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Who Should be Called to the Lab?

A comprehensive comparison of students and non-students in classic experimental games

Michèle Belot (Nuffield Centre for Experimental Social Sciences)

Luis Miller (Nuffield Centre for Experimental Social Sciences)

Raymond Duch (Nuffield Centre for Experimental Social Sciences)

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A comprehensive comparison of students and non-students in classic experimental games

Michele Belot, Raymond M. Duch, and Luis M. Miller

Centre for Experimental Social Sciences
Nuffield College, University of Oxford *

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Abstract

This study compares the behavior of students and non-students in a number of classic experimental games. We find that students are more likely to behave as homo-economicus agents than non-students in games involving other-regarding preferences (Dictator Game, Trust Game and Public Good Game). These differences persist even when controlling for demographics, cognitive ability and risk preferences. In games that do not engage other-regarding preferences (Beauty-contest and Second-price Auction) there is limited evidence of differences in behaviour between subject pools. In none of the five games is there evidence of significant differences in comprehension between students and non-students. Within subject analyses indicate that students are highly consistent in their other-regarding preferences while non-student subjects are inconsistent across other-regarding games. Our findings suggest that experiments using students will provide a lower bound estimate of other-regardedness in the general population while exaggerating the stability of other-regarding preferences.

Keywords: lab experiments, convenience samples, other-regarding preferences, consistency
JEL classification: C72, C81, C91.

*Corresponding Author is Raymond Duch, Nuffield College, Oxford United Kingdom OX1 1NF. E-mail: raymond.duch@nuffield.ox.ac.uk. Telephone: 44 (0)1865 278515 Fax: 44 (0)1865 278621

1 Introduction

Are students so different? Most subject pools in experiments conducted in social sciences are drawn from undergraduate student populations (Morton and Williams, 2009; Peterson, 2001). Based on a survey of 60 laboratory experimental economics papers published in major experimental economic journals, Danielson and Holm (2007) found that only four did not use students as subjects. Reliance on student subject pools is a potential concern because some recent studies suggest systematic differences in the experimental behaviour of students versus non-student subjects (Carpenter, Burks and Verhoogen, 2005; Carpenter, Connolly and Myers, 2008). A comprehensive second-order meta-analysis of social science experiments conducted by Peterson (2001) suggests that student samples tend to be much more homogeneous than non-student samples and that the treatment effect sizes differ significantly across these two types of samples.[1]

How does the behaviour of students and non-students differ? There are two stylized facts that emerge from experimental work comparing student and non-student subject pools. First, students are more likely than non-students to resemble the homo economicus in other-regarding games. Carpenter, Burks and Verhoogen (2005) compares students and workers in both a laboratory and field setting and finds that workers were more generous in the Dictator Game while students made more generous offers in the Ultimatum Game. A comparison of contributions of students versus community members in a charitable donation version of the Dictator Game found that students were much less generous (Carpenter, Connolly and Myers, 2008).

Second, in experiments with more complex game forms, that do not engage other-regarding preferences, there is less evidence of differences in equilibrium play between student and non-student subject pools. A classic study is the comparison of newspaper and lab Beauty-contest experiments (Bosch-Domenech et al., 2002) which concludes that the lab and field generate comparable results. Depositario et al. (2009) find no difference in the bidding behaviour of students and non-students in a uniform price auction.

[1] There is, though, some limited evidence that the experimental behaviour of student and non-student subject pools is similar (Ball and Chech, 1996; Egas and Riedl, 2008).

Hence there is evidence that the behaviour of student and non-student subject pools differ. And casual observation suggests that these subject pool differences might vary according to the type of experiments concerned. We have implemented a set of experiments designed to calibrate the overall magnitude of these subject pool differences and to assess whether, and how, these differences vary across types of games. The design allows us not only to explore whether game type conditions subject pool differences in homo-economicus behaviour but also whether a number of other factors might help to explain these differences.

We assess, and explain, subject pool effects by comparing systematic differences in equilibrium play between students and non-students. Deviation from equilibrium play of course has been widely observed in lab experiments (Berg, Dickhaut and McCabe, 1995; Fehr, Kirchsteiger and Riedl, 1993; Guth, Schmittberger and Schwarze, 1982). Our experiments compare equilibrium play across a range of experiments, employing a within-subject design. The results confirm that these deviations are large. But of particular interest here is that the magnitudes of these deviations vary substantially across student and non-student subject pools.

We test five possible explanations for these subject pool differences: demographic composition; risk preferences, other-regarding preferences; cognitive abilities; and design effects. An obvious point of departure for explaining student versus non-student differences in equilibrium play is simply the demographic composition of the subject-pools. The subject pools are in fact quite distinct on demographic characteristics that one might expect to affect behaviour (gender, intelligence, and age). But we find that these covariates contribute little to our understanding of subject pool differences in equilibrium choices. Moreover, in some cases the distinctiveness of the covariate distributions across subject pools suggests that controlling on these variables in multi-variate analyses of lab experiment data is an inappropriate strategy for addressing subject pool differences.

Risk aversion varies significantly within both subject pools. Approximately a third of our respondents exhibited inconsistent responses on the risk aversion measure which limited the analyses incorporating this variable. With that caveat in mind, we find no evidence that subject pool differences can be explained by differences in risk aversion.

By varying the types of experimental games that each subject plays, we explore whether variations in preferences or cognitive skills across subject pools might result in differential equilibrium behaviour. The subjects all play the identical five games; some of which are primarily other-regarding (Trust Game, Dicator Game, and Public Goods Game) and others which have game forms that are strategically challenging (Beauty-contest and Second-price Auction). Hence we can observe the behaviour of the same subjects over very different types of games. Our results suggest that differences in other-regarding preferences most certainly account for the significantly higher levels of equilibrium behaviour by students in games that engage other-regarding preferences. The fact that the differences across other-regarding games are most dramatic for the simplest of games (the Dictator Game) would seem to rule out other explanations such as comprehension.

The within subject design allows us to explore whether decisions are consistently other-regarding (or consistently non-other-regarding). Across other-regarding games, student choices exhibit high levels of consistent homo-economicus preferences. On the other hand, while non-students are highly generous in other-regarding games, their choices suggest much more inconsistent preferences.

Our experiments provide little evidence that variations in equilibrium behaviour can be accounted for by differential levels of strategic reasoning abilities (or cognitive skills) in student versus non-student subject pools. Levels of non-equilibrium choice are much lower in the case of our two games with strategically challenging game forms. At the same time, though, we find much weaker evidence of student versus non-student differences in levels of non-equilibrium behaviour in these strategic reasoning games. If strategic reasoning abilities were an explanation here we would expect large differences across student/non-student subject pools or across education and IQ groupings but this was not the case. Hence, comprehension of the game form is a much more likely explanation for the non-equilibrium choices that are made in our two games with strategically challenging game forms.

Accordingly we explore whether design effects explain non-equilibrium behaviour in our five experiments. Some subject types may simply not understand the game form of the experiment. Or there might be features of the experimental context that confound the treatment effects. "House money effects" or the contrived and abstract nature of the lab environment might trigger non-equilibrium behaviour. And if in fact these design effects are important, we would expect them to be more salient for non-student as opposed to student subjects. For example, non-students might be more sensitive to "house money effects" associated with monetary incentives; or students might perform better in cognitively challenging experiments because they have a better understanding of the game form (Chou et al., 2009).

We implement two treatments in order to test for design effects. First, we conduct a repeated public goods game and find that learning (which one might expect would reduce differential comprehension rates) does not significantly reduce the differences in equilibrium behaviour by the two subject pools. Second, we implement a non-obtrusive test of cooperative behaviour that takes place outside of the, potentially contrived and abstract, lab environment. Again, differences in other-regarding behaviour persist between subject pools. Hence, neither result suggests that our subject pool differences are an artifact of the experimental design.

2 Experimental design

To explore these issues we exploited the opening of the new lab at the Nuffield Centre for Experimental Social Sciences in Oxford. This was the first experiment carried out in the lab. Hence, the subjects have not yet been exposed to experiments carried out in the lab. Accordingly, past participation in CESS lab experiments or information about CESS experiments cannot explain outcomes in this experiment. Subjects may have a prior about experiments based on past experiences or information they have about experiments in social sciences. But, because the lab is new, it does not have a reputation and hence subjects cannot know, even through their peers, what types of experiments are carried out in this lab. Moreover, since we are comparing subject decisions across a number of classic experimental economic games, we rule out the potential differential effect of previous experimental participation.

At the time of the experiment, the CESS subject pool consisted of 1,000 students and non-students: 75 percent are students and 57 percent are females. Half of the students are freshmen and they come from more than 30 different disciplines. Half of the non-students are private employees and there is also a significant number of workers, self-employees, public employees, and unemployed. Student and non-student subjects were told they could earn on average between £10 and £15 per hour. The typical gross pay of students working for Oxford university is £12, and the average salary of an administrator in the UK in 2008 was £16,994, which corresponds to an hourly rate of £8.5.[2]

2.1 Experimental set-up

Table 1 describes the six sessions of the experiment; two with students, two with non-students and two with a mixed population (a total of 128 subjects). All sessions were conducted at 5.30 or 6 pm. Each session lasted for about one hour and a half. Upon arrival, subjects were assigned randomly to a desk in the lab. We read out the instructions.[3] We presented the experiment as consisting of two parts. The first part involved 6 different situations, which we asked subjects to treat independently of each other. We read the instructions for the first situation; subjects were instructed to take a decision after we read the instructions. We then moved on to the second situation and so on. Subjects received no feedback about their payoffs in each of these situations until all six situations were completed (except in the last situation, which featured a repeated interaction). Earnings for this part were determined by selecting one of the six situations randomly (subjects were informed of the payment scheme at the very beginning). The second part of the experiment was an IQ test consisting of 26 questions. Subjects received £0.20 for each correct answer.[4]

The situations were presented in the following sequence for all subjects (identified only by their number): 1) Trust game (TG), 2) Guessing game (GG), 3) Dictator game (DG), 4) Second price sealed bid auction, 5) Elicitation of risk preferences, 6) Repeated public good game.

[2]Source: http://www.mysalary.co.uk/

[3]The instructions can be found in appendix B.

[4]We separated the experiment into two parts because they were fundamentally different. The first part, experimental games, was administered using z-Tree (Fischbacher, 2007) and different situations involved a combination of one's own decisions, others' decisions and chance. The second part, the IQ test, was administered using a different program (MS Access) and it did not involve strategic interaction nor chance moves. In this latter situation, participants' payoffs depended only on their knowledge and effort.

Session	1	2	3	4	5	6
Population	Student	Mixed	Mixed	Non-student	Student	Non-student
No. of subjects	20	24	24	24	20	16
Date	19/02/09	20/02/09	23/02/09	24/02/09	25/02/09	27/02/09
Av. earnings	£7.9	£8.7	£18.5	£8.4	£8.1	£12.2
Situation paid	Auction	Guessing	Trust	Guessing	Auction	Risk

Table 1: Session characteristics

2.2 Description of the games

1. **Dictator game:** We conducted a single-blind two-person dictator game. Random pairs of subjects were formed and assigned the role of 'sender' or 'receiver' .[5] The sender was told that she had been allocated £10 and was asked to indicate the amount she wished to transfer to the receiver.

2. **Trust game:** We conducted a two-person binary trust game. Random pairs of subjects were formed and assigned the role of 'sender' and 'receiver' . The sender received £10 and had to choose whether to transfer them to the receiver or to keep them. The money was then tripled by the experimenter and the receiver had to decide whether to keep the resulting £30 or split them evenly (£15-£15).

3. **Guessing game:** All participants in one session took part in the same guessing game. They had to guess the closest number to two-thirds of the average guess. The winner won £20, the rest nothing. In the event of ties, one of the winners was selected randomly.

4. **Repeated public good game:** Our public good game uses a standard Voluntary Contribution Mechanism (VCM) whereby the amount of public good produced is determined by the total contribution in the group (Ledyard, 1995). The game has a unique equilibrium of full free-riding (dominant strategy in the one-shot game, unique Nash in the finitely repeated game). Our experimental environment mirrors those of previous experiments. Four subjects are endowed with 20 tokens. A subject

[5]Both in the dictator game and the trust game we used the labels 'person 1' and 'person 2' for senders and receivers respectively.

can either keep these tokens for herself or invest them into a so-called ‘project’ by contributing g_i. The payoff function was the following:

$$\pi_i = 20 - g_i + 0.4 \sum_{j=1}^{4} g_i$$

The size of the project is just given by the sum of all contributions g_i to it. The marginal payoff of a contribution to the public good is 0.4 tokens.

5. **Second price sealed bid auction:** We conducted a second-price auction with all the subjects in one session bidding for a single unit of a commodity under a sealed-bid procedure. The high bidder paid the price of the second highest bidder and earned profit equal to her valuation less the price paid. Other bidders earned zero profit. In the event of ties, one of the winners was selected randomly. Private valuations were assigned randomly with one-fourth of the subjects bidding for £4 and a similar proportion bidding for £6, £8, and £10.

2.3 Experimental Treatments

In each session, each subject played five different games. Three of our games invoke other regarding preferences: the Dictator Game (non strategic and one-shot); the Trust Game (strategic and one-shot); and the Public Good Game (strategic and repeated). We can evaluate learning in the Public Good Game because it is repeated. Two of our games involve cognitive capabilities and no other-regarding preferences: Beauty-contest (or Guessing Game); and the Second-price sealed bid auction.

Our analyses concentrate on the Nash equilibrium strategy based on monetary payoffs -the homo-economicus equilibrium strategy, assuming selfishness and rationality and common knowledge of rationality. For the Guessing Game, in which the Nash equilibrium is for subjects to report 0, this is not very informative because only one subject does so in our sample. Alternatively, it is common for this game (Camerer, Ho and Chong, 2002) to define equilibrium strategies according to depths of levels of reasoning. Level 0 subjects are non-sophisticated subjects and simply pick a number at random. Level 1 subjects best respond to Level 0 subjects whose random picks will average around 50, thus report a guess equal to 2/3 of 50. Level 2 subjects best respond to Level 1 subjects, thus report a guess equal to $(2/3)^2$ of 50. More generally, the level of depth of reasoning, k, commands a best response equal to $50 * (2/3)^k$. We will consider the probability of reasoning of level 1 or more. The Nash equilibrium strategy for the other games are the following: Dictator Game – donate zero; Trust Game – send zero and send back zero; Public Goods Game – contribute zero. For the second-price sealed bid auction, the equilibrium is defined as bidding the private value or the private value +0.1£ (Kagel and Levin, 1993).

Our primary concern is comparing behaviour between student and non-student subject pools. Accordingly, two sessions include only students and two consist only of non-students. To test whether behavior is conditional on the characteristics of the subjects one is playing with, we supplemented these four homogeneous sessions with two heterogeneous sessions with student and non-student subjects.

2.4 Individual-level covariates

We will describe variations in student and non-student characteristics and their implication for the analysis of experimental data from different types of subject pools. Accordingly we have information on the subjects' age, gender, ethnicity and professional status.

We have two measures of cognitive ability. First, in the case of each experiment, we asked them to describe an example of a possible outcome, immediately after the instructions were read. The goal was to obtain a measure of understanding without priming them in any particular way. Second, subjects took a short 12-minute version of an IQ test, consisting of 4 components: Numerical computation (calculation), numerical reasoning (e.g. logical series), abstract reasoning (figures and series) and verbal ability (e.g. analogies). Subjects had a limited amount of time for each component (2/3 minutes), and they received £0.20 for each correct answer.[6]

[6]We selected 26 questions in total from an on-line psychometric test battery: www.psychometric-success.com. We sampled the questions according to their levels of difficulty, including very easy, easy and more difficult questions. The level of difficulty is typically increasing in such tests, so we selected questions from the beginning, the middle and the end, to ensure we could capture well differences in cognitive ability across subjects.

We also elicit risk preferences in the first part of the experiment (situation 5). Since we expect non-students to be quite heterogeneous in their mathematical ability, we chose a simple strategy method to elicit risk preferences (Chetan, Eckel and Rojas, 2010). They are presented with eight successive choices between a safe amount (£5) and a lottery. The lottery gave them a 50% chance of earning a positive amount (which decreased by £1 in each case from £14 in the first case to £7 in the last case) and a 50% chance earning nothing. We told them that we would select one of the cases randomly to determine the payoff for this situation.

3 Experimental results

3.1 Covariate differences across subject pools

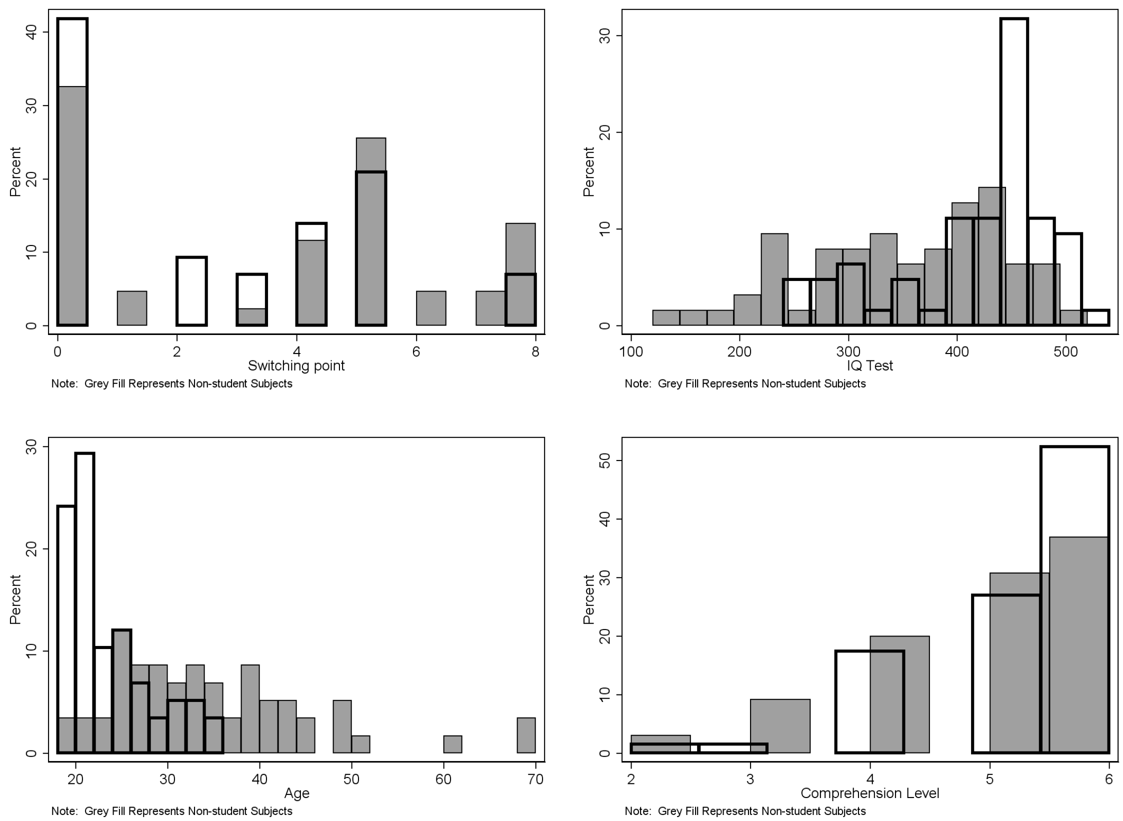
The socio-demographic composition of student and non-student subject pools may account for differences in subject pool behaviour. In our multivariate analysis we explore whether co-variate controls account for behavioural differences across subject pools. But the power of these controls is very much contingent upon the range of values these variables assume within each of the subject pools. To the extent that there is restricted variance in the covariates, our multivariate results could be highly sensitive to model specification (Gelman and Hill, 2007). A key requirement in introducing these controls is that the range of values on the covariates be similar across the convenience samples (or treatment groups) that are being compared – in our case the student and non-student subjects (Rubin, 1978). Hence, we are concerned not simply with comparing measures of central tendency but also with whether the range of values on the covariates for our student subjects is similar to that of the non-student subject pool.

The degree of consistency in responses to the risk items is low given the simplicity of the question: About a third of the subjects (in both pools) switch back and forth (at least once) between the 'safe' and the 'risky' alternative.[7] Figure 1a presents the distribution of risk

[7]Non-unique switching points is not uncommon in these types of experiments – Holt and Laury (2002) elicit risk preferences in a similar fashion and report 19.9 percent of the subjects exhibited non-unique switching behviour.

attitudes among those who remain consistent across the eight different cases. Non-students tend to be less risk averse on average. More students are at the zero point, rejecting any uncertainty in terms of the pay-offs. Nevertheless, encouragingly, we get a similar range of values across both the student and non-student subjects.

Figure 1: Distribution of Covariance Variables: Student and Non-Student Subjects



Students have a much higher average IQ score (350 for non-students and 415 for students) which is not entirely surprising. More interestingly, Figure 1b indicates that there is a considerable range of IQ scores for which there is no overlap between students and non-students. While non-students have a range of IQ scores that go from just above 100 to 500, student IQ scores range from about 250 to 500.

Virtually by definition the non-student subject pool will be older than the student subjects (34 against 23 years old). Figure 1c presents the age distribution in the two subject pools. And while hardly surprising, the plots indicate that there is a significant age range for which there is no overlap in the non-student and student subject pools. The age range between 35 and 70, which exists in the non-student subject pool, does not exist in the student sample.

Finally, we measure comprehension by tallying each of our experimental checks to come up with a total comprehension score that ranges between zero and six. On average, comprehension is higher amongst student subjects (5.3) than non-student subjects (4.9). Figure 1d presents the comprehension distribution overlays for student and non-student subjects. Again there are regions of the comprehension distribution for which students effectively have no observations – i.e., the very low comprehension levels. This reinforces a widely held belief that aggregate comprehension of the game form may be higher in student subject pools.

As we expected students and non-students have different average covariate values. A more noteworthy finding is that for a number of the covariates there are ranges of values for which there are no overlap for students and non-students. This is the case for IQ, age and comprehension. Controlling on these covariates in a conventional multi-variate model will generate results that are sensitive to model specification because the fitted model is forced to extrapolate beyond the support of the data. Hence, in the case of this particular student/non-student subject pool, controlling on IQ, age and (or) comprehension may not provide meaningful insights about treatment effects because there are observations for which we have no empirical counterfactuals (for example, we do not have an opportunity to observe whether low IQ students are more other-regarding).[8] More generally, this suggests

[8]There are of course strategies for dealing with the problem of covariate imbalance, such as matching,

caution in introducing control variables when estimating treatment effects on experimental data that are generated from convenience samples which frequently are imbalanced on the covariates of interest.

3.2 Aggregate differences in game behaviour across subject pools

We begin by comparing student versus non-student behaviour for each of the five games. Table 2 reports the percentage of subjects playing the homo-economicus equilibrium strategy, that is the equilibrium strategy if the subject is rational and selfish (cares about his own monetary payoffs). The differences between students and non-students are striking: In all games with the exception of the auction game, we find that students are more likely to behave as a homo-economicus agent.[9]

Game	(Non-students)	(Students)	Mann-Whitney test of equality (p-value)
Dictator	17	57	.00
Trust - sender	18	65	.00
Trust - receiver	13	44	.01
Public Good	9	24	.03
Guessing	32	56	.00
Auction	26	37	.21

Table 2: Percentage playing the homo-economicus equilibrium strategy

In general our dictator game results are in line with previous literature. Dictator games in which the recipients are anonymous result in average donations that fall between 10 and 15 percent (Hoffman and Smith, 1994; Hoffman, McCabe and Smith, 1996; Eckel and Grossman, 1996). The average donation for our student sample was 16 percent which is similar to these other findings (which, again, are typically based on student subject pools). Fifty-seven percent of our student samples donated nothing which again is consistent with other findings – Eckel and Grossman (1996), for example, find that 63 percent of their

although these have been developed with a particular emphasis on observational and field experiment data (Heckman, Ichimura and Todd, 1997; Rubin, 1973).

[9] Independent analyses for each game are shown in appendix A.

student subject pool donate nothing when the recipient is anonymous. Seventeen percent of our non-students donated nothing which is less than a third of the student subject pool result. Duch and Palmer (2004) similarly find a reluctance of general population samples to make no donation – in their representative sample from Benin only eight percent made this choice. The average donation of our non-students is 35 percent of the maximum; about twice the average donation of the students. This 2-1 non-student-student ratio is roughly the difference in donations that Carpenter, Burks and Verhoogen (2005) find between Middlebury students and Kansas City workers.

Our binary trust game results are also consistent with other studies. Subjects in trust experiments typically invest and reciprocate. Relatively small percentages of trustors give nothing: In their base-line treatment Berg, Dickhaut and McCabe (1995) find that 90 percent of first movers invest; in a similar trust experiment Ortmann, Fitzgerald and Boeing (2000) and Eckel and Wilson (2004) find that 80 percent of first movers invest. In a binary trust experiment very similar to ours, Casari and Cason (2009) report that 73 percent of their student subjects (designated as trustors) trusted by investing. Our non-student participants behaviour resemble these results: 82 percent of first movers were trusting. But our student subjects are clear outliers; only 35 percent of our student trustors trust.

Rates of reciprocation in binary trust games typically are in the range of 50-60 percent. Casari and Cason (2009), for example, report a rate of reciprocation of 60 percent for their student subjects. Fifty-six percent of our non-student trustees reciprocate while 87 percent of non-student second-movers reciprocated. In the case of reciprocation we find that our students are roughly consistent with reciprocation results in other studies while our non-student subjects are very much a positive outlier.

Our third other-regarding game is the public good game – this is the one game that involved repeated play (10 rounds). As Table 2 indicates, students are considerably more likely to behave in the homo economicus fashion by free-riding: in the first round of the public goods game, 24 percent of the students contributed nothing to the public good while only 9 percent of non-students were strictly non-cooperative. Gachter, Herrmann and Thoni (2004) conduct a similar, although one-shot, public good game with students and non-students. Their results suggest, consistent with ours, that non-students are more cooperative than students: non-students contributed an average of 10.2 tokens (out of 20) while students contributed 8.8 (Gachter, Herrmann and Thoni, 2004). The average contribution of our non-students in the first round was 11.1 (out of 20) compared to 9.9 for students; in the last round the average contributions, respectively, were 5.4 and 4.8.[10] On balance the patterns in both studies are similar and provide quite convincing evidence that students are less cooperative than non-student subject pools.

All three of our experiments involving other-regarding preferences demonstrate, consistent with virtually all of the experimental literature, that subjects, in general, are more trusting and more cooperative than classic theory would predict. And of course there have been considerable efforts to incorporate this "kindness" into models of economic behaviour (Fehr and Schmidt, 1999). The three empirical results presented above leave little doubt that students are dramatically more non-cooperative and less trusting than non-student subjects. Why is this the case? Andreoni (1995) correctly directs our attention to two contributing explanations for cooperation: confusion and kindness.

Comparing these other-regarding games in terms of their complexity suggests that the differences across subject pools are not simply the result of confusion. The dictator game is extremely simple; the trust game is somewhat more complex in that first movers should consider the likely behaviour of second movers; and finally the public good game requires more complex strategic calculations related to collective action and free riding. But there is no evidence that non-students are less cooperative in the very simple, as opposed to more complex, other-regarding games. In fact, Table 2 suggests that, very roughly, the ratio of non-cooperative students to non-cooperative non-students is a reasonably constant 3 to 1 for all three games. Second, confusion implies that over the course of repeated games non-students learn at a slower rate than students. But the learning results from the public goods game suggest no difference in the rate of learning by students and non-students. Hence, the student/non-student differences in our other-regarding games suggest that non-students, rather than being confused, are simply "kinder" than students which results in higher levels of trust and cooperation.

[10] The differences between their results and ours might be associated with the fact that theirs is a one-shot game and ours is a repeated public goods game. Although one might speculate that this would increase the free-riding in the Gachter, Herrmann and Thoni (2004) experiment.

Our two other games – the Auction and Beauty-contest games – are a more direct test of whether there are systematic differences in the student/non-student comprehension and reasoning abilities. They involve no other-regarding preferences and are distinctly more cognitively challenging than the other three games. In these games students are more likely to make the homo-economicus equilibrium choice; but the magnitude of these differences is much smaller than it was for other-regarding games. Table 2 suggests that for the Beauty-contest the ratio of student equilibrium choices to non-students equilibrium choices is roughly 2 to 1 compared to the 3-1 ratio for the other regarding games. And for the second-price sealed bid auction game the ratio is even smaller 1.5 to 1.

The unique equilibrium for the Beauty-contest is 0, yet typically the percentage of subjects choosing 0 in experiments is less than 10 percent (Camerer, 1997; Nagel, 1995) – only one non-student subject in our experiment selected 0. On balance, students exhibit higher levels of iterative reasoning although the differences are less stark than with other-regarding games. Thirty percent of our non-student subjects made choices consistent with at least level 1 iterative reasoning compared to 56 percent for the student subjects. Although in the case of, roughly, level 2 iterative reasoning, students and non-students are more similar (21 and 15 percent, respectively).

The average guess of our student subjects is 38 which is consistent with similar guessing games results (in which the average is multiplied by two-thirds). Nagel (1995) finds that the average guess is 35. Camerer (1997) conducted multiple Beauty-contest experiments (with a multiple of .70 applied to the mean) and found the average guesses, for mostly student subject pools, ranged between 31 and 40. Our non-student subject pool exhibited somewhat less iterative rationality with an average guess of 46.

In a second-price sealed bid auction, individuals should bid their private value (Kagel and Levin, 1993). Thirty-seven percent of our student subjects bid their private value. Again this is consistent with other experimental results; Kagel and Levin (1993) find that 37 percent of students bid their private value in a second-price sealed bid auction. Our non-students are somewhat less likely to behave rationally and bid their private value – 26 percent of non-students behave in this fashion. But these differences are not statistically different. And an analysis of average bids for students and non-students (see Table 11 in Appendix A) suggests that both students and non-students on average have bids that approximate their private value. Hence, for this particular game form there is essentially no difference in student and non-student behaviour.

We included strategic reasoning games in our experiment to explore whether variations in strategic reasoning skills across convenience samples might lead to different results in these games. There is little evidence here that student subjects exhibit more developed strategic reasoning skills than the non-students. For the strategic reasoning games, students are only weakly more likely than non-students to behave in a homo-economicus fashion in the case of the Beauty-contest; and for the auction game they have the same levels of equilibrium behaviour. Particularly given the very large student/non-student differences we saw in the case of other-regarding games, it seems reasonable to conclude that in the case of strategic reasoning games students are only marginally more homo-economicus than non-student subjects.

There is some evidence here that cognitively demanding game forms significantly reduce equilibrium behaviour. Both students and non-students are less likely to make equilibrium choices in the Public Good game than in the other two other-regarding games. One could attribute this to the complexity of the game form. On the other hand we note that students exhibit levels of equilibrium play in the two strategic reasoning games that are not very different than in the other-regarding games. And for non-students, levels of equilibrium play are clearly higher in the cognitively demanding games but of course its difficult to draw any conclusions here because of the confounding affect of non-student other-regardedness. We defer to the multi-variate analyses in order to tease out the independent impact of complexity of game form on equilibrium behaviour.

3.3 Within-subject Behaviour Across Games

The frequency with which lab experiment subjects exhibit non-equilibrium behaviour has lead some to conclude that there are "types" in the population that correspond to some version of "egoists" and "non-egoists" (Bolton and Ockenfels, 2000; Bowles and Gintis, 2004; Habyarimana et al., 2009). For the most part the evidence regarding types has focused on other regarding preferences and it has relied on student subject pools. Our experiment allows us to address a more general question: Are there types in the population, or at least in our convenience samples, that consistently play homo-economicus in both other-regarding games and in games that do not engage other-regarding preferences? And in the case of both of these behavioural types, our experiments allow us to determine whether we are more, or less, likely to identify these types if we rely on a student versus non-student subject pool.

The stark contrasts between student and non-student behaviour suggest that homo-economicus and non-homo-economicus types will be concentrated in the student and non-student subject pools respectively. Homo-economicus types are subjects that consistently make equilibrium choices across the five experimental games. There are no non-students who are pure homo-economicus and in fact only four non-students make more than two equilibrium choices. But even amongst the student subjects, there is only one subject that makes all five equilibrium choices; and only 19 students (about a third of the student subjects) make more than two equilibrium choices. This result is driven largely by the fact that there are few student (or non-student) subjects that are consistently homo-economicus in the two games that are relatively cognitively demanding and do not engage other regarding preferences. There is no compelling evidence here of a significant group of consistent homo-economicus individuals in the subject pools.

There are distinct other-regarding types although their behaviour differs by subject pool. If we define other-regarding types simply in terms of whether they never (or always) play equilibrium in other-regarding games then the non-student subject pool is unquestionably other-regarding: over 70 percent of non-student subjects selected none of the homo-economicus choices in the three other-regarding experiments (and none play equilibrium). The student subjects are somewhat less distinct on this count: 35 percent of students never play equilibrium while 10 percent always give zero.

An even stronger claim, again based primarily on other-regarding games played by student subjects, is that other-regarding preferences conform to axioms of revealed preference. Theories of other-regarding preferences presume that inequity averse parameter estimates can be recovered from revealed preferences which assume consistent within-subject choices across other-regarding experiments, where choices are defined in terms of degrees of inequity aversion (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999). Andreoni and Miller (2002) find that subjects' choices across variants of the same game are consistent with the axioms of revealed preferences although the results of Blanco, Engelmann and Normann (2010) are less consistently supportive for subjects playing different other-regarding games.

We can assess the consistency of subject choices, across different types of other-regarding games, by correlating the amount subjects give across the three other-regarding games; and, of course, we can compare consistency of students and non-students. For students, the amount given in the dictator game has a .47 correlation with the trustor's contribution in the binary trust game (and only .05 for non-students); the dictator amount has a .46 correlation with the Public Goods Game contribution for students (-.13 for non-students); and the PGG and trustor's contributions have a .19 correlation for students (.03 for non-students).

Non-student subjects are consistent in not playing equilibrium although the magnitudes of their contributions exhibit high degrees of inconsistency across other-regarding games. Students, on the other hand, are less likely to consistently play non-equilibrium; more likely to consistently make equilibrium choices; and exhibit high degrees of consistency in the magnitudes of their contributions. The differences in within-subject choices (across different types of other-regarding games) by non-students versus students suggest that experimental results concerning axioms of revealed preference may be conditioned by the nature of the subject pools selected.

3.4 Design Effects

Design effects result when treatment effects are confounded with how the theoretical treatment is implemented - whether respondents understood the game form or whether the lab environment itself affects behaviour. We examine whether design effects might account for differences in subject pool behaviour.

Subject comprehension of the game form is one feature of the experimental design that might account for differences across subject pool types. We argued that the student/non-student differences in other-regarding games suggest that these groups have significantly different levels of trust and inequity aversion. It seems unlikely that these differences could be attributed to comprehension. If the student/non-student differences for the other-regarding games were the result of comprehension we might expect these differences to erode with repeated play of other-regarding games. Our repeated play of the public goods game suggests that both students and non-students become more homo-economicus over the repeated plays but differences in cooperative behaviour persist. By the final play of the public goods game both our student and non-student subjects make highly non-cooperative decisions: 53 percent of students choose to give nothing (none chose the option of 1 token); 29 percent of non-students give nothing while 12 percent give 1. Students maintain their non-cooperative edge but it declines over the course of the repeated plays. Our student results closely tract the results of Andreoni (1995) who conducted similar 10-round public goods games with student subject pools: In the first round of his PGG experiment, 20 percent contribute nothing and in round 10, 45 percent contribute nothing (Andreoni, 1995). Figure 2 compares the non-cooperation in the Andreoni (1995) experiments with the evolution of non-cooperation by students and non-students in our experiment. Note our two subject pools behave similarly over the course of repeated plays in that their non-cooperative behaviour increases quite significantly. Moreover, both rates of increase in non-cooperation closely resemble the findings of Andreoni (1995).

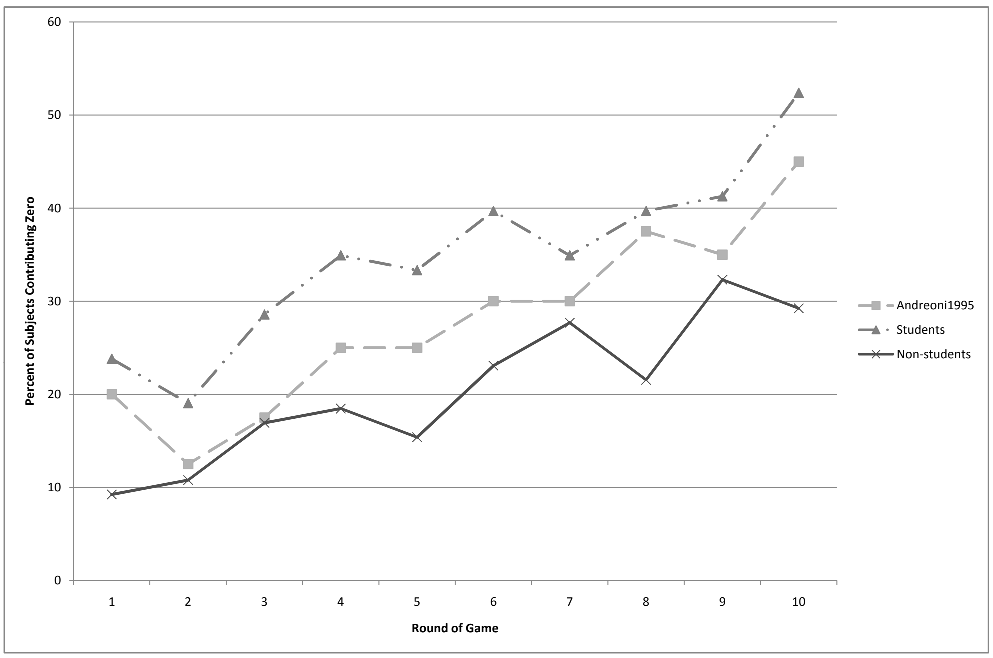


Figure 2: Percentage of Subjects Contributing Nothing by Round in Repeated Public Goods Game

Table 3 reports the results for a simple test for differences in learning behaviour. We model the probability of contributing zero as a function of the student subject pool dummy, the number of rounds, and the interaction of the student dummy and number of rounds. We find that there is a positive trend. The proportion of subjects contributing zero increases by roughly 2 percentage points in the early rounds and this rises to about 3.2 percent in the final rounds. But we fail to find any evidence of differences in trends between the two subject pools (the interaction term is indistinguishable from zero). Rates of learning by students and non-students are similar suggesting that both subject pools respond similarly, over iterative play, to the non-cooperative incentives of the game. This seems inconsistent with the notion that the two subject pools have differing levels of comprehension (or respond differently to the lab environment). Also, if the differences in other-regardedness we observed are simply the result of differential comprehension levels, we would expect them to diminish significantly over repeated play. The fact that differences in other-regardedness persist in spite of learning lends support for the notion that these two subject pools have different levels of inequity aversion and trust.

Probability of contributing 0 (probit estimates)	
Student dummy	0.16
	(0.08)
Number of rounds	0.03
	(0.007)
Student x Number of rounds	-0.002
	(0.01)
Observations	1280
Wald χ^2	49.13
Standard errors are clustered by subject	

Table 3: Learning in the public good game

Another possible explanation for these significant subject pool differences is that the lab context differentially triggers other-regardedness in non-students versus students. Levitt and List (2007*b*,*a*) suggest that the "nature and extent of scrutiny associated with the lab" might induce subjects to conform to social norms against wealth maximizing choices (Levitt and List, 2007*a*). Students might be more comfortable than non-students with the scrutiny associated with the lab environment and hence less sensitive to norms regarding selfish behaviour. This might explain lower levels of other-regardedness amongst students. We have an indirect measure of whether our student/non-student differences in other-regardedness are replicated outside of the laboratory environment.

Two weeks after the experiment, subjects received a thank-you e-mail that also asked them how they had been informed about the lab. This requests a form of cooperation that is only beneficial to us; is not personal in any way; and in principle involves little cost. This test enables us to obtain a different measure of other-regarding preferences outside the lab and in a natural context (this kind of e-mail is relatively common practice and there is no reason to expect differences in familiarity between students and non-students). Again, we find systematic differences between students and non-students. 53% of the students replied to the e-mail against 72% of the non-students (this difference is significant at the 2% level). Thus, the student/non-student differences we reported earlier do not seem to be an artefact of the laboratory setting.

3.5 Explaining differences in behaviour - A multivariate analysis

We now turn to a multivariate analysis of the difference in subject pool behaviour. Our primary concern is whether students differ from non-students; whether the differences can be attributed to preferences or cognitive reasoning abilities; whether these differences are conditional on covariates; and whether design effects, i.e., features of the experimental session, exaggerate or moderate these differences.

The subjects are effectively assigned to two experimental treatments: they are assigned to one of $j = 6$ sessions; and all subjects play each of the $k = 5$ games. In our estimations, the two treatments will be modeled as second-level characteristics in a two-level hierarchical model. Accordingly, these two, second-level treatments, result in $6 \times 5 = 30$ treatment cells. And of course the model is non-nested because subjects are not nested within a particular game treatment, i.e., they play each game.

Our central concern is in fact with individual-level variation within these cells – specifically, the gap in equilibrium behaviour between student and non-student subjects. This is captured by the following equation:

$$\log\left[\frac{\Pr(y_{ijk}=1)}{1-\Pr(y_{ijk}=1)}\right] = \beta_{0jk} + \beta_{1jk}P_{ijk} + \beta_2 Z_{ijk} \qquad (1)$$

where i indexes subjects; j indexes sessions; k indexes the type of game the subjects are playing; and y_{ijk} is a dichotomous variable assuming a value of 1 when the subject selects the homo-economicus predicted equilibrium behaviour. The estimate of interest is the mean probability of behaving according to the equilibrium prediction (recall y_{ijk} is a dichotomous variable). The dummy variable, P_{ijk}, indicates whether a subject is associated with the student subject pool and hence β_{1jk} is our estimate of the degree to which students respond differently from non-students to the treatments. Given the two treatment effects, we estimate for β_{0jk} and for β_{1jk} the random effects associated with the j sessions and k games:

$$\beta_{0jk} = \pi_0 + \eta_j + \zeta_k \qquad (2)$$

$$\beta_{1jk} = \theta_0 + \tau_j + \omega_k \qquad (3)$$

We also speculated that this non-student gap in equilibrium behaviour may be moderated by other individual-level characteristics. Accordingly, we include a moderating variable, Z_{ijk}, that potentially affects the magnitude of the student/non-student gap in equilibrium behaviour.

We explore four individual-level covariates that possibly explain differences in behaviour:

- *Cognitive ability:* Students and non-students may differ in the level of understanding of the instructions, the game, and how they derive the equilibrium strategy of a game. Cognitive ability matters in the understanding of the instructions and of the game, as well as in backward induction and strategic thinking. For instance, using a sample of 1,000 trainee truckers, Burks et al. (2009) find that an individual's cognitive skills are related to their preferences in different choice domains.

- *Demographics:* Students presumably are younger and come from a more homogeneous socio-economic background. Regarding age, there is a body of empirical literature suggesting that trust is positively correlated with age (Midlarsky, 1989; Yen, 2002). Sutter and Kocher (2007) find that trust increases almost linearly from early childhood to early adulthood, but stays rather constant between different adult age groups. Croson and Gneezy (2009) point out that the literature suggests gender differences in three areas: risk preferences, social preferences and competitive behavior. Gender and intelligence have been correlated with overbidding in Common Value Auctions (Casari, Ham and Kagel, 2007).

- *Risk Attitudes:* A large class of games in economics involve risk, and differences in behavior in these games could therefore be due to differences in risk attitudes. As we pointed out earlier only 86 of the 128 subjects report consistent results on the risk preference game – including this variable in the multivariate analysis significantly reduces the number of observations in the multivariate analysis because we treat the inconsistent respondents as missing values. The risk measure, for consistent respondents, ranges in value from -4 for those who always select the lottery to +4 for those who never select the lottery. The zero value is assigned to those who switch to the certain outcome when the certain outcome and the expected value of the lottery are equal.

The β_2 coefficient captures the effect of Z_{ijk} on subject choices in these games.[11] To the extent that the covariates are moderating the treatment effect, their coefficients should account for much of the differences across subject pools, reducing the magnitude of β_1 in particular.

The estimates for Equation 1 are reported in Model 1 of Table 4. Our hypothesized student effect here is only weakly significant: while the coefficient on the student dummy variable indicates that students are more likely than non-student subjects to play the equilibrium

[11] Z_{ijk} of course could be a vector of covariates – to simplify this exposition we treat it as a single variable.

homo-economicus strategy, the estimate is quite imprecise. The standard deviation for ω_k, which is the random component of the student effect associated with the game treatment, suggests why this is the case. Note that the standard deviation for ω_k is about 1.0 which is almost twice the standard deviation of the fixed student effect (.54). This suggests that the model is not capturing variations in the student effect that are associated with features of the game treatments. This is not particularly surprising since we know from our earlier discussion that student effects appear to be conditioned on game type.

Model 2 in Table 4 incorporates the individual-level covariates.[12] The results from adding these particular covariates are conclusive: the student/non-student differences are not conditioned on the set of covariates available for our subject pools.[13] The IQ test, comprehension, age and gender variables are all statistically insignificant in this model and they remain insignificant in subsequent model specifications. Note that the variance terms capturing the random effects associated with sessions and games essentially remain the same. Not surprisingly, the likelihood-ratio test and Wald test of the joint significance of the four covariates both indicate their statistical insignificance.

[12] The risk preference covariate is treated separately below.

[13] A reasonable argument might be that our estimates of the effects of introducing these covariate controls are unreliable because of the problems noted earlier regarding the non-overlapping of covariate values for student and non-student subjects. But we performed analyses that only included the cases that had overlapping values and our conclusions were similar.

Table 4: Generalized Linear Mixed Model Results

	Model 1	Model 2	Model 3	Model 4	Model 5
Student	0.9532	1.15	0.90	2.82	3.04
	(.54)	(.57)	(.59)	(.72)	(.89)
Hetero			.41	.42	.76
			(.41)	(.41)	(.51)
Student X Hetero			.36	.35	.03
			(.52)	(.52)	(.65)
Game Type				-0.48	-.47
				(.55)	(.54)
Student X Game Type				-.84	-0.73
				(.31)	(.36)
IQ Test		0.001	0.001	.001	0.0003
		(.002)	(.002)	(.002)	(.002)
Age		-.003	-0.003	-.003	0.01
		(.02)	(.02)	(.02)	(.02)
Gender		-.33	-0.32	-.65	-.51
		(.25)	(.25)	(.25)	(.31)
Risk Averse					0.04
					(.06)
Comprehension		-.74	-.81	-0.65	-0.33
		(.36)	(.41)	(.36)	(.43)
Intercept	-2.17	-1.75	-1.97	-1.12	-1.71
	(.49)	(1.18)	(1.17)	(1.62)	(1.85)
StDev η_j	.23	.25	.00	.07	.09
StDev ζ_k	.97	1.05	1.03	.98	.91
StDev τ_j	.04	.15	.00	.00	.07
StDev ω_k	1.03	.93	.93	.21	.14
Observations	576	506	506	506	346

One of our second-level treatment effects is designed to estimate whether the student/non-student gap in equilibrium behaviour is affected by the homogeneity of the session. For example, other-regarding preferences might be undermined in homogeneous contexts because subjects are less inhibited to play competitively (Palacios-Huerta and Volij, 2009). Hence, student/non-student differences in equilibrium behaviour may be minimized in homogeneous experimental sessions. Accordingly, the session treatment (T_j) in our model assumes two values: a homogeneous treatment in which subjects play with their own "type" (either students or non-students); and a heterogenous treatment in which the subjects are both students and non-students. Our expectation was that the student effect will vary by session type although the random effects associated with this treatment suggest this is probably not the case.

Features of game-type, the other second-level treatment in our design, are also hypothesized to condition the student effect. The subjects in our experiment play five different experimental games that vary in terms of whether they are cognitively challenging or other-regarding. To measure this feature of game type we define G_k as a trichotomous variable indicating whether the game was other-directing; both cognitively challenging and other-directing; or cognitively challenging.

Our student gap in equilibrium behaviour, captured by the coefficient β_{1jk}, will vary as a function of our two second-level experimental treatments. The second-level equation that incorporates these two treatment effects is:

$$\beta_{1jk} = \theta_0 + \theta_1 T_j + \theta_2 G_k + \tau_j + \omega_k \quad (4)$$

We would also incorporate fixed effects associated with the two second-level experimental treatments, i.e., the notion that the probability of equilibrium behaviour varies systematically as a function of each of the two treatments:

$$\beta_{0jk} = \pi_0 + \pi_1 T_j + \pi_2 G_k + \eta_j + \zeta_k \quad (5)$$

The full model with substitution is the following:

$$\log\left[\frac{\Pr(y_{ijk}=1)}{1-\Pr(y_{ijk}=1)}\right] = [\pi_0+\pi_1 T_j+\pi_2 G_k+\eta_j+\zeta_k]+[\theta_0+\theta_1 T_j+\theta_2 G_k+\tau_j+\omega_k]P_{ijk}+\beta_2 Z_{ijk} \tag{6}$$

Of particular interest to us in Equation 6 is θ_0, which is an estimate of the general student/non-student gap in equilibrium behviour, and the θ_1 and θ_2 coefficients that indicate the extent to which this effect is conditioned on the two experimental treatments.[14]

Model 3 in Table 4 incorporates our first second-level treatment effect. The model estimates the π_1 and θ_1 terms associated with the T_j treatment: a dummy variable for students and non-students sessions and its interaction with the Student effect dummy variable. As expected from the random effects results earlier, the coefficients on both variables are not statistically significant suggesting there is no strong evidence here that equilibrium behaviour, or the student/non-student gap in equilibrium behaviour, is affected by the homogeneity/heterogeneity treatment effects.

Model 4 adds estimates for π_2 and θ_2 which are associated with the Game Type treatment (G_k). Note that the student effect in this model, captured by θ_0, is now highly significant. The student-game interaction term, (θ_2), is also statistically significant suggesting that the student effect is exaggerated in games that are other-regarding. Students generally exhibit more homo-economicus behaviour than non-students but they are particularly homo-economicus when they play games that are inherently other-regarding. Finally note that, when we explicitly model the impact of game treatment on the student effect,the standard deviation on the random effects associated with game type, ω_k, falls to about .2 from .9.

Finally, Model 5 includes the Risk Attitude variable. Risk Attitude is not significant in this model. Note though that, for reasons pointed out earlier, we loose a third of our cases. Accordingly we are hesitant to draw any strong conclusions from the Risk Attitude

[14] Rearranging terms in Equation 6 gives the following:

$$\log\left[\frac{\Pr(y_{ijk}=1)}{1-\Pr(y_{ijk}=1)}\right] = T_j[\pi_1+\theta_1 P_{ijk}]+G_k[\pi_2+\theta_2 P_{ijk}]+P_{ijk}[\theta_0+\tau_j+\omega_k]+\beta_2 Z_{ijk}+\pi_0+\eta_j+\zeta_k \tag{7}$$

The equation was estimated as a generalized linear mixed model in R. For details on the GLMM estimation see (McCullagh and Nelder, 1989; Raudenbush and Bryk, 2002).

result in this model. Our very tentative conclusion is that it does not help explain the student/non-student gap in equilibrium behaviour.

This multivariate analysis suggests three insights about differences in the equilibrium behaviour of student and non-student subject pools. First, students exhibit more homo-economicus behaviour than non-students. Second, students are particularly homo-economicus when they play games that are inherently other-regarding – the student/non-student gap in equilibrium behaviour is comparatively small for games that do no invoke other-regarding preferences. Third, demographic characteristics that one might expect to be associated with variations in equilibrium behaviour – gender, age, intelligence, and risk aversion – have no significant effect on the behaviour of our subjects. This, of course, is subject to our caveat regarding truncated values on some of these demographic variables.

3.5.1 Modeling Amount of Contributions for each Game

We can explore the same themes from the previous section by modeling the amount of money subjects chose in each of the different games. Here we estimate a separate model for each of the games, giving maximum opportunity for the effects to vary across games. The dependent variables are 1) the amount donated in the dictator game (hence only subjects that were randomly selected to donate rather than receive); 2) the amount sent in the trust game (we only report the amount sent – the results are very similar for the amount returned); 3) the contribution in the first round of the public goods game; 4) the amount guessed in the Beauty-contest; and 5) the amount bid in the auction game (note that this model includes a control for the subject's private value).

The results are reported in Table 5 through Table 7. For each game, we estimate regression models for the total sample and for the sub-sample of subjects who, based on our comprehension check, demonstrated that they understood the game. And in the case of each of these two samples we estimate a model with only the student dummy variable and the measure of IQ; and another model that includes these two variables plus gender and age.[15]

[15]Note that for reasons explained earlier we lose large numbers of observations when we include Risk Attitudes in the individual models. Accordingly we do not estimate a model with Risk Attitudes because

These results confirm the multivariate findings in the previous section: First, the student dummy variable is virtually the only statistically significant variable in these equations. Again, consistent with the previous estimates, the student effect is quite pronounced in the other-regarding games – the dictator and trust games – but is not statistically significant in the games that we characterize as more cognitively demanding – the Public Goods, Beauty-contest and Auction Games. Finally, two of the covariates, I.Q. and gender, never reach statistical significance in any of the 20 equations. The one equation in which covariates matter is in the Beauty-contest game where younger subjects are significantly more likely to give lower guesses.

The student effect in the more cognitive demanding games is on balance directionally consistent with the notion that students play homo-economicus, although in any one of these three models it is not statistically significant. First, in the case of the Public Goods Game none of the explanatory variables predict the subjects' choices. In fact, the variance explained by our models is effectively zero.[16] In the Beauty-contest game the student effect approximates statistical significance in the reduced models with only student and IQ but the introduction of age as a control variable clearly reduces it to insignificance. In the Auction Game the student dummy variable is not statistically significant. Hence consistent with the multivariate analysis, there are decidedly smaller differences between students and non-students in the case of more cognitively demanding games.

the missing values on this variable results in a loss of about one-third of the cases.

[16]This result uses the contributions from the first of ten rounds of the Public Goods Game. We obtain the same results using the ultimate round.

Table 5: OLS Regression Model Results with Amounts Chosen as Dependent Variable

	Total 1	Total 2	Understand 1	Understand 2
Dictator				
Student	-1.97	-1.49	-2.06	-1.56
	(.54)	(.69)	(.53)	(.68)
IQ Test	0.001	0.002	0.001	0.002
	(.003)	(.004)	(.003)	(.004)
Age		0.03		0.03
		(.04)	(0.05)	(.04)
Gender		-0.25		-0.15
		(.57)		(.56)
Intercept	3.26	1.82	3.43	1.84
	(1.16)	(2.19)	(1.14)	(2.16)
$Adjusted R^2$	.17	.09	0.19	.11
Observations	64	56	63	55
Trust (Amount Sent)				
Student	-4.93	-5.87	-4.72	-5.96
	(1.17)	(1.40)	(1.22)	(1.52)
IQ Test	0.007	0.006	0.01	0.008
	(.007)	(.007)	(.01)	(.008)
Age		0.01		-0.02
		(.07)		(.09)
Gender		0.21		-0.07
		(1.18)		(1.26)
Intercept	5.61	5.95	4.25	5.92
	(2.61)	(4.01)	(2.90)	(4.37)
$Adjusted R^2$	.20	.28	0.19	.27
Observations	63	55	57	49

Table 6: OLS Regression Model Results with Amounts Chosen as Dependent Variable

	Total 1	Total 2	Understand 1	Understand 2
Public Goods Round 1				
Student	-0.72	-1.24	-2.21	-2.60
	(1.39)	(1.66)	(1.71)	(2.13)
IQ Test	0.007	-0.01	0.0004	0.0002
	(.008)	(.009)	(0.01)	(.01)
Age		-0.01	-0.02	
		(.09)		(.12)
Gender		-1.17		-0.84
		(1.42)		(1.73)
Intercept	13.54	14.34	12.68	13.62
	(2.93)	(5.13)	(3.55)	(6.26)
$Adjusted R^2$	0.0	0.0	0.0	0.0
Observations	126	111	91	82

Table 7: OLS Regression Model Results with Amounts Chosen as Dependent Variable (Continued)

	Total 1	Total 2	Understand 1	Understand 2
Beauty-contest Guess				
Student	-5.04	1.39	-8.01	-1.52
	(4.46)	(5.20)	(5.32)	(6.23)
IQ Test	0.04	-0.04	-0.05	-0.04
	(.03)	(.03)	(.03)	(.03)
Age		0.67	0.88	
		(.28)		(.38)
Gender		2.38		-2.53
		(4.43)		(5.47)
Intercept	61.62	34.97	63.86	33.86
	(9.32)	(16.07)	(12.68)	(19.15)
$Adjusted R^2$	.04	.08	.08	.09
Observations	126	111	90	80
Auction Bid				
Private Value	0.45	0.42	0.38	0.39
	(.13)	(.15)	(.16)	(.17)
Private * Student	0.26	0.25	0.41	0.35
	(.19)	(.21)	(.21)	(.23)
Student	-1.69	-1.28	-2.50	-1.54
	(1.44)	(1.69)	(1.63)	(1.83)
IQ Test	-0.0004	-0.001	-0.003	-0.004
	(.003)	(.003)	(.01)	(.004)
Age		0.05		0.06
		(.03)		(.04)
Gender		-0.70		-1.03
		(.51)		(.56)
Intercept	2.79	2.22	4.24	3.20
	(1.44)	(2.36)	(1.76)	(2.64)
$Adjusted R^2$	.23	.21	0.26	.26
Observations	126	111	101	90

4 Discussion and conclusion

Our results suggest that in experiments that invoke other-regarding considerations, student and non-student subject pools will make choices that are very different - students are more likely to behave as homo-economicus agents than is the case for non-students. The student subject pool was dramatically less kind and less trusting than the non-student subjects. Fairly consistently across experiments, non-students were more other regarding by a ratio of three-to-one. Our results suggest these differences were not the result of features of the experimental design such as comprehension or the unnatural nature of the lab experiment context. Rather we are inclined to conclude that non-students are more inequity averse and trusting.

While student subject pools may understate the extent of other-regardedness in the population, they will likely overstate the degree of consistency in the magnitudes of contributions in other-regarding games. Within-subject correlations of contributions in other-regarding games is significantly higher amongst students than non-students. Hence experiments based on student subject pools may exaggerate the extent to which other-regarding choices are consistent with the axioms of revealed other-regarding preferences.

In experiments that are more cognitively demanding and that are unlikely to invoke other-regarding considerations, student and non-student differences are comparatively quite small. Our results suggest that equilibrium behaviour in the strategic reasoning games was low, in the classic sense, for both subject pool types. But in comparing equilibrium play across the two game types, homo-economicus behaviour is in fact equal to, or higher than, play in simple other-regarding games. Part of the explanation is that for non-students equilibrium play in other-regarding games is very low (depressed, we argue, by their inequity aversion and trust preferences). Student behaviour in the two strategic reasoning games was somewhat more homo-economicus than non-students. But the differences are small compared to those found in other-regarding games and they are not significant in the multivariate analyses. There is no strong evidence here to suggest differences in the strategic reasoning abilities of student and non-student subject pools. The overall low levels of equilibrium behaviour in strategic reasoning games, amongst both students and non-students, is what mostly stands out.

Student and non-student subject pools can differ quite significantly in terms of characteristics that likely matter for the decisions they take in a typical economic experiment. Students are better educated, score higher on standard IQ tests, exhibit higher levels of comprehension for most experiments, and, of course, are younger. And it is particularly noteworthy that for many of these characteristics, there is virtually no overlap with respect to extreme values. Hence, for many student subject pools we never observe subjects who are elderly, have low IQs or exhibit low comprehension levels. Given that student subjects typically have values on key demographics that can be highly skewed or truncated, controlling on these covariates in the multivariate analysis of experimental data may generate misleading results.

With this caveat in mind, we explored whether the introduction of covariates as controls attenuated or exaggerated student/non-student differences. It turns out that these differences are very robust to multivariate analyses that control for a variety of demographic and cognitive ability covariates. With only minor exceptions these individual-level demographic covariates are not statistically significant in our multivariate models.

To the extent that experimenters are concerned with identifying subjects with motivations conforming to the classic assumptions of game theoretic models, they should focus on student subject pools and avoid non-students. This is particularly the case with games that incorporate other regarding features, such as trust or dictator games. In the case of games in which cognitive skills are critical to the internal validity of a particular experiment, the results are more nuanced and the student/non-student differences are not large. Similarly, student subjects are more likely to provide experimenters with choices that are consistent with the axioms of revealed other-regarding preferences.

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Appendix A: Behavior in games

	Non-students	Students	Mann-Whitney test of equality (p-value)
% sending £10	82	35	.00
% sending £15 back	87	56	.01

Table 8: Behaviour in the Trust Game

	Non-students	Students
% reporting 0	1.5	0
% reporting 20 or 21	3.1	6.3
% reporting reporting 32, 33 or 33.3	6.2	10.1
% reporting 50	7.7	6.4
% reporting < 20	15.4	20.6
% reporting < 33.3	32.3	54.0
% reporting < 50	64.6	74.6
average guess	45.9	38.3

Table 9: Behaviour in Guessing Game

	Non-students	Students	Mann-Whitney test of equality (p-value)
average donation (std. dev)	£3.5 (1.9)	£1.6 (2.1)	.00

Table 10: Behaviour in the Dictator Game

	Percentage bidding private value		Average bid	
Private value	Non-students	Students	Non-students	Students
4	28.6	26.7	4.8	3.7
6	46.2	30.8	5.1	4.9
8	18.8	61.5	5.6	7.7
10	18.2	31.8	7.4	7.7

Table 11: Behaviour in Auction

	Non-students	Students
Consistency rate	66.1%	68.3%
Average Switching Point	3.6	2.5

Table 12: Risk elicitation task

	Non-students	Students
Average Contribution (first round)	55.5%	49%
Average Contribution (ten rounds)	41.2%	42.5%

Table 13: Behaviour in PGG

Game	**Students**	**Non-students**	**Mann-Whitney test of equality (p-value)**
Dictator	100	98	.33
Trust Game	97	89	.10
Public Good	83	66	.02
Guessing	91	68	.09
Auction	92	73	.01

Table 14: Percentage who understand the instructions

Appendix B: Instructions

Dear participants,

Welcome and thank you for participating to our experiment. The experiment will last for about one and a half hour. Please do remain quiet from now on until the end of the experiment. You will have the opportunity to ask questions in a few minutes.

The experiment consists of two main parts. Throughout the whole experiment, you will receive instructions on the computer screen and be told what to do.

PART I In this first part, you will be asked to make choices in six different situations. Each of these six is an entirely different and independent situation. You should treat each as being independent of the others. We will describe each situation successively and explain precisely what you have to do. In each situation your earnings will depend on your choices, possibly the choices of others and chance. To make sure you understand what you have to do, you will be asked to think of an example for each situation before you take the actual decision. This is only to make sure you understood, and these examples have no implications at all for your earnings. When each situation is over, there will be a very short pause and then we will introduce the next situation. You will only be informed about the outcomes of all six situations **at the end of PART I**. We will show you a screen with the outcomes corresponding to each of the six situations.

The computer will then 'roll a die' to pick one of the six situations, which will determine your actual earnings. That is, **your actual earnings will depend on the choices you made in one of the six situations only** -this single situation will be selected at random by the computer for all participants in this room.

PART II In this second part, you will be asked to answer questions and your earnings will depend on your answers (your earnings in this part will depend only on **your** answers). Again, you will be informed about the outcome of this part only **at the end of PART II**.

We ask you to remain quiet during the whole experiment. Those who do not respect the silence requirement will be asked to leave the experimental room. Once the experiment is finished, please remain seated. You will be called up successively by the number on your table; you will then receive an envelope with your earnings and you will be asked to sign a receipt.

Important note: The CESS lab has a **strict 'no deception' policy**. That means that **under no circumstances** will participants to experiments be deceived. All the information you will receive from us is true. For example, if we tell you that you have been paired to another participant in the room, this is indeed the case. Finally, note that your participation is considered voluntary and you are free to leave the room at any point if you wish to do so. In that case, we will only pay you the participation fee of £4.

Please leave these instructions on your table when you leave the room. You can take notes on these pages if you wish to do so.

If you have any questions, please raise your hand now.

PART I

SITUATION 1

We have formed pairs of people at random in the room. Each pair is made up of **person 1** and **person 2**. None of you will know with whom you have been paired. The experimenter is the only one who knows who is paired with whom.

Person 1 will receive £10 and then can choose whether to transfer these £10 to **person 2** or not to transfer any money and keep the £10.

- If **person 1** decides to keep the £10, **person 2** earns nothing.
- If **person 1** decides to transfer the £10 to **person 2**, the money will be tripled by us before it is passed on to **person 2**. That is, person 2 will receive £30.

Person 2 will be able to choose whether to transfer £15 back to **person 1** or not to transfer anything back in the event her/his account is credited with £30.

- If **person 2** transfers £15 back, both **person 1** and **person 2** will earn £15.
- If **person 2** does not transfer £15 back, **person 2** will earn £30 and **person 1** will earn nothing.

To make sure you understand the set-up, could you please provide an **example** (of your choice) of a possible outcome for this situation:

Decision of Person 1:

Decision of Person 2:

Earnings of Person 1: £

Earnings of Person 2: £

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to the computer and follow the next instructions on the screen.

SITUATION 2

Each of you has to guess a number between 0 and 100. We will calculate the average of the guesses of all participants in the room and multiply this number by two-thirds. The person whose guess is closest to this number will receive £20 (in case of a tie, the computer will select randomly one of the winners and he/she will receive £20).

To make sure you understand the set-up, could you please provide an **example** (of your choice) of a possible outcome for this situation:

Average of all guesses:

The winner would be the person with the guess closest to what number?

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to the computer and follow the next instructions on the screen.

SITUATION 3

You have been paired randomly with another person in the room. Each pair is made up of **person 1** and **person 2**. None of you will know with whom you have been paired. The experimenter is the only one who knows who is paired with whom. **Person 1** will receive £10 and will have the opportunity to give a portion of her (his) £10 to **person 2**. **Person 1** can transfer any (round) amount between 0 and £10.

To make sure you understand the set-up, could you please provide an **example** (of your choice) of a possible outcome for this situation:

Decision **Person 1**: Transfer £to **person 2**

Earnings **Person 1**:

Earnings **Person 2**:

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to the computer and follow the next instructions on the screen.

SITUATION 4

You will now have the opportunity to bid for an amount of money. There are 4 possible amounts of money you could bid for: £4, £6, £8 or £10. The computer will determine at random for each participant which of these four amounts you could bid for. This will appear on the computer screen. Thus, some of the participants will be able to bid for £4, some for £6, some for £8 and some for £10. All participants in this room bid in the same auction.

You will be asked to introduce a bid. The person with the highest bid (among all participants in this room) will win the amount of money that has been allocated to that person. Bids will remain private and will only be known to the experimenter.

Important: The winner will have to pay the amount of money corresponding to the bid of the **second highest bidder**.

Thus, if you are the highest bidder of the auction; you will earn the following: Your Prize (£4, £6, £8 or £10) - Second highest bid.

If you are not the highest bidder, you will not win or have to pay anything. In case of a tie, the computer will select randomly one of the highest bidders and this will determine who has won the auction.

To make sure you understand the set-up, could you please provide an **example** (of your choice) of a possible outcome for this situation, supposing there would only be 4 participants, who could win respectively £4, £6, £8 and £10

Bid submitted by person 1 (for £4):

Bid submitted by person 2 (for £6):

Bid submitted by person 3 (for £8):

Bid submitted by person 4 (for £10):

Who would be winning the auction? Person

Earnings of the person winning the auction: £(Prize) - £(Second highest prize) = £

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to the computer and follow the next instructions on the screen

SITUATION 5

We will now propose eight different choices between a fixed amount of money and an all-or-nothing lottery. The lottery will work as follows. The computer will roll a six-sided die. The general rule is that you will earn nothing if the die indicates 1, 2 or 3; and earn something (as indicated for each case) if the die indicates 4, 5 or 6. Finally the computer will pick at random a number between 1 and 8 to determine which of 8 cases applies to determine your earnings in this situation.

You will be asked to indicate your preferred option in each of the cases.

For example:

Case 1: Choice between

A: £5 with certainty or B: £0 if the die shows 1, 2 or 3; £14 if the die shows 4, 5 or 6.

To make sure you understand the set-up, could you please provide an **example** (of your choice) of a possible outcome in this case:

Your choice (A or B):

Outcome of the lottery (die: 1,2,3,4,5 or 6):

Your earnings: £

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return now to the computer and follow the next instructions on the screen

SITUATION 6

We have formed groups of four people at random in the room. You have been assigned to one of these groups. None of you will know who is in his/her group. The experimenter is the only one who knows who is in which group.

Each member of the group has to decide on the division of 20 tokens. You can put these 20 tokens on your private account or you can invest them fully or partially into a project. Each token you do not invest will automatically be transferred to your private account.

Your income from the private account: For each token you put on your private account, you will receive exactly one point. Nobody except you earns something from your private account.

Your income from the project: We will add up the contributions made by the four members of your group to the project. Each member will then receive an income from the project calculated as follows:

Income from the project = Sum of all contributions of the members of the group x 0.4

Your total income

Your total income is the sum of your income from the private account and your income from the project.

We will ask you to make such a decision 10 times in a row. We will add up the points you have earned in each period and calculated your final earnings using the following exchange rate: £1 = 25 points.

Note that your group will remain the same throughout this situation.

To make sure you understand the set-up, could you please provide an example (of your choice) of a possible outcome for this situation:

Your contribution to the project: tokens

Sum of all contributions to the project (including yours): tokens

Your income from the private account: points

Your income from the project: points

Your total income: points

If you have any questions about the set-up, please raise your hand now and wait for the experimenter to come to you.

Please return to the computer and follow the next instructions on the screen.

PART II

We will now ask you 4 sets of multiple-choice questions. Each set has to be completed within a certain amount of time. You can pass questions and come back to them later within the same set. Once a set is completed, it is not possible to change or come back to the answers you have submitted. You will earn 20p per good answer submitted.