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Can ethics instruction make economics students more pro-social?

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ABSTRACT

Numerous studies suggest that economics students act in a more self-interested manner, on average, than other students. According to much of the literature on the topic, this is due, at least in part, to the economics training itself with its emphasis on self-interest. The current study turns this question on its head and asks whether teaching ethics in economics classes can produce more pro-social behavior. A classroom experiment examines possible effects of two types of ethics instruction, moral duty and enlightened self-interest, on two economically important types of pro-social behavior, viz., generosity and cooperation. The main findings are that generosity is higher following instruction that stresses moral duty, that cooperation is not significantly affected but is positively correlated with generosity, and that business and economics majors are less cooperative than other majors.

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1. Introduction

Economics students act in a more self-interested manner, on average, than other students, according to most of the now sizeable literature on the topic. In the seminal paper, [Marwell and Ames \(1981\)](#) point to two possible explanations: selection of self-interested persons into economics courses or the effects of the economics training itself with its emphasis on rational self-interest. There is compelling evidence that selection plays a role, e.g., [Frey and Meier \(2003\)](#). But much of this work also indicates at least a partial role for the training itself with its emphasis on self-interest in causing the attitudes or behavior of economics students to be more selfish and/or less fair, e.g., [Faravelli \(2007\)](#), [Haucap and Müller \(2014\)](#), [Hole \(2013\)](#), [Ifcher and Zarghamee \(2018\)](#), and [Frank et al. \(1993\)](#). The current study turns this question on its head, in a sense, and asks whether formal ethics instruction in economics classes can produce more intrinsically motivated pro-social behavior.¹ This paper reports the results of a classroom experiment that, to my knowledge, represents the first study of this question. Specifically, it examines possible effects of two types of ethics instruction on two types of incentivized

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¹ Note that the case for ethics instruction in economics does not rest on the claim that economics students behave worse than others (although that would give the proposal greater urgency). The case can be made solely on the basis of two conditions: (1) economics students engage in some socially undesirable behavior, and (2) ethics training in economics classes provides some measure of remedy. Although the first condition is surely satisfied, evidence of the latter has heretofore been lacking.

behavior, which is also an apparent first. The so-called Moral duty treatment encourages students to act unconditionally on moral rules, whereas the Enlightened self-interest treatment seeks to convince students that moral conduct ultimately promotes their own well-being. Effects are investigated on two of the most economically important behaviors using two of the most widely employed experimental designs: the dictator game measures generosity as the amount of a fixed sum one subject is willing to share with an anonymous counterpart, and the prisoner's dilemma captures cooperation as the willingness of two subjects to shun the dominant strategy in order to secure higher joint earnings.

It is of potentially considerable consequence, if the content of economics courses affects pro-social behavior, either favorably or unfavorably: (1) economists occupy a wide range of influential private and public sector positions, (2) the undergraduate degree is a leading major for post-graduate studies in many important fields including business and law, (3) economics is often required for other majors, including for the most popular undergraduate major, viz., business (Snyder et al., 2018), as well as for the influential MBA degree, and (4) an estimated 40% of US undergraduates take introductory economics during their studies (Siegfried and Walstad, 2014). There has been a rise in support for ethics education across many fields, especially in higher education (e.g., witness the UNESCO Ethics Education Programme), but special concerns have been raised about the ethical training of those who, through their participation in or influence on the economy, have often been seen as complicit in such events as the 2007–08 financial crisis and subsequent Great Recession. In the decade following the 2001 accounting scandals (e.g., at Enron and WorldCom), the percentage of business schools requiring an ethics course for their MBA programs more than doubled from 34% to 79% (Beyond Grey Pinstripes). Within economics, there are also growing efforts to strengthen the emphasis on ethics in economics teaching and research, e.g., see Bruni and Sugden (2013), Sandel (2013), and Schiller and Schiller (2011), including calls to establish ethical guidelines, e.g., Atkinson (2011) and DeMartino (2011), amid reports of moral lapses among professional economists, e.g., List et al. (2001). Indeed, the American Economic Association recently adopted a Code of Professional Conduct (April 20, 2018) that promotes as integrity, honesty, transparency, freedom, respect, fairness, equal opportunity, and responsibility.

To be clear, the current research project is distinct from an important area of economic research on what is sometimes called “moral suasion.” Contrary to its name, in economics, moral suasion does not necessarily have a basis in moral preferences. Rather, it involves simple and usually short statements designed to motivate others to take specific actions, often to prime compliance with specific norms or laws, at times accompanied by an implicit threat. In the case of norms, they might be social, and not specifically moral, norms, such as shaking hands when meeting someone, and even appeals to arguably moral actions might be motivated by norm compliance rather than moral preferences, e.g., an experimenter demand effect in response to the suggestion that the subject contribute his or her full endowment in a public goods game. Results of studies in the laboratory and the field suggest that moral suasion is sometimes, although not always, effective. Moreover, the moral content in these studies, when present, is usually limited to a single sentence or less, and the evidence on the efficacy of the specifically moral content in these studies is mixed. Bursztyn et al. (2015) find a moral message increases repayment of credit card debt, whereas both moral and non-moral suggestions cause significant increases in contributions to public goods in Dal Bo and Dal Bo (2014). Indeed, even absent any explicit moral arguments or language, mere encouragement to comply with norms sometimes affects behavior, e.g., with book returns to libraries (Apestequia et al., 2013) and energy conservation (Ito et al., 2018). The current study differs from these in several ways: (1) the content is always explicitly ethical, (2) the messages are protreptic arguments presented in a coherent ethical framework rather than brief prompts, (3) the instruction is targeted broadly at moral decision-making across many contexts rather than at specific actions, and (4) this study is conducted in economics classes rather than in a laboratory or the field.

Despite the apparent absence of research on the teaching of formal ethics in economics classes, there is an extensive literature on classroom ethics instruction in other disciplines, the results of which are inauspicious. Meta-analyses of the effects of ethics instruction in business (Waples et al., 2009) and the sciences (Antes et al., 2009) find minimal effects or, more commonly, none at all. The picture is even more dismal, if one considers the variety of methodological issues that often plague these studies and prompt skepticism about even modest findings. In many cases, one or more features increase the likelihood of a type I error (that is, a false positive), e.g., the lack of control groups and/or anonymity, selection and/or social desirability biases, and self-reported rather than actual behavior. Konow (2017) employs a survey design typical of these studies while seeking to avoid various suspect elements of prior research. The aim was not only to avoid a type I error but also a type II error: given the modest prior results, design features were chosen to minimize a false negative that might result from an inappropriately chosen or insufficiently calibrated effect. Thus, the effect examined was not behavioral but rather one better suited to self-reports, namely, impartial moral views. Specifically, the focus of this study was on possible effects of five weeks of intensive instruction about distributive justice theories on views about the fairness of allocations or actions in various vignettes. The treatment took place in required rather than elective courses, there was a control, the experimenter differed from the instructor, respondents were anonymous, and elicitation was timed to detect even short-run effects while also examining possible delayed effects. Despite these measures, this study failed to find systematic effects.

Only two previous studies, to my knowledge, consider the effects of classroom ethics instruction on actual (as opposed to self-reported) and incentivized behavior. The studies of Mayhew and Murphy (2009) and Bloodgood et al. (2008) both compare the incidence of cheating among business students, who are required to take ethics training, with that of other business students. The former study examined fourth year accounting students, who participated in an additional, zero credit ethics program, and found subsequent reduction of misreporting of incentivized quiz results, but only among students assigned to a condition in which their payments and actions were disclosed to the experimenter and fellow participants. There was no effect on misreporting among students whose decisions were anonymous (indeed, there was slightly higher misreporting

following the ethics program, although that was not significant). Moreover, the comparison group in this study was not a true control, since they were taken from a different cohort of Master of Accounting students and had not participated in an alternative non-ethics program. Bloodgood et al. examined cheating among students enrolled in undergraduate management classes, distinguishing those who reported having taken a required business ethics course from those who had not yet taken the course. There was no significant main effect of ethics instruction, although cheating was significantly lower among students who reported higher ACT scores and/or lower attendance at religious services. Even this effect on a subsample comes with several qualifications, though: cheating overall was quite limited in this study (about 16% of participants), deliberately misleading instructions were used to identify cheating, and the assignment to treatment and control was not random. Thus, neither study finds any significant main effect of ethics instruction, and the results on intrinsic motivation are mixed with the former study refuting any such role and the latter finding it only among a limited subsample. Moreover, both studies employed methods that differ from the usual standards of experimental economics, and it is unclear, therefore, whether their results would be robust to experimental economic methods.

The present study examines effects of exposure to ethics lectures on the monetarily incentivized, intrinsically motivated behavior of students in economics classes. An experiment is employed because of its ability to restrict and anonymize interactions, thereby suppressing extrinsic, non-moral motives, such as reputation concerns or social desirability biases, that can otherwise insinuate themselves into behavior outside a controlled setting. The focus is on short-term effects for two reasons. First, given the modest findings of prior research, it seems optimistic to expect a brief, one-time stimulus to produce a costly behavioral change that is not only significant but also long-lasting. Indeed, any effect, however brief, would be a striking result in light of previous non-results. Second, efforts outside the classroom to motivate moral action frequently target the short-term, and many of these are economically important. Charitable organizations, for instance, routinely exhort potential donors to immediate action through door-to-door appeals, televised advertisements, telethons, radio funding drives, and mailed solicitations that stress the urgency of their causes (see [Andreoni, 2006](#)). The fact that many efforts at moral motivation are repeated at regular intervals suggests not only that their effects decay but also that repetition is seen as necessary and effective. For millions of people, moral instruction occurs in conjunction with regular religious practice, such as exposure to sermons at weekly services. [Beattie \(2017\)](#) finds that newspaper reporting on global warming favorably affects car usage, but the duration of this pro-social effect is only about one or two weeks. These and other considerations inform the experimental design and procedures, which are addressed in [Section 2](#). [Section 3](#) presents the results and [Section 4](#) a discussion of them.

2. Experimental design, procedures and predictions

With respect to the choice of subject pool, the research question calls for the use of college-aged economics students. A college-aged sample actually offers several other advantages for our purposes. [Almås et al. \(2010\)](#) and [Harbaugh et al. \(2003\)](#), among others, offer compelling evidence of gradual change in the type and strength of moral preferences among children with age. But the results of [Sutter and Kocher \(2007\)](#) with samples ranging from average ages of 8 to 68 suggest that such preferences stabilize by college age. Thus, one can expect this group to have stable moral preferences that mostly reflect those of the larger adult population. Further, the current study adopts the same understanding of “economics students” as most previous studies of the negative effects of economics training: these are students who have been exposed at least to the fundamentals of economics but who include both majors and non-majors. Previous work has found more selfish behavior among economics majors and, for that matter, also business majors (e.g., [Frey and Meier, 2003](#)). But there is no obvious reason to expect any effects on pro-social behavior, whether favorable or unfavorable, to be restricted to these groups. Indeed, [Bauman and Rose \(2011\)](#) find the reverse: although economics majors exhibit a selection effect, the effects of economics training itself are limited to non-majors in their sample. Thus, this study employs students enrolled in an introductory economics course at a comprehensive university, 59% of whom majored in business or economics and the rest in a wide variety of other majors.

The aims and methods of ethics instruction vary widely. On the limited occasions it surfaces in economics classes, however, ethics is normally treated rather dispassionately as an academic subject, e.g., in the context of welfare economics or behavioral economics. Given the goals of this study, though, the ethics lectures should aim to influence or motivate individual behavior, so the lecturers selected here specialized in business ethics. This choice has several advantages. Business is arguably the closest field to economics that has a well-established ethics specialty geared at achieving behavioral changes. Moreover, the current studies and future careers of the students in these classes were more likely to be in business than any other field. Finally, within business ethics, there are very different methods of instruction, and the faculty available at this university offered the opportunity to test different approaches.

The experiment took place in three sections of introductory microeconomics, each of which had a different guest lecturer and constituted, therefore, a separate treatment. To maintain comparability of the three sections, the procedures were identical except for the lecturer, the three sections had the same regular instructor, the experimenter was the same, and all three treatments were conducted on the same day during the twelfth week of classes. In addition, data collection proceeded in two waves: the same three treatments were repeated 1 year later, again during the 12th week of classes with the same lecturers, regular instructor, and experimenter with a change only in the timing and order of treatments on the day of the experiment between the first and second waves. And, although university policy did not allow the experimenter to

Table 1
Sequence of the experiment.

-
1. Instructor introduces guest lecturer
 2. Guest gives lecture and departs
 3. Students complete evaluation of lecturer
 4. Instructor introduces experimenter
 5. Students receive show up fees and sign receipts
 6. Students decide dictator transfer, estimate average transfer, and make prisoner dilemma choice
 7. Students complete post-experimental questionnaire
 8. Payments are made anonymously in following class meeting
-

assign students to treatments, the standard enrollment procedures for these classes provided a quasi-random assignment of students.²

The sequence of phases in the experiment is summarized in Table 1. Each class began with the introduction of the guest professor by the regular instructor. Each lecturer spoke for thirty minutes (plus or minus 2 min) and departed. The instructor then asked the students to complete an evaluation of the guest lecturer. After collecting the evaluations, the regular instructor then introduced the experimenter as a different guest to the class. Students were given \$3 show-up fees in cash and signed receipts for them – since they were already present, this was merely to reassure them that monetary payments were real. The experiment was not computerized in light of the classroom setting. Subjects made three decisions (discussed in detail below), after which they completed a post-experimental questionnaire. They received their payments anonymously in the following class meeting.

The content of the lectures differed across treatments as follows. Treatment A involved what I will call “Enlightened self-interest.” This lecturer began by observing that a common view is that there is tension between business (or economics) and ethics, since the former is based on self-interest and the latter on the interests of others. He stated that the view that the single-minded pursuit of self-interest promotes the interests of others conflicts with ethics, which requires that we seek to promote the common good, and that what is needed is a blend of competition and cooperation. This lecturer’s first main argument for an ethical approach is that people are not self-sufficient but rather are interconnected and benefit from the division of labor, specialization, and public services. He focused on how the promotion of cooperation is necessary for the happiness and material well-being of people. His-second main argument was that the aggressive pursuit of individual interests and the disregard for ethics is ultimately self-defeating. He claimed that engaging in unethical behavior has cognitive and affective consequences that make people stupid and greedy and cited examples of business people who initially profited from pursuing their narrow interests but eventually came to ruin. Thus, the aggressive pursuit of individual interests does not profit the individual in the long run. Treatment B advocated for what I will call “Moral duty.” This professor began by exploring the meaning of ethics and critiquing ethical claims that are relativistic or biased. He discussed the importance of intentions in evaluating the morality of acts. He then outlined the position that there are different levels of moral development, beginning with the avoidance of punishment, noting its weaknesses as a basis for ethics, e.g., the fact that laws can be unjust. He condemned uncritical compliance with the behavior of others and stated that the highest level of moral motivation is following one’s values no matter what the outcome. He concluded by outlining an approach to moral reasoning, which requires maintaining the focus on the interests of others and on moral principles. The third condition, C, was the control: a statistics professor discussed applications of statistics to microeconomics.

The two ethics treatments instantiate a common division between schools of thought in philosophical ethics, specifically, the distinction between the Good and the Right. Theories of the Good argue that judgments about the morality of conduct be based on the outcome or consequences of conduct. For example, utilitarianism is the moral philosophy that equates the Good with happiness and judges conduct based on its promotion of that end. Enlightened self-interest is an alternative approach that embodies elements of ethical hedonism, the moral philosophy that holds the Good to be one’s own well-being and that bases morality on the need for cooperation for the achievement of that outcome. Theories of the Right reject outcomes as the basis for ethics and focus instead on compliance with moral rules or duties. Moral duty has much in common with Kantian ethics, the preeminent theory of the Right: both stress intentions, the primacy of moral rules and duties, and the use of reason.

An additional design question concerns what kind of behavioral effect to measure. One consideration is that behavior is not simply more or less moral but rather that morality varies on multiple, possibly independent, dimensions, such as generosity, fairness, and trust. Moreover, different types of ethics instruction might affect these behaviors in different ways. A novelty of this study, as far as I am aware, is the ability to examine how different types of ethics instruction might affect different types of moral behavior. Specifically, the experiment focuses on two of the most economically important types of moral behavior, generosity (or fairness) and cooperation, which are measured using two of the most widely tested experimental designs, viz., a dictator game and a prisoner’s dilemma, respectively. More precisely, every subject made three decisions, each of which was completed before introducing and moving on to the next. First, all allocated as dictators in a

² Specifically, students were, in large part, randomly assigned registration dates while caps on sections were gradually raised, which effectively forced many students, more or less randomly, into one section or the other.

dictator game: each subject was endowed with \$10 and could transfer any integer amount between \$0 and \$10 to an anonymously matched counterpart in a different class. Next, every subject estimated how much, on average, all subjects in their room had transferred to their counterparts in the first decision. This was incentivized by deducting one dollar from their total earnings for each dollar of error in their integer-valued estimates from the (rounded) average.³ This estimate could shed light on possible motives for the dictator decision, e.g., a desire to comply with the perceived norm. Finally, all subjects participated in a prisoner's dilemma with an anonymous student in a different class (who was a different counterpart from the one in the first decision): mutual cooperation earned each \$8, mutual defection earned each \$4, and cooperating when the other defected earned the co-operator \$0 and the defector \$10. The dictator experiment preceded the prisoner's dilemma, since the former involves unconditional consideration of others whereas the latter decision potentially triggers strategic considerations. Of course, an order effect cannot be ruled out, but this choice reflected the aim to minimize the chance of such an effect given the greater concern that strategic thinking in the prisoner's dilemma might spill over to unconditional regard in the dictator game than the converse. Thus, this order was maintained across the two waves for this reason and also in order to avoid confounding inferences about the source of differences, if any, given a change in the protocol between the two waves (see Section 4 the discussion at the end of this section below).

What effects, if any, might we predict the ethics lectures to have on behavior in the two main decisions? Enlightened self-interest argues from the stance of individual interests and the view that the promotion of those interests often requires cooperation with others. If some students are persuaded by this argument but view it as applying only when cooperation yields immediate benefits, this group is expected to be no more generous in the dictator game and no more cooperative in the prisoner's dilemma, given the dominant strategies in these simple, anonymous, one-shot decisions. If, however, they embrace individual interests but see pro-social behavior as a rule to follow generally in cooperative situations, they would still be no more generous in the dictator game but would cooperate more in the prisoner's dilemma. Moral duty emphasizes unconditional compliance with moral rules and rejects punishment as a basis for ethics. If effective, therefore, it seems reasonable to expect an increase in dictator giving, based on an unconditional desire to follow rules, in this case of fairness or generosity, and in the absence of any punishment opportunities, which are ruled out by design. This treatment might also encourage cooperation, if students regard cooperation as a moral rule. Nevertheless, some arguments tilt the other way. In the dictator game, the interests of self and other are diametrically opposed and sanctions are missing, making a clear case for unconditional moral behavior based on pro-social preferences. Cooperation, on the other hand, can also be understood as a behavior rather than a moral preference. It is potentially affected by moral preferences, such as altruism or fairness, but these can interact with self-interest and expectations in ways that muddy the waters. For example, even a player in a prisoner's dilemma whose sole concern is fairness would choose to defect, if the other player is expected to defect. Moreover, the strategic nature of this decision might contribute to framing it in terms of strategic self-interest, rather than morality, perhaps reinforced by the perception of defection as a form of punishment, which was described in amoral terms in the Moral duty treatment and which, therefore, might crowd out cooperation. These considerations, therefore, make the effect of Moral duty on cooperation ambiguous and render it an empirical question. To summarize, the predictions are for possibly increased cooperation in the prisoner's dilemma in the Enlightened self-interest treatment and increased dictator generosity in the Moral duty treatment, whereas the effects are ambiguous in the other cases. The following section employs two-tailed tests, therefore, both to simplify the presentation of the results across treatments and to acknowledge the tentative nature of these conjectures given the lack of solid prior evidence of such effects on incentivized intrinsically motivated behavior, let alone of these specific types of instruction on these particular behaviors. Nevertheless, the significance or insignificance of the effects at conventional levels is not sensitive to the choice of one- or two-tailed tests, as we will see.

The experimental procedures were chosen with the aim of isolating any treatment effects on the willingness to sacrifice material interests in order to act on intrinsic moral preferences (the complete procedures can be found in the appendix). Thus, the goal was to avoid extraneous forces. One such potentially confounding factor is strategic self-interest, in this context, the attempt to advance one's material interests by feigning moral preferences. The chosen experimental designs provided no basis for this. The dictator game and prisoner's dilemma both have dominant strategies. The fact that they were one-shot with rematching between decisions offered no opportunity for reputation effects. And no forms of communication were possible given evidence from numerous experimental studies that the ability to signal one's type can affect cooperation, even if only through cheap talk.⁴

Another possible confound is social desirability biases, i.e., the extrinsic motive to present oneself favorably to others. In this regard, pains were taken to ensure double-blind anonymity, i.e., neither the experimenter nor the other subjects were able to associate decisions or questionnaire responses with specific students. Subjects collected their materials, which were identified only by subject IDs, and deposited them confidentially and one at a time. A randomly chosen student distributed payments in the following class meeting outside the presence of the experimenter, and blank slips ensured equal thickness of payment envelopes.

³ Subjects were asked to estimate the mean rather than a more narrow measure like the mode, given the sometimes wide fluctuations of the latter in dictator games. This was incentivized with a linear loss function given a desire for easy comprehension and the goal of focusing limited time and subject attention on the main decisions 1 and 3.

⁴ For example, Davidson and Stevens (2013) find that trust and trustworthiness increase in the investment game, if the trustee is known to have accepted a brief code of ethics.

Mean Gifts in the Dictator Game

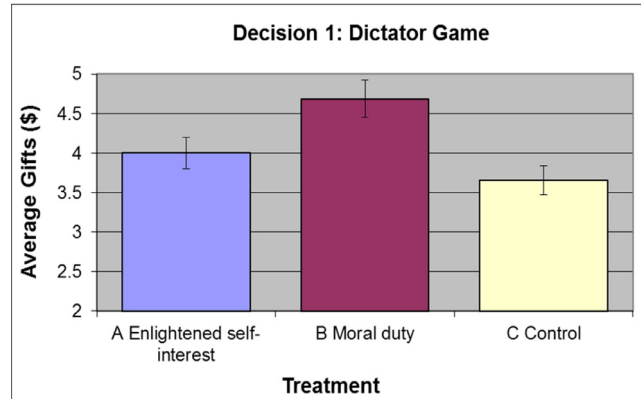


Fig. 1. Mean gifts in the dictator game. Notes: Whiskers represent 95% confidence intervals.

Finally, experimenter demand effects emerge, if subjects change their behavior in response to perceived cues about the experiment. Numerous measures were taken to address this issue. The lectures took place in a classroom rather than laboratory setting and as part of a regular class meeting rather than by invitation to an experiment. Students were informed in the prior class meeting that there would be guests on that date but were not told any further details. This was to prepare them for multiple unrelated visitors on that day and to normalize the visits. The experimenter was a different person from both the regular instructor and the lecturer, and the instructor introduced the experimenter without explicitly making (or disavowing) any connection to the previous lecture. The design was “between subjects,” i.e., the only procedural difference between treatments was the identity of the guest lecturer. This method was selected over various “within subjects” designs, which offer no obvious way to distance the lectures from the experiments: they involve either experiments both before and after each lecture or a sequence of all three lectures each followed by a round of the experiments (whereby the latter version has the additional disadvantage of risking order effects).

The behavioral and self-reported evidence indicates the above measures were effective. Qualitative and quantitative differences in decisions across treatments, including negative results, reported below suggest demand effects, if any, were not pervasive. Subject explanations of the reasons for their decisions in the post-experimental questionnaire are especially compelling. They were asked: “Why did you choose the amount that you did for this decision?” about the dictator game and “Why did you choose the action that you did for this decision?” about the prisoner’s dilemma. Subjects were very candid about any suspicions they harbored about the experiment. Indeed, in the first wave, subjects were informed there would be multiple decisions at the start, and some comments in the post-experimental questionnaire suggest some subjects suspected generosity in the dictator decision might benefit them in a subsequent decision. Although any such inference would be erroneous, the protocol for the second wave was adjusted such that subjects were informed of further decisions only after they had completed the dictator decision.⁵ But, to the matter at hand, all of the 167 participants responded to both of the questions about their choices in the dictator game and the prisoner’s dilemma, and not a single comment hints at any inference of a connection between the lectures and the experiment, indeed, no mention was made whatsoever of the lectures.

3. Results

I begin with graphical representations of the results in Figs. 1 and 2. Fig. 1 suggests that the Moral duty treatment has the largest positive effect on dictator generosity, whereas Fig. 2 indicates higher cooperation in the Enlightened self-interest treatment. As suggested by the 95% confidence intervals and reported in the more detailed analysis below, however, only the former effect is significant. Table 2 summarizes the mean values by decision and treatment. In the dictator decision, the mean transfer across all conditions was \$4.11 (the median was \$4).

The distribution of transfers by treatment is presented in Fig. 3, which illustrates the greater frequency of transfers of one-half or more in the Moral duty treatment than in the other two conditions. Over all treatments, 36% of dictators split earnings equally, but a sizeable majority did not, indeed, 14% of dictators transferred more than one-half. Given this pattern of transfers, I will refer to giving in this first decision in general terms as “generosity” and avoid a more specific attribution of motives (e.g., fairness). The level of giving in this study is high relative to most other dictator experiments and is likely explained by the subject pool here: Eckel and Grossman (2000) report that “captive” classroom subjects are significantly

⁵ The analysis of the results reported below suggests that the change in protocol had a marginally significant effect on dictator transfers in the expected direction but that the main results were not qualitatively impacted.

Cooperation Rates in the Prisoner's Dilemma

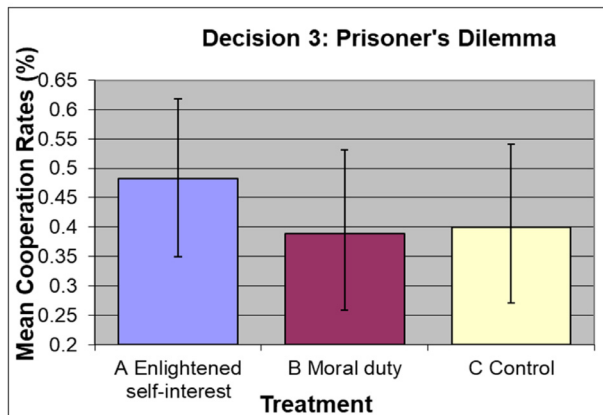


Fig. 2. Cooperation rates in the prisoner's dilemma. Notes: Whiskers represent 95% confidence intervals.

Table 2
Experimental decisions by treatment.

	(1) Dictator game (dollars given) mean (std dev)	(2) Estimated gift (dollars) mean (std dev)	(3) Prisoner's dilemma (cooperation rate) % N
A Enlightened self-interest	4.00 (2.24)	3.31 (1.74)	48.3 58
B. Moral duty	4.69 (2.59)	3.63 (2.03)	38.9 54
C. Control	3.65 (2.37)	3.47 (1.74)	40.0 55

Distribution of Dictator Game Gifts by Treatment

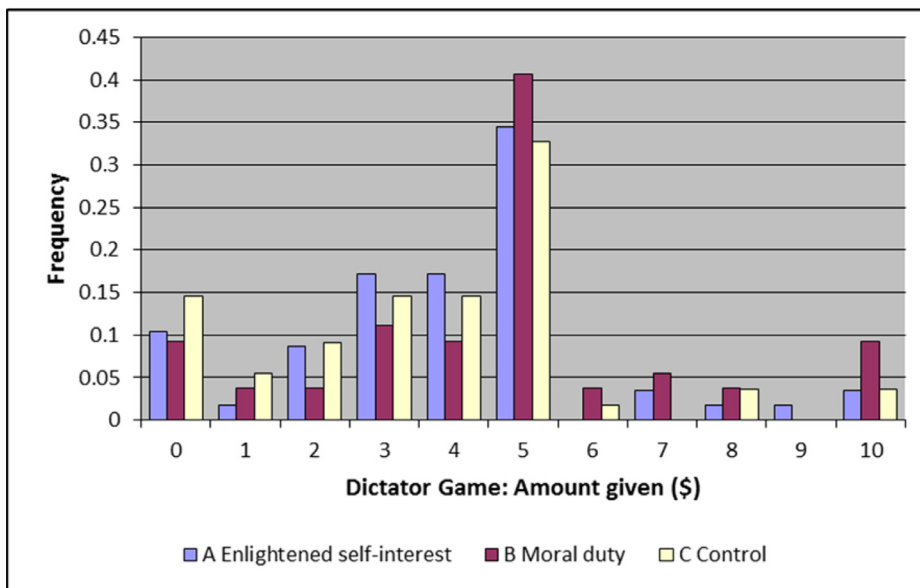


Fig. 3. Distribution of dictator game gifts by treatment.

more generous in dictator games than “volunteer” subjects recruited in the usual manner. The frequency of zero gifts is also quite low in the current study, which also likely explains the absence of significant differences on the extensive margin: the proportion of dictators choosing to transfer non-zero amounts is consistent with the overall pattern of giving across treatments but this fraction is not significantly greater in the Enlightened self-interest ($z=0.677$, $p=0.498$) or Moral duty treatments ($z=0.851$, $p=0.394$) than in the Control, according to two-tailed z -tests. On the intensive margin, mean dictator gifts are higher in the ethics treatments than in the control. Two-tailed t -tests of the corresponding propositions regard-

Table 3
Regression analysis.

	Dictator game (dollars given)	Estimated gift (dollars)		Prisoner's dilemma (cooperation rate)	
	(1)	(2)	(3)	(4)	(5)
A Enlightened self-interest	0.32 (0.48)	−0.40 (0.36)	−0.52* (0.31)	0.02 (0.10)	0.01 (0.10)
B Moral duty	1.02** (0.48)	−0.04 (0.36)	−0.44 (0.32)	−0.06 (0.10)	−0.10 (0.10)
Business/Economics	−0.06 (0.41)	−0.63** (0.31)	−0.60** (0.27)	−0.16* (0.08)	−0.16* (0.08)
Protocol	−0.62* (0.37)	−0.15 (0.28)	0.09 (0.25)	−0.02 (0.08)	0.00 (0.08)
Decision 1			0.39** (0.05)		0.03** (0.02)
Intercept	4.02** (0.53)	4.09** (0.40)	2.53** (0.40)	0.53** (0.11)	0.42** (0.13)
R-squared	0.05	0.03	0.28	0.03	0.05

Notes: Estimated coefficients (standard errors).

* $p < 0.10$,

** $p < 0.05$, $N = 167$.

ing differences in means reveal that the \$1.04 difference in the Moral duty treatment is statistically significant ($t = 2.168$, $p = 0.032$) whereas the \$0.35 difference is not significant at conventional levels in the case of the Enlightened self-interest treatment ($t = 0.796$, $p = 0.428$). Both of these results are consistent with the predictions presented in the prior section and are unaffected by the decision discussed there to report two-tailed tests, although note that the prediction for Moral duty justifies a one-tailed test and produces a $p = 0.016$.

Jumping to the other main decision, the prisoner's dilemma (decision 2 is discussed in the more detailed analysis below), the average cooperation rate across all conditions is 42.5%. The cooperation rate is highest in the Enlightened self-interest treatment, followed by the Control and the Moral duty treatment. As discussed in Section 2, arguments cut both ways regarding the effect of the ethics treatments on cooperation. The 1.1% point difference between cooperation in the Moral duty treatment and the Control is small and insignificant according to a two-tailed test ($z = -0.119$, $p = 0.907$). This result favors the arguments that cooperation is viewed as a behavior rather than a moral rule and that even moral motivations have ambiguous implications for cooperation. In the Enlightened self-interest treatment, the rate of cooperation is 8.3% higher than in the Control, but this difference is not significant according to a two-tailed test ($z = 0.885$, $p = 0.379$) or even a one-tailed test ($p = 0.188$). Enlightened self-interest was predicted to increase cooperation, if its arguments are construed as applying broadly, but not, if its effects are limited to cases with short-run benefits, or if it tends to frame cooperation in terms of strategic self-interest. This result seems consistent with evidence from many prior experiments casting doubt on cooperation being affected by the priming of internalized preferences (e.g., Ellingsen et al., 2012) and indicating targeted prompts only have a significant, reliable positive effect on cooperation, if they are somehow reinforced, e.g., by revealing subject identities (e.g., Rege and Telle, 2004), communication (Brosig et al., 2003) or punishment (Dal Bo and Dal Bo, 2014). Such features were deliberately excluded in this study, however, given the focus on intrinsic moral preferences.

Multi-variate analysis allows further tests of these conclusions and of other questions of interest. Table 3 presents the results of OLS regressions of the three decisions on dummy variables for the two ethics treatments (the Control is the omitted category), for students with a major in business or economics, and for the second wave to address the aforementioned change in protocol. When the other subject data from the questionnaire are included, none proves significant at conventional levels, so the analysis here centers on the independent variables of a priori interest. For reasons explained below, the decision 1 is added as a regressor in separate regressions for the estimated gift and the prisoner's dilemma in columns 3 and 5, respectively.

The main effects for decision 1 on dictator giving appear in column 1 and are quite robust to the inclusion of controls, both in terms of magnitude and significance. The Moral duty treatment is associated with a significant increase in dictator giving of slightly more than one dollar, whereas the 30-some cent increase in the Enlightened self-interest treatment is insignificant. Business and economics students are less generous than other majors, but the effect is small and insignificant. The revised protocol, which the reader will recall delays mentioning subsequent decisions until after the dictator decision, does produce a reduction in dictator giving, consistent with expectations, although this effect is only marginally significant ($p = 0.100$).

Decision 2 is the dictator's estimate of the average gift of other dictators in the same session. The reader will recall that decision 2 was included to illuminate a possible relationship between perceived norms and behavior, so regressions are reported without and with decision 1 in columns 2 and 3, respectively. The negative signs on the coefficients for the ethics treatments seem opposed to expectations: perhaps the ethics treatments trigger a stronger desire for subjects to view themselves favorably relative to others and, therefore, to reduce their reported beliefs about the generosity of others. In any case, though, these effects are not significant at conventional levels (only treatment A in regression 3 is barely marginally

Table 4
Ratings of lecturers.

	Speaking skills	Personal likability	Enthusiasm	Knowledge	N
Mean scores					
A. Enlightened self-interest	4.28	4.45	4.10	4.77	58
B. Moral duty	4.69	4.58	4.60	4.89	55
C. Control	4.20	4.53	4.64	4.73	55
Two-tailed <i>t</i> -tests of differences in means (<i>p</i> -values)					
A vs. C	0.542	0.527	0.000	0.650	
B vs. C	0.000	0.678	0.718	0.097	

Notes: Responses on five point Likert scale where 5 = Very high and 1 = Very low. Note also that there is one more observation in these ratings than the behavioral data: this is due to one subject in treatment B, who later reported to the experimenter that he answered the lecturer survey but chose not to participate in the subsequent experiment, because he did not wish to provide his social security number on the receipt. Otherwise, all students in all treatments participated in the experiment.

significant with a $p=0.100$). Business and economics students are significantly less optimistic about the generosity of their fellow students ($p=0.046$ and $p=0.026$, respectively). The coefficient of 0.39 on decision 1 indicates gifts and estimated gifts are strongly correlated ($p < 0.001$). This is consistent with the false consensus effect Iriberry and Rey-Biel report in their dictator games (2013).⁶ An alternative explanation for the pattern might be norm-compliance, i.e., a desire to match one's behavior to what others are expected to do. But then one would expect estimated transfers of the group to differ across treatments as do the actual transfers, which is not the case, according to two-tailed *t*-tests of differences in means in the three pairwise comparisons of estimated transfers across the three conditions, i.e., A vs. B, A vs. C, and B vs. C ($0.38 < p < 0.67$).⁷ Returning to Table 2, we also find that subjects give more, on average, than their estimates of others' giving, although this varies in size and significance: decision 1 exceeds decision 2 by \$0.69 in treatment A ($p=0.009$), by \$1.06 in B ($p=0.002$), and by \$0.18 in C ($p=0.545$) (two-tailed *t*-tests). Thus, dictators only overestimate their generosity relative to others in the ethics treatments. It appears that exposure to ethics instruction can affect the behavior of dictators but not their expectations regarding the behavior of others.

Finally, the conclusions from the multi-variate analysis of the prisoner's dilemma in Table 3 regarding treatment effects are qualitatively the same as those from the earlier tests of differences in proportions, except that the effect of the Enlightened self-interest treatment falls in both size and significance. Consistent with much prior work, business and economics students appear to be less cooperative in regressions 4 and 5 ($p=0.055$ and $p=0.056$, respectively). The change in protocol has no effect on cooperation – in any case, an effect was expected at most for the dictator decision. This study lends itself to examination of a possible relationship between cooperative motives and generosity, which is why decision 1 is included in regression 5. In fact, this result indicates a significant and positive relationship between the two, indeed, the effect size is relatively large: cooperation is an estimated 16% points higher for the median “giver” (i.e., the median dictator among those who transferred anything, who gave \$5) than for “nongivers.”⁸ Thus, in this study, more generous dictators are also likely to act more pro-socially in the simultaneous prisoner's dilemma by cooperating.

It is possible that any variation in the experiment is not related to the content of the lectures themselves, but rather to some personal qualities of the lecturers. To explore this, the evaluation form completed by students right after the lectures elicited responses on a five point Likert scale (with 5 = Very high and 1 = Very low) to the following four questions: How do you rate the lecturer's overall speaking skills?, ... lecturer in terms of personal likability? ... lecturer in terms of enthusiasm? ... lecturer's knowledge of the subject matter? The results are summarized in Table 4. First, we note that all three lecturers received high average ratings with all scores exceeding 4. Comparing the Control to A and B, respectively, on the four questions, only two of the eight comparisons reveal differences at conventional levels of significance, one in the direction favoring the ethics treatment and the other favoring the Control. Thus, there is no systematic evidence of a general pattern.⁹

4. Discussion

Recent calls for increased emphasis on ethics in economics teaching and research follow experimental and field evidence of moral lapses by current and former economics students. Given the results of prior studies and the types of effects often

⁶ Note, as Engelmann and Strobel (2012) demonstrate, a consensus effect need not be false; i.e., it might be rational to use information about oneself; this is only false, if one knows about others but still assigns too great a weight to oneself.

⁷ I thank Colin Camerer and Alexander Koch for comments that contributed to this point.

⁸ Of course, this means that a regression with dictator gifts as the regressand also finds a significant correlation with cooperation in the prisoner's dilemma. A separate regression confirms that fact and that the results are qualitatively unchanged and even quantitatively similar to those in regression (1), so it is not reported here.

⁹ It would be interesting to include these ratings in regressions, but it was unclear how to design the experiment so as to relate lecturer ratings to experimental decisions at the individual level without violating anonymity or raising suspicions about the connection between the lectures and the subsequent experiments.

targeted by philanthropies, religious institutions and others, this study has focused on short run effects of ethics instruction. Specifically, it has examined the effects among students in economics classes of two types of ethics instruction on two of the most economically significant types of moral behavior, generosity and cooperation, using well established experimental designs. The results indicate that intrinsically motivated generosity is significantly greater following Moral duty instruction, the apparent first finding in the literature of a main effect of ethics education on intrinsic, incentivized moral motivation. The other main positive findings are that cooperation is positively correlated with generosity, and economics and business students are less cooperative than other majors. As predicted, Enlightened self-interest is not associated with any significant increase in dictator giving. The predictions of the two ethics treatments on intrinsically motivated cooperation were ambiguous, and the results, in fact, revealed no significant effects. This is consistent with much previous evidence on cooperation suggesting that effects on cooperation might only be achieved with the kind of extrinsic incentives that were ruled out by design in the current study.

What, if any, are the implications of a positive finding regarding ethics for the practice of economics instruction? The moral behavior of economics students is of considerable social and economic importance, an observation that was the basis for the previously stated motivation for exploring ethics training. The result here on Moral duty appears to be the first of its kind and can be seen as a promising indication of potential for boosting the regard of economics students for the welfare of others. Such pro-social concerns can provide a means to help, for example, increase philanthropic contributions and solve principal-agent problems, e.g., between owners and managers or managers and workers.

Despite the potential significance of ethics training in economics, it would be premature, of course, to jump from these results to specific policies, such as modifications of economics curricula. The reasons go beyond the obvious need to determine the robustness of results through replication. Even a robust set of results represent a necessary, but not sufficient, condition for policy. Any proposal needs to pass the opportunity cost test and demonstrate that the benefits of ethics instruction outweigh those of the content it would presumably displace. An important consideration is whether ethics training, in fact, must be repeated at regular intervals and, if so, whether repetition produces cumulative and long-lasting behavioral benefits, as Konow and Earley (2008) believe is the case with virtuous behavior. In addition, research into other types of ethics training and other kinds of behavioral effects is needed. For example, ethics instruction encompasses not only formal training, such as the classroom lectures examined here, but also experiential training, such as volunteering. Along these lines, Xiao and Houser (2018) report some promising results: college students who are properly incentivized to volunteer are subsequently more likely to express an interest in future volunteering. Finally, given the richness of moral preferences and in the interests of brevity, this paper focused on generosity and cooperation, but future work could examine other important types of pro-social behavior, such as honesty (e.g., Gneezy, 2005) and reciprocal altruism (e.g., Fehr and Gächter, 1998).

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Declaration of Competing Interest

None.

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Supplementary materials

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