

Does studying 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.

KEYWORDS

economics education, social preferences

JEL CLASSIFICATION

A20, D64, C91, D90

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

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argument goes, studying economics may also promote policy opinions typically considered conservative (Colander, 2005; O'Roark & Wood, 2011; Stigler, 1959).

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, e.g., 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.

The results could depend on the fact 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

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

substantial amount. A one-semester class may thus offer a sufficient intervention to change a student's preferences and is therefore worthy of study.

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 (Birch & Marlin, 1982; Murphy et al., 1995; Murphy & Zajonc, 1993; Zajonc, 1965). Exposing students to self-interest as the norm for human behavior might thus have the unintended effect of making students more likely to adopt that behavior themselves. With exposure, students may experience instruction around self-interest as the description of an injunctive norm: that is, a description about how economists think people *ought* to behave, rather than merely a description of how they *do* behave (Cialdini et al., 1998; Pickup et al., 2020).

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 (Dana et al., 2007; Mazar et al., 2008). 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 (Dana et al., 2007; Gino et al., 2013; Shalvi et al., 2015).

Finally, it has long been recognized in social psychology that actions can affect preferences as part of a cognitive dissonance reduction response (Ariely & Norton, 2008; Festinger, 1957). 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.

²Included are Marwell and Ames (1981), Carter and Irons (1991), Wang et al. (2011), Frank et al. (1993) and Rubinstein, 2006. A few studies have instead found economists to be more generous or less opportunistic than others (Yezer et al., 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.

³For example O’Roark and Wood (2011) and Colander (2005).

A much smaller set of articles 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.⁴

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 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 et al. (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 (e.g., 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 (Bauman & Rose, 2011; Frey & Meier, 2003) 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.

The differences between our study and those based on a brief experimentally induced exposure to economics (Ifcher & Zarghamee, 2018; Molinsky et al., 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).

⁴An earlier exercise with a similar logic is briefly presented in Frank et al. (1993, p. 168). They administer two questions concerning an ethical dilemma to students in two introductory microeconomics courses and an astronomy course, at the beginning and at the end of the semester. Different from the other studies reviewed here, no material incentive is involved. Results are presented informally through a histogram, suggesting that economics students display some differential movement towards less honest responses, but no formal statistical test is performed. This exercise can be seen as a precursor to the difference-in-difference studies described here.

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 & Dal Bó, 2014).

The more extended and natural-setting exposure to economics in our study could have both temporary state-dependent effects and longer term learning effects, by which preferences change in a durable (not state-dependent) manner.

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 (DG) and reciprocity (TG), 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 (2022) 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). The four intermediate microeconomics courses all had the same enrollment prerequisites and identical description in the online enrollment system.

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, employing the topic modeling analysis of Bowles and Carlin (2020).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

⁵Specifically, Figure 1 was obtained by applying to the three textbooks used in our experiment the topic model developed by Bowles and Carlin (2020), to identify three important meta-topics that are at the heart of microeconomics research. We thank Sahana Subramanyam for assistance in performing this analysis and producing Figure 1.

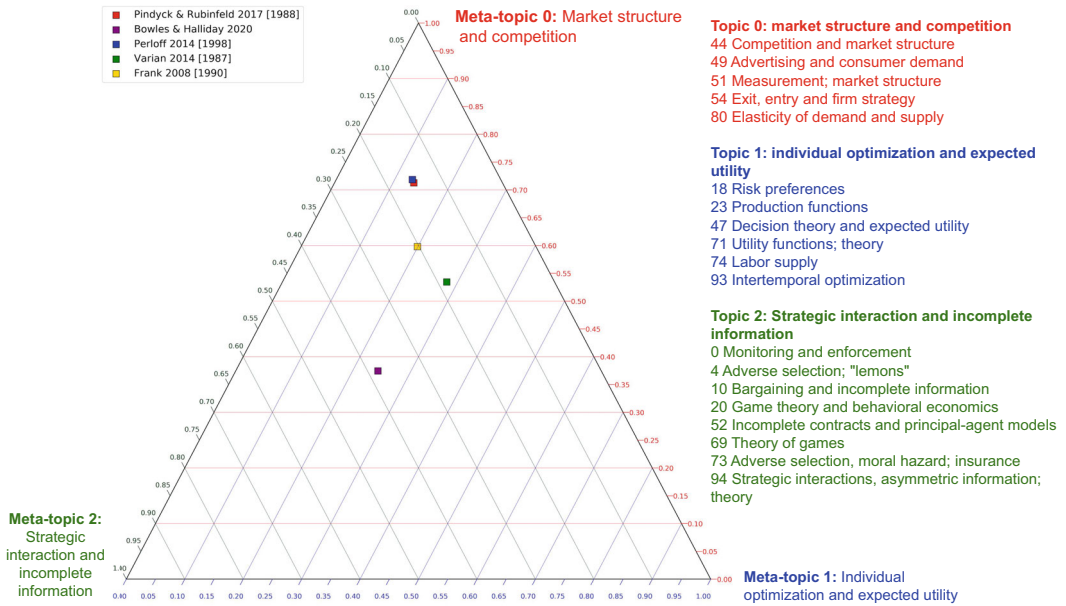


FIGURE 1 The location of microeconomics textbooks in a three meta-topics space. *Notes:* This Figure was obtained by applying the topic modeling analysis developed by Bowles and Carlin (2020) to the textbooks used in the microeconomics courses in our experiment. Coordinates of the textbooks are the topic weights for the meta-topics at the vertices. For example, Varian has a location of (0.53, 0.28, and 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).

constrained maximization, whereas Bowles and Halliday (2022) 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 (2022) 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.⁶

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.

In the Trust Game (TG), participants are anonymously and randomly paired (Berg et al., 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.⁷ In each pair, one was randomly selected as first-mover and the other as second-mover. We performed the random matching of participants 1 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 1 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.⁸

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

⁶This matching subsidy creates a strong incentive for altruistic individuals to donate through the experiment. This addresses a major concern with the DG with charities: in the absence of matching, altruistic participants might personally send a share of their payoff to the same charity or a different one post-experiment (Knowles & Servátka, 2015, p. 57). It is also consistent with Dictator Games run alongside Trust Games in previous literature (Ashraf et al., 2006).

⁷Participants were matched across the entire sample, not within each treatment group.

⁸While 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.

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].

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.⁹

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 C.

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 from all 11 policy statements. 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 Table D.1 in Appendix D 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 construct five indexes from the 11 policy statements. They are calculated as simple sums of scores in questions which share a common topic covering five areas: pro-

⁹For clarity and brevity in exposition we sometimes refer to amounts donated in the DG and amounts passed back as Player 2 in the TG as "generosity." This can be interpreted more specifically as unconditional altruism in the DG and trustworthiness in the TG (see e.g. Chaudhuri and Gangadharan (2007)).

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 D.

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:

$$y_{it} = \alpha_i + \gamma \text{Post}_t + \beta \text{Econ}_i * \text{Post}_t + u_{it}, \quad (1)$$

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.¹⁰

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:

$$y_{it} = \alpha_i + \gamma \text{Post}_t + \beta^W \text{Conventional}_i * \text{Post}_t + \beta^{\text{PW}} \text{PostWalrasian}_i * \text{Post}_t + u_{it}, \quad (2)$$

where *Conventional* is a dummy equal to 1 if a student is enrolled in a conventional intermediate microeconomics course; *Post Walrasian* is a dummy for being enrolled in what we called the Post-Walrasian intermediate microeconomics course.¹¹ 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.

¹⁰Ideally, 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 & 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 & 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 & Webb, 2018). The Ferman and Pinto (2019) and Conley and Taber (2011) approaches are not applicable either, in our setting, as they require a large number of non-treated clusters. 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.

¹¹The 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.

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	62	98	63.3
Conventional + SP	13	25	52.0
Conventional II	49	70	70.0
Nutrition and metabolism	46	62	74.2
Total	207	295	70.2

4 | RESULTS

4.1 | Summary statistics and consistency checks

Table 1 summarizes sample and sub-sample sizes and participation rates. Two hundred and seven students responded to both rounds of the survey.¹² 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 70.2%.

Table 2 reports the demographic distribution of participants and the share of economics and business majors across courses. Almost all participants are between 19 and 25 years old. Among participants from the nutrition course, who constitute our control group, there is no economics or business major, subjects are almost evenly distributed between the 19–21 and 22–25 age categories, and the share of women is nearly 91%. Among economics students in our sample, the share of economics or business majors is 94%, a large majority (84%) is in the 19–21 age category, and the share of women is only 26%. 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 F.1–F.5 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 generosity/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 0.12 ($p = 0.015$).

¹²We 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.

TABLE 2 Distribution of respondents by gender, region of origin, major and age.

Course	Female		Major				Age			
	Asia	Europe	Other	US	Economics	Business	16–18	19–21	22–25	>26
Post Walrasian	0.22	0.07	0.00	0.91	0.84	0.08	0.07	0.81	0.12	0.00
Conventional I	0.26	0.15	0.02	0.83	0.77	0.17	0.05	0.88	0.07	0.00
Conventional + SP	0.08	0.19	0.00	0.81	0.69	0.31	0.00	0.50	0.27	0.23
Conventional II	0.35	0.10	0.02	0.88	0.86	0.10	0.05	0.91	0.04	0.00
Nutrition	0.91	0.08	0.04	0.88	0.00	0.00	0.00	0.42	0.53	0.04
Econ versus non Econ										
Non Econ	0.91	0.08	0.04	0.88	0.00	0.00	0.00	0.42	0.53	0.04
Econ	0.26	0.12	0.01	0.86	0.80	0.14	0.05	0.84	0.09	0.02
Total	0.41	0.11	0.02	0.86	0.63	0.11	0.04	0.75	0.19	0.02

Note: For each gender, region of origin, major and age range 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.

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 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 generosity 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 well above half of their endowment in the Dictator Game with charities (59% for economics students and 65% for non-economics students) and passed back more than a third of their initial payoff when acting as Player 2 in the Trust Game (36% for economics students and 39% for non-economics ones). 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 0.29 among economics students and 0.22 in the control group.

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 (5.9% percentage points lower for economics students in the DG, and 2.6% 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 0.08 additional units relative to nutrition students (SE 0.05). Given our study design, these (rather small) pre-treatment differences could reflect not only selection into studying economics, but also possible effects of previous economics courses taken by the students in our sample.¹³

¹³Appendix J reports the distribution of the number of economics courses attended before the start of our experiment, in the treatment and control groups. All the intermediate microeconomics students have attended at least one prior economics course—indeed introductory economics is a pre-requisite for intermediate microeconomics—and most of them have attended two. (Specifically, the pre-requisites for enrolling in the intermediate microeconomics course were an introductory microeconomics course, and one course in mathematics for social sciences.) Around 80% of the nutrition students in the control group have no prior economics education, while the remaining 20% have attended either one or two.

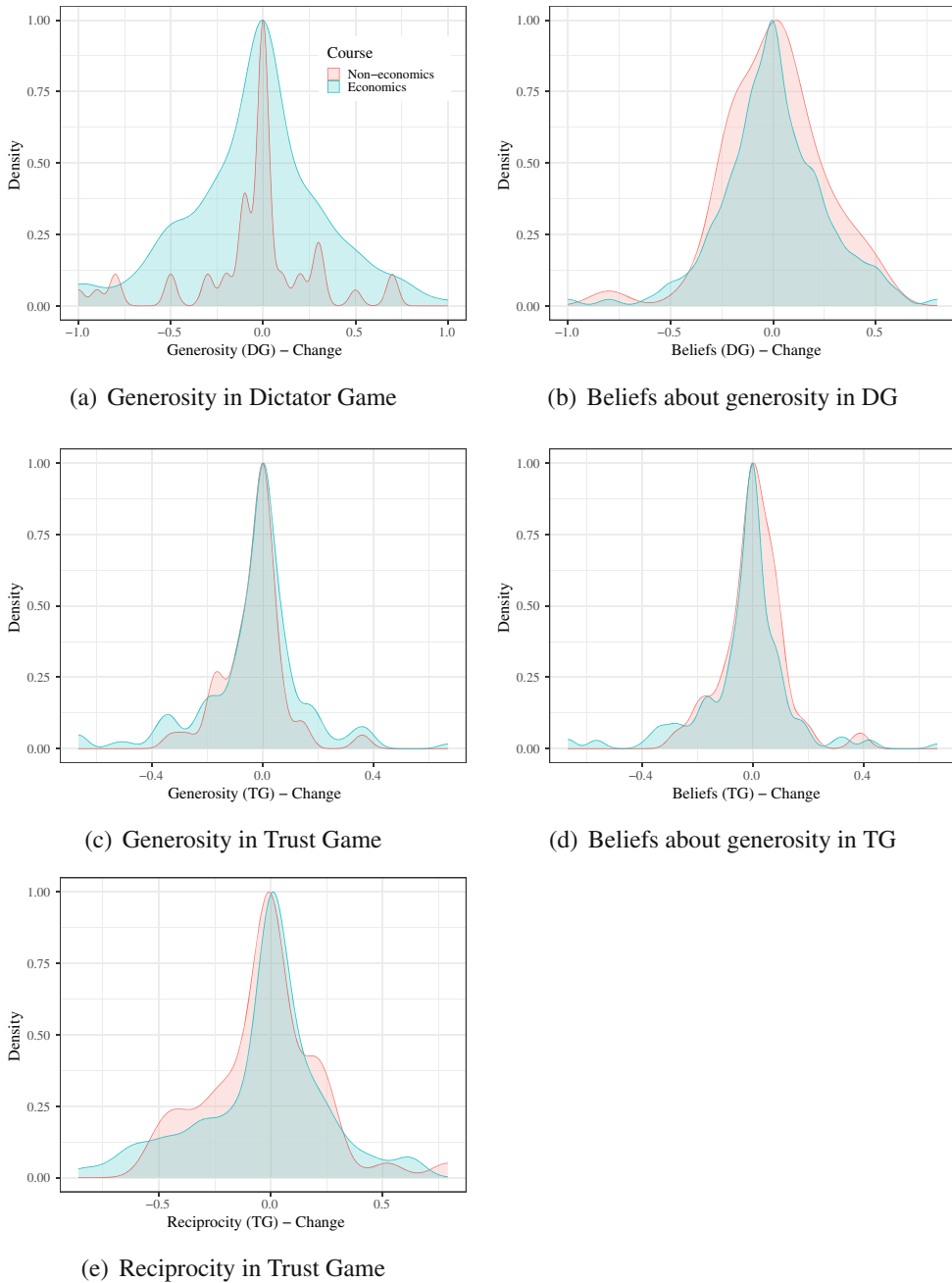


FIGURE 2 Experimental measures of social preferences—distribution of changes during the semester. *Notes:* Smoothed density plots for the distribution of changes during the semester. Distribution of changes for economics students in light blue; distribution of changes for the control group (nutrition students) in red. See section 3.3 and Appendices C and D for the definition of each variable.

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

TABLE 3 Difference-in-differences (DiD) estimates of the effect of intermediate microeconomics on students' social preferences and beliefs.

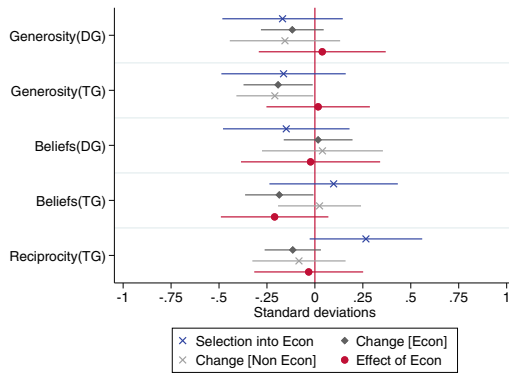
	(1) Generosity in Dictator Game [d _{fsi dg}]	(2) Generosity in Trust Game [d _{fsi tg p2}]	(3) Beliefs about Generosity (DG) [guess d _{fsi dg}]	(4) Beliefs about Generosity (TG) [guess d _{fsi tg p2}]	(5) Reciprocity in Trust Game [recip]
Mean before (Econ)	0.591 (0.027)	0.359 (0.012)	0.467 (0.017)	0.321 (0.012)	0.294 (0.023)
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)	-0.059 (0.058)	-0.026 (0.026)	-0.033 (0.037)	0.014 (0.025)	0.079 (0.05)
Change (Econ)	-0.041 (0.029)	-0.030 (0.015)	0.004 (0.02)	-0.027 (0.013)	-0.034 (0.022)
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)	0.013 (0.059)	0.003 (0.022)	-0.005 (0.041)	-0.031 (0.021)	-0.010 (0.043)
Standardized DiD (Effect of Econ)	0.038 (0.169)	0.017 (0.138)	-0.023 (0.185)	-0.210 (0.143)	-0.032 (0.145)
<i>N</i>	414	414	414	414	414

Note: 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 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 (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.

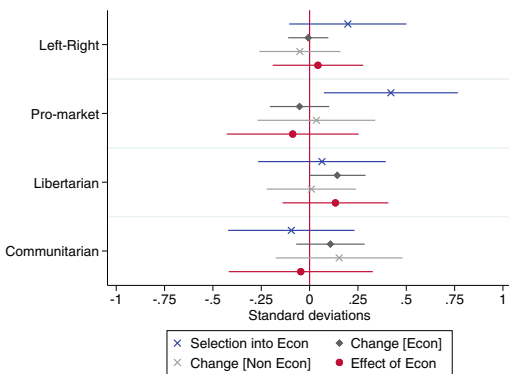
during the semester. They show that both economics and non-economics students tend to display stability of social preferences and of beliefs about others' social preferences. Changes in average levels of generosity and reciprocity and in beliefs during the semester are small in both groups.

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 1 in our sample). The fifth panel reports the same estimated average effect after standardizing the outcome variables, to help interpret effect sizes. Standardized effects are also reported in Figure 3a.

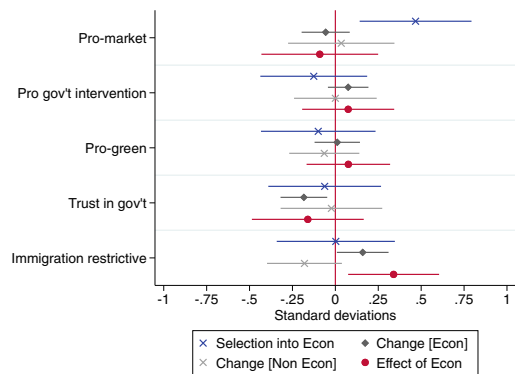
The estimated average treatment effect of intermediate microeconomics on social preferences is close to zero. The estimated effect on generosity in the DG amounts to +1.3 percentage points (with a standard error of 5.9 pp), or 0.04 standard deviations (SE 0.17). The estimated



(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 C and D 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.

effect on generosity in the TG is +0.3 percentage points (SE 2.2 pp), or 0.02 standard deviations (SE 0.14). The estimated effect on reciprocity is -0.03 standard deviations (SE 0.15).

When using Player 2 behavior in the TG to measure generosity, the null effect is also fairly precisely estimated. We can rule out at the 0.05 significance level a decrease in generosity bigger than 4 percentage points or 0.25 standard deviations.

With respect to beliefs, Figure 3a shows that the estimated effect of economics on beliefs about other people's generosity in the DG is indistinguishable from zero. On generosity in the TG, however, the effect of economics on beliefs is -0.21 standard deviations (SE 0.14). Though

imprecisely estimated, the effect suggests that economics students may modestly 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 G.1 we estimate the effect of intermediate microeconomics including only female students, obtaining similar results.¹⁴

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 Table H.1. 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 3b use the four principal components detected by our PCA; Table 5 and Figure 3c 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. Specifically, the tables report both raw and standardized effects, while figures display standardized coefficients. Below, we focus on the standardized measures.

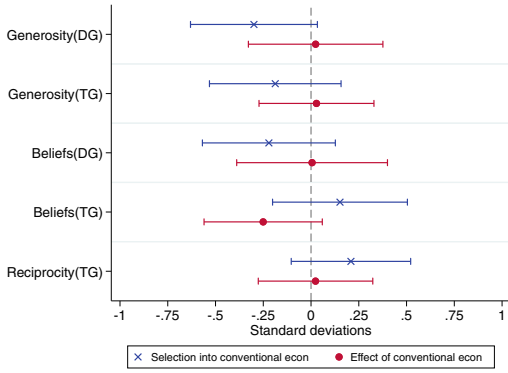
We first consider our measure of selection into economics: the difference in pre-treatment average policy opinions between economics students and the control group. On average, students enrolled in intermediate microeconomics are substantially and significantly more “pro-market.” This is found both in the PCA analysis and in the analysis using simple averages. The ‘pro-market’ component from the PCA is higher by 0.42 standard deviations for economics students (SE 0.18); the average agreement with statements expressing a positive view of markets is higher by 0.47 standard deviations (SE 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 0.106 for the PCA component and 0.030 for the simple average.¹⁵ Economics students also display a higher pre-treatment average for the “Left-right” component, by 0.20 standard deviations. This means that they are, on average, politically to the right of the control group students. This difference is, however, imprecisely estimated (SE 0.15) 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'

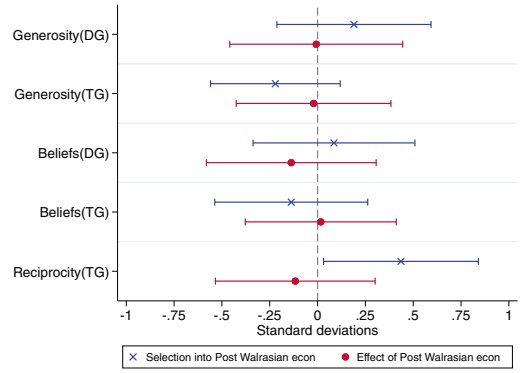
¹⁴The total number of female students in our sample is 84, and they are equally distributed between the control and the treated group (42 in each). We are not able to estimate effects for males only, because there are only four male students in the nutrition course that serves as a control group (Table 2).

¹⁵This result is robust to using alternative methods to adjust for multiple hypothesis testing. Specifically, the adjusted p-values are respectively 0.073 and 0.027 if using the Bonferroni-Holm method, and 0.071 and 0.027 when using the Sidak-Holm method. We use the “wyoung” command in STATA (Jones et al., 2018) in order to perform adjustment for multiple hypothesis testing.

Social preferences and beliefs

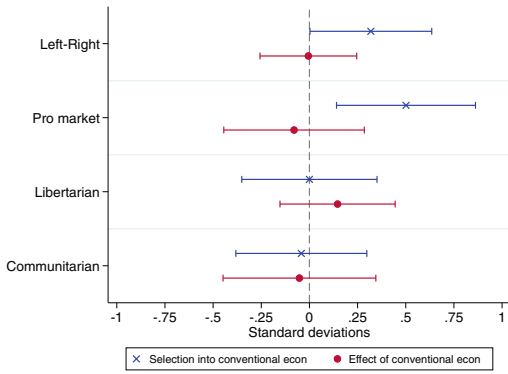


(a) Conventional curriculum

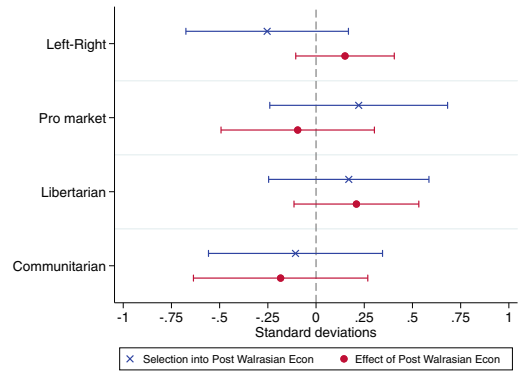


(b) Post Walrasian curriculum

Policy views: principal components

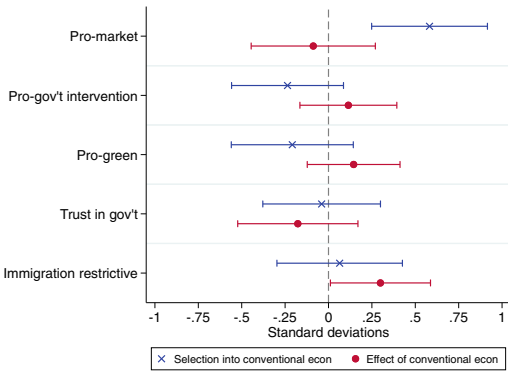


(c) Conventional curriculum

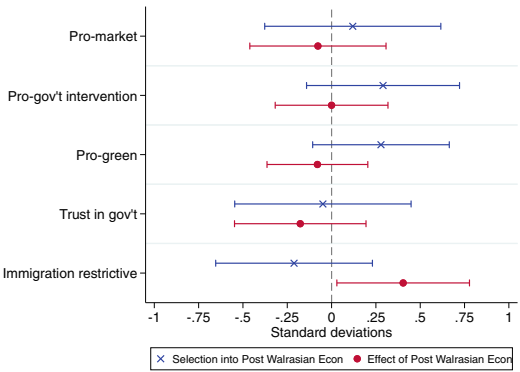


(d) Post Walrasian curriculum

Policy views: simple averages



(e) Conventional curriculum



(f) Post Walrasian curriculum

FIGURE 4 Legend on next page.

TABLE 4 Difference-in-differences estimates of the effect of intermediate microeconomics on students' policy views (principal components).

	(1) <i>Left-Right</i>	(2) <i>Pro market</i>	(3) <i>Libertarian</i>	(4) <i>Communitarian</i>
Mean before (Econ)	0.086 (0.127)	0.148 (0.105)	-0.045 (0.085)	-0.082 (0.077)
Mean before (Non Econ)	-0.240 (0.221)	-0.418 (0.213)	-0.113 (0.158)	0.015 (0.153)
Selection (into Econ)	0.326 (0.255)	0.566 (0.238)	0.068 (0.18)	-0.097 (0.172)
Change (Econ)	-0.012 (0.087)	-0.070 (0.105)	0.153 (0.08)	0.111 (0.093)
Change (Non Econ)	-0.083 (0.177)	0.047 (0.209)	0.010 (0.126)	0.157 (0.172)
DiD (Effect of Econ)	0.071 (0.197)	-0.117 (0.234)	0.142 (0.149)	-0.047 (0.196)
Standardized DiD (Effect of Econ)	0.043 (0.119)	-0.087 (0.174)	0.134 (0.14)	-0.046 (0.19)
<i>N</i>	414	414	414	414

Note: 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 (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.

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 0.34 standard

FIGURE 4 Effect of intermediate microeconomics—Conventional versus post Walrasian curriculum. *Notes:* The Figures displays 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 C and D 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.

TABLE 5 Difference-in-differences (DiD) estimates of the effect of intermediate microeconomics on students' policy preferences (simple averages).

	(1) <i>Pro-Market</i>	(2) <i>Pro-Gov't intervention</i>	(3) <i>Pro-Green</i>	(4) <i>Trust in gov't</i>	(5) <i>Immigration restrictive</i>
Mean before (Econ)	0.151 (0.026)	0.291 (0.029)	0.523 (0.031)	0.264 (0.037)	-0.357 (0.047)
Mean before (Non Econ)	0.003 (0.045)	0.337 (0.05)	0.565 (0.064)	0.293 (0.07)	-0.359 (0.095)
Selection (into Econ)	0.148 (0.053)	-0.046 (0.058)	-0.042 (0.072)	-0.030 (0.079)	0.002 (0.106)
Change (Econ)	-0.018 (0.023)	0.027 (0.022)	0.005 (0.028)	-0.087 (0.033)	0.096 (.046)
Change (Non Econ)	0.011 (0.05)	0.000 (0.045)	-0.027 (0.044)	-0.011 (0.072)	-0.109 (0.067)
DiD (Effect of Econ)	-0.029 (0.055)	0.027 (0.05)	0.032 (0.052)	-0.076 (0.079)	0.205 (0.082)
Standardized DiD (Effect of Econ)	-0.091 (0.174)	0.075 (0.137)	0.076 (0.124)	-0.161 (0.166)	0.339 (0.135)
<i>N</i>	414	414	414	414	414

Note: 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 D 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 (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.

deviations (SE 0.135) 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 0.056.¹⁶

To put the effect we have found on students' opinions on immigration policy in context, note 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 equal to -0.36 for both economics and non-economics students (on a scale that ranges from -1 to 1). The index increases on average by 0.096 (SE 0.046) during the semester for economics students, while in the control group it *decreases* by a similar magnitude. 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.

¹⁶Using the Bonferroni-Holm method produces an adjusted *p*-value of 0.064; the Sidak-Holm method gives an adjusted *p*-value of 0.062.

There is also some sign of a possible modest negative effect of economics on trust in government, but it is rather small and quite imprecisely estimated. Trust in the government of the State of Massachusetts decreases by 0.16 standard deviations among economics students relative to the control group (SE 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 0.78.

While the aggregations we have performed allow us to convey results in a more compact and informative way, in Appendix Figure H.4 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 G.1).

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.

In Appendix I we also test whether the microeconomics courses have any moderating (or polarizing) effect on students' policy opinions, following the methodology of Makowsky and Miller (2014). To this end, we calculate their index of “attitude extremity” in our sample. This is defined as the average squared deviation from the neutral mid-point of the Likert scale, expressed as a percentage of the maximum possible deviation (Makowsky & Miller, 2014, p. 838). We find little differential change in “attitude extremity” among economics students: intermediate microeconomics does not seem to have any substantial moderating (or polarizing) effect in our sample.

5 | CONCLUSION

This article 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.

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.¹⁷ 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 (e.g., 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.¹⁸

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.

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¹⁷See 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.

¹⁸This 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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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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