

Does economics make you selfish?

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Abstract

It is widely held that studying economics makes you more selfish and politically conservative. Few studies, however, can disentangle the impact of economics education from selection effects. We use a difference-in-differences strategy to estimate the effect of four different intermediate microeconomics courses on students' experimentally elicited social preferences and beliefs about others, and on their policy opinions. We find no discernible effect of studying economics (whatever the course content) on self-interest or beliefs about others' self-interest. Results on policy preferences also point to little effect, except that economics may make students relatively more favorable to restricting immigration.

1 Introduction

The concern that 'economics makes you selfish' is widely held. From opinion pieces in *The New York Times* (Bauman 2011) to popular broadcasts on the US National Public Radio (NPR) (Vedantam 2017) and the BBC (Stafford 2013), mass media has popularized the idea that studying economics has a detrimental effect on generosity and cooperativeness. Similarly, the argument goes, studying economics may also promote policy opinions typically considered conservative (Colander 2005; O'Roark and Wood 2011).

Some existing evidence suggests that indeed economics students behave more selfishly and less cooperatively in a variety of economic games, and donate less to charities (Marwell and Ames 1981; Carter and Irons 1991; Frank, Gilovich, and Regan 1993; Wang, Malhotra, and Murnighan 2011). In contrast, studies looking at real-world donations and using a difference-in-differences design to remove selection effects have tended to find little effect of studying economics (Frey and Meier (2003)). An important unresolved question is to what extent the evidence about economics students reflects selection into economics, and to what extent it reflects a genuine effect of economics education. That is, do students who choose to study economics start off as more selfish (selection) or does economics as a discipline train students to behave self-interestedly and to consider the self-interest of others as normatively acceptable (framing and indoctrination)?

To interrogate this behavior more deeply and discern the impact of economics education from selection and framing effects, we use a transparent difference-in-differences strategy. Specifically, we estimate the effect of a one-semester intermediate microeconomics course on students' social preferences and policy opinions. We administer an online survey at the beginning and at the end of the semester to five classes – four intermediate microeconomics classes and one non-economics class as a control. We use a Trust Game (TG), a triple Dictator Game (DG) with charities in the role of receiver, and two belief-elicitation questions about subjects' expectations on the behavior of others in the same games. We use these games to extract measures of deviation from self-interest (DFSI), which is the extent to which individual behavior deviates from the (Nash-equilibrium) prediction of self-interest. We use the belief-elicitation questions to measure the extent to which people expect others to deviate from self-interest. We also include questions eliciting students' policy preferences on topics such as economic and environmental regulations, trust in government, market efficiency, and immigration.

Our sample includes undergraduate students enrolled at [Institution Blinded for Review]. The intermediate microeconomics courses include two courses following a standard curriculum (*Conventional I* and *II*), a course that exposes students substantially to externalities, contractual incompleteness, and social preferences (*Post-Walrasian*),¹ and a fourth course that is predominantly conventional but with some exposure to social preferences (*Conventional plus social preferences*). The variation in microeconomics curricula allows us to study different effects based on the approach to economics being taught.

We find that a one-semester intermediate microeconomics course has little effect on experimental measures of social preferences and expectations about other people's social preferences. Our estimates of the effect of an intermediate microeconomics course on measures of altruism and reciprocity are close to zero and stable across curricula.

Results about policy preferences are more nuanced. We find no overall effect on the political orientation of students' policy preferences, nor on their more specific views on market efficiency, government intervention, and environmental regulations. The one exception is immigration, where economics seems to make students *relatively* more restrictionist than non-economics students, though still not on average restrictionist. Similar to the experimental measures, the policy results differ little based on economics course content.

Our paper is the first, to the best of our knowledge, to provide difference-in-differences experimental evidence on the effect of studying economics on students' social preferences, beliefs about other people's social preferences, and policy opinions. We are also the first to try to assess the effects of differing course content on behavior and attitudes. Our results contrast with several papers arguing that studying economics makes people selfish.

¹We adopt the language of Stiglitz (1993) and Bowles and Gintis (2000) in conceiving of an economics course that focuses on contractual incompleteness and strategic interaction as Post-Walrasian, whereas Walrasian economics presume information is freely available and interactions are parametrically competitive rather than strategic.

Our results help to reconcile conflicts in prior evidence about whether economics students start off as more selfish (selection), learn to behave selfishly and conservatively (framing and indoctrination), or are encouraged to behave selfishly when shown particular economic content (priming). We shed light on these conflicts because we use experimental outcomes while at the same time employing a difference-in-differences approach and focusing on semester-long courses. Our results suggest that what accounts for previous contrasting results is not the use of real-world as opposed to experimental outcomes, but the use of difference-in-differences as opposed to cross-sectional designs, and the focus on longer-term exposure as opposed to priming. The null effects we find suggest that previous cross-sectional studies finding substantial impacts might be affected by selection bias, while studies that have randomly assigned a brief economics class might capture framing effects (context-dependent preferences) rather than lasting changes in preferences.

2 Economics and Preferences: Theory and Evidence

Why worry that studying economics might cause students to behave selfishly?

In most undergraduate microeconomics courses self-interest is taught as the standard for human maximizing behavior. Accordingly, preferences are exogenous and self-regarding, and agents act on the belief that everyone else is similarly selfish. Economics students may tend to behave in accordance with the theory they study, because of exposure and ‘moral wiggle room’ effects.

Various evidence suggests that repeated exposure to an idea or behavior may propagate that idea or behavior. First, consider the powerful effect of mere exposure. By exposure we mean the introduction and repeated interaction with an idea, such as a student who learns about self-interest in economics and is repeatedly shown the many ways in which rational, self-interested actors behave (and is not similarly exposed to other ways in which people – or other relevant economic agents – might behave). The effect of exposure on social learning has been well documented (Zajonc 1965; Murphy and Zajonc 1993; Murphy, Monahan, and Zajonc 1995; Birch and Marlin 1982). Even the sight of money and the discussion of coins (rather than non-monetary objects) in a recent experiment induced children to behave in less pro-social ways and to behave less helpfully toward others (Gasiorowska, Zaleskiewicz, and Wygrab 2012).

Second, given what they learn, economics students may be provided moral wiggle room for what they would otherwise consider immoral behavior, and a way to reconcile their own self-interest with a positive self-concept (Mazar, Amir, and Ariely 2008). How might learning microeconomics achieve these results? In microeconomics, students learn to demonstrate that in a perfectly competitive market the non-cooperative pursuit of self-regarding preferences results in a Pareto-efficient equilibrium. This may provide a moral and social justification for self-interested behavior. A student who believes that self-interest promotes efficiency will be able to maintain a positive and pro-social self-perception while at the same time acting selfishly, when she would otherwise see self-interest as immoral or contrary to social norms (Gino, Ayal, and Ariely 2013; Shalvi et

al. 2015; Dana, Weber, and Kuang 2007). The moral wiggle room provided by economic theory would reinforce the exposure effect.

Several empirical papers argue that studying economics makes people selfish. These studies find that economics students are more likely to free ride in Public Good Games (Marwell and Ames 1981), offer and accept lower amounts in Dictator Games and Ultimatum Games (Carter and Irons 1991; Wang, Malhotra, and Murnighan 2011) and defect in Prisoners' Dilemma games (Frank, Gilovich, and Regan 1993). In addition to this, Frank, Gilovich, and Regan (1993) find that economics students donate less to charities. Other work provides survey evidence that economics students report a more favorable attitude towards greed and self-interest (Rubinstein 2006; Wang, Malhotra, and Murnighan 2011; Frank, Gilovich, and Regan 1993). Regarding policy preferences, the idea that studying economics would make students more conservative goes back at least to Stigler (1959). More recent evidence in favor of this view are, for instance, O'Roark and Wood (2011) and Colander (2005).²

Other studies, however, reach different conclusions. For example, Frey and Meier (2003) study (real-world) giving behavior and observe that economics students donate less than others to start with (before taking any economics class),³ and this difference remains stable after having taken economics courses. This result suggests that any differences in the extent of self-interested behavior between economics and non-economics students may be driven by selection, rather than by a causal effect of economics education. Bauman and Rose (2011) use a similar design and confirm a substantial selection effect, while obtaining more nuanced results regarding learning effects. Other studies using real-world (as opposed to experimental) outcomes even question the very idea that economists behave on average more selfishly. For example, Yezer, Goldfarb, and Poppen (1996) use a 'lost letter' experiment: envelopes containing \$10 in cash and with an address written on them were left in classrooms. In their sample, economics students are actually *more* likely to return the letter. Laband and Beil (1999) find that professional economists are *less* likely to cheat on their professional association dues, compared to sociologists and political scientists.

Considering priming versus selection, Ifcher and Zarghamee (2018) address selection bias by randomly assigning experimental subjects to one of three conditions – Normative, Positive, and Control – immediately before performing incentivized games and find that the normative description including language "(1) that all individuals are self-interested and (2) that all individuals attempt to maximize their payments" shifts behavior towards self-interest. In this case, the open question is whether this result reflects a framing effect or a lasting change in preferences. Relatedly, Molinsky, Grant, and Margolis (2012) show that unobtrusively priming economic schema just before delivering bad news to an

² Specifically, Colander (2005) reports that "10 percent of first-year [economics] students considered themselves conservative; by the fourth and fifth year, this number had risen to 23 percent. There was also a large drop by year in students who considered themselves radical; that percentage fell from 13 percent of first year students to only 1 percent of fourth-year and higher students."

³ However, when breaking down this statistics by field, Frey and Meier (2003) find that it is only students of business economics who donate less, while the donations of other economics students are broadly in line with those of non-economics students.

individual causes them to express less compassion to those in need.

Existing studies using experimental outcomes like those above are either cross-sectional or analyze the framing effect of brief exposure to economic ideas. These tend to support the idea that ‘economics makes you selfish.’ Studies using real-world or observational outcomes often reach different conclusions. Frey and Meier (2003) and Bauman and Rose (2011) are closer to ours in focusing on the longer-term impact of economics courses while also using some type of difference-in-differences design to control for selection. There are three main differences between our paper and these two studies. First, we look at a variety of experimental outcomes, including donations but also strategic games, while their focus is only on (out-of-lab) donations. Our focus on (among others) experimental outcomes allows better comparability with the previous literature which finds significant differences in experimental outcomes between economics’ and other students. Second, we are able to estimate also effects on policy preferences and beliefs about other people’s behavior. Third, our design allows us to test for different effects based on course content among economics courses.

3 Research design

We administered an online survey at the beginning and at the end of the semester to a group of undergraduate students enrolled in four intermediate microeconomics courses and one non-social science course. The survey includes questions on personal characteristics and policy preferences, and four economic games with real monetary stakes – a Trust Game (TG), a Triple Dictator Game with charities (DG), and two belief elicitation questions about the behavior of others in the same games.⁴ We use these to obtain individual-level measures of ‘deviation from self-interest’ due to generosity and reciprocity, and beliefs about the social preferences of others. Participants completed the survey at a time of their convenience, from a link we sent in our invitation email.

3.1 Sample and courses

Students from four different intermediate microeconomics courses and from one course outside of the social sciences comprise our sample. The course *Nutrition and Metabolism* serves as a control non-economics course. The economics courses vary: two courses (Conventional I and Conventional II) are fairly standard intermediate microeconomics courses using Pindyck and Rubinfeld (2012) and Perloff (2011); a third (*Post Walrasian*) course

⁴Consistent with Ashraf, Bohnet, and Piankov (2006), we use a triple dictator game for comparison of the behavior across the dictator game and the trust game. That is, the proposer in the triple dictator game will allocate 3 times the endowment between herself and the charity, which is consistent with what the proposer will do in the trust game. Similarly, the trustee in the trust game makes a choice using the strategy method of three times the total endowment that the proposer would give them, thus allocating an amount similar to what the dictator is deciding on. Our choice to multiply the dictator amounts given is also consistent with work on charitable giving with “matching subsidies”, such as Knowles and Servátka (2015).

uses Bowles and Halliday (2020) and focuses on strategic interactions and contractual incompleteness alongside standard topics of optimization (crucially it contains behavioral experiments and models of social preferences); finally, the fourth course (*Conventional plus social preference*), is an online course using Frank (2008).

To clarify why we think different economics courses could lead to different outcomes, we present Figure 1. In Figure 1 we show the location of the textbooks used in the intermediate microeconomics courses under investigation in a simplex covering three important and over-arching ideas in modern economics (Bowles et al. (2019) refer to these ideas as “meta-topics” as they are aggregations of underlying sets of topics).⁵ The location of a given textbook within the simplex identifies a book’s relative emphasis. For example, Pindyck and Rubinfeld (2012) and Perloff (2011) place their emphasis on market structure and competition. Varian (2014), by way of contrast, puts greater emphasis on individual constrained maximization, whereas Bowles and Halliday (2020) places a greater weight on strategic interactions, contractual incompleteness, and bargaining.

With respect to the content of each book, one can also compare the coverage of how economists conceive of and teach preferences. In each book, a model of constrained utility maximization is the main model of individual decision-making. Frank (2008) and Bowles and Halliday (2020) teach standard self-interested preferences while also explaining the evidence for alternatives to self-interest, such as altruism, difference aversion, conditional cooperation, and so on. Both books explain the evidence from results in experimental economics that underlie the alternative models of preferences.

3.2 Experimental design

The survey administered to our sample includes standard demographic and academic information, questions eliciting students’ policy opinions, incentivized choice experiments (economic games), and incentivized belief-elicitation questions regarding a subject’s beliefs about the behavior of others in the same games. The wording of all the policy questions is available in Appendix E, with topics covering immigration, the functioning of markets, government regulation, and climate change.

The survey asked participants to play four incentivized games: a Triple Dictator Game (DG), a Trust Game (TG), and two belief-elicitation tasks about the behavior of other participants in these games. The order in which the two games were presented was randomized: each participant was equally likely to play the DG first or the TG first. After completion of the survey, we randomly selected one of the four games for payment.

In the Triple Dictator Game (DG) with charities the respondent is allocated \$10 and given the possibility to donate a portion to a local non-profit charitable organization from a list of three. The list included non-partisan, non-controversial, and apolitical organizations. Any amount donated would be tripled, consistent with Ashraf, Bohnet, and Piankov (2006).

⁵ Specifically, Bowles et al. (2019) use topic modeling – a machine learning algorithm used to analyze texts (Gentzkow, Kelly, and Taddy 2019) – to identify three important meta-topics that are at the heart of microeconomics research.

We then ask the subject to guess the average contribution of the other participants. The subject’s pay-off depended on how close they were to the actual average: their payoff was \$12 minus the absolute value of the guessing error. The guessing error is defined as the difference between a subject’s guess and the average donation of all other respondents.

In the Trust Game (TG), participants are anonymously and randomly paired. Within each pair, one player is randomly assigned the role of first mover, while the other is the second mover. The first mover is allocated \$10. She must transfer a share of this \$10 of her choice to the second mover (the amount sent may be zero if the first mover chooses so). The first player is also informed that whatever she sends will be tripled by the experimenter. Once the first mover chooses a value, the experimenter will triple it and transfer it to the second mover. The second mover is then told to make a similar choice: transfer some share of the now-tripled money back to the first mover (the amount given back may be zero, should the second mover choose so).⁶

Subjects played the games asynchronously with matching occurring later. Each subject specified how they would play both roles (first mover and second mover) and we used the strategy method for the case of the choices as the second mover. Each participant was therefore asked to specify (1) how much they would send as first mover; (2) how much they would send back as second mover for each possible transfer of the first mover in whole numbers.

To determine payoffs, each participant was then (after completion of the surveys) randomly paired with another participant. In each pair, one was randomly selected as first-mover and the other as second-mover. We performed the random matching of participants one week after the opening of the survey (including all who had responded within the first week), and then at the end of the survey (including all participants who filled the survey during the second week). In this way, we were able to guarantee that each participant would receive her payoff within one week after survey completion. Subjects also performed a belief-elicitation task, similar to the one regarding behavior in the DG and with the same payoff rule, with respect to Player 1’s behavior in the Trust Game.⁷

Respondents also stated their best guesses about the average responses as Player 2 of all other participants, for each possible amount received from Player 1. Payoff was then based on the accuracy of their guesses. A subject’s payoff is \$12 minus the subject’s average guessing error. To define the average guessing error, we take the absolute value of the difference between the subject’s guess and the average amount transferred as Player 2 by all other players, for each possible amount received from Player 1, and then take an average across all possible amounts received from Player 1.

⁶We use the standard trust game as investigated in Berg, Dickhaut, and McCabe (1995). We use an incentivized online survey measure (with choices realized ex-post); recent evidence suggests that these measures are strongly correlated, even if they were not incentivized in the manner we provide (Glaeser et al. 2000; Aksoy et al. 2018).

⁷While we included this belief-elicitation question in the survey for symmetry, we will not use it in estimation, because the behavior of Player 1 in the TG does not have a clear interpretation in terms of deviation from self-interest.

3.3 Experimental measures of social preferences

We use the four experiments to obtain two measures of self-interest, a measure of reciprocity, and two measures of beliefs about others' self-interest. Each measure is standardized such that it falls in the range $[0, 1]$.

First, we measure how much behavior deviates from self interest (DFSI). For example, in the Dictator Game if a player gives \$10 and the self-interested choice would be 0, then this amount would be divided by 10 (the maximum possible transfer) to give a measure of 1; if a player gives 5, their DFSI measure would be 0.5, and so on. In the Trust Game, if Player 2 returns to Player 1 everything she receives, their DFSI is 1; if they return half the amount received, their DFSI is 0.5, and so on.

Second, we measure how much a subject believes the behavior of others will deviate from self interest (what we call *guess DFSI*). This is the same as the above measure, but based on the elicited beliefs.

Third, we measure reciprocity using behavior by Player 2 in the TG. Specifically, we look at the covariation between the share of her endowment that Player 1 transfers to Player 2 and the share of this transfer passed back by Player 2 to Player 1. If Player 2 increases the share she returns one-to-one with the share she receives, their measured reciprocity is 1. A Player 2 who returns the same share, regardless of the transfer received, has a reciprocity measure of 0. We provide further details about each measure in Appendix F.

3.4 Policy preferences

We aggregate the information contained in the students' answers to the policy questions into six measures of policy preferences, using both a Principal Component Analysis (PCA) and a simple aggregation based on topic. Details of both measures are provided in Appendix G.

The first principal component, which contains a higher share of the variance, is labelled 'progressive', for brevity and because it appears to have a straightforward intuitive interpretation in terms of 'how progressive', as opposed to conservative, a students' policy positions are (details in Appendix G). We use it as a first outcome variable of interest in our analysis of policy preferences.

We then consider five indexes based on common topics. These indexes are calculated as simple sums of scores in questions which share a common topic covering five areas: pro-market, pro-government intervention, pro-green policies, trust in government, and immigration restrictiveness. Each sum of individual scores is divided by its maximum possible value, so that all indexes range from -1 to +1.

3.5 Estimation strategy

We estimate the effect of a semester-long intermediate microeconomics course on our outcomes of interest using a difference-in-differences (DiD) strategy. We employ the following fixed-effects regression:

$$y_{it} = \alpha_i + \gamma Post_t + \beta Econ_i * Post_t + u_{it} \quad (3.1)$$

where i indexes individuals; t indexes the survey round ($t = 0$ for beginning-of-semester and $t = 1$ for end-of-semester); y is an outcome of interest; α_i captures individual fixed-effects; $Post$ is an indicator equal to 1 if $t = 1$ and 0 otherwise; $Econ$ takes on 1 if the respondent is enrolled in an intermediate microeconomics course, 0 otherwise. The β coefficient provides the difference-in-differences estimate of the effect of the ‘intermediate microeconomics’ treatment. Standard errors are clustered at the individual level.⁸

To capture possible heterogeneity in effects based on the specific approach to economics being taught, we also examine the effect of ‘Conventional’ and ‘Post Walrasian’ microeconomics courses separately, using the following specification:

$$y_{it} = \alpha_i + \gamma Post_t + \beta^W Conventional_i * Post_t + \beta^{PW} PostWalras_i * Post_t + u_{it} \quad (3.2)$$

where *Conventional* is a dummy equal to 1 if a student is enrolled in a conventional intermediate microeconomics course; *PostWalras* is a dummy for being enrolled in what we called the Post-Walrasian intermediate microeconomics course.⁹ β^W is our estimate of the effect of the ‘conventional microeconomics’ treatment, while β^{PW} provides the estimate of the effect of the ‘Post-Walrasian microeconomics’ treatment. The excluded category is always the non-economics control group.

⁸Ideally, we would want to cluster standard errors at the treatment group level (economics vs. non-economics students). This, however, is not possible, as it would result in only two clusters. Also clustering at the course level would result in a too small number of clusters for reliable statistical inference (we would have five clusters, four of which are treated). The standard Liang-Zeger clustering adjustment tends to perform poorly (severely underestimating standard errors) with a small number of clusters (Cameron and Miller 2015). This problem cannot be solved by using wild-bootstrap methods to adjust for clustering: although they are robust to a small number of clusters, they cannot be applied in a difference-in-differences setting in which treatment is assigned at the cluster level and there are few treated clusters (MacKinnon and Webb 2018); in this setting, both restricted (WCR) and unrestricted (WCU) versions of the wild-bootstrap method would provide severely biased estimates of standard errors (MacKinnon and Webb 2018). We therefore cluster standard errors at the individual level. Inability to account for higher-level clustering of error terms is a limitation of this study, which is imposed by the structure of our data.

⁹The courses that we called *Conventional I* and *Conventional II* are included in the ‘Conventional’ treatment; the *post-Walrasian* course represents the *PostWalras* treatment. We exclude from this ‘disaggregated’ portion of the analysis the *Conventional + SP* course, because it is not clear in which of the two groups it should be included. All the results we will present are robust to including the *Conventional + SP* course either in the *Conventional* or in the *PostWalras* treatment.

4 Results

4.1 Summary Statistics and consistency checks

Table 1 summarizes sample and sub-sample sizes and participation rates. 202 students responded to both rounds of the survey.¹⁰ Participation rates are quite high, ranging from 52% in the *Conventional + SP* course to 92.5% in the Post-Walrasian course. In the overall sample, the participation rate is 68.5%.

Table 2 reports the demographic distribution of participants across courses. In the nutrition course which we use as a control group nearly 91% of respondents are women. In contrast, the share of female students is only 27% on average across economics courses. This is broadly in line with national gender ratios. As long as the stark differences in gender composition between treated and control groups are absorbed by the individual fixed effects, they should not affect our estimates. They would, however, be potentially problematic if male and female students displayed differential *trends* in social preferences and policy opinions. We devote particular attention to assessing systematic gender differences in (changes in) behavior, and present robustness tests that estimate our main regressions separately by gender.

Appendix Figures 4 to 8 plot frequency distributions for our measures of social preferences before treatment, and for their *changes* over the course of the semester, by gender. According to all measures, around 40% of respondents did not change their level of altruism/reciprocity at all, 20% displayed only small changes, and 20% displayed large changes. The distribution of the outcomes, and of their changes during the semester, displays little systematic differences by gender.

The measures of generosity from the DG and from the TG are positively and significantly, although not strongly, correlated, with a Pearson correlation coefficient of 0.18 ($p = 0.0003$). Expectations about other people's generosity from the two games are also positively and significantly but not strongly correlated, with a Pearson correlation coefficient of 0.11 ($p = 0.0315$).

4.2 Effect on social preferences and beliefs

Table 3 reports our baseline difference-in-differences estimates of the average effect of intermediate microeconomics courses on students' social preferences and beliefs about social preferences. The top panel of Figure 2 visually summarizes the key results. To interpret effect sizes, we report estimates of the effect of economics using the measures of social preferences and beliefs as defined in Section 3.3 (which have an interpretation in terms of percentage changes in generosity/reciprocity) and after standardizing each measure to have a mean of 0 and a standard deviation of 1 (so coefficients are interpreted in terms of standard deviations).

Four main results stand out. First, average initial (pre-treatment) levels of altruism are quite high in both groups, resulting in large deviations from the Nash equilibrium

¹⁰We disregard observations for students who only participated in the first round or only in the second round as we need observations from both survey rounds.

predictions of self-interest. This is shown in the top panel of Table 3, which reports pre-treatment averages for economics and nutrition students. On average, participants donated more than 60% of their endowment in the Dictator Game with charities and passed back almost 40% of their initial payoff when acting as Player 2 in the Trust Game. Average levels of reciprocity are positive and moderately strong. For a unit increase in the share passed on by Player 1, the share passed back by Player 2 increases by approximately 0.3.

Second, and consistent with most previous literature, economics students display slightly lower levels of generosity in both games. However, they display higher levels of reciprocity. This is shown in the second panel of Table 3, which reports a measure of selection into economics: the difference in pre-treatment averages between economics and nutrition students. Figure 2b displays this measure of selection bias, expressing it in terms of standard deviations. The difference in generosity is relatively small (5 percentage points lower for economics students in the DG, and 2.8 percentage points lower in the TG) and we cannot reject the null hypothesis of no difference at any conventional significance level. Pre-treatment beliefs about other students' generosity do not appear to differ much between economics and the control group (slightly lower for economics students in the DG, but slightly higher in the TG). Regarding reciprocity, for each unit increase in the share of the endowment passed on by Player 1, economics students increase the share they pass back as Player 2 by 0.09 additional units relative to nutrition students (s.e. 0.05).

Third, social preferences and beliefs about social preferences remain stable for both economics and non-economics students. The third panel of Table 3 and Figure 2a display changes during the semester. They show that both economics and non-economics students tend to display stability of social preferences and of beliefs about others' social preferences. Changes in average levels of altruism and reciprocity and in beliefs during the semester are small in both groups.

Fourth – and most important – economics education seems to have little effect on social preferences. The fourth panel of Table 3 reports the estimated effect of intermediate microeconomics (obtained through the estimation of equation 3.1 in our sample). The fifth panel reports the same estimated average effect after standardizing the outcome variables, to help interpreting effect sizes. Standardized effects are also reported in the top panel of Figure 2.

The estimated average treatment effect of intermediate microeconomics on social preferences is close to zero. The estimated effect on generosity in the DG amounts to +1.4 percentage points (with a standard error of 6 pp), or 0.04 standard deviations (s.e. 0.17). The estimated effect on generosity in the TG is +0.2 percentage points (s.e. 2 pp), or 0.015 standard deviations (s.e. 0.14). The estimated effect on reciprocity is -0.04 standard deviations (s.e. 0.15).

When using Player 2 behavior in the TG to measure generosity, the null effect is also quite precisely estimated. We can rule out at the 0.05 significance level a decrease in generosity bigger than 4.6 percentage points or 0.3 standard deviations.¹¹

¹¹A relevant caveat to this observation is that – because of the structure of our data – we were not

With respect to beliefs, Figure 2 shows that the effect of economics on beliefs is precisely estimated to be zero for four out of the five measures, suggesting that the effects are not large for those outcomes. On generosity in the TG, however, the effect of economics on beliefs is -0.22 standard deviations (s.e. 0.146). Though imprecisely estimated, the effect suggests that economics students may reduce their belief in others' generosity in trusting interactions.

To assess whether these results are affected by the gender differences between the treatment and control groups, in Appendix Table 5 we estimate the effect of intermediate microeconomics separately for male and female students. Coefficients are small and we cannot reject the null hypothesis of no effect at any conventional significance level.¹²

To capture possible differences in treatment effects based on course content, we separate the impact of different course curricula. Results are summarized in the top panel of Figure 3. More details are provided in Appendix Tables 6. We find little to no difference. The estimated effect of both conventional and Post-Walrasian variants of intermediate microeconomics is close to zero and we cannot reject the null hypothesis of no effect at any conventional significance level, across all the experimental measures of social preferences and beliefs.

4.3 Effects on policy preferences

Table 4 and the bottom panel of Figure 2 report our results about the effects of intermediate microeconomics courses on students' policy preferences, using the six aggregate measures of policy preferences. For symmetry with the analysis of social preferences, we report estimated effects using the indexes in the range $[-1, 1]$ and with standardization. Below, we focus on the standardized measures.

We first consider our measure of selection into economics: the difference in pre-treatment average policy opinions between economics students and the control group. This measure is shown in the second panel of Figure 4 and in Figure 2d. On average, students enrolled in intermediate microeconomics are substantially and significantly more 'pro-market' (by around 0.5 standard deviations, s.e. 0.17). After accounting for multiple hypothesis testing through the Westfall and Young (1993) method, the adjusted p-value for the selection effect in the 'pro-market' variable is 0.034.¹³ Selection effects are rather small and not significantly different from zero for the other five indexes of policy preferences.

We then turn to our difference-in-differences estimates of the effect of intermediate microeconomics. We find no effect on how 'progressive' students' opinions are, nor on

able to take into account clustering at the course or discipline level when calculating standard errors (as explained in footnote 8).

¹²Of course, the fact that we have few female students in the economics courses and few male students in the nutrition course (Table 2) reduces the statistical power of this robustness test.

¹³ This result is robust to using alternative methods to adjust for multiple hypothesis testing. Specifically, the adjusted p-value is 0.026 if using the Bonferroni-Holm method or the Sidak-Holm method. We use the 'wyoung' command in STATA (Jones, Molitor, and Reif 2018) in order to perform adjustment for multiple hypothesis testing.

their opinions on free markets, government intervention, and green policies. We do, however, find effects on their opinions on immigration policy: economics seems to make students favor more restrictive immigration. Specifically, their support for the statement ‘Immigrants from other countries should be prohibited except where it can be shown that they will contribute to the quality of life of the current resident population ’ (Statement Q9, the only component question of the ‘immigration restriction’ index) increases by 0.33 standard deviations (s.e. 0.13) among economics students relative to the control group. After accounting for multiple hypothesis testing through the Westfall and Young (1993) method, the adjusted p-value for this effect is 0.092.¹⁴

To put the effect we have found on students’ opinions on immigration policy in context, it is worth noting that at the beginning of the semester economics students (as well as the control group) on average disagree with the restrictive view of immigration (first panel of Table 4 and Figure 13a). The average pre-treatment value for the ‘immigration restrictiveness index’ is -0.36 for both economics and non-economics students (on a scale that ranges from -1 to 1). The index increases on average by 0.093 (s.e. 0.047) during the semester for economics students. Notwithstanding this significant increase, at the end of the semester economics students remain on average substantially more likely to disagree than to agree with the restrictionist view of immigration.

There also seems to be a modest negative effect of economics on trust in government, but it is quite imprecisely estimated. Trust in the government of [State Blinded for Review] decreases by 0.2 standard deviations among economics students relative to the control group (s.e. 0.17). However, a 95% confidence interval for this effect cannot reject the null hypothesis of zero effect, and, after accounting for multiple hypothesis testing through the Westfall and Young (1993) method, the p-value for this effect is 0.64.

While the aggregation we have performed allows us to convey results in a more compact and informative way, in Appendix D (Tables 13 to 15) we also look at effects on each single policy statement, reaching similar conclusions: there is no substantial effect on any single policy statement, except for the effect on immigration policy.

The estimated effects on policy preferences also appear to display little difference based on course content. The bottom panel of Figure 3 reports separately the effect different courses. Most importantly, the positive effect on the ‘immigration-restrictive’ variable is visible in both types of courses. There is no discernible effect on any other policy opinion in any of the two types of courses. The only significant difference in results is in selection effects: the higher pre-treatment value of the pro-market variable among economics students seems to be mainly driven by the courses with a conventional curriculum.

5 Conclusion

This paper revisits the question ‘does economics make you selfish?’ In particular, we estimate the impact of semester-long intermediate microeconomics courses on social

¹⁴ Using the Bonferroni-Holm method produces an adjusted p-value of 0.088; the Sidak-Holm method gives an adjusted p-value of 0.084.

preferences, policy opinions, and beliefs about other people’s social preferences.

Our results suggest that social preferences and the beliefs about social preferences of other people appear rather stable over the course of a semester, and unaffected by the course a student takes. We find some evidence of selection effects: economics students start the semester with a more favorable opinion of market competition. Economics students also display lower generosity and higher reciprocity in experimental games, but these differences are not large nor statistically significant.

Results about the effect of economics education on policy preferences are more nuanced. We find no effect on how ‘progressive’ students’ policy opinions are, nor on their opinions on markets, government intervention, and green policies. We find a relevant effect on economics students’ opinions on immigration policy, which become relatively more restrictive, although not on average restrictionist.

Overall, our results contrast with those of several previous cross-sectional studies on this topic, which have found a negative relationship between economics education and experimental measures of social preferences. This discrepancy could be explained by selection bias, for which we do find some evidence. This interpretation is reinforced by the fact that previous studies using some form of DiD approach (but employing real-world donations rather than experimental outcomes) found results similar to ours (Frey and Meier 2003). Our results can be reconciled with effects like those found in Ifcher and Zarghamee (2018) if they mainly capture framing effects (context-dependent preferences), while we study more long-lasting effects over the course of a semester (endogenous preferences).

We do not conclude, on the basis of this single study, that economics does not make you selfish. However, the evidence we provide complements, generalizes and reinforces findings from previous studies using difference-in-differences designs and casting doubts on this popular idea. We outlined at the outset a line of reasoning that might lead us to expect the commonplace view that studying economics leads to more self-interested behavior. But there are also cogent reasons to expect the opposite.¹⁵ Montesquieu, Voltaire, Smith and other 18th century thinkers held that markets promote honesty and cooperativeness towards others, and that these predispositions are as important as self-interest in making markets work. Students in today’s economics courses might well marvel that in markets, even when interacting with total strangers, adherence to social norms of respect for others and reciprocating goodwill can be the basis for mutually beneficial exchange. Exposure to this message could promote social preferences as well as self-interest.

¹⁵This is one explanation offered of the findings of a cross cultural experimental project showing that greater exposure to markets was associated with more generous and more fair minded behavior in an experimental ultimatum game, a result celebrated by the Wall Street Journal as “the civilizing effect of the market” (Henrich et al. 2001).

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Figures

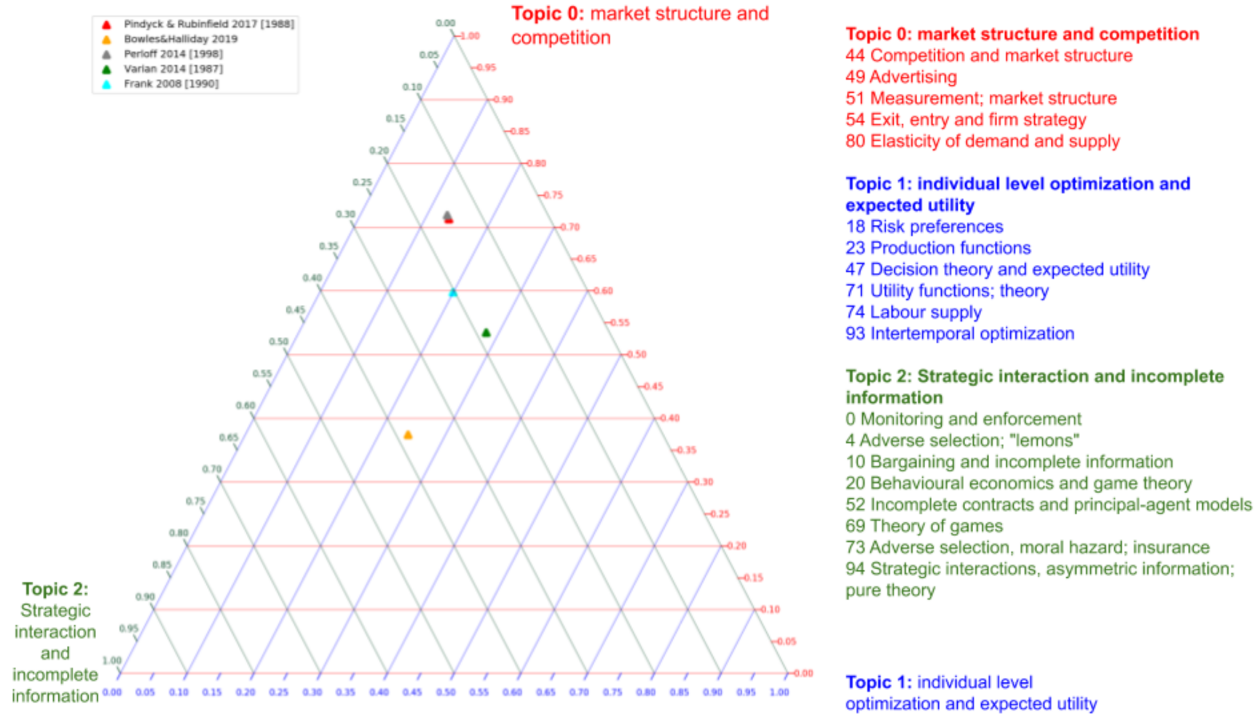
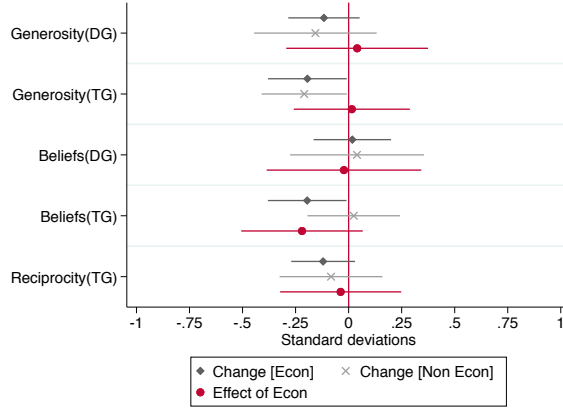
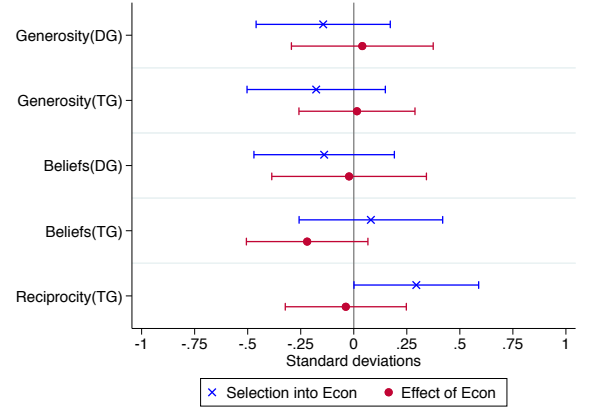


Figure 1: The location of microeconomics textbooks in a 3-Metatopic space. Coordinates of the textbooks are the topic weights for the Metatopics at the vertices. For example, Varian has a location of (0.53, 0.28, 0.19), that is, a weight of 0.58 on market structure (meta-topic 0), a weight of 0.28 on individual optimization and expected utility (meta-topic 1), and a weight of 0.19 on strategic interaction and incomplete information (meta-topic 2). Source: Bowles et al. (2019).

Social preferences and beliefs

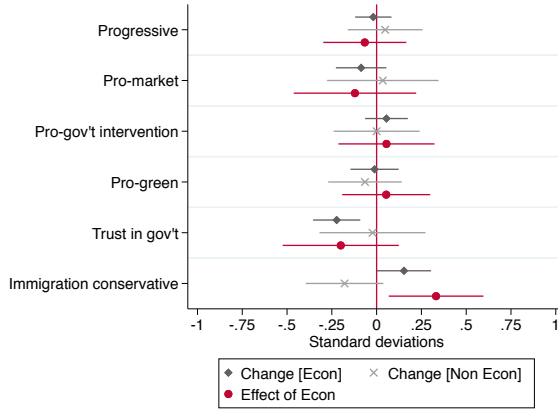


(a) Changes during the semester and effect of Econ

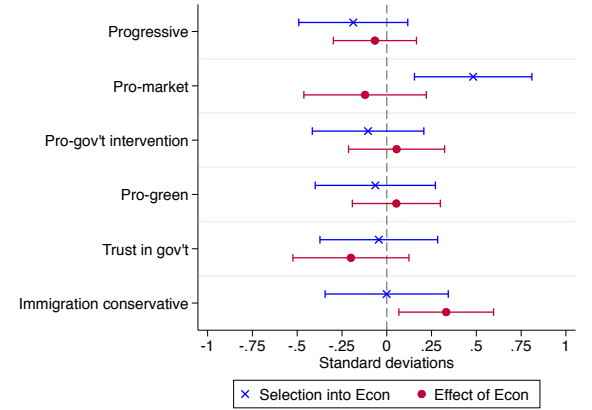


(b) Selection vs. effect of Econ

Policy preferences



(c) Changes during the semester and effect of Econ

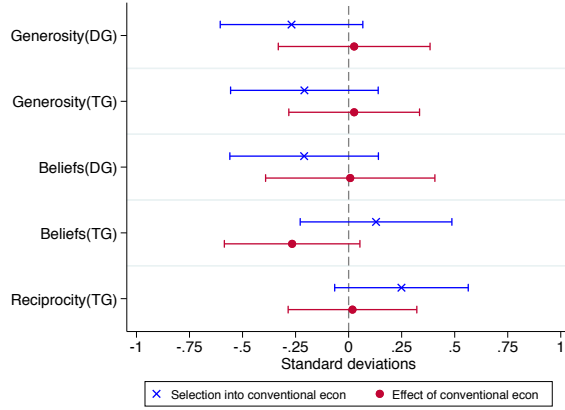


(d) Selection vs. effect of Econ

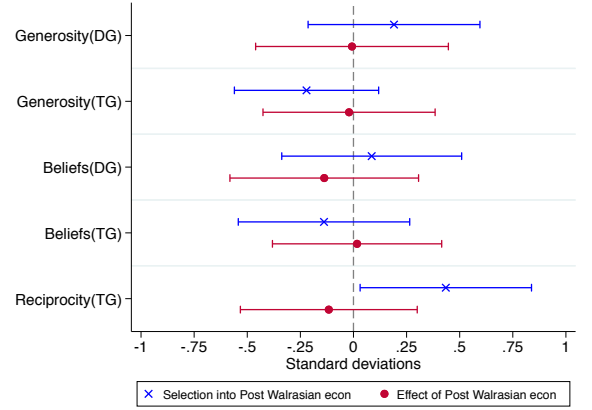
Figure 2: Effect of Intermediate Microeconomics on students' social preferences, beliefs and policy preferences

Notes: The Figures display visually our difference-in-differences (DiD) estimates of the effect of an Intermediate Microeconomics course on students' social preferences, beliefs about other students' social preferences, and policy preferences. See Section 3.3 and Appendixes F and G for the definition of each outcome variable. All outcome variables are standardized. For each outcome of interest, the left panel displays the average change during the semester among Intermediate Microeconomics students ('Change [Econ]') and non-economics students ('change [Non econ]'), and the DiD estimate of the effect of Intermediate Microeconomics ('Effect of Econ', given by the difference between the two changes). The right panel displays differences between averages for Intermediate Microeconomics and non-economics students in the first (pre-treatment) survey round ('Selection into Econ') and again the DiD estimate of the effect of Intermediate Microeconomics ('Effect of Econ'). Dots represent point estimates, bars are 95% confidence intervals from standard errors clustered at the individual level.

Social preferences and beliefs

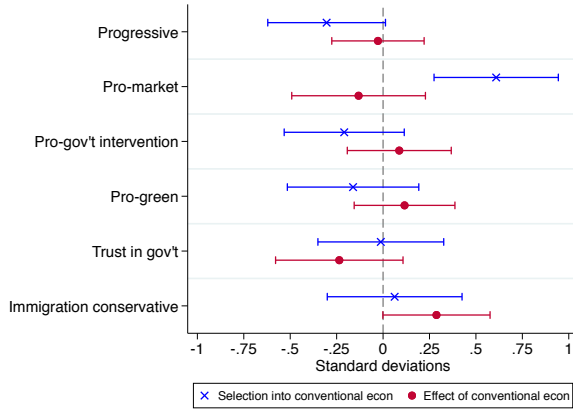


(a) Conventional curriculum

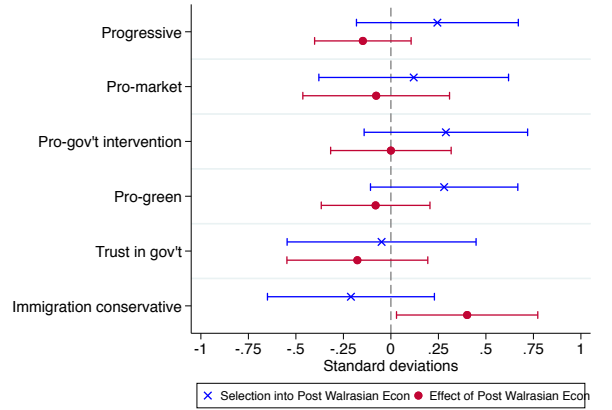


(b) Post Walrasian curriculum

Policy preferences



(c) Conventional curriculum



(d) Post Walrasian curriculum

Figure 3: Effect of Intermediate Microeconomics on students' social preferences, beliefs and policy preferences – Conventional vs. Post Walrasian curriculum

Notes: The Figures display visually our difference-in-differences (DiD) estimates of the effect of an Intermediate Microeconomics course on students' social preferences, beliefs about other students' social preferences, and policy preferences. See Section 3.3 and Appendixes F and G for the definition of each outcome variable. For each curriculum and for each outcome of interest, the graphs displays differences between averages for Intermediate Microeconomics and non-economics students in the first (pre-treatment) survey round ('Selection into Econ') and the DiD estimate of the effect of Intermediate Microeconomics ('Effect of Econ'). Dots represent point estimates, bars are 95% confidence intervals from standard errors clustered at the individual level.

Tables

Table 1: Number of respondents who participated in both rounds of the survey, by course

Course	Frequency	Total Enrolled	Participation rate
Post Walrasian	37	40	92.5%
Conventional I	60	98	61.2%
Conventional + SP	13	25	52.0%
Conventional II	46	70	65.7%
Nutrition and metabolism	46	62	74.2%
Total	202	295	68.5%

Table 2: Distribution of respondents by gender and region of origin

	Female	Asia	Europe	Other	US
Course					
Post Walrasian	0.22	0.07	0.03	0.00	0.91
Conventional I	0.27	0.16	0.00	0.02	0.82
Conventional + SP	0.08	0.19	0.00	0.00	0.81
Conventional II	0.37	0.09	0.00	0.02	0.89
Nutrition	0.91	0.08	0.00	0.04	0.88
Econ vs Non Econ					
Non Econ	0.91	0.08	0.00	0.04	0.88
Econ	0.27	0.12	0.01	0.01	0.86
Total	0.42	0.11	0.00	0.02	0.87

Notes: For each gender and region of origin indicated in column, this Table reports the share of respondents, by course and by treatment group. Here 'region of origin' is defined as the region where a student attended high school.

Table 3: Difference-in-differences (DiD) estimates of the effect of Intermediate Microeconomics on students' social preferences and beliefs

	(1) Generosity in Dictator Game <i>[dfsi dg]</i>	(2) Generosity in Trust Game <i>[dfsi tg p2]</i>	(3) Beliefs about generosity (DG) <i>[guess dfsi dg]</i>	(4) Beliefs about generosity (TG) <i>[guess dfsi tg p2]</i>	(5) Reciprocity in Trust Game <i>[recip]</i>
Mean Before (Econ)	0.600 (0.028)	0.357 (0.013)	0.469 (0.018)	0.319 (0.012)	0.303 (0.024)
Mean Before (Non Econ)	0.650 (0.051)	0.385 (0.023)	0.500 (0.033)	0.307 (0.022)	0.215 (0.044)
Selection (into Econ)	-0.050 (0.058)	-0.028 (0.026)	-0.031 (0.037)	0.012 (0.024)	0.088 (0.05)
Change (Econ)	-0.040 (0.03)	-0.031 (0.015)	0.004 (0.021)	-0.028 (0.014)	-0.036 (0.023)
Change (Non Econ)	-0.054 (0.051)	-0.033 (0.016)	0.009 (0.035)	0.004 (0.016)	-0.025 (0.037)
DiD (Effect of Econ)	0.014 (0.059)	0.002 (0.022)	-0.005 (0.041)	-0.032 (0.021)	-0.011 (0.044)
Standardized DiD (Effect of Econ)	0.040 (0.171)	0.015 (0.14)	-0.022 (0.186)	-0.220 (0.146)	-0.038 (0.146)
N	404	404	404	404	404

Notes: This table reports difference-in-differences (DiD) estimates for the effect of a semester-long intermediate microeconomics course on students' social preferences and beliefs about other students' social preferences. See Section 3.3 and Appendix F for the definition of each outcome variable. All outcome variables range from 0 (perfect self-interest) to 1 (maximum possible deviation from self-interest). The 'Mean before' panel reports the average of the outcome variables in the first (pre-treatment) survey round for Economics and non-Economics students; 'Selection' is the difference in 'Mean before' between Economics and non-Economics students; 'Change' is the average change in the outcome variable between the first (pre-treatment) and the second (post-treatment) survey round. 'DiD (Effect of Econ)' reports our estimates of the effect of intermediate microeconomics, using the DiD specification in equation 3.1; 'Standardized DiD (Effect of Econ)' reports the same estimated average effect after standardizing the outcome variables. Standard errors clustered at the individual level in parentheses.

Table 4: Difference-in-differences (DiD) estimates of the effect of Intermediate Microeconomics on students' policy preferences

	(1) <i>Progressive</i>	(2) <i>Pro-Market</i>	(3) <i>Pro-Gov't inter- vention</i>	(4) <i>Pro-Green</i>	(5) <i>Trust in gov't</i>	(c) <i>Immigrati servative</i>
Mean Before (Econ)	-0.067 (0.128)	0.155 (0.027)	0.299 (0.029)	0.538 (0.031)	0.272 (0.037)	-0.359 (0.048)
Mean Before (Non Econ)	0.241 (0.221)	0.003 (0.046)	0.337 (0.05)	0.565 (0.064)	0.293 (0.07)	-0.359 (0.095)
Selection (into Econ)	-0.308 (0.256)	0.152 (0.053)	-0.038 (0.058)	-0.027 (0.072)	-0.021 (0.08)	-0.000 (0.107)
Change (Econ)	-0.031 (0.085)	-0.027 (0.023)	0.020 (0.022)	-0.005 (0.029)	-0.106 (0.032)	0.093 (.047)
Change (Non Econ)	0.078 (0.175)	0.011 (0.05)	-0.000 (0.045)	-0.027 (0.044)	-0.011 (0.072)	-0.109 (0.067)
DiD (Effect of Econ)	-0.109 (0.195)	-0.038 (0.055)	0.020 (0.05)	0.022 (0.052)	-0.095 (0.078)	0.202 (0.082)
Standardized DiD (Effect of Econ)	-0.066 (0.118)	-0.121 (0.174)	0.055 (0.137)	0.053 (0.125)	-0.200 (0.165)	0.332 (0.135)
N	404	404	404	404	404	404

Notes: This table reports difference-in-differences (DiD) estimates for the effect of a semester-long intermediate microeconomics course on students' policy preferences. See Section 3.3 and Appendix G for the definition of each outcome variable. All outcome variables range from -2 to 2. The 'Mean before' panel reports the average of the outcome variables in the first (pre-treatment) survey round for Economics and non-Economics students; 'Selection' is the difference in 'Mean before' between Economics and non-Economics students; 'Change' is the average change in the outcome variable between the first (pre-treatment) and the second (post-treatment) survey round. 'DiD (Effect of Econ)' reports our estimates of the effect of intermediate microeconomics, using the DiD specification in equation 3.1; 'Standardized DiD (Effect of Econ)' reports the same estimated average effect after standardizing the outcome variables. Standard errors clustered at the individual level in parentheses.

A Survey Details and Questionnaire

A.1 Timeline, sample and recruitment

A.1.1 Timeline

We administered the same online survey, at the beginning and towards the end of the semester.

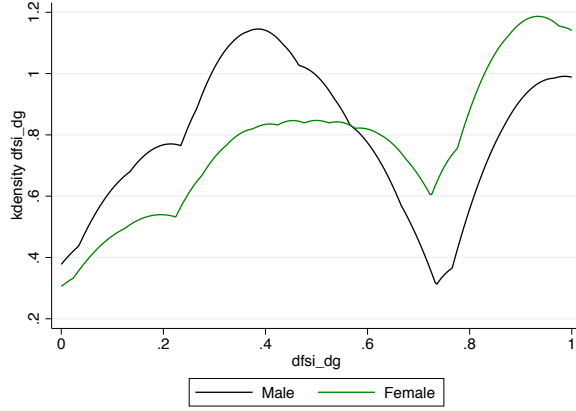
We administered the first round between January 14 and January 28, 2019.¹⁶ The second round was conducted between April 8 and April 24, 2019.

A.1.2 Recruitment

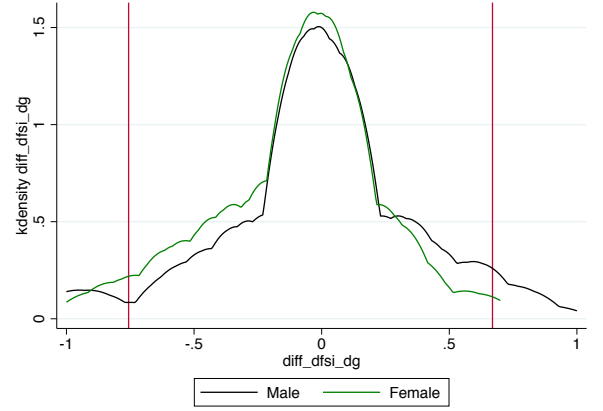
Students in the sample received an invitation email signed "[Blinded for Review] research Group." The invitation email and two subsequent reminders were forwarded to students by the course Professor and/or by a Teaching Assistant (TA). To encourage participation, students who filled the survey in both rounds received extra-credits in the course in question, amounting to 1.25% of the final grade in the *Walrasian I* course, 2% in the *Walrasian II* course, 3% in the *Post Walrasian* course, 2% in the *Walrasian + SP* course and 2% in the *Nutrition and Metabolism* course. The invitation email explicitly stated that the Professor of the course was not involved in the research project and that he/she would not be able to access a student's responses, and that the only information which would be shared with the Professor would be the list of students who participated, in order to grant them extra-credits.

¹⁶We allowed students that enrolled in the course after January 28 to take the survey between January 29 and February 4. 9 students from the *Nutrition and Metabolism* course participated in the survey between January 28 and February 4. Results are unchanged if we exclude these 'late participants'.

B Behavior in games by gender

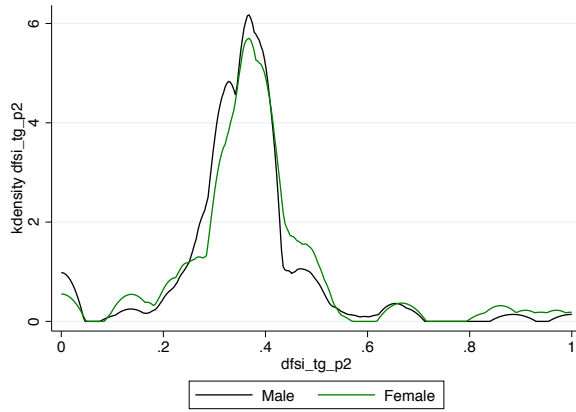


(a) beginning of semester

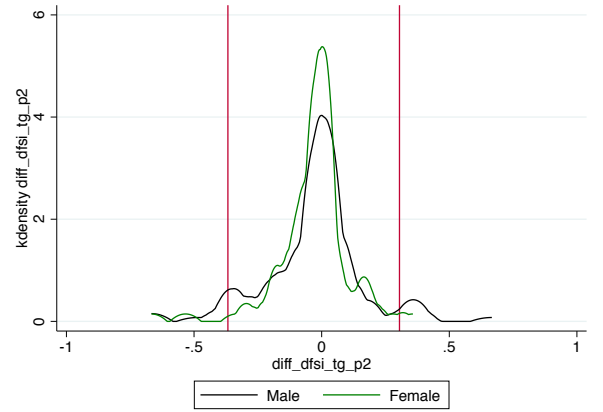


(b) change over the semester

Figure 4: Deviation from self-interest in the DG game, by gender

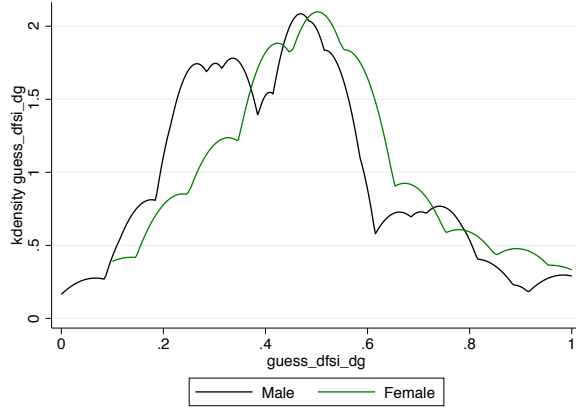


(a) beginning of semester

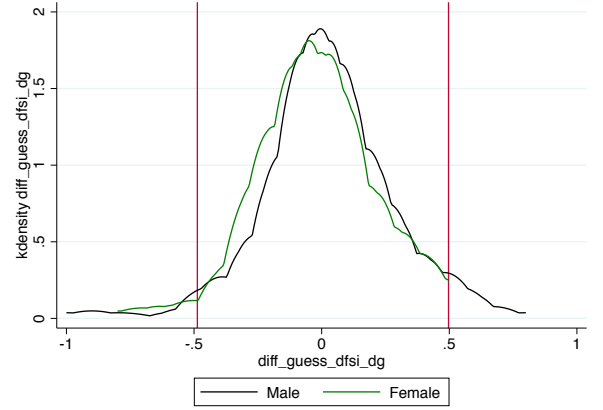


(b) change over the semester

Figure 5: Deviation from self-interest in the TG game (Player 2), by gender

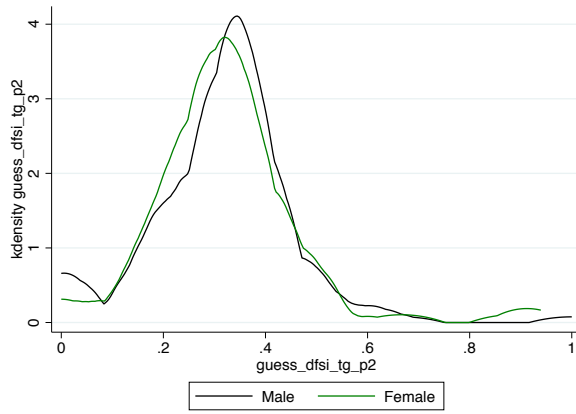


(a) beginning of semester

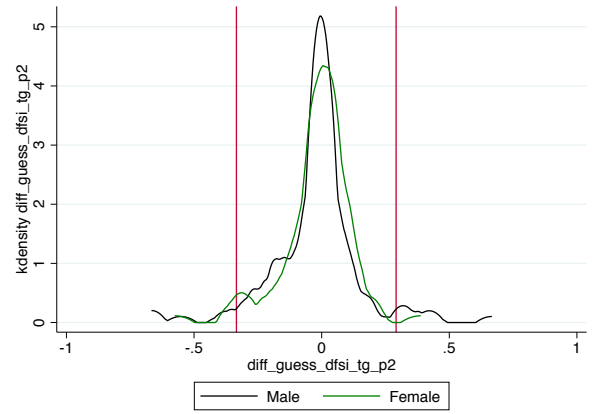


(b) change over the semester

Figure 6: Guess about deviation from self-interest in the DG game, by gender

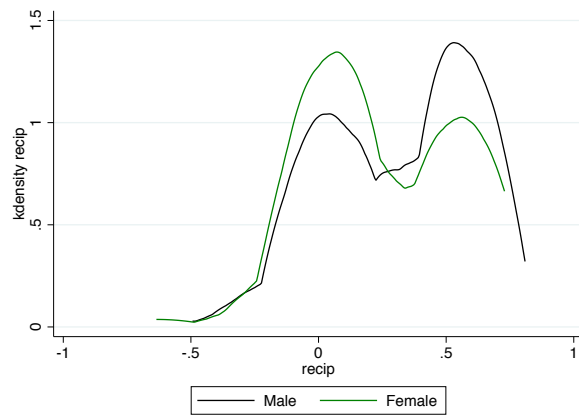


(a) beginning of semester

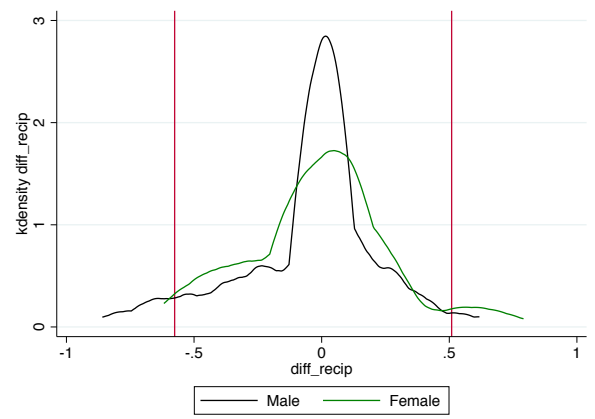


(b) change over the semester

Figure 7: Guess about deviation from self-interest in the TG game (Player 2), by gender



(a) beginning of semester



(b) change over the semester

Figure 8: Reciprocity in the TG game (Player 2), by gender

C Estimates by gender

Table 5: Difference-in-differences (DiD) estimates of the effect of Intermediate Microeconomics on students' social preferences and beliefs, by gender

	Male					Female			
	(1) <i>dfsi dg</i>	(2) <i>dfsi tg p2</i>	(3) <i>guess dfsi dg</i>	(4) <i>guess dfsi tg p2</i>	(5) <i>dfsi recip</i>	(6) <i>dfsi dg</i>	(7) <i>dfsi tg p2</i>	(8) <i>guess dfsi dg</i>	(9) <i>guess dfsi tg p2</i>
Mean Before (Econ)	0.579 (0.033)	0.351 (0.014)	0.454 (0.021)	0.310 (0.014)	0.308 (0.027)	0.657 (0.050)	0.373 (0.026)	0.510 (0.032)	0.341 (0.021)
Mean Before (Non Econ)	0.650 (0.104)	0.367 (0.039)	0.475 (0.055)	0.373 (0.043)	0.384 (0.108)	0.650 (0.053)	0.387 (0.026)	0.502 (0.036)	0.301 (0.024)
Selection (into Econ)	-0.071 (0.109)	-0.016 (0.042)	-0.021 (0.059)	-0.063 (0.045)	-0.077 (0.112)	0.007 (0.073)	-0.014 (0.036)	0.007 (0.048)	0.041 (0.032)
Change (Econ)	-0.011 (0.037)	-0.032 (0.018)	0.021 (0.025)	-0.025 (0.017)	-0.046 (0.026)	-0.121 (0.047)	-0.027 (0.026)	-0.043 (0.035)	-0.038 (0.023)
Change (Non Econ)	-0.000 (0.036)	-0.006 (0.020)	0.025 (0.055)	-0.002 (0.009)	-0.090 (0.099)	-0.060 (0.056)	-0.036 (0.018)	0.007 (0.039)	0.004 (0.018)
DiD (Effect of Econ)	-0.011 (0.051)	-0.026 (0.027)	-0.004 (0.060)	-0.023 (0.019)	0.044 (0.103)	-0.062 (0.073)	0.008 (0.031)	-0.050 (0.052)	-0.042 (0.029)
Standardized DiD (Effect of Econ)	-0.030 (0.145)	-0.159 (0.165)	-0.018 (0.281)	-0.158 (0.129)	0.145 (0.335)	-0.182 (0.215)	0.055 (0.209)	-0.219 (0.228)	-0.296 (0.205)
N	236	236	236	236	236	168	168	168	168

Notes: This table reports difference-in-differences (DiD) estimates for the effect of a semester-long intermediate microeconomics course on students' social preferences and beliefs about other students' social preferences, by gender. See Section 3.3 for the definition of each outcome variable. All outcome variables range from 0 (perfect self-interest) to 1 (maximum possible deviation from self-interest). The 'Mean before' panel reports the average of the outcome variables in the first (pre-treatment) survey round for Economics and non-Economics students; 'Selection' is the difference in 'Mean before' between Economics and non-Economics students; 'Change' is the average change in the outcome variable between the first (pre-treatment) and the second (post-treatment) survey round. 'DiD (Effect of Econ)' reports our estimates of the effect of intermediate microeconomics, using the DiD specification in equation 3.1; 'Standardized DiD (Effect of Econ)' reports the same estimated average effect after standardizing the outcome variables. Standard errors clustered at the individual level in parentheses.

D Detailed estimates of the effect of different Intermediate Microeconomics curricula

Table 6: Difference-in-differences (DiD) estimates of the effect of Intermediate Microeconomics – Walrasian vs. Post Walrasian curriculum

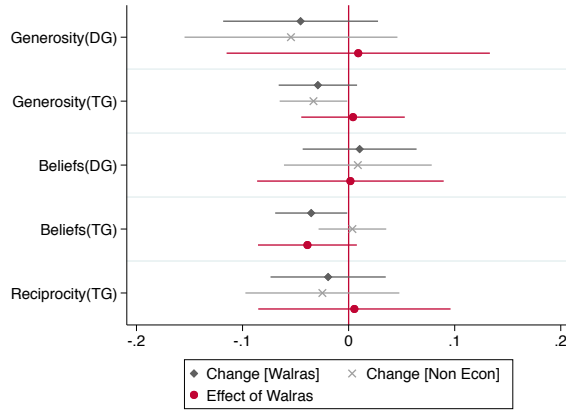
	(1) Generosity in Dictator Game <i>dfsi dg</i>	(2) Generosity in Trust Game <i>dfsi tg p2</i>	(3) Beliefs about generosity (DG) <i>guess dfsi dg</i>	(4) Beliefs about generosity (TG) <i>guess dfsi tg p2</i>	(5) Reciprocity in Trust Game <i>recip</i>
Mean Before (Walras)	0.557 (0.034)	0.352 (0.015)	0.454 (0.022)	0.326 (0.014)	0.290 (0.029)
Mean Before (Post Walras)	0.716 (0.052)	0.350 (0.014)	0.519 (0.034)	0.287 (0.020)	0.345 (0.048)
Mean Before (Non Econ)	0.650 (0.049)	0.385 (0.023)	0.500 (0.033)	0.307 (0.022)	0.215 (0.038)
Selection (Walras)	-0.093 (0.059)	-0.033 (0.028)	-0.046 (0.039)	0.019 (0.027)	0.074 (0.048)
Selection (Post Walras)	0.066 (0.072)	-0.035 (0.027)	0.019 (0.048)	-0.020 (0.030)	0.130 (0.062)
Change (Walras)	-0.045 (0.037)	-0.029 (0.019)	0.010 (0.027)	-0.035 (0.017)	-0.019 (0.028)
Change (Post Walras)	-0.057 (0.062)	-0.036 (0.028)	-0.022 (0.035)	0.006 (0.025)	-0.059 (0.052)
Change (Non Econ)	-0.054 (0.051)	-0.033 (0.016)	0.009 (0.036)	0.004 (0.016)	-0.025 (0.037)
DiD (Effect of Walras)	0.009 (0.063)	0.004 (0.025)	0.002 (0.045)	-0.039 (0.024)	0.005 (0.046)
DiD (Effect of Post Walras)	-0.002 (0.080)	-0.003 (0.033)	-0.030 (0.050)	0.002 (0.030)	-0.035 (0.063)
Standardized					
DiD (Effect of Walras)	0.026 (0.182)	0.026 (0.157)	0.008 (0.204)	-0.266 (0.163)	0.018 (0.155)
DiD (Effect of Post Walras)	-0.007 (0.231)	-0.021 (0.207)	-0.138 (0.227)	0.017 (0.203)	-0.116 (0.212)
N	378	378	378	378	378

Notes: See Section 3.3 and Appendix F for the definition of each outcome variable. All outcomes range from 0 (perfect self-interest) to 1 (max. possible deviation from self-interest). ‘Mean before’ is the average of the outcome variables in the first (pre-treatment) survey round; ‘Selection’ is the difference in ‘Mean before’ between Economics and non-Economics students; ‘Change’ is the average change in the outcome variable between the first (pre-treatment) and the second (post-treatment) survey round. ‘DiD (Effect of Econ)’ reports our DiD estimates of the effect of intermediate microeconomics; ‘Standardized DiD (Effect of Econ)’ reports the same estimated average effect after standardizing the outcome variables. Standard errors clustered at the individual level in parentheses.

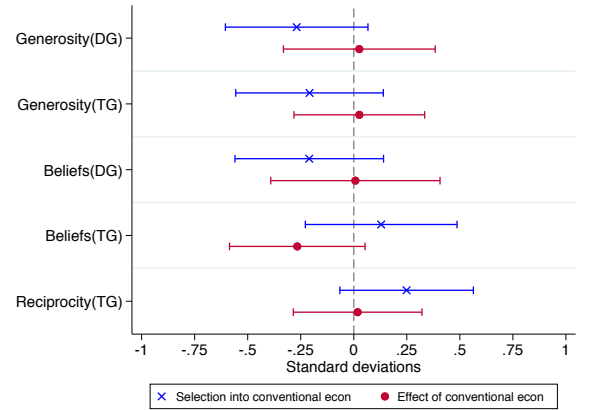
Table 7: Difference-in-differences (DiD) estimates of the effect of Intermediate Microeconomics – Walrasian vs. Post Walrasian curriculum

	(1) <i>Progressive</i>	(2) <i>Pro-Market</i>	(3) <i>Pro-Gov't inter- vention</i>	(4) <i>Pro-Green</i>	(5) <i>Trust in gov't</i>	(6) <i>Immigration conservative</i>
Mean Before (Walras)	-0.260 (0.147)	0.195 (0.028)	0.261 (0.033)	0.498 (0.039)	0.288 (0.042)	-0.321 (0.060)
Mean Before (Post Walras)	0.644 (0.281)	0.041 (0.066)	0.443 (0.062)	0.682 (0.052)	0.270 (0.098)	-0.486 (0.097)
Mean Before (Non Econ)	0.241 (0.222)	0.003 (0.046)	0.337 (0.050)	0.565 (0.065)	0.293 (0.070)	-0.359 (0.095)
Selection (Walras)	-0.501 (0.266)	0.192 (0.054)	-0.076 (0.060)	-0.068 (0.076)	-0.006 (0.082)	0.038 (0.113)
Selection (Post Walras)	0.403 (0.358)	0.038 (0.080)	0.106 (0.080)	0.117 (0.083)	-0.023 (0.120)	-0.128 (0.136)
Change (Walras)	0.033 (0.113)	-0.031 (0.029)	0.032 (0.027)	0.021 (0.038)	-0.123 (0.042)	0.066 (0.059)
Change (Post Walras)	-0.165 (0.121)	-0.014 (0.037)	-0.000 (0.038)	-0.061 (0.042)	-0.095 (0.054)	0.135 (0.094)
Change (Non Econ)	0.078 (0.176)	0.011 (0.050)	-0.000 (0.045)	-0.027 (0.044)	-0.011 (0.072)	-0.109 (0.067)
DiD (Effect of Walras)	-0.045 (0.209)	-0.042 (0.058)	0.032 (0.052)	0.048 (0.058)	-0.112 (0.083)	0.175 (0.089)
DiD (Effect of Post Walras)	-0.243 (0.214)	-0.024 (0.062)	0.000 (0.059)	-0.034 (0.061)	-0.084 (0.090)	0.244 (0.115)
Standardized						
DiD (Effect of Walras)	-0.028 (0.127)	-0.132 (0.184)	0.087 (0.143)	0.116 (0.138)	-0.236 (0.175)	0.288 (0.147)
DiD (Effect of Post Walras)	-0.147 (0.130)	-0.077 (0.197)	-0.000 (0.162)	-0.080 (0.146)	-0.177 (0.189)	0.401 (0.190)
N	378	378	378	378	378	378

Notes: See Section 3.3 and Appendix G for the definition of each outcome variable. All outcomes range from -2 to 2. ‘Mean before’ is the average of the outcome variables in the first (pre-treatment) survey round; ‘Selection’ is the difference in ‘Mean before’ between Economics and non-Economics students; ‘Change’ is the average change in the outcome variable between the first (pre-treatment) and the second (post-treatment) survey round. ‘DiD (Effect of Econ)’ reports our DiD estimates of the effect of intermediate microeconomics; ‘Standardized DiD (Effect of Econ)’ reports the same estimated average effect after standardizing the outcome variables. Standard errors clustered at the individual level in parentheses.

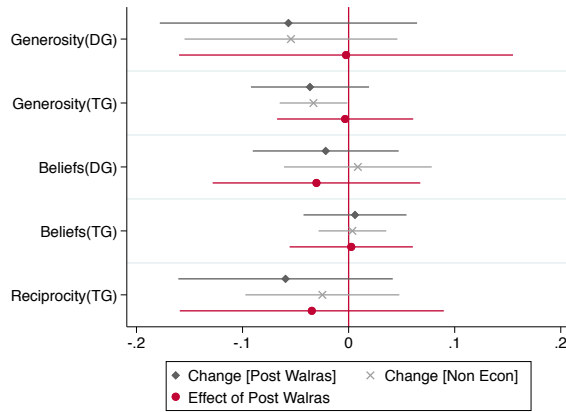


(a) Changes during the semester and effect of Walrasian Econ

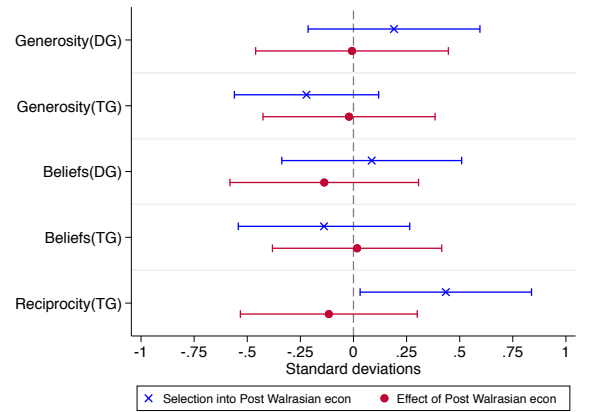


(b) Selection vs. effect of Walrasian Econ

Figure 9: Effect of Walrasian Intermediate Microeconomics on social preferences

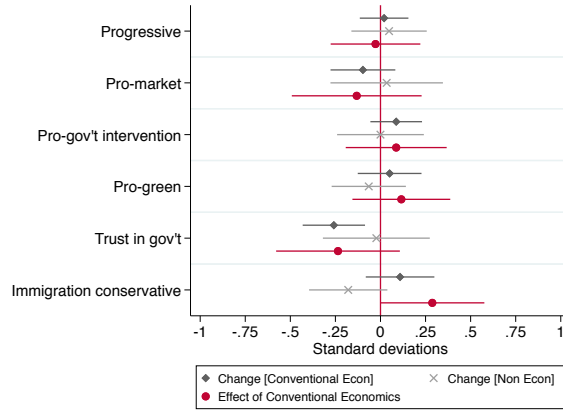


(a) Changes during the semester and effect of Post Walrasian Econ

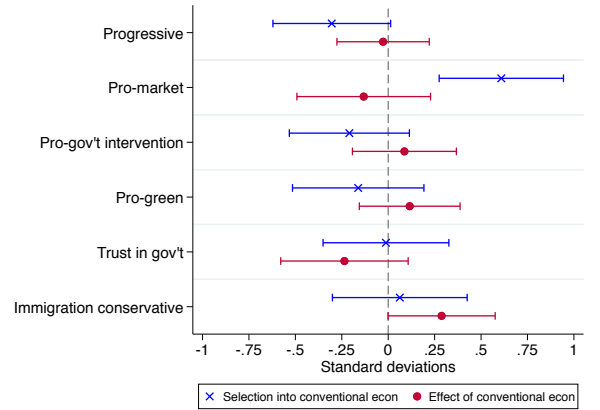


(b) Selection vs. effect of Post Walrasian Econ

Figure 10: Effect of Post Walrasian Intermediate Microeconomics on social preferences

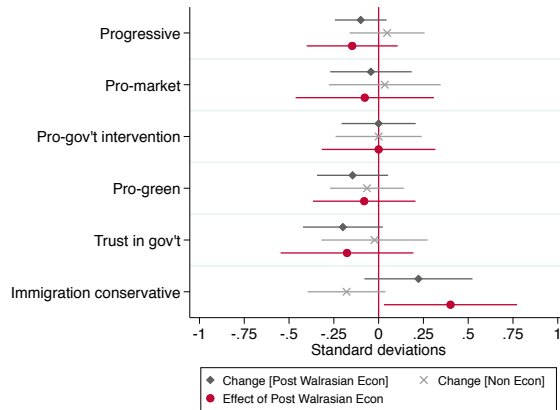


(a) Changes during the semester and effect of Walrasian Econ

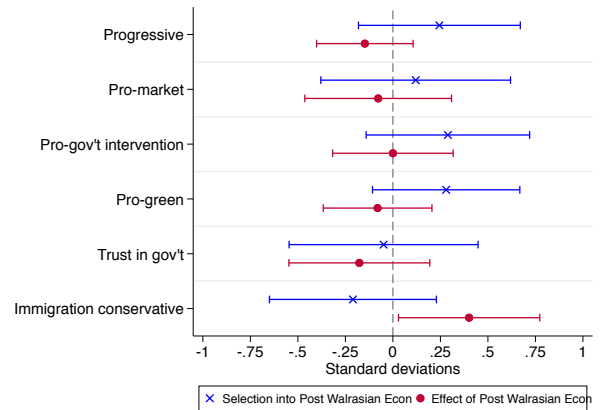


(b) Selection vs. effect of Walrasian Econ

Figure 11: Effect of Walrasian Intermediate Microeconomics on policy preferences

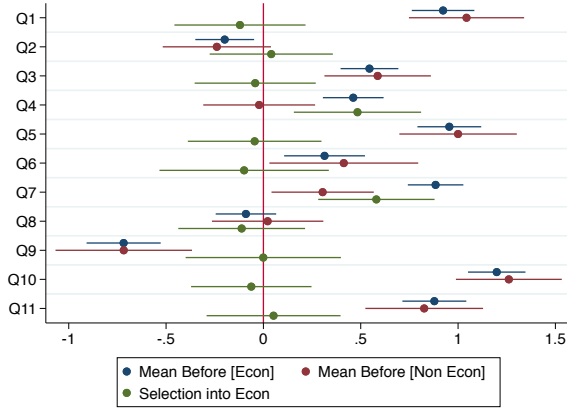


(a) Changes during the semester and effect of Post Walrasian Econ

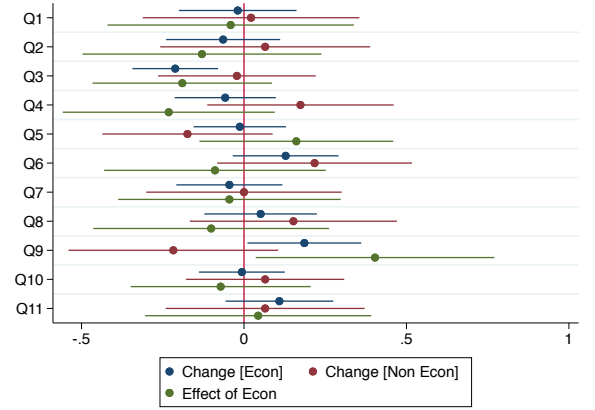


(b) Selection vs. effect of Post Walrasian Econ

Figure 12: Effect of Post Walrasian Intermediate Microeconomics on policy preferences

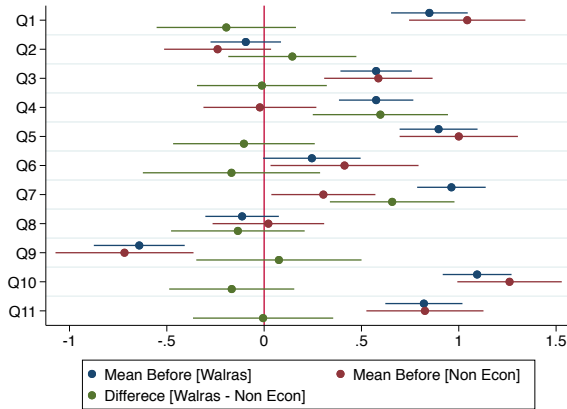


(a) Pre-treatment averages

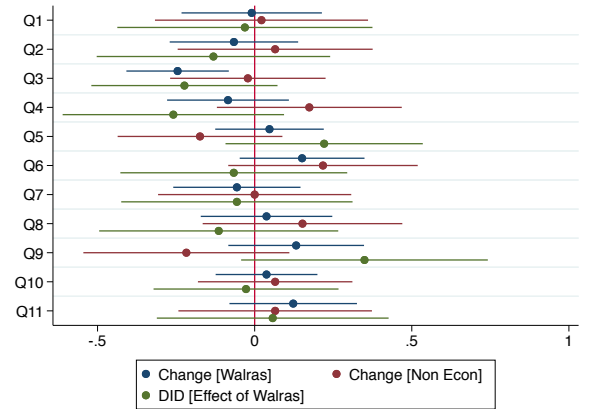


(b) Changes during the semester and effect of econ

Figure 13: Effect of intermediate microeconomics on students' social preferences: all policy questions



(a) Pre-treatment averages



(b) Changes during the semester and effect of conventional econ

Figure 14: Effect of conventional intermediate microeconomics on students' social preferences: all policy questions

E Policy opinions

We asked the respondents to rank the following statements on a five-point Likert scale (from 'Strongly Disagree' to 'Strongly Agree').

Q1: The US government should take more responsibility to ensure that everyone's basic

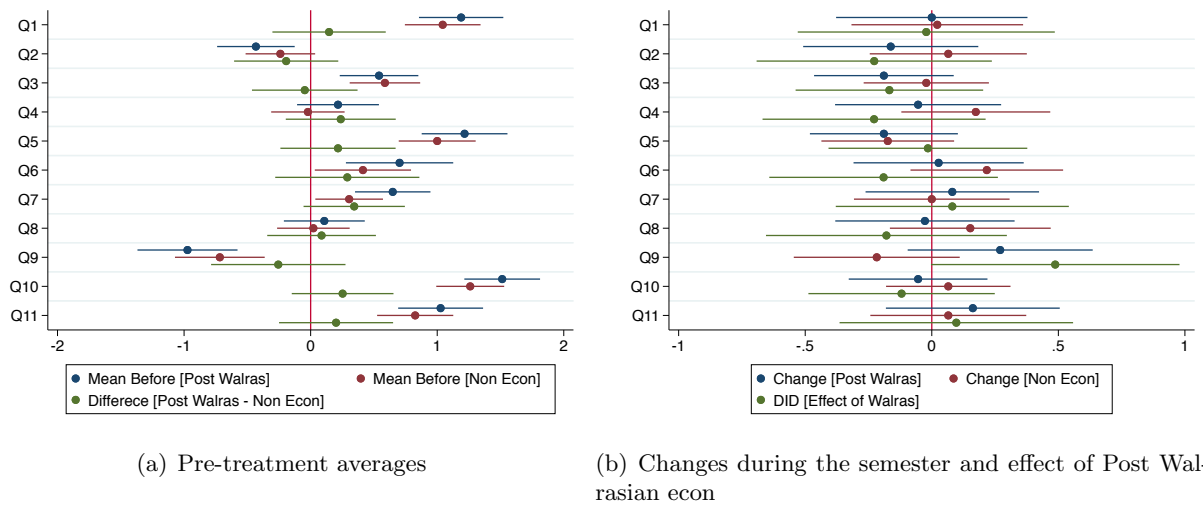


Figure 15: Effect of Post Walrasian intermediate microeconomics on students' social preferences: all policy questions

needs are satisfied.

Q2: In most situations, government intervention cannot make the market system work better.

Q3: I tend to trust the government of the [State Blinded for review]

Q4: I tend to trust the functioning of the free market.

Q5: The Government should impose a carbon tax, defined as a tax on the CO2 emissions that a firm produces.

Q6: The minimum wage in the US should be raised from the current 7.25 dollars per hour to 14 dollars or more (which would mean around \$27000 a year for a full time worker before deducting taxes).

Q7: Market competition is mostly good. It weeds out those (people, companies, etc.) who are not doing a good job, while rewarding good ideas.

Q8: Market competition can be harmful. It brings out the worst in people and creates a society of winners and losers.

Q9: Immigrants from other countries should be prohibited except where it can be shown that they will contribute to the quality of life of the current resident population.

Q10: We owe it to people in the future to pass on to them a planet with environmental conditions no worse than they are today even if this means tightening our belts now.

Q11: Even if pornography is offensive to some, the government should not prohibit its sale to adults.

E.0.1 Demographic and academic information

We collected information on the age and gender of the respondent, their region of origin, the year of study (freshman, sophomore, junior, senior) and their major. We also asked if they had ever taken an economics course before this semester and if so how many. We asked them to list all the courses they were taking this semester. We also asked for the highest level of education completed by both parents as a proxy for the socio-economic status of the family.

F Measures of Self-interest and Reciprocity

Generosity in the DG [*dfsi dg*] For the Dictator Game, an entirely self-interested actor would donate nothing and keep everything for herself. The deviation from self interest in the Dictator Game (*dfsi dg*) would therefore be the total amount donated minus the total amount that a purely self-interested actor would donate (zero). We divide this by the total amount that could be donated so that the deviation from self interest ranges from zero (entirely self interested) to one (entirely altruistic). This measure captures deviations from self-interest due to generosity.

$$\text{Generosity in Dictator Game } [dfsi\ dg] = \frac{\text{Donation in the DG} - 0}{\text{Max possible donation}} \quad (\text{F.1})$$

Beliefs about generosity in the DG [*guess dfsi dg*] We use Equation F.2 to extract a measure of beliefs about other people’s deviation from self-interest (due to generosity), using the ‘guessing game’ about donations in the DG. An individual that expects all others to be self-interested, would expect the average donation to be zero. We divide the guess by the maximum donation possible, such that the deviation from self-interest expectation ranges from zero (for a subject who expects all others to donate nothing) to one (for a subject who expects all others to donate their entire endowment).

$$\text{Belief about generosity in DG } [guess\ dfsi\ dg] = \frac{\text{Guess about average donation in the DG} - 0}{\text{Maximum possible donation}} \quad (\text{F.2})$$

Generosity in the TG [*dfsi tg p2*] As we used the strategy method for Player 2’s choices in the trust game, we define *dfsi tg p2* as the average amount returned as Player 2 divided by the maximum average possible (defined as the average for a hypothetical Player 2 who always returns everything she receives). A self-interested actor would always return 0. *dfsi tg p2* therefore ranges from zero (entirely self interested) to one (entirely altruistic).

$$\text{Generosity in Trust Game } [dfsi\ tg\ p2] = \frac{\text{Average amount returned as Player 2} - 0}{\text{Max possible average}} \quad (\text{F.3})$$

Beliefs about generosity in the TG [*guess dfsi tg p2*] For the guessing game about actions of Player 2 in the TG, an individual who expects all others to be ‘homo economicus’ would expect everyone to keep the entire sum at their disposal, independently of the amount received, implying an average amount returned of zero. We divide the guess by the maximum possible average, such that *guess dfsi tg p2* ranges from zero (for a subject who expects all others to always return zero) to one (for a subject who expects all others to always return the whole available amount).

$$\text{Belief about generosity in TG } [guess\ dfsi\ tg\ p2] = \frac{\text{Guess about average amount returned as Player 2} - 0}{\text{Maximum average possible}} \quad (\text{F.4})$$

Reciprocity in the TG [*recip*] We also estimate a measure of deviation from self-interest due to reciprocity. This is based on Player 2 behavior in the Trust Game. Specifically, we define reciprocity (*recip*) as the average effect of increases in the share of her initial endowment that P1 transfers to P2 on the share of this transfer passed back by P2 to P1. For instance, if Player 2 does not vary the share passed back as the share she receives increases, her reciprocity is 0; if instead the share passed back increases one-by-one with the share received, reciprocity takes a value of 1. We estimate this average effect by running the following regression separately for each individual observation in our sample

$$\frac{\text{P2 transfer to P1}}{\text{amount available to P2}} = \alpha + \phi \frac{\text{P1 transfer to P2}}{\text{P1's initial endowment}} + \epsilon$$

and then defining

$$\text{Reciprocity in TG } [recip] = \phi \quad (\text{F.5})$$

G Measures of policy preferences

To measure policy preferences on each single policy question, we scored the responses such that "Strongly Disagree" would take a value of -2; "Disagree" would take a value of -1, "Neither agree nor disagree" would take a value of 0; "Agree" would take a value of 1 and "Strongly Agree" would take a value of 2.

We then perform a standard PCA on these scores.

We find that the first principal component contains a much higher share of the variance than the others. Moreover, it appears to have a straightforward intuitive interpretation in terms of positioning of a students' preferences on the conservative-progressive scale, with higher values indicating more progressive policy opinions.

The first principal component assigns positive weights to questions 1, 3, 5, 6, 8, 10 and 11, and negative weights to questions 2, 4, 7 and 9.

- 'Pro-market' = $(+Q2 + Q4 + Q7 - Q8)/8$
- 'Pro-government intervention' = $(+Q1 + Q5 + Q6 - Q2)/8$
- 'Pro-green policies' = $(+Q5 + Q10)/4$
- 'Trust in government' = $+Q3/2$
- 'Immigration-restrictive' = $+Q9/2$

where Q_i represents the score (defined as above) from the response to question i .

- a course with conventional content, offered by the Department of Economics and using the Pindyck and Rubinfeld (2012) textbook (*Conventional I*);
- a course with conventional content, offered by the Department of Resource Economics and using the Perloff (2011) textbook (*Conventional II*);¹⁷
- a course with an innovative 'behavioral' curriculum, stressing externalities, incomplete contracts and social preferences, offered by the Economics Department and using the Bowles and Halliday (2020) textbook (*Post Walrasian*);
- an online course with a largely standard curriculum, apart from one section on the presence of social preferences, offered by the Economics department and using the Frank, Gilovich, and Regan (1993) textbook (*Conventional plus Social Preferences*);
- a course on *Nutrition and Metabolism* offered by the Food Science department, which we use as a control group.

The economics course which we call for brevity 'Post Walrasian' was taught by one of the authors. It incorporates research from behavioral economics into every aspect of the course, and does not present 'homo economicus' as the norm for behavior. Importantly, the 'Post-Walrasian' approach of the course was not signaled beforehand to students in any way: the brief course description that students could see in the course enrollment platform used by the University was identical to that of the conventional course offered by the same Department (*Conventional I*) and suggested no difference between the courses' content.

¹⁷This course is called 'Price Theory' but is completely equivalent in content and pre-requisites to a intermediate microeconomics course.