Does economics make you selfish?

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Abstract

It is widely held that studying economics makes you more selfish and politically conservative. We use a difference-in-differences strategy to disentangle the causal impact of economics education from selection effects. We estimate the effect of four different intermediate microeconomics courses on students' experimentally elicited social preferences and beliefs about others, and 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 somewhat less opposed to highly restrictive immigration policies.

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 (Stigler 1959; Colander 2005; O'Roark and Wood 2011).

There is some evidence (cited in the next section) that economics students are more conservative and self-regarding than their peers. But an important unresolved question is to what extent this reflects differential selection into economics rather than a causal effect of economics education.

To address this question we use a transparent difference-in-differences strategy to identify the causal effect of a one-semester intermediate microeconomics course on students' social preferences and policy opinions. We administered 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 used a Trust Game (TG), a triple Dictator Game (DG) with charities in the role of receiver, and two incentivized tasks eliciting subjects' expectations about the behavior of others in the same games.

Subjects' behavior in these games provides a measure of the extent to which an individual deviates from the (Nash-equilibrium) prediction of self-interest (which we term deviation from self interest, or DFSI). Our belief-elicitation tasks measure the extent to which subjects expect others to deviate from self-interest. We also included 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 the University of Massachusetts Amherst. The intermediate microeconomics courses include two courses following a standard curriculum. Because we know that differences in course content can have substantial long term effects on social and political attitudes (as documented, for example, by Cantoni et al. 2017, who study a recent curriculum reform in Chinese schools) we also look at the possible effects of different content of the economics courses. Our sample also includes students in an intermediate microeconomics course that, while listed in the course catalogue as identical to the conventional courses, devotes substantial attention to a variety of other topics: social preferences, asymmetric information, incomplete contracts, game theory, fairness and Pareto-efficiency as normative criteria, the benefits of cooperation (e.g. in commons tragedies), and competition. We call this course *Post-Walrasian*.[1] We also include students in a fourth course that is predominantly conventional but with some exposure to social preferences. Our control subjects are in a large course on nutrition.

We find that a one-semester intermediate microeconomics course has little to no effect on experimental measures of social preferences or on expectations about other people's social preferences. Our estimates of the effect on measures of altruism and reciprocity are close to zero and do not differ across the differing content of the courses. We also find little evidence of an effect on the students' policy preferences or political orientations. The one exception concerns immigration: studying intermediate microeconomics (whatever the course content) seems to make students less opposed to highly restrictive immigration policies.

2 Economics and Preferences: Theory and Evidence

Theoretically, studying economics might shift behavior towards self-interest through three main mechanisms: exposure, moral wiggle room, and reducing cognitive dissonance.

First, consider the powerful effect of mere exposure. By exposure we mean the introduction to and repeated interaction with an idea. In particular, a student learns about self-interest in economics courses, 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). Presenting self-interest as the norm for human behavior might thus have the unintended effect of making students more likely to adopt that norm themselves.

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

[1]The course was taught by one of the authors of this paper (Girardi) using the pre-publication draft of a textbook written by two of us (Bowles and Halliday).

their own self-interest with a positive self-concept (Mazar, Amir, and Ariely 2008; Dana, Weber, and Kuang 2007). How might learning microeconomics produce 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).

Finally, it has long been recognized in social psychology that actions can affect preferences as part of a cognitive dissonance reduction response (Festinger 1957; Ariely and Norton 2008). The 'effort justification' variant of this body of theory proposes that, as Xiao and Houser (2018) put it 'when one engages in a strenuous activity that one would not typically choose, one develops the perception that the activity is attractive in order to justify the effort.' By this reasoning, the effort that economics students spend choosing a strategy to maximize their payoff in a game, or a level of output to maximize the profits of a firm, or their market basket to maximize their self-regarding utility, could induce a shift towards more self-regarding preferences.

Empirically, a substantial literature has appeared in support of the idea that economists are more self interested.[2] There is also some (more limited) evidence that economists tend to hold more conservative policy preferences.[3] These studies do not identify a causal impact of studying economics, as distinct from a selection effect concerning who chooses to study economics.

A much smaller set of papers has addressed our question, namely, is there a causal effect of the study of economics on social values and policy preferences? Two identification strategies have been deployed. The first is to observe students' attitudes or behavior over time, contrasting those in economics courses with those taking other courses. Frey and Meier (2003) study (real-world) giving behavior of students in economics and other courses over their period at university. They find no evidence that studying economics reduces contributions. Bauman and Rose (2011), using a similar design, find no evidence that taking economics courses reduces the contributions of economic majors to a public interest group. However, they find a negative effect on the contributions of non-economics majors who take economics courses.

The second strategy is to implement a controlled experiment, briefly exposing randomly selected subjects to economic concepts or language, and a control group to an exposure that is otherwise similar but unrelated to economics, and then observing the difference in the before-after measures of interest. Ifcher and Zarghamee (2018) randomly assign some experimental subjects to the treatment – economics exposure – by

[2] Included are Marwell and Ames (1981), Carter and Irons (1991), Wang, Malhotra, and Murnighan (2011), Frank, Gilovich, and Regan (1993) and Rubinstein (2006). A few studies have instead found economists to be more generous or less opportunistic than others (Yezer, Goldfarb, and Poppen 1996). Konow (2019) shows that providing ethics instruction to students taking an economics course can increase generosity, though economics and business majors are less generous on average than other majors.

[3] For example O'Roark and Wood (2011) and Colander (2005).

means of language affirming "(1) that all individuals are self-interested and (2) that all individuals attempt to maximize their payments." Subjects then play incentivized games. The authors find that compared to subjects exposed to non-economic language, the exposure to economics shifts behavior towards self-interest.

In another experiment, Molinsky, Grant, and Margolis (2012) asked mid-career business leaders acting as "managers" to convey to a "subordinate," some bad news, for example reassignment to an undesirable location or dissatisfaction with the subordinate's job performance. Immediately prior to this, managers had been randomly selected to create a sensible phrase from a scrambled bunch of words, some of which contained economic content (for example, in unscrambled form: "analyse costs and benefits"), and some that did not (the control). In communicating the bad news to the subordinate the managers who had been exposed to the economic words experienced less empathy and conveyed less compassion to the subordinate than did those in the control group.

Our study belongs to the strand of literature that uses a difference-in-differences approach, comparing medium-term changes in students' behavior and beliefs among those with a sustained exposure to economics teaching and those without. The two other studies of this type (Frey and Meier 2003; Bauman and Rose 2011) measure a single outcome – giving behavior – in a natural setting. Our study draws upon a wide range of incentivized experimentally-elicited behaviors and beliefs, and measures of political orientation and policy opinions. Moreover, we are the first to study the effects of different course content.

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. 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 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. A course in 'Nutrition and Metabolism' serves as a control non-economics course. The economics courses vary: two courses (which we call *Conventional I* and *Conventional II*) are fairly standard intermediate microeconomics courses using Pindyck and Rubinfeld (2012) and Perloff (2011); a third (*Post Walrasian*) course 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 preferences*), is an online course using Frank (2008).

Figure 1 clarifies why we hypothesize that different economics courses could lead to different outcomes. It shows 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).[4] 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 A.4, 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).

We then ask the subject to guess the average contribution of the other participants. The subject's payoff depended on how close they were to the actual average: their

[4] 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.

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 (Berg, Dickhaut, and McCabe 1995). 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 mover 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).

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 guaranteed 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.[5]

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. Their 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.

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 $[0, 1]$.

[5]While 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.

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 B.

3.4 Policy preferences

We aggregate the information contained in the students' evaluation of the 11 policy statements into a smaller set of variables. We employ two alternative approaches to do this.

The first approach uses a Principal Component Analysis (PCA) to extract the four main principal components. We give them interpretative labels, based on the topics of the statements to which they give larger (positive or negative) weights. We interpret the first component as positioning a subject's policy views on a left-right scale ('Left-right'). The second component appears to measure support for and positive view of free markets ('Pro-market'). The third and fourth are labeled, respectively, 'Libertarian' and 'Communitarian'. See Appendix C for details, including the weights that each component gives to each statement.

The second approach takes simple averages of scores in statements which concern the same topic. Specifically, we consider five indexes. They 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. Details on these indexes are provided in Appendix C.

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:

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

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; $Econ$ equals 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.[6]

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:

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

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.[7] 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.

[6]Ideally, 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.

[7]The 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.[8] 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 E.4 to E.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

We start by simply looking at the distribution of changes in our outcomes of interest during the semester, comparing economics and non-economics students. As shown in Figure 2, changes during the semester are distributed similarly in the two groups, suggesting little effect of economics on social preferences and beliefs. This result is confirmed by our difference-in-differences estimations, which we now describe.

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 3 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

[8] We 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.

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 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. The blue bars in Figure 3a display 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 3a 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 Figure 3a.

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.

With respect to beliefs, Figure 3(a) shows that the estimated effect of economics on beliefs about other people's generosity in the DG is practically zero. 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 F.9 we estimate the effect of intermediate microeconomics including only female students, obtaining similar results .[9]

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 4. More details are provided in Appendix Tables G.2. 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

Tables 4 and 5, and the bottom panels of Figure 3, report our results about the effects of intermediate microeconomics courses on students' policy preferences. In particular, Table 4 and Figure 3(a) use the four principal components detected by our PCA; Table 5 and Figure 3(b) use simple averages of statements sharing a common topic. For symmetry with the analysis of social preferences, we report estimated effects in terms of average changes in the indexes and in terms of standard deviations. 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. On average, students enrolled in intermediate microeconomics are substantially and significantly more 'pro-market'. This is found both in the PCA analysis and in the analysis using simple averages. The 'pro-market' component from the PCA is higher by 0.45 standard deviations for economics students (s.e. 0.18); the average agreement with statements expressing a positive view of markets is higher by 0.48 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 0.052 for the PCA component and 0.032 for the simple average.[10] Economics

[9]The total number of female students in our sample is 84, and they are equally distributed between the control and the treated group (42 in each). We are not able to estimate effects for males only, because there are only 4 male students in the nutrition course that serves as a control group (Table 2).

[10] This result is robust to using alternative methods to adjust for multiple hypothesis testing. Specifically, the adjusted p-values are respectively 0.053 and 0.022 if using the Bonferroni-Holm method, and

students also display a higher pre-treatment average for the 'Left-right' component, by 0.19 standard deviations. This means that they are, on average, politically to the right of the control group students. This difference is, however, imprecisely estimated (s.e. 0.16) and we cannot reject the null hypothesis of no selection effect for this variable. Selection effects are rather small and indistinguishable from zero for all other measures of policy preferences.

We then turn to our difference-in-differences estimates of the effect of intermediate microeconomics. We find no effect on any of the four principal components that summarize students' policy positions, nor on their average 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 0.082.[11]

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 5). 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 the State of Massachusetts 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 aggregations we have performed allow us to convey results in a more compact and informative way, in Appendix Figure G.13 we also look at effects on each single policy statement, reaching similar conclusions: there is no substantial impact on any single policy statement, except for the effect on immigration policy.

Results are similar when including only female students, so they do not appear to be driven by gender differences between the treated and the control groups (Appendix

0.052 and 0.022 when using 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.

[11] Using the Bonferroni-Holm method produces an adjusted p-value of 0.073; the Sidak-Holm method gives an adjusted p-value of 0.071.

F.9).

The estimated effects on policy preferences also appear to display little difference based on course content. The bottom panel of Figure 4 reports separately the effect of different microeconomics courses. Most importantly, the positive effect on the 'immigration-restrictive' variable is visible in both the 'conventional' courses and the 'post-Walrasian' one. 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 values for the pro-market variable and the 'Left-right' components among economics students seem to be mostly 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 preferences, policy opinions, and beliefs about other people's social preferences.

The economics students in our sample start the semester with a more favorable opinion of market competition and relatively more conservative policy views, and display lower generosity and higher reciprocity in experimental games. But other than economics students being substantially more "pro market", these effects of differential selection into economics are relatively small and imprecisely estimated.

We found little to no causal effect of studying economics on social preferences and beliefs about other people's social preferences. Differences in these outcomes between economics students and the control group did not change during the semester, and are also unaffected by the content of the economics course. We find no effect on an aggregate "left-right" measure of political positions, nor on views of markets, government intervention, and green policies. The sole evidence of a substantial effect is that economics students come to express less opposition to a highly restrictive statement about immigration policy. This effect is economically relevant, but only marginally significant when accounting for multiple-hypothesis testing. Further research will be needed to assess the robustness of this result, and, should it prove robust, evaluate the mechanisms.

Overall, our results are thus consistent with the two other difference-in-differences studies of the effects of a substantial exposure to economics teaching (Frey and Meier 2003; Bauman and Rose 2011). We do not conclude, on the basis of our study of a single semester in intermediate microeconomics, that economics does not make you selfish. It could be that the main effect of studying economics occurs at the introductory level, or that a single semester is too brief an exposure to produce a detectable effect. The Bauman and Rose (2011) study, however, suggests caution in accepting this explanation of our results. They found that the negative effect of studying economics among non-majors was larger for the intermediate than for the introductory courses, and estimated that an additional single semester of economics (at whatever level) reduced contributions by a substantial amount.

The differences between our study and those based on a brief experimentally induced

exposure to economics (Ifcher and Zarghamee 2018; Molinsky, Grant, and Margolis 2012) arise because we are measuring different things. The experimentally induced exposure to economics leveraged by these studies provides a frame or a prime, suggesting the type of problem that is being addressed or activating particular mental modules. The framing or priming then constitutes a particular state in which the decision-maker acts. The results of these experiments show that social preferences are state-dependent (a psychologist would say, situation-dependent).

While the duration of these state-dependent effects has not adequately been studied, an implication of this interpretation is that the effects of brief experimentally induced exposure to economics should be temporary. An example of such transient state-dependent effects is a standard repeated public goods experiment in which moral or neutral messages are delivered to subjects: the immediate and substantial positive effect of the moral messages entirely vanished after 10 rounds of play (Dal Bó and Dal Bó 2014).

The more extended and natural-setting exposure to economics in our study could have both state-dependent effects and longer term learning effects, by which preferences change in a durable (not state-dependent) manner. A conclusion consistent with the evidence from previous studies (along with our own) would be that exposure to economics has state-dependent effects on preferences, but does not produce the durable changes in preferences associated with the term endogenous preferences.

We outlined at the outset a line of reasoning that might lead us to affirm the commonplace view that studying economics leads to more self-interested behavior. But there are also cogent reasons to expect the opposite. 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.[12] 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' property rights and reciprocating goodwill (eg, not stealing the other's goods) can be the basis for mutually beneficial exchange. Exposure to this message could promote social preferences as well as self-interest.[13]

One possible explanation for our results is that the potential mechanisms we outlined at the outset, through which studying economics would promote self-interest, are just not active, or not powerful enough to produce a discernible effect. It is also possible, however, that these mechanisms are present, but are offset by "doux commerce" mechanisms, as the ones we just described, working in the opposite direction.

[12]See Bowles (2016). Smith, for example, contrasted the probity of merchants with the untrustworthiness of ambassadors and provided a verbal model of the reasons for the difference.

[13]This was the primary 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

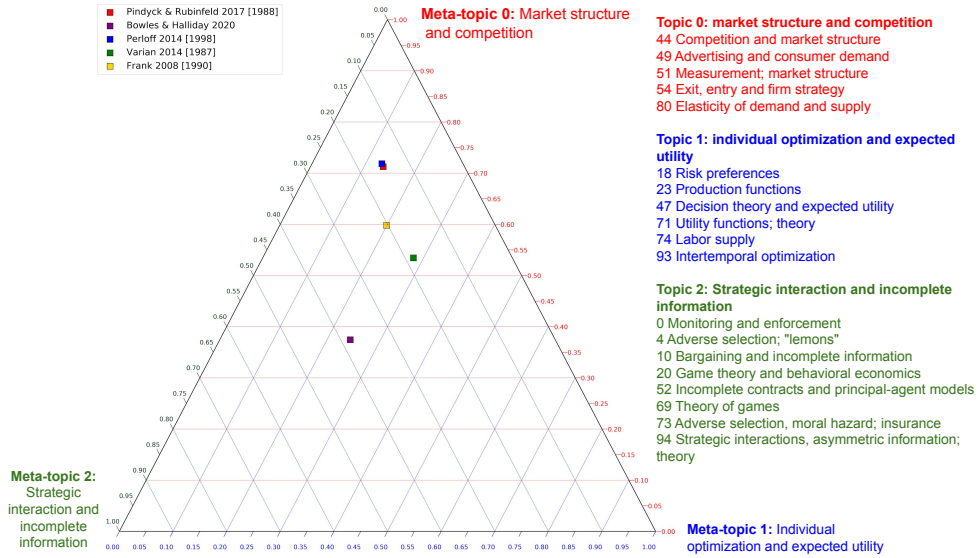
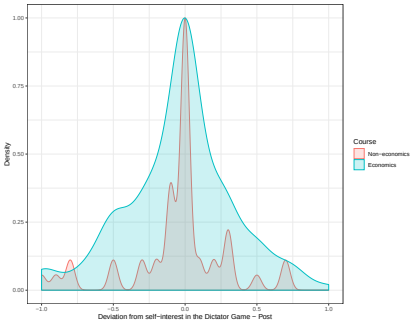
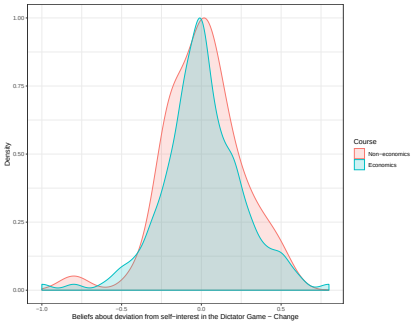


Figure 1: The location of microeconomics textbooks in a 3 meta-topics space.

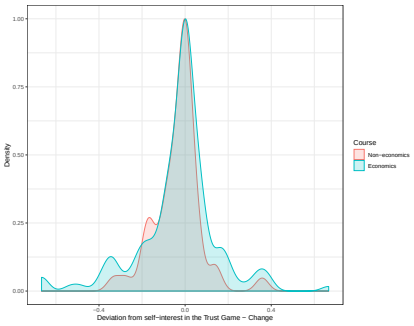
Notes: Coordinates of the textbooks are the topic weights for the meta-topics at the vertices. For example, Varian has a location of (0.53, 0.28, 0.19), that is, a weight of 0.58 on market structure (meta-topic 0), a weight of 0.28 on individual optimization and expected utility (meta-topic 1), and a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



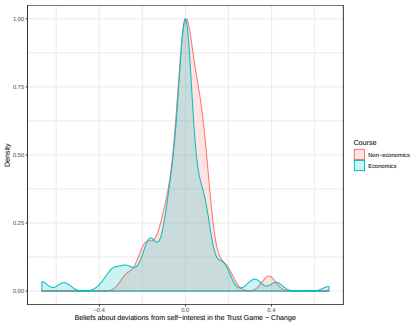
(a) Generosity in Dictator Game



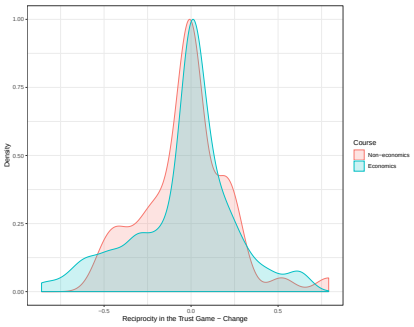
(b) Beliefs about generosity in DG



(c) Generosity in Trust Game



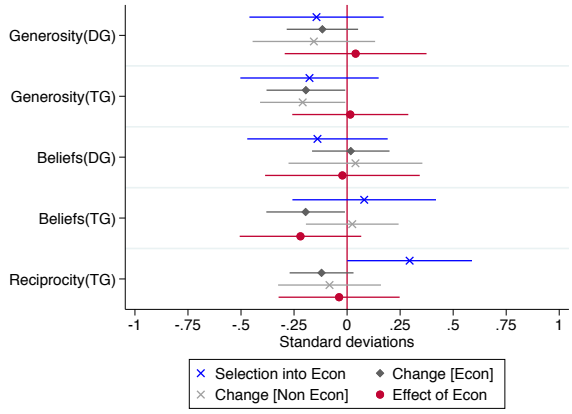
(d) Beliefs about generosity in TG



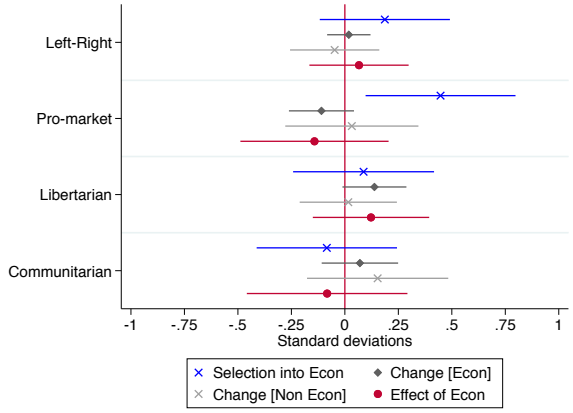
(e) Reciprocity in Trust Game

Figure 2: Experimental measures of social preferences - distribution of changes during the semester

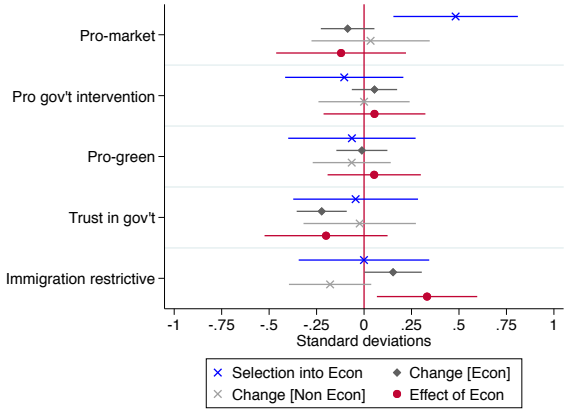
Notes: Smoothed density plots for the distribution of changes during the semester. Distribution of changes for economics students in light blue; distribution of changes for the control group (nutrition students) in red. See Section 3.3 and Appendices B and C for the definition of each variable.



(a) Social preferences and beliefs



(b) Policy views: principal components

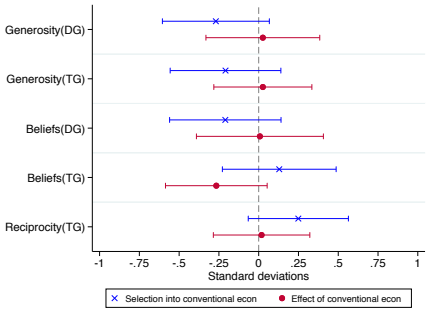


(c) Policy views: simple averages

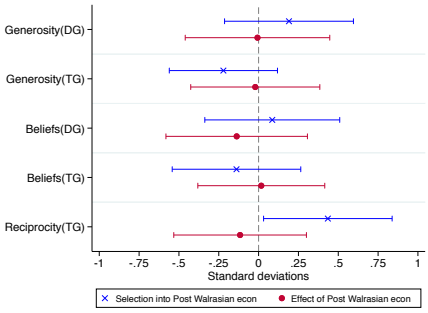
Figure 3: Effect of Intermediate Microeconomics on students' social preferences, beliefs and policy preferences

Notes: The Figures display visually our difference-in-differences 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 Appendices B and C for the definition of each outcome variable. All outcome variables are standardized. For each outcome of interest, the graphs display: differences between averages for Intermediate Microeconomics and non-economics students in the first (pre-treatment) survey round ('Selection into Econ'); the average change during the semester among Intermediate Microeconomics students ('Change [Econ]') and non-economics students ('change [Non econ]'); and our difference-in-differences estimate of the effect of Intermediate Microeconomics ('Effect of Econ', given by the difference between the two changes). Dots represent point estimates, bars are 95% confidence intervals from standard errors clustered at the individual level.

Social preferences and beliefs

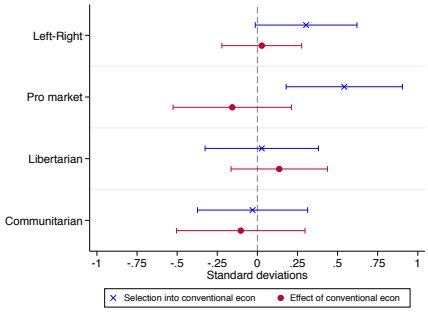


(a) Conventional curriculum

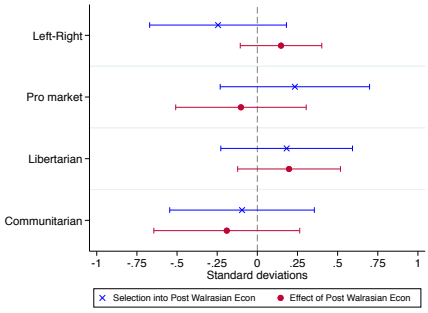


(b) Post Walrasian curriculum

Policy views: principal components

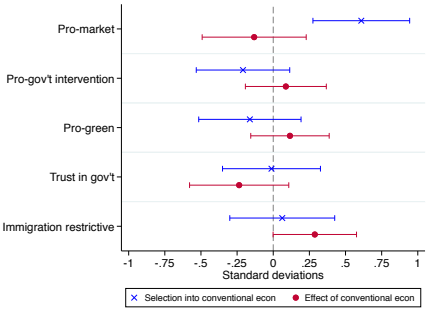


(c) Conventional curriculum

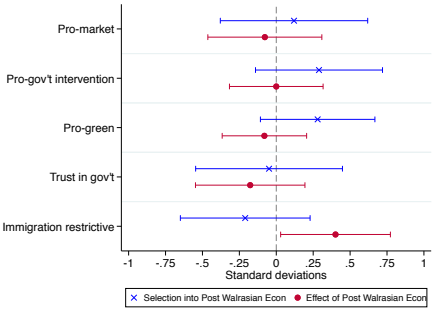


(d) Post Walrasian curriculum

Policy views: simple averages



(e) Conventional curriculum



(f) Post Walrasian curriculum

Figure 4: 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 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 Appendices B and C 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 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 *[dfsi dg]*	(2) Generosity in Trust Game *[dfsi tg p2]*	(3) Beliefs about generosity (DG) *[guess dfsi dg]*	(4) Beliefs about generosity (TG) *[guess dfsi tg p2]*	(5) Reciprocity in Trust Game *[recip]*
Mean Before (Econ)	0.600	0.357	0.469	0.319	0.303
	(0.028)	(0.013)	(0.018)	(0.012)	(0.024)
Mean Before (Non Econ)	0.650	0.385	0.500	0.307	0.215
	(0.051)	(0.023)	(0.033)	(0.022)	(0.044)
Selection (into Econ)	-0.050	-0.028	-0.031	0.012	0.088
	(0.058)	(0.026)	(0.037)	(0.024)	(0.05)
Change (Econ)	-0.040	-0.031	0.004	-0.028	-0.036
	(0.03)	(0.015)	(0.021)	(0.014)	(0.023)
Change (Non Econ)	-0.054	-0.033	0.009	0.004	-0.025
	(0.051)	(0.016)	(0.035)	(0.016)	(0.037)
DiD (Effect of Econ)	0.014	0.002	-0.005	-0.032	-0.011
	(0.059)	(0.022)	(0.041)	(0.021)	(0.044)
Standardized DiD (Effect of Econ)	0.040	0.015	-0.022	-0.220	-0.038
	(0.171)	(0.14)	(0.186)	(0.146)	(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 B 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 estimates of the effect of Intermediate Microeconomics on students' policy views (principal components)

	(1) *Left-Right*	(2) *Pro market*	(3) *Libertarian*	(4) *Communitarian*
Mean Before (Econ)	0.067	0.187	-0.038	-0.066
	(0.128)	(0.102)	(0.087)	(0.08)
Mean Before (Non Econ)	-0.241	-0.409	-0.131	0.021
	(0.221)	(0.215)	(0.157)	(0.152)
Selection (into Econ)	0.308	0.596	0.094	-0.087
	(0.256)	(0.238)	(0.18)	(0.172)
Change (Econ)	0.031	-0.146	0.148	0.072
	(0.085)	(0.103)	(0.082)	(0.094)
Change (Non Econ)	-0.078	0.043	0.017	0.157
	(0.175)	(0.211)	(0.124)	(0.173)
DiD (Effect of Econ)	0.109	-0.189	0.131	-0.085
	(0.195)	(0.235)	(0.149)	(0.197)
Standardized DiD (Effect of Econ)	0.066	-0.142	0.122	-0.083
	(0.118)	(0.177)	(0.139)	(0.192)
N	404	404	404	404

Notes: This table reports difference-in-differences estimates for the effect of a semester-long intermediate microeconomics course on students' policy views. We use Principal Component Analysis to extract the four main components from the 11 policy statements that we ask participants to score. 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 5: Difference-in-differences (DiD) estimates of the effect of Intermediate Microeconomics on students' policy preferences (simple averages)

	(1) *Pro-Market*	(2) *Pro-Gov't intervention*	(3) *Pro-Green*	(4) *Trust in gov't*	(5) *Immigration restrictive*
Mean Before (Econ)	0.155	0.299	0.538	0.272	-0.359
	(0.027)	(0.029)	(0.031)	(0.037)	(0.048)
Mean Before (Non Econ)	0.003	0.337	0.565	0.293	-0.359
	(0.046)	(0.05)	(0.064)	(0.07)	(0.095)
Selection (into Econ)	0.152	-0.038	-0.027	-0.021	-0.000
	(0.053)	(0.058)	(0.072)	(0.08)	(0.107)
Change (Econ)	-0.027	0.020	-0.005	-0.106	0.093
	(0.023)	(0.022)	(0.029)	(0.032)	(.047)
Change (Non Econ)	0.011	-0.000	-0.027	-0.011	-0.109
	(0.05)	(0.045)	(0.044)	(0.072)	(0.067)
DiD (Effect of Econ)	-0.038	0.020	0.022	-0.095	0.202
	(0.055)	(0.05)	(0.052)	(0.078)	(0.082)
Standardized DiD (Effect of Econ)	-0.121	0.055	0.053	-0.200	0.332
	(0.174)	(0.137)	(0.125)	(0.165)	(0.135)
N	404	404	404	404	404

Notes: This table reports difference-in-differences estimates for the effect of a semester-long intermediate microeconomics course on students' policy preferences. Outcome variables are simple averages of scores for policy statements concerning the same topic. See Section 3.4 for the precise 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.

Online Appendix

A Experimental Design, Survey Details and Questionnaire

A.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.[14]
- The second round was conducted between April 8 and April 24, 2019.

The complete surveys (in PDF) are included as online appendices.

The steps of the experimental design are explained in the section "Experimental Design" of the main paper. Nonetheless, we reiterate them here for clarity. The survey includes the following:

- 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 A.4, 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.

[14] We 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'.

A.1.1 Triple Dictator Game and belief elicitation

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).

We then ask the subject to guess the average contribution of the other participants. The subject's payoff 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.

A.1.2 Trust Game and belief elicitation

In the Trust Game (TG), participants are anonymously and randomly paired (Berg, Dickhaut, and McCabe 1995). 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 mover 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).

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.

A.1.3 Matching Rules and Payments

To determine payments, 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 guaranteed 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.[15]

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. Their 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.

A.2 Sample

Our sample comprises students from the following five courses:

- 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*);[16]
- 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.

As shown in Table 1 in the main text, a total of 295 students were enrolled in these courses. 202 of them completed both rounds of our survey.

[15] While 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.

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

A.3 Recruitment

To be eligible to participate in the experiment, a subject had to be registered for one of the following five courses.

- Conventional I
- Conventional II
- Post-Walrasian
- Conventional + SP
- Nutrition

Each subject received a recruitment email at each stage of the experiment (start of the semester and end of the semester). The text of the email is included in the experimental instructions appendix. The email contained a link to the survey and experimental tasks using Qualtrics. Only subjects who completed the surveys at both stages were included in the sample.

Students in the sample received an invitation email signed 'Research Group on Human Behavior – UMass Amherst'. 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.

A.3.1 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.

A.4 List of policy statements

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 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 Commonwealth of Massachusetts.

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.

B 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.

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

Beliefs about generosity in the DG [*guess dfsi dg*] We use Equation B.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).

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

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).

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

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).

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

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

$$\text{Reciprocity in TG } [\textit{recip}] = \phi \tag{B.5}$$

C Aggregate 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 aggregate the information into a smaller number of variables, using two alternative approaches. First, we perform a standard Principal Component Analysis (PCA) on the scores given by students to these statements. We extract the first four components (which together explain 60.8% of variation in the data) and use them in estimation. Table C reports the loading matrix for all components and the proportion of variance explained by each.

The second approach consists in taking simple average scores of statements concerning the same topic. Specifically, we consider the following five indexes:

- '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 Qi represents the score (defined as above) from the response to question i (questions are listed in Appendix A, Section A.4).

Table C.1: Principal component analysis - Loading matrix

	Comp 1	Comp 2	Comp 3	Comp 4	Comp 5	Comp 6	Comp 7	Comp 8	Comp 9	Comp 10	Comp 11
Q1 (Govt. ensure basic needs met)	0.421	0.123	-0.126	0.029	0.256	-0.182	0.108	0.564	-0.592	0.110	-0.020
Q2 (Govt. can't improve markets)	-0.182	0.318	-0.112	0.610	-0.401	0.171	-0.385	0.073	-0.182	0.260	-0.191
Q3 (Trust in the state Govt.)	0.278	0.373	-0.248	-0.081	-0.020	0.497	0.513	-0.319	0.012	0.320	0.002
Q4 (Trust in the market)	-0.184	0.562	-0.107	0.009	-0.117	0.051	0.157	0.297	0.127	-0.625	0.320
Q5 (Pro carbon tax)	0.426	0.149	-0.089	-0.099	0.263	0.272	-0.454	-0.021	0.228	-0.343	-0.509
Q6 (Pro $15 min wage)	0.402	-0.001	-0.294	0.250	-0.046	-0.349	-0.054	0.162	0.628	0.250	0.282
Q7 (Market competition good)	-0.094	0.526	0.168	-0.033	0.140	-0.642	0.123	-0.307	0.023	0.095	-0.367
Q8 (Market competition harmful)	0.218	-0.250	0.192	0.686	0.082	-0.017	0.415	-0.181	-0.027	-0.387	-0.139
Q9 (Severely restrict immigration)	-0.302	0.145	0.156	0.266	0.784	0.216	-0.108	0.049	0.125	0.210	0.238
Q10 (Sustain the environment)	0.406	0.177	0.344	0.009	-0.064	-0.037	-0.358	-0.423	-0.265	-0.075	0.546
Q11 (Don't prohibit pornography)	0.155	0.120	0.772	-0.069	-0.208	0.177	0.127	0.390	0.258	0.194	-0.114
Proportion Explained	24.688%	16.122%	10.421%	9.590%	7.575%	6.789%	5.843%	5.280%	5.086%	4.661%	3.945%

D Detailed results of the survey eliciting policy views

Figure D.1: Scoring of policy statements: beginning of the semester

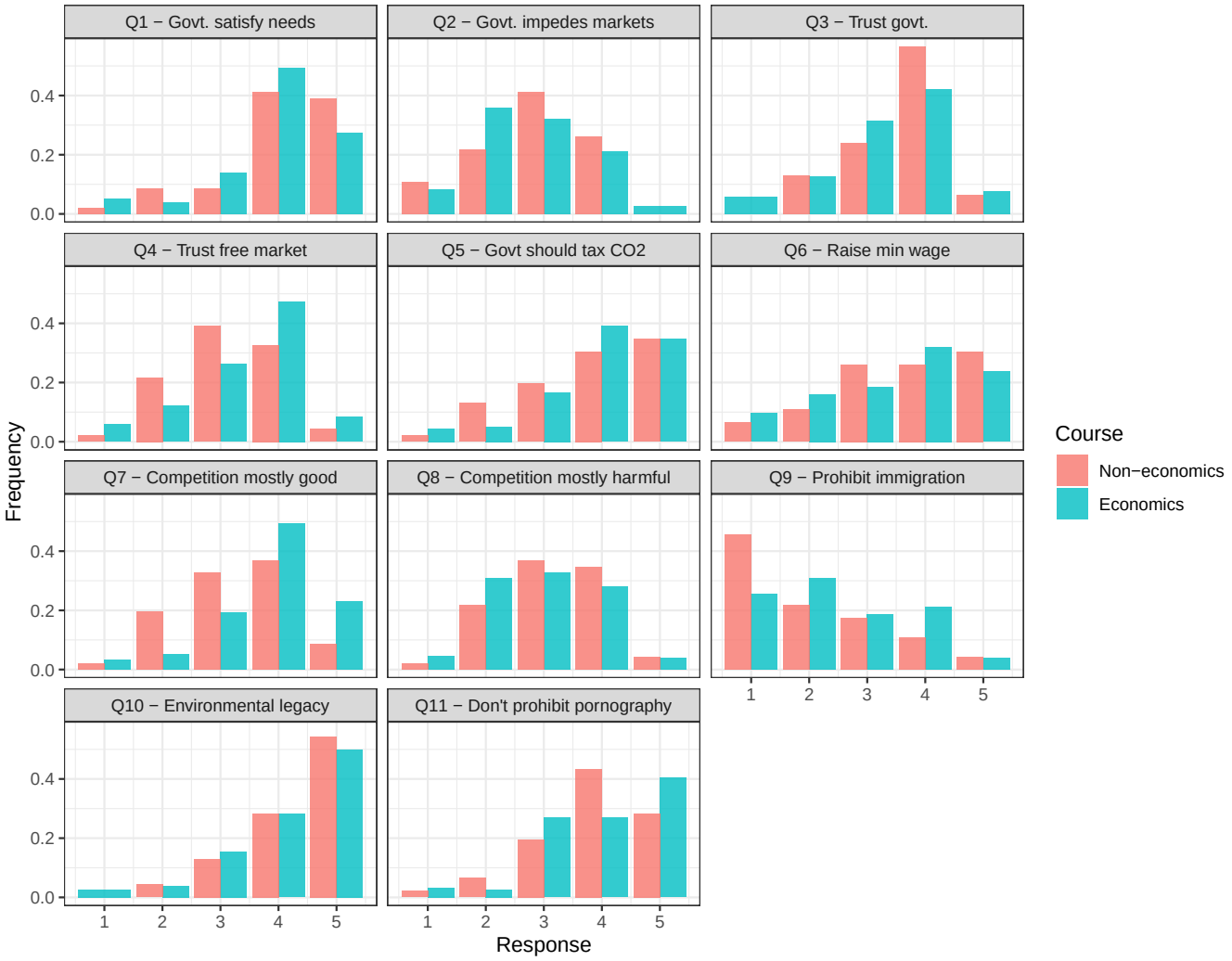


Figure D.2: Scoring of policy statements: end of the semester

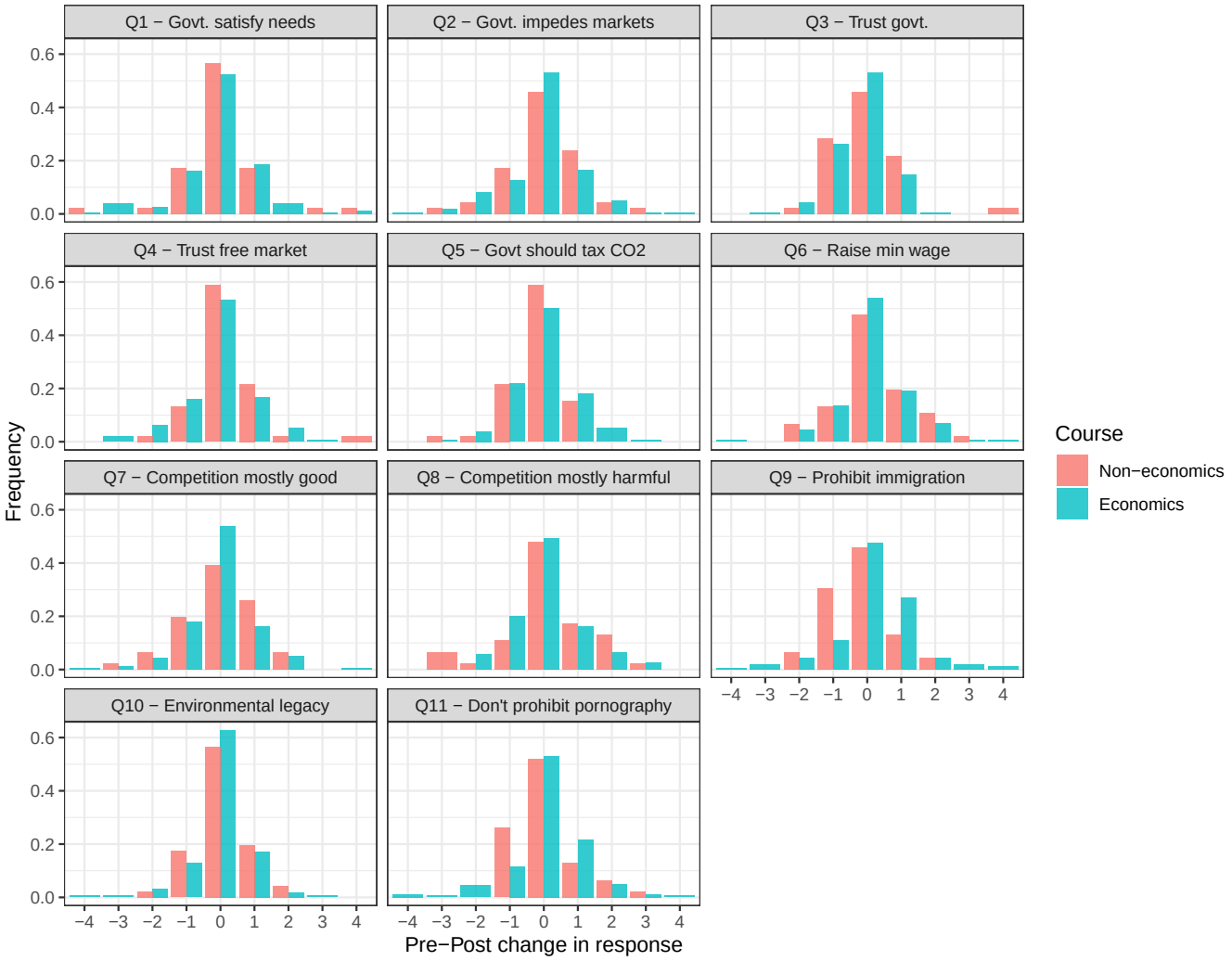
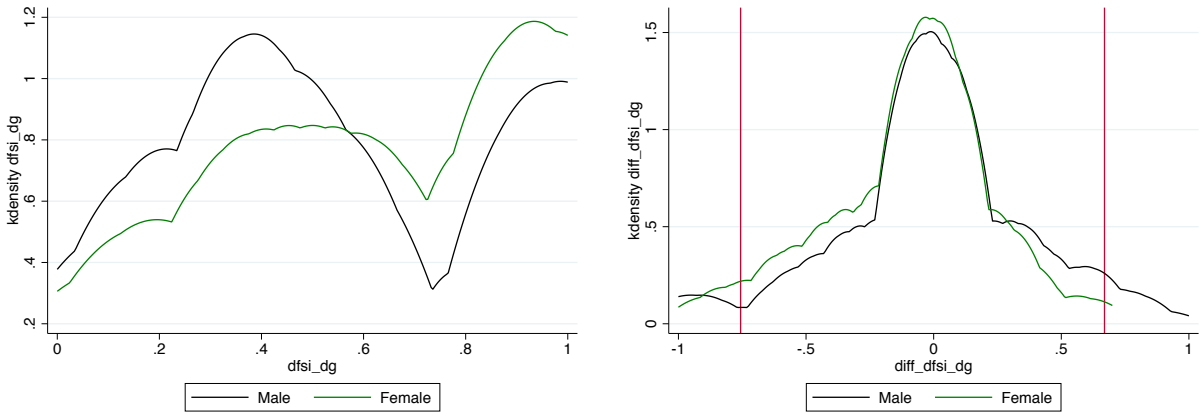


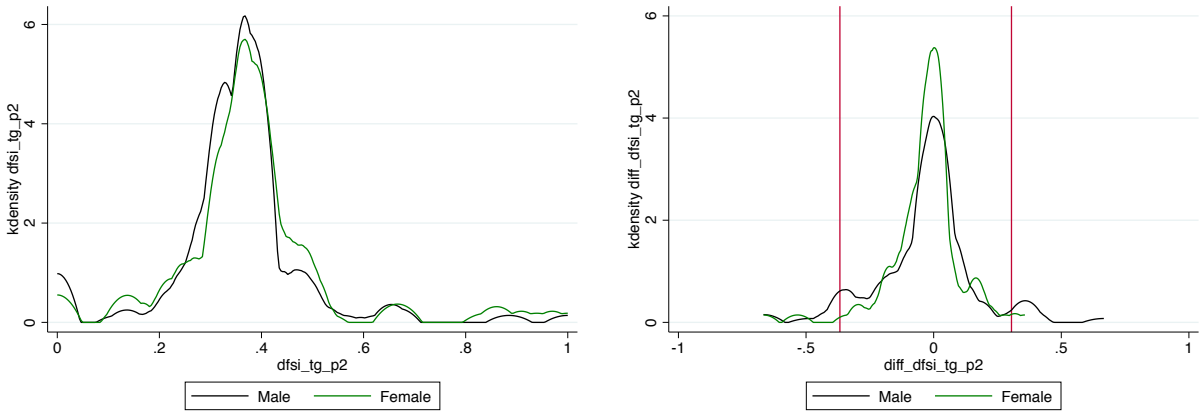
Figure D.3: Scoring of policy statements: change during the semester

E Behavior in games by gender



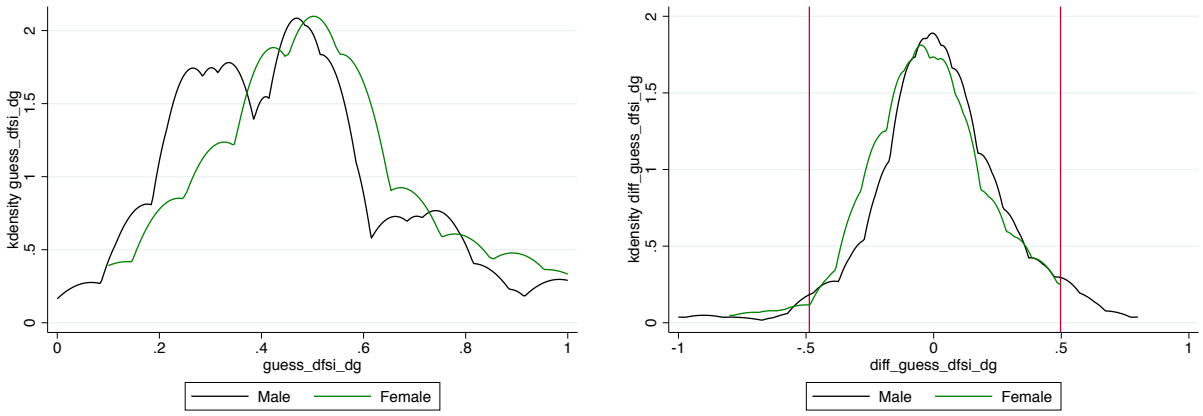
(a) beginning of semester (b) change over the semester

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



(a) beginning of semester (b) change over the semester

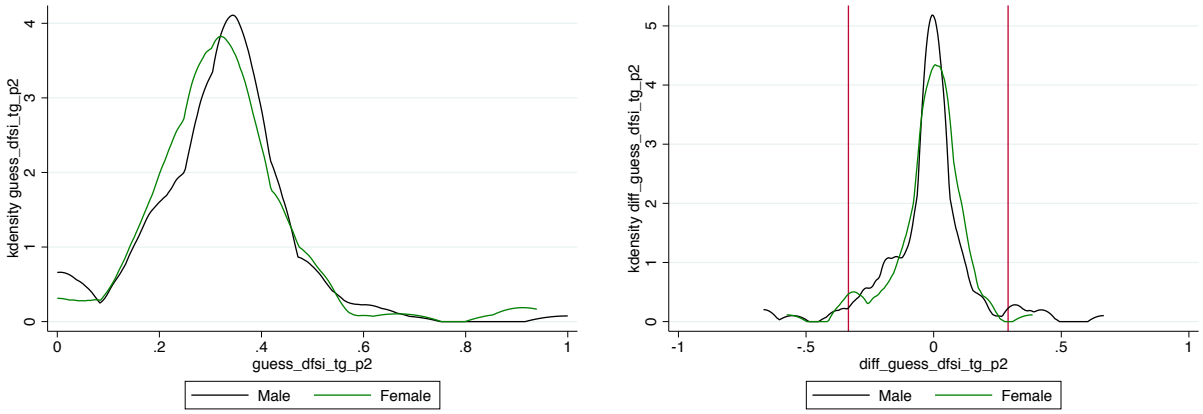
Figure E.5: Deviation from self-interest in the TG game (Player 2), by gender



(a) beginning of semester

(b) change over the semester

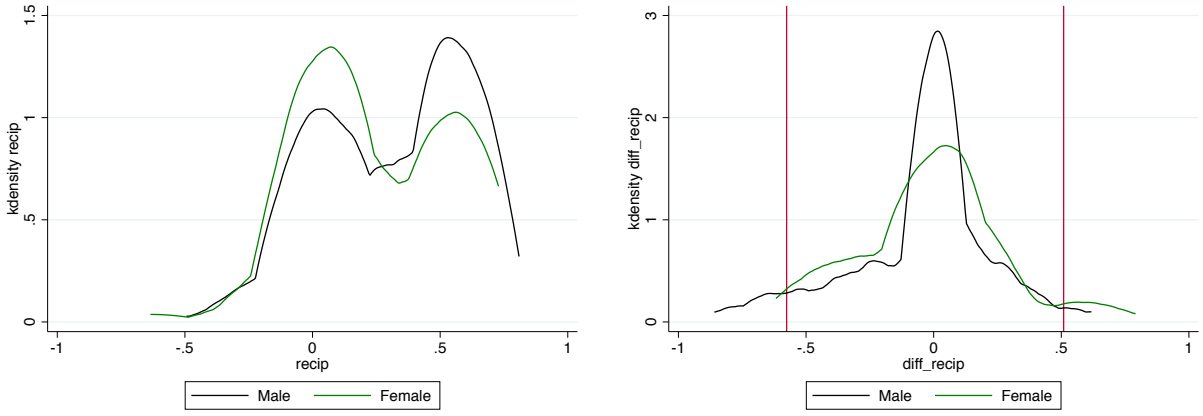
Figure E.6: Guess about deviation from self-interest in the DG game, by gender



(a) beginning of semester

(b) change over the semester

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

F Estimates including only female students

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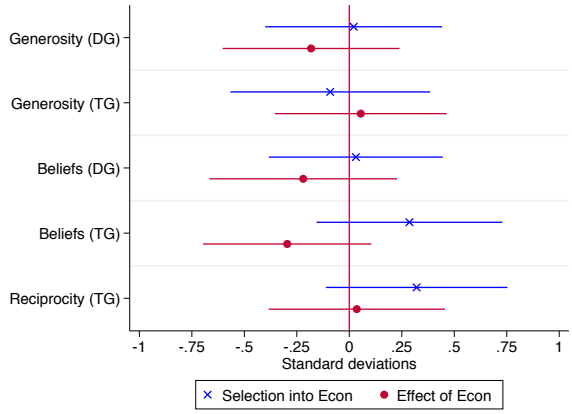
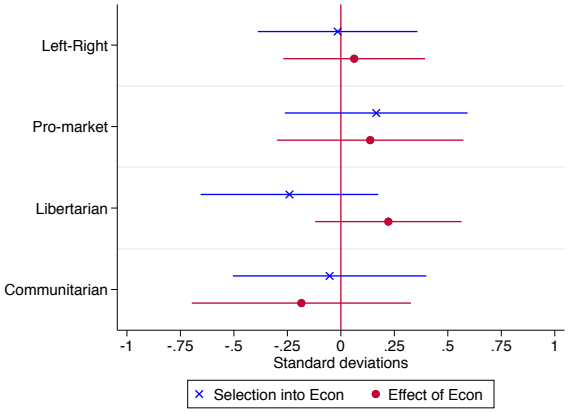
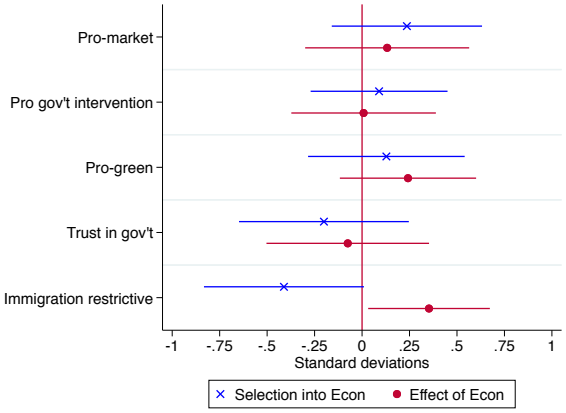


(a) Social preferences and beliefs



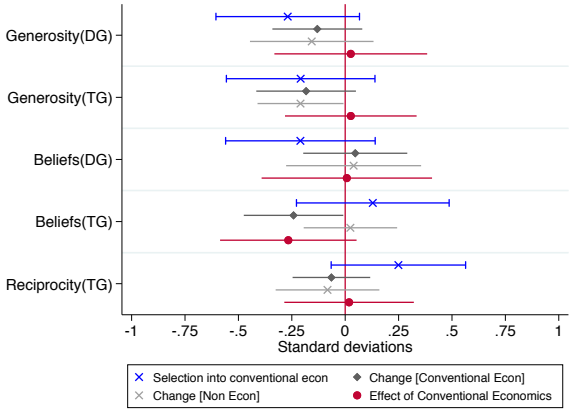
(b) Policy views: principal components



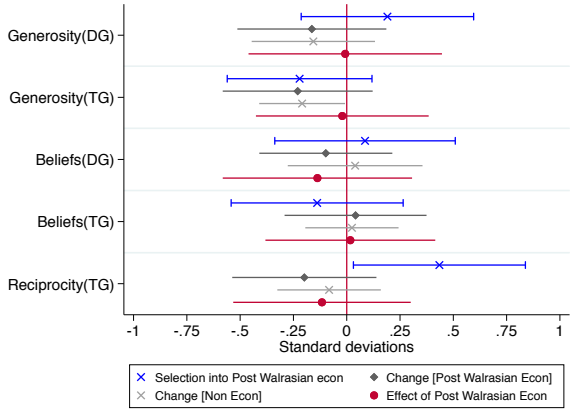
(c) Policy views: simple averages

Figure F.9: Effect of Intermediate Microeconomics on the social preferences, beliefs and policy preferences of female students

G Details on the effect of different course content

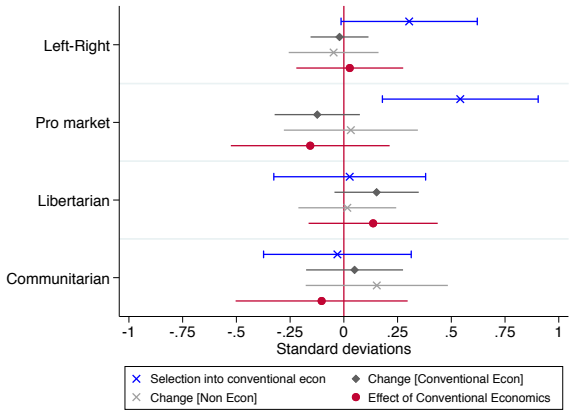


(a) Walrasian course

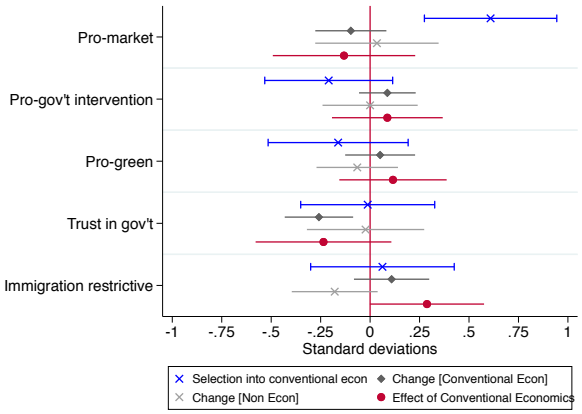


(b) Post Walrasian course

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

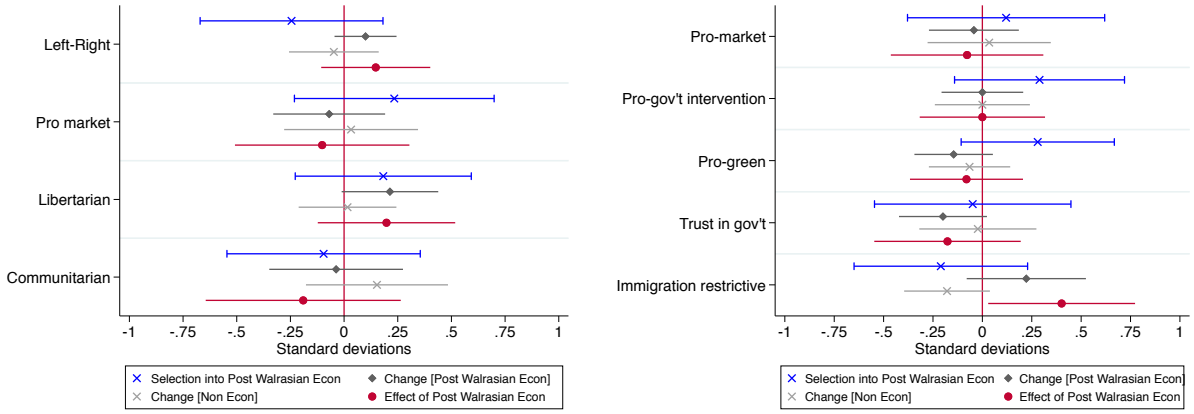


(a) Principal Components



(b) Simple Averages

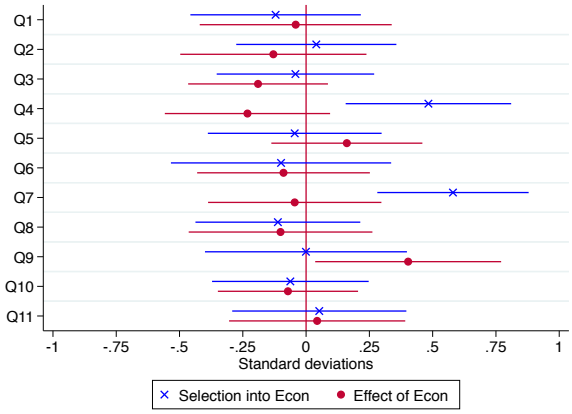
Figure G.11: Effect of Walrasian Intermediate Microeconomics on policy preferences - Principal components and simple averages



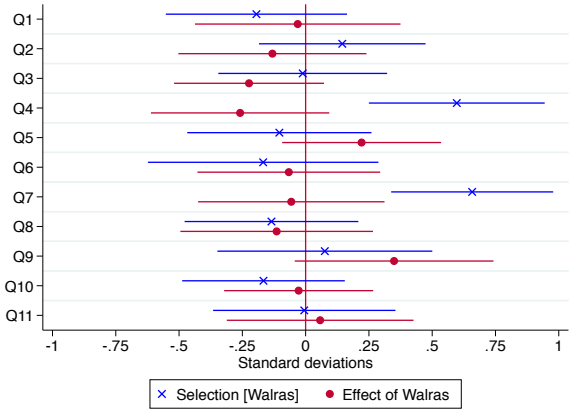
(a) Principal components

(b) Simple Averages

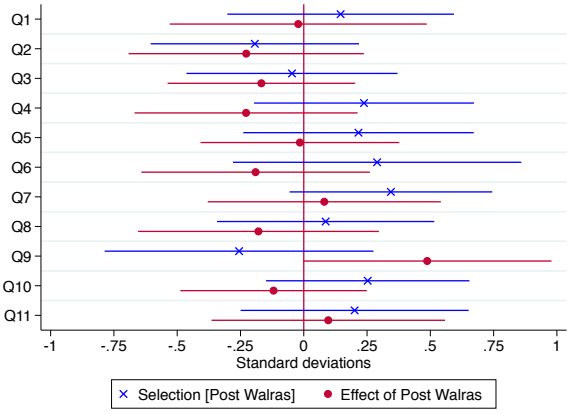
Figure G.12: Effect of Post Walrasian Intermediate Microeconomics on policy preferences - principal components and simple averages



(a) Intermediate Microeconomics



(b) Walrasian Course



(c) Post Walrasian Course

Figure G.13: Effect of Intermediate Microeconomics – all single policy statements

Table G.2: Difference-in-differences (DiD) estimates of the effect of Intermediate Microeconomics on social preferences – Walrasian vs. Post Walrasian curriculum

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

Notes: See Section 3.3 and Appendix B 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 G.3: Difference-in-differences (DiD) estimates of the effect of Intermediate Microeconomics on policy views – Walrasian vs. Post Walrasian curriculum (simple averages)

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

Notes: See Section 3.4 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.

Table G.4: Difference-in-differences (DiD) estimates of the effect of Intermediate Microeconomics on policy views – Walrasian vs. Post Walrasian curriculum (principal components)

	(1) *Left-Right*	(2) *Pro market*	(3) *Libertarian*	(4) *Communitarian*
Mean Before (Walras)	0.260	0.313	-0.102	-0.010
	(0.147)	(0.119)	(0.111)	(0.095)
Mean Before (Post Walras)	-0.644	-0.097	0.064	-0.077
	(0.281)	(0.231)	(0.159)	(0.180)
Mean Before (Non Econ)	-0.241	-0.409	-0.131	0.021
	(0.222)	(0.216)	(0.158)	(0.153)
Selection (Walras)	0.501	0.721	0.029	-0.031
	(0.266)	(0.246)	(0.193)	(0.180)
Selection (Post Walras)	-0.403	0.311	0.195	-0.098
	(0.358)	(0.316)	(0.224)	(0.236)
Change (Walras)	-0.033	-0.165	0.163	0.051
	(0.113)	(0.135)	(0.107)	(0.118)
Change (Post Walras)	0.165	-0.092	0.229	-0.038
	(0.121)	(0.177)	(0.123)	(0.163)
Change (Non Econ)	-0.078	0.043	0.017	0.157
	(0.176)	(0.212)	(0.124)	(0.173)
DiD (Effect of Walras)	0.045	-0.208	0.146	-0.106
	(0.209)	(0.251)	(0.164)	(0.210)
DiD (Effect of Post Walras)	0.243	-0.136	0.211	-0.195
	(0.214)	(0.276)	(0.175)	(0.238)
Standardized				
DiD (Effect of Walras)	0.028	-0.156	0.136	-0.103
	(0.127)	(0.188)	(0.153)	(0.204)
DiD (Effect of Post Walras)	0.147	-0.102	0.198	-0.190
	(0.130)	(0.207)	(0.163)	(0.232)
N	378	378	378	378

Notes: See Section 3.4 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.