DISCUSSION PAPER SERIES

IZA DP No. 10171

The Rapid Evolution of Homo Economicus: Brief Exposure to Neoclassical Assumptions Increases Self-Interested Behavior

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August 2016

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zur Zukunft der Arbeit
Institute for the Study
of Labor

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ABSTRACT

The Rapid Evolution of Homo Economicus: Brief Exposure to Neoclassical Assumptions Increases Self-Interested Behavior*

Economics students have been shown to exhibit more selfishness than other students. Because the literature identifies the impact of long-term exposure to economics instruction (e.g., taking a course), it cannot isolate the specific course content responsible; nor can selection, peer effects, or other confounds be properly controlled for. In a laboratory experiment, we use a within- and across-subject design to identify the impact of brief, randomly-assigned economics lessons on behavior in games often used to measure selfishness: the ultimatum game (UG), dictator game (DG), prisoner's dilemma (PD), and public-goods game (PGG). We find that a brief lesson that includes the assumptions of self-interest and strategic considerations moves behavior toward traditional economic rationality in UG, PD, and DG. Despite entering the study with higher levels of selfishness than others, subjects with prior exposure to economics instruction have similar training effects. We show that the lesson reduces efficiency and increases inequity in the UG. The results demonstrate that even brief exposure to commonplace neoclassical economics assumptions measurably moves behavior toward self-interest.

JEL Classification: A2, D6, C9, C7, A1

Keywords: economics instruction, self-interest, game theory, laboratory experiment, social preferences

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* Invaluable assistance provided by Marianne Farag and Tiffany Iskandar. Financial support from the Leavey School of Business is gratefully acknowledged.

1. Introduction

Humanity is slowly coming to grips with a potentially existential collective-action problem: global climate change. Climate scientists predict that by the end of the century average temperatures will increase by up to 10 degrees Fahrenheit, the sea level will rise by up to 6 feet, and the frequency of severe weather-events will increase (IPCC, 2013; DeConto & Pollard, 2016). Changes appear to have begun already. For example, the last 15 years are 15 of the 17 hottest since record-keeping began in 1880; sea levels have risen eight inches in that time (NOAA/NCEI, 2016; NOAA, 2015). Scientists believe that the impact cannot be completely undone, but collective action would presumably limit the damage. We believe it is incumbent on the profession to consider how it can help ameliorate this and other collective-action problems.

There is reason to believe, though, that components of economics instruction may be counterproductive: compared to other students, economics students are more likely to act in their own self-interest, are (and believe others to be) less honest, and view greed more favorably (e.g., Frank et al., 1993). While there is evidence that economics students act more selfishly prior to economics instruction ("selection effects": e.g., Frey & Meier, 2003), there is also evidence that economics instruction generates even more selfishness ("training effects": e.g., Frank et al., 1993). Training effects are concerning given that approximately 40% of undergraduates take at least one economics course (Siegfried & Walstad, 2014); and the most popular major in the U.S. is business, which requires economics coursework (Snyder et al., 2016).

What might be causing training effects? We believe that introductory economics instruction often relegates to "fine print" the nuances of the assumptions, definitions, and limitations necessary to contextualize theoretical predictions. If students fail to understand or consider the fine print, it is conceivable that they might come to conclusions like the following: people are motivated by nothing but self-interest, people acting in their own self-interest generate efficient outcomes, perfectly competitive markets are the norm, efficiency should be privileged over other concerns, and redistribution is always inefficient. As such, they may fail to recognize collective-action problems or that self-interest does not lead to their efficient resolution. Further, students may not keep in mind the distinction between the technical economic definition of efficiency and its everyday usage as an unqualified good (e.g., the Dictionary.com definition of efficient is "performing or functioning in the best possible manner with the least waste of time and effort..."). Individuals with equity-concerns, for example, may then understand efficiency to imply a fair distribution of resources.

A limitation of the existing training-effect literature is that it identifies the impact of long-term exposure to economics instruction (e.g., majoring in economics or taking a semester-long course) and thus cannot isolate the specific content causing the impact; nor can selection, peer effects, or other confounds be properly controlled for. We contribute to the literature by testing the impact of economics instruction on selfishness with the control afforded by a laboratory experiment. Specifically, we use a within- and across-subject design to identify the impact of brief, randomly-assigned economics lessons on behavior in games often used to measure selfishness: the ultimatum game (UG), dictator game (DG), prisoner's dilemma (PD), and public-goods game (PGG). We believe that our approach corrects for the confounds in the existing literature and identifies specific course-content that promotes selfishness--a necessary first step in understanding the mechanism linking economics instruction and selfishness, and limiting its deleterious impact.

We find that a brief lesson that includes the assumption of self-interest and strategic considerations moves behavior toward traditional economic rationality in UG, PD, and DG. Further, we find that the impact of the lesson is at least as strong for subjects with prior exposure to economics instruction as for those with no prior exposure, despite the former entering the study with higher levels of selfishness than the latter. Lastly, we show that the lesson reduces efficiency and increases subject-payment inequality in the UG. The results demonstrate that even brief exposure to commonplace neoclassical economics assumptions measurably moves behavior toward self-interest.

2. Literature Review

It is well established that individuals exposed to economics instruction--economists, economics majors, and even undergraduates who take a single economics course[1]--have different attitudes and behaviors vis-a-vis selfishness than those not exposed. Economics students have been shown to behave more in accordance with traditional economic rationality in games (PGG: Marwell & Ames, 1981; UG: Carter & Irons, 1991; PD: Frank et al., 1993); give less to charity (Bauman & Rose, 2011; Frank et al., 1993; Frey & Meier, 2003); exhibit less honesty (Frank et al., 1993; Frank & Schulze, 2000); view greed more favorably (Wang et al., 2011); and place greater importance on individualism than collectivism (Gandal et al., 2005). To our knowledge, only one study presents contravening evidence: Yezer et al. (1996) find that "lost envelopes" containing money are more likely to be mailed to the addressee if left in economics classes than other classes.

The bulk of the extant literature compares economics students to other students. As such, identification relies on the effects of long-term exposure to economics instruction (e.g., semester-long courses or entire majors). For example, a seminal study finds that economics Ph.D. students contribute less in the PGG than do other students (Marwell & Ames, 1981). This approach has several weaknesses. First, it cannot distinguish between selection and training effects. Second, training effects may be confounded with factors unrelated to instructional content (e.g., classmates and professors). Third, if there are training effects, the specific course content responsible cannot be identified.

Some of these concerns have been addressed better than others. For example, a number of studies use a within-subject design whereby students' behavior is measured before and after exposure to at least a term of economics instruction (Bauman & Rose, 2011; Frank et al., 1993; and Frey & Meier, 2003). Selection effects are identified using pre-instruction differences between the behavior of economics and other students. Training effects are identified using pre- to post-instruction differences in the behavior of economics students, as compared to other students. Bauman & Rose (2011) find evidence of selection-effects in the charitable donations of economics majors as compared to other majors; they also find evidence of training effects over a three-year period among students who have taken economics courses but do not major in

[1] Hereafter we will use "economics students" to refer to all three of these groups.

economics, and no evidence of training effects for economics majors. Frank et al. (1993) find evidence of selection-effects in the PD defection-rates of economics majors as compared to other majors; they find evidence of training effects using honesty-surveys for students in an introductory economics course as compared to astronomy. Frey & Meier (2003) find evidence of selection-effects in the charitable donations of economics majors as compared to other majors; they find no evidence of training effects. It is important to note that Bauman & Rose (2011) and Frank et al. (1993) cannot rule out that the training effects they identify are due to selection-effects insofar as students selecting into economics instruction are more susceptible to its selfishness-promoting content.

Further, studies using long-term exposure to economics instruction cannot distinguish between the impact of instruction and other factors. For example, economics students may be invited to join a business fraternity; or economics instruction may pique students' interest in financial markets, leading them to learn about, and invest in, stocks. Thus, extracurricular activities, and not course content, may explain the identified training effects. Training effects may also result from differences in the opinions or persuasiveness of professors or classmates.

Lastly, studies using long-term exposure to economics instruction cannot identify the specific course content responsible for training effects. For example, Frank et al. (1993) find a greater pre- to post-course honesty-decrease in an introductory microeconomics course taught by an instructor with a game-theoretic rather than a Maoist/development orientation. Importantly, this indicates that the magnitude of training effects is dependent on course characteristics, suggesting that economics courses can be designed so as to reduce training effects. While the authors attribute the variant impact to the game-theoretic instructor's "heavy emphasis on the prisoner's dilemma and related illustrations of how survival imperatives often militate against cooperation," this is just one of many possible differences between the two courses.

Given the confounds discussed above, the control afforded by laboratory experimentation is valuable. To our knowledge, only one experimental study, Molinsky et al. (2012), considers the relationship between economics and selfishness: they find that priming the concept of economics using a sentence-unscrambling task reduces compassion. An important limitation of the applicability to the current research question is that Molinsky et al. (2012) cannot help identify the course content within economics instruction that promotes selfishness, as economics instruction *ipso facto* primes the concept of economics.

3. Experimental Design

We conducted a laboratory experiment to test the impact of brief economics lessons on selfishness. The experimental design allowed for both within- and across-subject comparisons: each subject completed the experimental tasks before and after treatment, and treatment was randomly assigned across subjects. All tasks were incentivized.

The experiment was conducted at Santa Clara University (SCU) in the winter and spring quarters of 2016. 276 students participated. In an attempt to ensure that the sample was representative of the undergraduate student body, we emailed all first- and second-year students inviting them to participate. Prospective subjects were told that participation in the study would take about 60 minutes and that they would be paid for their participation, with a minimum, average, and maximum payment of $5, $20, and $40. Sessions lasted approximately 60 minutes, and subjects received a minimum, average, and maximum payment of $5.00, $18.75, and $45.75. The experiment was conducted in a classroom equipped with downview computer-desks using z-Tree (Fischbacher, 2007); screenshots are included in Appendix A. In brief, our experimental procedure was as follows (additional details provided below).

- Subjects read and signed the informed consent form.
- Subjects read instructions and were given the opportunity to make a real charitable donation.
- Subjects completed tasks 1-6 (randomly ordered): UG (as proposer and responder), DG (as dictator and recipient), PGG, and PD.
- Subjects completed a brief, randomly-assigned economics lesson
- Subjects completed tasks 7-12, a randomly-ordered repetition of tasks 1-6.
- Subjects' risk preferences were elicited.
- Subjects completed a questionnaire that included demographic and other items.
- Subjects received their payments and exited the session.

3.1. Real charitable donation opportunity

Subjects were informed that they were starting with $5 and could donate between $0 and $5 to the Santa Clara Fund. This exercise was intended to familiarize subjects with the z-tree program, to give subjects a sense of the decisions they would be making, and to measure subjects' baseline willingness to donate to a charity. Subjects were given the following information from the Santa Clara Fund website: "The Santa Clara Fund supports four areas of campus that are distinguishing factors of a Jesuit education: 60% is awarded as scholarships; 20% supports academic programs; 15% is awarded to help students travel abroad and participate in immersion trips; and 5% is available for student clubs and initiatives. One hundred percent of the gifts made to this fund serve SCU students."

3.2. Tasks 1-6

Tasks 1 and 2 involved a standard UG. Subjects were informed that a "proposer" who had been endowed with $20 would make an "offer" between $1 and $20 to a "responder." If the responder accepted the offer, then the proposer's payment would be $20 minus the offer and the responder would receive the offer. In contrast, if the responder rejected the offer, then both the proposer and responder would receive $0. Subjects were informed that they would play twice, once as proposer (Task 1) and once as responder (Task 2). Further, subjects were informed that they would be randomly matched with a subject in the session each time they played. Finally, the strategy method was used to elicit responders' responses to all possible offers.

Tasks 3 and 4 involved a standard DG. Subjects were informed that a "divider" who had been endowed with $20 would choose how to divide her $20 endowment with a "recipient;" divisions could be between $0 and $20. Subjects were informed that they would play twice, once as divider (Task 3) and once as recipient (Task 4). Further, subjects were informed that they would be randomly matched with a subject in the session each time they played.

Task 5 was a standard PGG. Subjects were informed that they had each been endowed with $20 and would choose how much of this endowment to allocate to the "4-person pile" (a public investment) with three other randomly selected subjects. All allocations to the 4-person pile were multiplied by 1.5 and divided evenly among the four subjects. Each subject's payment in Task 5 was the sum of what she did not allocate to the 4-person pile plus a quarter of the 4-person pile.

Task 6 was a standard PD. Subjects were informed that they would be randomly matched with another subject to play the "the box game" in which they would each choose between "Option A" (cooperate in the standard PD) and "Option B" (defect in the standard PD); they were shown a "box" (a standard PD payoff matrix) with diagonal payoffs of ($10, $10) and ($5, $5) and off-diagonal payoffs of ($0, $20) and ($20, $0).

The UG, DG, PGG, and PD were chosen to represent a range of experimental games that are commonly used to measure other-regarding preferences. The order of the games was randomized by subject. For UG (DG), the order of Tasks 1 (3) and 2 (4) was randomized as well. Subjects answered two unincentivized items testing their comprehension after instructions and before playing each game. It should be noted that, in an attempt to decontextualize the games, we referred to them as follows: the UG was called the "Offer Game," the DG was called the "Division Game," the PGG was called the "Four-Person Pile Game," and the PD was called the "Box Game." Finally, subjects received no feedback regarding the outcome of the games until the end of the session. The random matching of players in each task and lack of feedback were intended to reduce the scope for reciprocity and learning.

3.3. Brief, randomly-assigned economics lessons

Subjects read one of three randomly-selected lessons about game theory: normative, positive, or control. Each lesson started with the corresponding paragraph below.

- Normative: "HOW TO PLAY GAMES SUCH AS THOSE YOU JUST PLAYED. Normative economics helps economists understand how individuals should make decisions in games such as those you just played. To make normative economic assertions, economists build economic models. In such models, economists make the following assumptions: (1) that all individuals are self-interested and (2) that all individuals attempt to maximize their payments. Further, economists examine all the strategies available to an individual to determine which one maximizes his or her payment. Economists do this by working backward. First, economists consider all the choices the individual's opponent could make, and then, determine the choice that maximizes the individual's payment. Now we will apply normative economic analysis to the Box Game and the Offer Game to see what we can learn."

- Positive: "HOW TO PLAY GAMES SUCH AS THOSE YOU JUST PLAYED. Positive economics helps economists understand how individuals actually make decisions in games such as those you just played. To make positive economic assertions, economists can conduct laboratory experiments. In such experiments, economists recruit groups of people to play games like you just played. Further, economists record and examine the choices people make to learn how people play the games. They do this by building analytical databases and using statistical analyses. From such analyses, economists can determine what percentage of the people choose Option A or Option B in the Box Game; what the average offer is in the Offer Game; and how much a proposer has to offer, on average, in the Offer Game so that most responders (for example, more than 75%) accept the offer. Now we will apply positive economic analysis to the Box Game and the Offer Game to see what we can learn."

- Control: "HOW TO DESIGN GAMES SUCH AS THOSE YOU JUST PLAYED. Game theory helps economists understand strategic interactions. To design a game, that is, model a strategic interaction, economists must specify the number of players, the actions available for each player at each decision point, and the payment for each outcome. Games in which decisions are made concurrently are generally represented using a "normal form," the Box Game is such a game. Games in which decisions are made sequentially can be represented using an "extensive form," the Offer Game is such a game. Now we will discuss how the Box Game and the Offer Game are represented using game theory."

In order to cleanly identify the specific lesson-content that impacts behavior, the lessons were carefully designed to be parallel in structure and to mirror standard economics textbook styles (e.g., Dutta (1999); Mankiw (2015); Frank et al. (2015); Krugman & Wells (2015)). The "normative" lesson was designed to relay the economic model of how games are played and to include content that we believe may contribute to training effects in economics instruction: the assumption of own-payoff maximization and an explanation of strategic considerations. The primary goal of this study is to identify the impact of the normative lesson. Further, as introductory economics instruction usually distinguishes between normative and positive analysis early on, we also consider the impact of a "positive" lesson: subjects are informed of average behavior in economics games from the empirical experimental literature. A "control" lesson describes how games are designed with no normative or positive content. We chose the economics lessons to be about game theory for several reasons. For one, it was important that the lessons be brief, thorough, and self-contained, given the time constraints inherent to laboratory experimentation. Also, to minimize confounds, it was important that the lessons be on topics about which subjects had few preconceived notions, experience, or political opinion. Lessons about game theory seemed a natural fit.

Each of the three lessons also included an application of the corresponding paragraph above to the UG and PD. For example, in the normative lesson's UG-illustration, subjects read: "notice that if the responder accepts the offer, his or her payment will be greater than if he or she rejects the offer regardless of the proposer's offer... Thus, accepting the offer is the dominant strategy for the responder…" In the positive lesson's UG-illustration, subjects read: "The experiment was conducted at University of British Columbia… [P]roposers, on average, offered about 45% of the money they started the experiment with (to the responder)..." (See Appendix A for the complete lessons).

The decision not to include applications of the DG and PGG in the lessons enables the identification of spillovers of lesson-content to contexts not taught. This ensures that subsequent behavior is due to lesson-content and not simply the mechanics of the illustration. For example, the normative lesson's UG- and PD-illustrations include both the assumption of self-interest and strategic considerations. The DG does not involve strategic considerations, so if behavior in the DG is affected by the normative lesson, it is presumably due to the assumption of self-interest. Behavior in the PGG can be affected by both the assumption of self-interest and strategic considerations. Finally, if behavior in the UG or PD is affected by the normative lesson, it could be due to the assumption of self-interest, strategic considerations, or the mechanics of the illustration (i.e., replicating the application).

To reduce experimenter demand effects, interaction with the experimenter was minimized: all lessons were presented on individual computer screens with no audio, video, or lecture components. This also allowed for within-session randomization of the treatment, eliminating the confounding of treatment- and session-effects. It should be noted that, while we sought to minimize experimenter demand effects, one of the ways in which economics instruction may impact behavior in its natural setting is through the existence of analogous "instructor demand effects." Moreover, the use of a laboratory setting may not give rise to the usual external-validity critiques because the natural setting of economics instruction is similarly within university classrooms. As such, our experiment may even be considered an artefactual field experiment (Harrison & List, 2004).

3.4. Tasks 7-12

In Tasks 7-12, subjects repeat Tasks 1-6; the order of games and player-matching were randomized again. Subjects received abbreviated instructions: the full instructions were not repeated, nor were the items testing subjects' comprehension.

3.5. Risk-preference elicitation

Subjects' risk preferences were elicited using a 21-item multiple-price list in which subjects chose between a fixed payment ($0-$20) and a lottery with even odds of receiving a $0 or $20 payment.

3.6. Questionnaire

Subjects completed a questionnaire that included items regarding their demographic and other characteristics, for example, date of birth, gender, race, and family background. In addition, the questionnaire included an abbreviated 10-item version of the Narcissism Personality Inventory (NPI) and thirteen items regarding attitudes toward fairness, some of which were used in Konow (2003).

3.7. Payments

Subjects were given detailed information regarding the calculation of payments. One of Tasks 1-12 or the risk-preference elicitation task was randomly chosen for payment (payment-task). If the risk-preference elicitation task was chosen as the payment-task, then one of the 21 fixed payments was randomly chosen and the lottery was implemented. All randomization was implemented using a bingo spinner. Total subject-payments were the sum of the $5 show-up fee, the portion of the $5 not donated to the Santa Clara Fund, and the payment-task. Subjects were paid in cash. The payment was placed in an envelope with only the subjects' identification number on it. Subjects received their payment as they exited the session. Payments were double blind: one administrator prepared the envelopes, and another distributed them.

4. Results

We present the results by first comparing behavior in the normative, and then the positive, treatment to the control. For each comparison, we first consider behavior in the games used to illustrate the lessons (UG and PD), and then consider evidence of spillover effects (DG and PGG).

4.1. Normative treatment versus control

4.1.1. UG

Tables 1 & 2 and Figure 1 present evidence of significant training effects in the UG, with both proposers and responders behaving more in accordance with traditional economic rationality after the normative treatment than after the control. While normative-treatment and control pre-lesson mean offers are statistically indistinguishable (see Column 1 of Table 1), normative-treatment post-lesson mean offers are significantly lower than in control (across-subject comparison; see Column 2 of Table 1); the same pattern holds for minimum accepted offers (see Columns 1 & 2 of Table 2). Comparing pre- to post-lesson behavior, normative-treatment proposers reduce their offers by significantly more than control proposers ($4.06 versus $0.61, $p = 0.00$), and normative-treatment responders reduce their minimum accepted offers by significantly more than control responders ($2.02 versus $0.75, $p = 0.01$) (within-subject comparison; see Column 3 of Tables 1 & 2). The results are similar when we restrict to subjects who correctly answer the UG-comprehension-questions (pertaining to both the game's instructions and illustration) (see Column 6 of Tables 1 & 2).

Training effects are also evidenced by the proportion of subjects who change their behavior with treatment: 67% (22%) of normative-treatment (control) proposers reduce their offers, and 44% (19%) of responders reduce their minimum accepted offers (see Column 4 of Tables 1 & 2). Further, the proportion of normative-treatment (control) proposers who increase their offers is 2% (19%) (see Column 5 of Table 1). The proportion of control proposers who decrease and increase their offers are statistically indistinguishable (22% versus 19%).

4.1.2. PD

Table 3 presents evidence of training effects in the PD, with subjects behaving more in accordance with traditional economic rationality after the normative treatment than after the control. While mean defection rates are statistically indistinguishable for the normative treatment and control in both pre- and post-lesson comparisons (see Columns 1 & 2 of Table 3), the defection rate in the normative treatment increases by marginally significantly more than in control (0.23 versus 0.11, p = 0.06) (see Column 3 of Table 3). The results become fully significant when we restrict to subjects who correctly answer the PD-comprehension-questions (pertaining to both the game's instruction and illustration) (see Column 4 of Table 3).

4.1.3. DG

Table 4 presents evidence of significant training effects in the DG, with subjects behaving more in accordance with traditional economic rationality after the normative treatment than after the control. While mean offers are statistically indistinguishable before the normative treatment and control (see Column 1 of Table 4), offers are significantly lower after normative treatment than after control (see Column 2 of Table 4). Further, the normative-treatment dictators reduce their offers by more than control dictators ($1.78 versus $0.35, p = 0.00) (see Column 3 of Table 4). The results are similar when we restrict to subjects who correctly answer the DG-comprehension-questions (pertaining to the game's instruction) (see Column 6 of Table 4).

Training effects are also evidenced by the proportion of subjects who change their behavior with treatment: 45% (24%) of normative-treatment (control) dictators reduce their offers (see Column 4 of Table 4). The proportion of control proposers who decrease and increase their offers are statistically indistinguishable (24% and 18%).

4.1.4. PGG

Table 5 presents PGG behavior in the normative treatment and control. There is no evidence of training effects in the PGG using either across- or within-subject comparisons. The only exception is when we restrict to subjects who correctly answer the PGG-comprehension questions (pertaining to the game's instruction). Contributions to the public investment are reduced by marginally more in the normative treatment than in control ($1.05 versus $0.09, p = 0.09) (see Column 6 of Table 5).

4.2. Positive treatment versus control

We find almost no evidence of positive-treatment training effects (see Tables 6-10). The only differences that emerge suggest that subjects' post-lesson behavior is less in accordance with traditional economic rationality in the positive treatment than in control. In the UG, positive-treatment (control) responders increase (decrease) their minimum accepted offers on average by $0.10 ($0.75) (p = 0.06) (see Column 3 of Table 7); this difference becomes fully significant when we restrict to subjects who correctly answer the UG-comprehension-questions (pertaining to the game's instruction and illustration) (see Column 6 of Table 7). Also, the proportion of UG-responders whose minimum accepted offers increase is higher in the positive treatment than in control (0.25 versus 0.12, p = 0.02) (see Column 4 of Table 7). Further, the proportion of subjects whose PGG-contributions increase is marginally significantly higher in the positive treatment than in control (0.30 versus 0.19, p = 0.07) (see Column 4 of Table 10).

4.3. Subgroups

Below we present the results of subgroup analyses by prior exposure to economics instruction and by NPI score. We also conduct subgroup analyses by gender and family income and find no systematic differences; results not reported.[2]

4.3.1. Prior exposure to economics instruction

In the questionnaire, subjects are asked if they have ever studied game theory in any course. Of the 276 subjects, 72 answer affirmatively. Comparing the pre-lesson behavior of these subjects with those who answer negatively reveals a strikingly similar pattern to our normative-treatment training effects. Specifically, subjects who have studied game theory behave more in accordance with traditional economic rationality in the UG, PD, and DG than do those who have not; behavior in the PGG is statistically indistinguishable (see Table 11). Further, contributions to the SCU Fund are marginally significantly lower for subjects who have studied game theory than for those who have not.

Subjects are also asked if they had taken an economics course at SCU. Of the 276 subjects, 100 answer affirmatively. Comparing the pre-lesson behavior of these subjects with those who answer negatively reveals that subjects who have taken an SCU economics course behave more in accordance with traditional economic rationality in the PD, DG, and PGG than do those who have not; behavior in the UG is statistically indistinguishable (see Table 12). Further, contributions to the SCU Fund are significantly lower for subjects who have taken an SCU economics course than for those who have not.

It should be noted that the pre-lesson differences between those who have studied game theory and those who have not, and those who have taken SCU economics courses and those who have not, could be explained by either training or selection effects, as prior exposure to economics instruction is not random. Further, having taken an SCU economics course is likely correlated with being an economics major or a student in the business school, as SCU's business school requires that undergraduates take multiple economics courses.

One might expect that prior exposure to economics instruction (having studied game theory and/or taken an SCU economics course) might dampen the normative-treatment training effects. To examine whether this is the case we repeat the above subgroup analyses restricting the sample to subjects in the normative treatment. We pool subjects who have studied game theory and those who have taken an SCU economics course and compare their behavior to subjects with no prior exposure to economics instruction. Subjects with prior exposure to economics instruction are no less impacted by the normative treatment than are subjects without. Specifically, pre- to post-lesson differences in behavior are statistically indistinguishable for these subgroups, except for UG minimum accepted offers: normative-treatment training effects are larger for subjects with prior exposure to economics instruction than those without (see Table 13).

[2] In the questionnaire, subjects are asked to report their family's annual income on a nine-item response scale: Under \$20,000; \$20,00-\$40,000; \$40,000-\$60,000; \$60,000-\$80,000; \$80,000-\$100,000; \$100,000-\$150,000; \$150,000-\$200,000; \$200,000-\$500,000; Over \$500,000. The median response was \$100,000-\$150,000. The low-income (high-income) subgroup included subjects who reported income below \$100,000 (above \$150,000).

4.3.2. NPI

In the questionnaire, we include a 10-item subset of the 40-item NPI, which is used by social psychologists to measure narcissism (Raskin & Hall, 1979). The American Psychiatric Association defines narcissism as a "a pervasive pattern of grandiosity (in fantasy or behavior), need for admiration, and lack of empathy (APA, 1994)," and it has been linked to increased cheating (Von Hippel et al., 2005), romantic infidelity (Campbell & Foster, 2002), materialism, and impulsiveness (Rose, 2007).

Possible scores on our 10-item NPI are integer values from 0 (least narcissistic) to 10 (most narcissistic). The median score in our sample is 3. The low-narcissism (high-narcissism) subgroup includes subjects whose scores are less than or equal to 2 (greater than or equal to 4). Comparing the pre-lesson behavior of low- and high-narcissism subjects reveals that high-narcissism subjects behave more in accordance with traditional economic rationality in the UG (proposers only) and PD; behavior in other games is statistically indistinguishable (see Table 14). Further, contributions to the SCU Fund are significantly higher for low- than high-narcissism subjects. There is no evidence of the normative treatment impacting behavior differently for low- and high-narcissism subjects, except that normative-treatment training effects are marginally significantly lower for low- than high-narcissism subjects in the PD.

4.4. Efficiency and equity: an illustration

In this section, we consider the impact of the normative treatment on efficiency and equity in tasks 1 and 7 (pre- and post-lesson UG played as proposer). To measure efficiency, we use the proportion of the maximum possible social surplus that is actually realized (i.e., total subject payments divided by total possible subject payments within a task). This corresponds to the proportion of offers that are accepted in the UG, as accepted offers always generate $20 in social surplus and rejected offers always generate $0. Equity is measured as the Gini index of subject payments within a task and illustrated with the corresponding Lorenz curve. For each lesson-task pair, we create an "economy" that includes the proposers who receive the lesson and the responders with whom they are randomly matched. Specifically, when examining normative-treatment efficiency and equity in task 7, the economy comprises task-7 proposers receiving the normative lesson and their randomly-matched responders, regardless of the lesson received by the responder. Analogously, examining control efficiency and equity in task 7, the economy comprises task-7 proposers receiving the control lesson and their randomly-matched responders. The normative-treatment and control task-1 economies are constructed similarly.

Table 15 presents evidence that the normative lesson significantly reduces efficiency in task 7. The proportion of task-7 offers that are accepted is significantly lower in the normative treatment than in control (0.66 versus 0.85, $p = 0.00$) (see Column 2 of Table 15). The burden of the efficiency-loss is not equal, with responders bearing it entirely on average. The normative-treatment mean responder payment is significantly lower than in control ($3.64 versus $7.27, $p = 0.00$) (see Column 4 of Table 15). In contrast, the normative-treatment mean proposer payment is statistically indistinguishable from control ($9.46 versus $9.73) (see Column 3 of Table 15). It warrants mention that while normative-treatment proposers are more likely to receive a zero payment, their non-zero payments are larger than in control. Lastly, there is no evidence of pre-lesson efficiency differences: the proportion of normative-treatment and control task-1 offers that are accepted are statistically indistinguishable (0.82 versus 0.87) (see Column 1 of Table 15).

Further, the evidence suggests that the normative lesson reduces task-7 equity. The normative-treatment Gini index is significantly greater than in control (0.56 versus 0.29, p = 0.00) (see Column 6 of Table 15);[3] Figure 2a depicts the corresponding Lorenz curves. In contrast, the normative-treatment and control task-1 Gini indexes are statistically indistinguishable (0.31 versus 0.26) (see Column 5 of Table 15); Figure 2b depicts the corresponding Lorenz curves. In sum, the normative lesson not only promotes behavior that is more in accordance with traditional economic rationality, but it also reduces both efficiency and equity in UG-economies.

5. Discussion

In a laboratory experiment, we demonstrate that a brief normative economics lesson moves behavior toward traditional economic rationality (i.e., selfishness). In the UG, normative-treatment mean offers decrease by 46%, and minimum accepted offers decrease by 42%; in the PD, defection rates increase by 39%; and in the DG, mean offers decrease by 36%. In contrast, control mean offers do not significantly decrease in either the UG or DG; UG minimum accepted offers decrease by 15%; and PD defection rates increase by 17%. Those with prior exposure to economics instruction enter the study with higher levels of selfishness than others, and still the normative lesson is at least as impactful for them. Lastly, we use the UG to illustrate that the normative lesson can reduce both efficiency and equity.

Our experiment enables the identification of specific content that may contribute to the selfishness that economics students exhibit in the extant literature. Specifically, the normative lesson relays the assumptions of self-interest and strategic considerations. It does so without mention of traditional economic theory stipulating that self-interest leads to efficiency; as such, the lesson's impact should not be attributable to the belief that acting in one's self-interest will lead to the greater good. Thus, the theory that self-interest leads to efficiency is not necessary to make subjects more selfish. Further, in the UG as responder and DG, strategic considerations should be irrelevant, as decisions are made in the absence of simultaneous or subsequent decisions by other players; still we observe normative-treatment training effects, indicating that the assumption of self-interest is sufficient and strategic considerations not necessary.

The normative lesson seems to induce in subjects a privileging of own-payoff considerations over all other considerations. For example, subjects may become more likely to privilege own-payoff considerations over other-regarding preferences: in the UG, low normative-treatment offers may be due to the privileging of own-payoff considerations over social surplus maximizing. That said, the low normative-treatment minimum accepted offers cannot be explained by this, as reduced consideration of social surplus maximizing would suggest higher minimum accepted offers. Instead, low normative-treatment minimum accepted offers may reflect the privileging of own-payoff over emotional considerations, as high minimum accepted offers have been shown to reflect costly punishment driven by anger over perceived unfairness. Xiao & Houser (2005) find that UG-responders who are given the opportunity to express their

[3] Standard errors are calculated using bootstrapping (100 repetitions).

emotions in writing are more likely to forego costly punishment and accept low offers. Further, assumptions relayed in the normative lesson may impact behavior by reducing inhibitions against acting selfishly. Using charitable donations and the DG, Exley (2016) finds that selfishness is exacerbated when there is an available excuse for it.

It is of interest to note that normative-treatment training effects are not identified in the questionnaire-items regarding attitudes toward fairness. While this may be due to training effects having worn off or to the items' inadequate sensitivity, it could suggest that the items are orthogonal to the lessons. That is, subjects may be able to behave more selfishly while maintaining similar attitudes toward fairness. In the same vein, Mazar et al. (2008) provides experimental evidence that subjects commonly cheat but only to the extent that they can still maintain the belief that they are not cheaters.

If economics instruction does indeed change economics students' behavior, we are left with the question of how to teach economics in a way that does not undermine the greater good. For one, instructors should be aware that their authority in the pedagogic relationship may influence students' interpretation of lessons. An important critique of laboratory experimentation leveled by economists is that of experimenter demand effects, whereby subjects behave in accordance with what they believe to be the demands of the authoritative experimenter. Although the scope for such effects certainly exists in the classroom, no such critique has been leveled at analogous "instructor demand effects." We believe that instructor demand effects may bias students toward understanding assumptions as prescriptive or axiomatic. To minimize the possibility of such misunderstandings, instructors could, for example, explain that assumptions may be violated and provide illustrations. Further, instructors could discuss theoretical ramifications of assumption-violation and highlight the distinction between model-based theoretical predictions and real-world phenomena.

Lastly, it may not be enough to discuss the nuances of the assumptions, definitions, and limitations of our models: instruction needs curating. Consider the curatorial status quo of introductory game-theory instruction. The single-shot interaction (with no history, nor future) is the benchmark and is followed by finitely- and then infinitely-repeated interactions. This structure, when applied to the PD--often the first strategic interaction introduced in game theory--may lead students to conclude that defection, rather than cooperation, is prescriptive. In real-world applications, collaborating agents likely know--and have collateral against--each other, or are part of an overarching network with its own codes of conduct that give rise to reputational concerns. Applying single-shot self-interest to this scenario requires the subsuming of these considerations and leads to a dominant strategy of defection. The same holds in the finitely-repeated interaction, as backward induction relies upon framing the final repetition as a single-shot interaction with no relevant history and applies this logic recursively to all repetitions. Only in the infinitely-repeated interaction is there scope to consider--albeit abstractly--the concerns listed above (e.g., reputation and punishment) and hence a broader notion of self-interest in which cooperation can emerge. The choice of benchmark, sequencing, and even the terminology of "infinity" may obscure that the infinitely-repeated interaction may be most applicable. Students may believe that the concerns incorporated in the infinitely-repeated interaction, not to mention others' well-being, should not normally be taken into consideration.

More broadly, economics students may take the model of narrow self-interest they have learned and attempt to apply it in their everyday activities, at work, or in the voting booth.
In a period of rapid population growth, resource-intensive lifestyles, and increasingly conspicuous climate change, it is all the more important that students leave the classroom with a balanced notion of self-interest that better aligns with the resolution of collective-action problems.

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Table 1. UG proposer offers, normative treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Proportion decrease (4)	Proportion increase (5)	Change^ (6)
Normative	$8.76	$4.70	-$4.06	0.67	0.02	-$4.22
n = 87	($0.40)	($0.43)	($0.43)	(0.05)	(0.02)	($0.53)
Control	$8.37	$7.76	-$0.61	0.22	0.19	-$0.56
n = 100	($0.37)	($0.35)	($0.39)	(0.04)	(0.04)	($0.32)
Difference	$0.39	***-$3.06***	***-$3.45***	***0.45***	***-0.17***	***-$3.66***
	($0.54)	($0.56)	($0.58)	(0.07)	(0.04)	($0.62)
	{0.473}	{0.000}	{0.000}	{0.000}	{0.000}	{0.000}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the UG (normative n = 60; control n = 59). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 2. UG responder minimum accepted offers, normative treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Proportion decrease (4)	Proportion increase (5)	Change^ (6)
Normative	$4.82	$2.79	-$2.02	0.44	0.09	-$2.15
n = 87	($0.41)	($0.34)	($0.38)	(0.05)	(0.03)	($0.45)
Control	$5.05	$4.30	-$0.75	0.19	0.12	-$0.69
n = 100	($0.46)	($0.40)	($0.33)	(0.04)	(0.03)	($0.33)
Difference	-$0.23	***-$1.51***	**-$1.27**	***0.25***	-0.03	**-$1.46**
	($0.62)	($0.53)	($0.50)	(0.07)	(0.05)	($0.56)
	{0.709}	{0.005}	{0.011}	{0.000}	{0.539}	{0.010}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the UG (normative n = 60; control n = 59). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 3. PD defection rates, normative treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Change^ (4)
Normative	0.59	0.82	0.23	0.25
n = 87	(0.05)	(0.04)	(0.05)	(0.06)
Control	0.63	0.74	0.11	0.09
n = 100	(0.05)	(0.04)	(0.04)	(0.04)
Difference	-0.04	0.08	0.12	**0.16**
	(0.07)	(0.06)	(0.06)	(0.07)
	{0.543}	{0.216}	{0.055}	{0.015}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the PD (normative n = 71; control n = 88). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 4. DG offers, normative treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Proportion decrease (4)	Proportion increase (5)	Change^ (6)
Normative	$4.89	$3.10	-$1.78	0.45	0.10	-$1.81
n = 87	($0.48)	($0.43)	($0.40)	(0.05)	(0.03)	($0.42)
Control	$5.07	$4.72	-$0.35	0.24	0.18	-$0.28
n = 100	($0.47)	($0.49)	($0.31)	(0.04)	(0.04)	($0.34)
Difference	-$0.18	**-$1.62**	***-$1.43***	***0.21***	-0.08	***-$1.53***
	($0.67)	($0.66)	($0.50)	(0.07)	(0.05)	($0.53)
	{0.783}	{0.016}	{0.005}	{0.003}	{0.139}	{0.005}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the DG (normative n = 83; control n = 89). Bold indicates statistical significance at 0.05 level; bold italics indicate statistical significance at the 0.01 level.

Table 5. PGG contributions, normative treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Proportion decrease (4)	Proportion increase (5)	Change^ (6)
Normative	$6.99	$5.99	-$1.00	0.28	0.20	-$1.05
n = 87	($0.64)	($0.67)	($0.47)	(0.05)	(0.04)	($0.44)
Control	$6.83	$6.45	-$0.38	0.24	0.19	-$0.09
n = 100	($0.63)	($0.64)	($0.35)	(0.04)	(0.04)	($0.37)
Difference	$0.16	-$0.46	-$0.62	0.04	0.01	-$0.96
	($0.90)	($0.93)	($0.58)	(0.06)	(0.06)	($0.57)
	{0.860}	{0.620}	{0.287}	{0.578}	{0.926}	{0.094}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the PGG (normative n = 76; control n = 87). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 6. UG proposer offers, positive treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Proportion decrease (4)	Proportion increase (5)	Change^ (6)
Positive	$8.09	$7.83	-$0.26	0.28	0.24	-$0.10
n = 89	($0.37)	($0.33)	($0.27)	(0.05)	(0.05)	($0.31)
Control	$8.37	$7.76	-$0.61	0.22	0.19	-$0.56
n = 100	($0.37)	($0.35)	($0.39)	(0.04)	(0.04)	($0.32)
Difference	-$0.28	$0.07	$0.35	0.06	0.05	$0.46
	($0.52)	($0.49)	($0.49)	(0.06)	(0.06)	($0.44)
	{0.594}	{0.884}	{0.470}	{0.336}	{0.443}	{0.299}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the UG (positive n = 62; control n = 59). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 7. UG responder minimum accepted offers, positive treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Proportion decrease (4)	Proportion increase (5)	Change^ (6)
Positive	$4.20	$4.30	$0.10	0.22	0.25	$0.50
n = 89	($0.39)	($0.36)	($0.31)	(0.04)	(0.05)	($0.36)
Control	$5.05	$4.30	-$0.75	0.19	0.12	-$0.69
n = 100	($0.46)	($0.40)	($0.33)	(0.04)	(0.03)	($0.33)
Difference	-$0.85	$0.00	$0.85	0.03	**0.13**	**$1.19**
	($0.61)	($0.54)	($0.45)	(0.06)	(0.06)	($0.48)
	{0.167}	{0.995}	{0.062}	{0.559}	{0.023}	{0.015}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the UG (positive n = 62; control n = 59). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 8. PD defection rates, positive treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Change^ (4)
Positive	0.57	0.73	0.16	0.15
n = 89	(0.05)	(0.05)	(0.05)	(0.05)
Control	0.63	0.74	0.11	0.09
n = 100	(0.05)	(0.04)	(0.04)	(0.04)
Difference	-0.06	-0.01	0.05	0.06
	(0.07)	(0.06)	(0.06)	(0.06)
	{0.427}	{0.881}	{0.431}	{0.348}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the PD (positive n = 80; control n = 88). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 9. DG offers, positive treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Proportion decrease (4)	Proportion increase (5)	Change^ (6)
Positive	$5.36	$5.61	$0.25	0.25	0.25	$0.12
n = 89	($0.46)	($0.43)	($0.37)	(0.05)	(0.05)	($0.38)
Control	$5.07	$4.72	-$0.35	0.24	0.18	-$0.28
n = 100	($0.47)	($0.49)	($0.31)	(0.04)	(0.04)	($0.34)
Difference	$0.29	$0.89	$0.60	0.01	0.07	$0.40
	($0.66)	($0.66)	($0.48)	(0.06)	(0.06)	($0.51)
	{0.661}	{0.181}	{0.216}	{0.909}	{0.261}	{0.429}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the DG (positive n = 82; control n = 89). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 10. PGG contributions, positive treatment versus control

	Pre-lesson (1)	Post-lesson (2)	Change (3)	Proportion decrease (4)	Proportion increase (5)	Change^ (6)
Positive	$6.99	$7.39	$0.40	0.18	0.30	$0.28
n = 89	($0.60)	($0.63)	($0.35)	(0.04)	(0.05)	($0.38)
Control	$6.83	$6.45	-$0.38	0.24	0.19	-$0.09
n = 100	($0.63)	($0.64)	($0.35)	(0.04)	(0.04)	($0.37)
Difference	$0.16	$0.94	$0.78	-0.06	0.11	$0.37
	($0.87)	($0.90)	($0.50)	(0.06)	(0.06)	($0.53)
	{0.856}	{0.298}	{0.118}	{0.314}	{0.071}	{0.486}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). ^ Drop subjects who did not correctly answer quiz and lesson questions regarding the PGG (positive n = 75; control n = 87). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 11. Pre-lesson decisions, by prior game theory

	UG offer (1)	UG min accept offer (2)	PD defection (3)	DG offer (4)	PGG contribution (5)	SCU Fund contribution (6)
Prior game theory	$7.04	$3.97	0.71	$3.72	$6.26	$1.11
n = 72	($0.48)	($0.48)	(0.05)	($0.45)	($0.76)	($0.16)
No prior game theory	$8.88	$4.96	0.56	$5.59	$7.17	$1.50
n = 204	($0.23)	($0.28)	(0.03)	($0.32)	($0.41)	($0.11)
Difference	***-$1.84***	-$0.99	**0.15**	***-$1.87***	-$0.90	-$0.39
	($0.49)	($0.56)	(0.07)	($0.61)	($0.82)	($0.20)
	{0.000}	{0.078}	{0.026}	{0.002}	{0.270}	{0.053}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 12. Pre-lesson decisions, by prior SCU economics course(s) taken

	UG offer (1)	UG min accept offer (2)	PD defection (3)	DG offer (4)	PGG contribution (5)	SCU Fund contribution (6)
Taken econ course(s) n = 100	$8.37	$4.64	0.71	$4.21	$5.80	$0.87
	($0.38)	($0.41)	(0.05)	($0.41)	($0.53)	($0.11)
No econ course n = 176	$8.42	$4.74	0.53	$5.61	$7.57	$1.70
	($0.26)	($0.31)	(0.04)	($0.35)	($0.47)	($0.12)
Difference	-$0.05	-$0.10	***0.18***	**-$1.40**	**-$1.77**	-$0.83
	($0.45)	($0.51)	(0.06)	($0.56)	($0.74)	($0.18)
	{0.912}	{0.848}	{0.004}	{0.012}	{0.017}	{0.000}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 13. Normative-treatment training effects, by prior exposure to economics instruction

	UG offer (1)	UG min accept offer (2)	PD defection (3)	DG offer (4)	PGG contribution (5)
Change					
Prior econ exposure n = 43	-$4.65	-$2.84	0.23	-$1.63	-$0.84
	($0.62)	($0.58)	(0.07)	($0.46)	($0.61)
No prior exposure n = 44	-$3.48	-$1.23	0.23	-$1.93	-$1.16
	($0.60)	($0.47)	(0.08)	($0.65)	($0.72)
Difference	-$1.17	**-$1.61**	0.01	$0.30	$0.32
	($0.86)	($0.74)	(0.10)	($0.80)	($0.95)
	{0.176}	{0.032}	{0.959}	{0.706}	{0.736}

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 14. Normative-treatment training effects, by narcissism

	UG offer (1)	UG min accept offer (2)	PD defection (3)	DG offer (4)	PGG contribution (5)	SCU Fund contribution (6)
Pre-lesson						
High narcissism n = 122	$7.70 ($0.34)	$4.45 ($0.34)	0.66 (0.04)	$4.91 ($0.42)	$6.48 ($0.51)	$1.20 ($0.12)
Low narcissism n = 115	$8.99 ($0.32)	$4.95 ($0.42)	0.50 (0.05)	$5.63 ($0.41)	$7.70 ($0.58)	$1.63 ($0.15)
Difference	***-$1.29*** ($0.47) {0.006}	-$0.50 ($0.54) {0.357}	***0.16*** (0.06) {0.013}	-$0.72 ($0.58) {0.222}	-$1.22 ($0.77) {0.115}	**-$0.42** ($0.19) {0.030}
Change (normative only)						
High narcissism n = 44	-$3.95 ($0.60)	-$1.73 ($0.51)	0.18 (0.06)	-$1.66 ($0.58)	-$1.23 ($0.55)	
Low narcissism n = 38	-$4.11 ($0.70)	-$2.00 ($0.59)	0.37 (0.08)	-$2.13 ($0.62)	-$0.76 ($0.87)	
Difference	$0.15 ($0.92) {0.870}	$0.27 ($0.78) {0.727}	***-0.19*** (0.10) {0.058}	$0.47 ($0.85) {0.578}	-$0.46 ($1.01) {0.646}	

Standard error in parenthesis. p-value for difference of means between treatment and control group in brackets (two-way test). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Table 15. UG efficiency and equity, normative treatment versus control

	Pre-lesson efficiency (1)	Post-lesson efficiency (2)	Post-lesson proposer payment (3)	Post-lesson responder payment (4)	Pre-lesson Gini index (5)	Post-lesson Gini-index (6)
Normative	0.82	0.66	$9.46	$3.64	0.31	0.56
n = 87	(0.04)	(0.05)	($0.81)	($0.44)	(0.03)	(0.03)
Control	0.87	0.85	$9.73	$7.27	0.26	0.29
n = 100	(0.03)	(0.04)	($0.50)	($0.42)	(0.02)	(0.03)
Difference	-0.05	-0.19	-$0.27	***-$3.63***	0.05	0.27
	(0.05)	(0.06)	($0.93)	($0.61)	(0.03)	(0.04)
	{0.312}	{0.002}	{0.771}	{0.000}	{0.177}	{0.000}

Bootstrapped standard error in parenthesis (100 repetitions). p-value for difference of means between treatment and control group in brackets (two-way test). **Bold** indicates statistical significance at 0.05 level; ***bold italics*** indicate statistical significance at the 0.01 level.

Figure 1a. Pre- and post-lesson UG offers, normative treatment

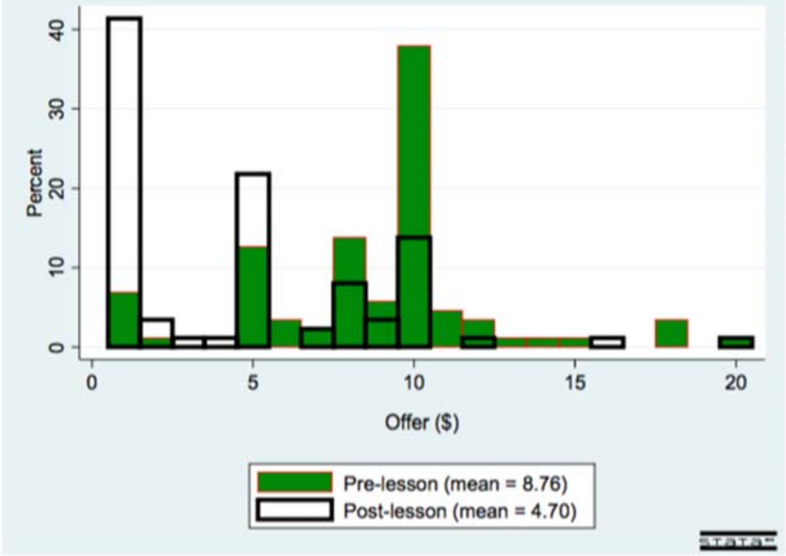

Figure 1b. Pre- and post-lesson offers, control

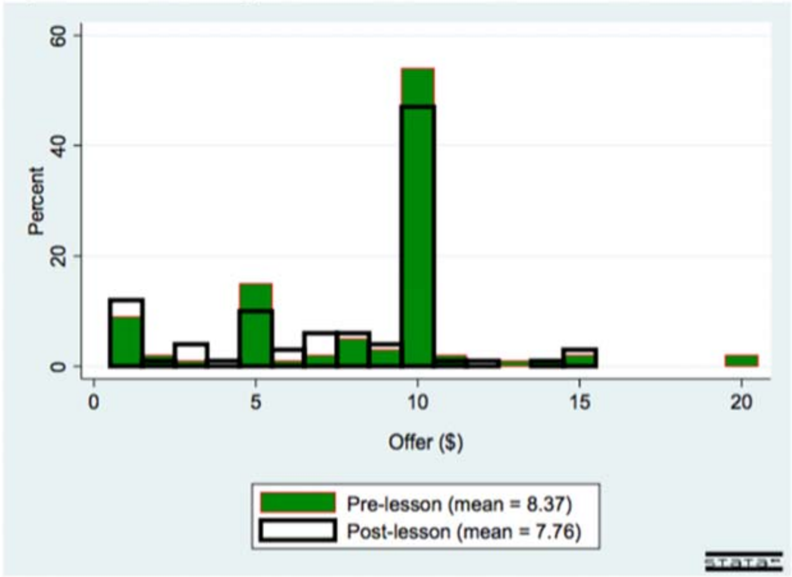

Figure 2a. UG post-lesson Lorenz curve, normative treatment versus control

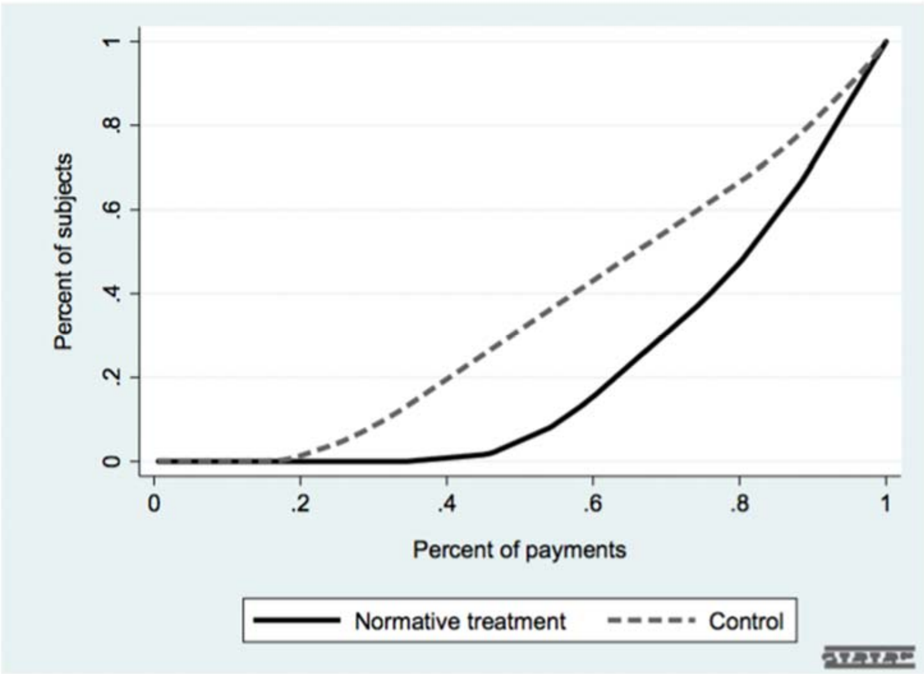

Figure 2b. UG pre-lesson Lorenz curve, normative treatment versus control