Can Ethics Instruction Make Economics Students More Pro-social?

James Konow
Chair of Economics and Ethics
Department of Economics
Kiel University, Germany
and
Department of Economics
Loyola Marymount University, USA

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 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.

Keywords: ethics, generosity, cooperation, fairness, classroom experiment
JEL classification: D63, C91, A2

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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 (2016), 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.

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 percent 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.,

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 might 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.
(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 concerns possible effects on students in economics classes of formal ethics instruction, by which I mean exposure to a coherent set of protreptic moral arguments. This is distinct from economics research on the priming of norm compliance, or what is sometimes called “moral suasion.” This important area of research involves simple and short appeals for compliance with specific norms or laws, which some studies in the laboratory and the field have found to be effective. Nevertheless, the moral content 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 can yield improvements, e.g., with book returns to libraries (Apesteguia et al., 2013) and energy conservation (Ito et al., 2018). Any effects in these studies, though, involve appeals to specific actions, leaving unanswered the effects of general moral instruction. The current method differs from these by being 1) always explicitly ethical, 2) coherent frameworks rather than brief prompts, 3) targeted broadly at moral decision-making rather than at specific actions, and 4) conducted in economics classes.

Despite the apparent absence of research on the teaching of formal ethics in economics classes, there is an extensive literature on such classroom instruction in other disciplines, the results of which are inauspicious. Meta-analyses of the effects of ethics instruction in business (Waples et al., 2008) 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) reports the results of a study that seeks to avoid various suspect elements of prior survey studies. Despite selecting an outcome and calibrating its measurement so as to detect even a small, short-term effect, this study also failed to find systematic effects.\(^2\) Finally, only two previous studies, to my

\(^2\) Specifically, the focus of this study was on possible effects of five weeks of intensive instruction about distributive justice on reported views of fairness. Although the views were elicited immediately following the instruction and represent a presumably more malleable effect than, say, costly behavioral changes, the differences between the treatment and a control group were generally small and insignificant.
knowledge, consider the effects of ethics instruction on actual (as opposed to self-reported) and incentivized behavior. The studies of Bloodgood, Turnley and Mudrack (2008) and Mayhew and Murphy (2009) both compare the incidence of cheating among business students, who are required to take ethics training, with that of other business students. Neither finds evidence of any main effects on intrinsically motivated behavior, and the only significant findings require parsing samples or relinquishing subject anonymity.

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 non-moral motives, such as reputation concerns or social desirability biases, that can otherwise insinuate themselves into behavior. 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. 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 and procedures

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 mirrors many prior ones on the negative effects of economics training by taking
“economics students” to mean those 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 in the twelfth week of 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, and all three treatments were conducted on the same day. In addition, data collection proceeded in two waves, one year apart, and the order of the ethics treatments was varied across the two waves. And, although university policy did not allow the experimenter to assign students to treatments, the standard enrollment procedures for these classes provided a quasi-random assignment of students.3

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 minutes) 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

3 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.
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 stated that a common view about business and economics is that there is tension between self-interest and the interests of others, but he argued that people are not self-sufficient and, therefore, benefit from cooperation. 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 promoting their narrow interests but eventually came to ruin. Thus, the narrow pursuit of self-interest does not profit the individual in the long run. Treatment B advocated for what I will call “Moral duty.” This professor began by critiquing ethical claims that are relativistic or biased by self-interest. He argued that there are different levels of moral motivation and that the highest level is following one’s values no matter the outcome. Moral reasoning, he concluded, requires maintaining the focus on the interests of others and on moral principles. These two ethics treatments instantiate a common division between schools of thought in philosophical ethics.4 The third condition, C, was the control: a statistics professor discussed applications of statistics to microeconomics.

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

\[4\] Specifically, philosophical ethics are often categorized based on the distinction between the Good versus the Right. Theories of the Good argue that conduct should be judged based on its consequences, and Enlightened self-interest is an example of this by underscoring the importance of cooperation for the consequence of one’s own well-being. Theories of the Right focus on compliance with rules or duties, and Moral duty is a version that stresses the primacy of duties to others.

<table>
<thead>
<tr>
<th>Table 1. Sequence of the Experiment</th>
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<tbody>
<tr>
<td>1. Instructor introduces guest lecturer</td>
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<tr>
<td>2. Guest gives lecture and departs</td>
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<tr>
<td>3. Students complete evaluation of lecturer</td>
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<tr>
<td>4. Instructor introduces experimenter</td>
</tr>
<tr>
<td>5. Students receive show up fees and sign receipts</td>
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<tr>
<td>6. Students decide dictator transfer, estimate average transfer, and make prisoner dilemma choice</td>
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<tr>
<td>7. Students complete post-experimental questionnaire</td>
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<tr>
<td>8. Payments are made anonymously in following class meeting</td>
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</table>
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 dictator game: each subject was endowed with $10 and could transfer any integer amount between $0 and $10 to an anonymously matched counterpart in 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. Finally, all subjects participated in a prisoner’s dilemma with an anonymous student in a different class from their own and from that used for 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 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 materials 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.\(^5\)

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.

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

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\(^5\) 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.
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. 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. 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 Figures 1 and 2. These suggest the possibility that different types of ethics instruction trigger different behavioral effects: appealing to moral duty appears to affect more strongly generosity whereas highlighting enlightened self-interest seems better at promoting cooperation. On reflection, such patterns seem plausible: moral duty encourages unconditional and unilateral moral action of which dictator generosity is an instance, whereas enlightened self-interest stresses mutual dependence and cooperation.

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6 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.
which are the core of the prisoner’s dilemma. As we will see in the more detailed summary and analysis below, however, only the former effect is significant at conventional levels.

*Figure 1*

**Mean Gifts in the Dictator Game**

![Bar chart showing mean gifts in different treatments](image1)

Notes: Whiskers represent 95% confidence intervals.

*Figure 2*

**Cooperation Rates in the Prisoner’s Dilemma**

![Bar chart showing cooperation rates in different treatments](image2)

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). Although 36% of dictators split earnings equally, 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). Dictator gifts are higher in the ethics treatments than in the control. One-tail t-tests of the corresponding
propositions regarding differences in means reveal that the $1.04 difference in the Moral duty treatment is statistically significant (t=2.168, p=0.016) but that the $0.69 difference is not significant at conventional levels in the case of the Enlightened self-interest treatment (t=0.796, p=0.214).

Table 2
Experimental Decisions by Treatment

<table>
<thead>
<tr>
<th>(1) Dictator game (dollars given)</th>
<th>(2) Estimated gift (dollars)</th>
<th>(3) Prisoner’s dilemma (cooperation rate)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>mean</strong> (std dev)</td>
<td><strong>mean</strong> (std dev)</td>
<td><strong>percent</strong></td>
</tr>
<tr>
<td>A Enlightened self-interest</td>
<td>4.00 (2.24)</td>
<td>3.31 (1.74)</td>
</tr>
<tr>
<td>B. Moral duty</td>
<td>4.69 (2.59)</td>
<td>3.63 (2.03)</td>
</tr>
<tr>
<td>C. Control</td>
<td>3.65 (2.37)</td>
<td>3.47 (1.74)</td>
</tr>
</tbody>
</table>

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. One-tail tests of the hypotheses that the proportion of cooperation is higher in the ethics treatments than the Control yields negative findings at conventional levels of significance, both for Enlightened self-interest (z=0.885, p=0.188) and for Moral duty (z=–0.119, p=0.547). Although the 8.2% point difference between cooperation rates in A and C does not achieve significance beyond the 20% level, if subsequent research proved it to be robust, it would be economically significant: with randomly matched players, mutual cooperation would occur, on average, 53% more frequently in the Enlightened self-interest treatment than in the Control (viz., in 23.2% of cases vs. 15.1% of cases).

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, for students with a major in business or economics, for the second wave to address the aforementioned change in protocol, and on the dollar value of Decision 1 in regressions 2 and 3 to examine possible correlations with Decisions 2 and 3, respectively. When the other subject data from the questionnaire are included, none prove significant at conventional levels, so the analysis here centers on the independent variables of a priori interest.
Table 3
Regression analysis

<table>
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<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
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<tbody>
<tr>
<td></td>
<td>Dictator game</td>
<td>Estimated gift</td>
<td>Prisoner’s dilemma</td>
</tr>
<tr>
<td></td>
<td>(dollars given)</td>
<td>(dollars)</td>
<td>(cooperation rate)</td>
</tr>
<tr>
<td></td>
<td>Estimate</td>
<td>Estimate</td>
<td>Estimate</td>
</tr>
<tr>
<td></td>
<td>(std error)</td>
<td>(std error)</td>
<td>(std error)</td>
</tr>
<tr>
<td>A Enlightened self-interest</td>
<td>0.32</td>
<td>−0.52*</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>(0.48)</td>
<td>(0.31)</td>
<td>(0.10)</td>
</tr>
<tr>
<td>B Moral duty</td>
<td>1.02**</td>
<td>−0.44</td>
<td>−0.10</td>
</tr>
<tr>
<td></td>
<td>(0.48)</td>
<td>(0.32)</td>
<td>(0.10)</td>
</tr>
<tr>
<td>Business/Economics</td>
<td>−0.06</td>
<td>−0.60**</td>
<td>−0.16*</td>
</tr>
<tr>
<td></td>
<td>(0.41)</td>
<td>(0.27)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Protocol</td>
<td>−0.62*</td>
<td>0.09</td>
<td>0.00</td>
</tr>
<tr>
<td></td>
<td>(0.37)</td>
<td>(0.25)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Decision 1</td>
<td>4.02**</td>
<td>2.53**</td>
<td>0.42**</td>
</tr>
<tr>
<td></td>
<td>(0.53)</td>
<td>(0.40)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.05</td>
<td>0.28</td>
<td>0.05</td>
</tr>
</tbody>
</table>

Notes: *p<0.10, **p<0.05, N=167.

The main effects regarding dictator giving in column 1 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).

Turning to the dictator’s estimate of the average gift of other dictators in the same session, i.e., decision 2 in Table 3, we see marginally significant or insignificant negative effects of ethics treatments (p=0.100 for A, and p=0.174 for B). Business and economics students are significantly less optimistic about the generosity of their fellow students’ (p=0.026). The coefficient of 0.39 indicates gifts and estimated gifts are strongly correlated (p<0.001). This is consistent with the false consensus effect Iriberri and Rey-Biel report in their dictator games.
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 actual transfers, which is not the case, according to two-tail tests of differences in means (0.38<p<0.67). 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-tail 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 (p=0.056). The change in protocol has no effect on cooperation – in any case, an effect was expected at most for the dictator decision. Cooperation is significantly and positively related to generosity. 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.” 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.

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7 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.

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

9 It would be interesting to include these ratings in regressions, but it was unclear how to design the experiment so as
Table 4
Ratings of Lecturers

<table>
<thead>
<tr>
<th></th>
<th>Speaking skills</th>
<th>Personal likability</th>
<th>Enthusiasm</th>
<th>Knowledge</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>A. Enlightened self-interest</td>
<td>4.28</td>
<td>4.45</td>
<td>4.10</td>
<td>4.77</td>
<td>58</td>
</tr>
<tr>
<td>B. Moral duty</td>
<td>4.69</td>
<td>4.58</td>
<td>4.60</td>
<td>4.89</td>
<td>55</td>
</tr>
<tr>
<td>C. Control</td>
<td>4.20</td>
<td>4.53</td>
<td>4.64</td>
<td>4.73</td>
<td>55</td>
</tr>
</tbody>
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Two-tail t-tests of differences in means (p-values)

<table>
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<tr>
<th></th>
<th>A vs. C</th>
<th>B vs. C</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.542</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>0.527</td>
<td>0.678</td>
</tr>
<tr>
<td></td>
<td>0.000</td>
<td>0.718</td>
</tr>
<tr>
<td></td>
<td>0.650</td>
<td>0.097</td>
</tr>
</tbody>
</table>

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.

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 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 main positive findings are that generosity is significantly greater following Moral duty instruction, cooperation is positively correlated with generosity, and economics and business students are less cooperative than other majors. Although students taught about Enlightened self-interest are more generous and more cooperative, the main effects are not significant. Enlightened self-interest arguments might simply be ineffectual, or perhaps insufficiently strong, in this context. Evidence from some public good games suggests that targeted prompts only have a significant, sustained effect on cooperation, if they are somehow reinforced, e.g., by communication (Brosig, Ockenfels and Weimann, 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.

What, if any, are the implications of a positive finding regarding ethics for the practice of

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.
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 (2015) 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).
References


APPENDIX – NOT FOR PUBLICATION

Composite Experimental Instructions

Key: Comments on procedures not read aloud appear in [brackets]. Text that is unique to versions is denoted: <Version 1> {Version 2}.

[Guest lecturers introduced by regular professor. Guest gives lecture and departs. Regular professor handouts and then collects an evaluation of lecturer form. Professor introduces experimenter, who announces experiment on economic decision-making, gives $3 show-up fee, and asks students to sign receipts and not to talk or communicate with one another.]

Please note that your participation is voluntary. You have the right to withdraw at any time and to forfeit all payments you have received and will receive from your participation.

You will now collect your materials for the experiment. Each of you will go individually to the study carrel in the back of the classroom. Behind the study carrel there is a box with envelopes. You may select any one envelope you wish and then proceed to your seat. Please keep your envelope closed until you are told to open it.

Please now refer to the sheet that states “General Instructions” at the top. For the moment, leave your envelope closed. I will now go over the General Instructions, which you may read along with me.

General Instructions

<The packets you now have contain four envelopes labeled 1 through 4. You will complete the forms contained in these envelopes in order, return them to their envelopes and then return the envelopes to the packet. The first three envelopes involve decisions about the allocation of earnings and the fourth is a questionnaire. Please leave these materials in your packet until instructed to take them out.>

Each person in this room, which we will call Room A, will be randomly paired with a different person in a different room, which we will call Room B. You will not be paired with any of the people in this room. You will never know the identity of your counterpart in Room B, nor will your counterpart ever know who you are.

We are employing a number of measures to guarantee your anonymity, that is, to ensure that no one, including your counterpart and me, the experimenter, will ever be able to trace any decision to you personally. I will now explain these measures. You chose your envelope, and, when you are finished, you will return your envelopes to the box from which you took them one at a time and confidentially. The materials in your envelopes are identified only by a subject ID. Since you chose your own envelope, only you know your subject ID. Before returning your envelope, you will remove from it a slip with your subject ID. You will keep this slip and use it later to claim your earnings. After the experiment, I will prepare and seal the payments for each person by subject ID. Then a student will be randomly chosen to distribute the payments. I will leave the classroom while the student assistant matches subject IDs to sealed payment envelopes and sees that each subject receives the correct envelope. You will then pocket your envelope and open it later. That way only you will know your earnings: I will not know which person has a particular subject ID, and the student assistant will not know how much the earnings of any person are. In addition, the payment envelopes will contain not only the correct earnings in bills
but also blank slips, where necessary, in order to ensure that all payment envelopes have the same thickness.

Please now put aside the General Instructions, but you may review them at any time. Open your envelope and take out a sheet that states “Decision 1” {“Allocation Decision”} at the top and a slip with your subject ID on it. Please pocket your subject ID slip now, which you will later use to claim your earnings. I will now go over the <Decision 1> {Allocation Decision} instructions, which you may read along with me.

<Decision 1> {Allocation Decision}

A sum of $10 has been allocated to each of the subjects here in Room A. {The subjects in Room B have not and will not receive any such payments.} You may, however, choose to transfer an amount of your sum to your counterpart in Room B. Transfers can only be made in whole dollar amounts, as indicated in the table below. Please indicate below how much, if any, of your $10 you wish to transfer to your counterpart in the other room by circling that amount. You will have five minutes for this decision. When you are finished, please put this form back in your envelope and seal it.

The amount I choose to transfer to my counterpart is

$0
$1
$2
$3
$4
$5
$6
$7
$8
$9
$10

If you have a question, please raise your hand, and I will approach you to answer your question. Please continue to maintain silence throughout the experiment. You may begin.

{We will now hand out packets that contain additional materials. Please keep your packet closed until you are told to open it.}

Time is up! {Subjects are instructed to return forms to the envelopes.}

Please open your packet and take out the sheet that says “Further Instructions” at the top. Leave the other materials in the packet. I will now go over those instructions, which you may read along with me.

Further Instructions

{The packet you just received contains three envelopes. The first two envelopes involve two additional decisions you will make and the third is a questionnaire. Please leave these materials in your packet until instructed to take them out. After completing these forms, you will place all materials, including the Allocation Decision you just completed, in this packet. Then, as stated previously in the General Instructions, you will return your Allocation Decision envelope...}
(enclosed now in your packet with the additional materials) to the box from which you originally took the envelope one at a time and confidentially. All of your decisions are still completely anonymous. The packet and the additional materials you just received are unmarked, including the subject ID spaces, which have been left blank. The additional materials, therefore, cannot be connected to you personally but only to a subject ID through your Allocation Decision form, which you also put in the packet. As explained previously in the General Instructions, the payments for the experiment will be made using a student assistant in a way such that no one will ever be able to trace any decision to you personally.}

Please now remove the envelope labelled “Decision 2” from your packet. Leave the other envelopes in the packet. Put the other materials in your packet, including the <Decision 1> Allocation Decision} envelope, the General Instructions form and the Further Instructions form. Take the form out of the “Decision 2” envelope. I will now go over the instructions, which you may read along with me.

Decision 2

Your task at this stage of the experiment is to estimate to the best of your ability how much on average subjects in Room A have transferred of their $10 sum to their counterparts in Room B. For purposes of calculation, this average will be rounded to the nearest whole dollar amount. If you correctly estimate this amount, you will receive your total earnings from all decisions in this experiment. That is, you will receive the sum of what you kept from the first Allocation Decision plus whatever you might earn in Decision 3. For every dollar error in your estimate, however, your earnings will be reduced by one dollar. For example, if your estimate is $1 above or $1 below the average transfer, your total earnings will be reduced by one dollar. As another example, if your estimate is $2 above or $2 below the average, your earnings will be reduced by two dollars. Your estimate of the average transfer from Room A subjects to Room B subjects can only be made in whole dollar amounts, as indicated in the table below. Please indicate below your best estimate of this value by circling that amount. You will have five minutes for this decision. When you are finished, please put this form back in the Decision 2 envelope and seal it.

I estimate that the average transfer of Room A subjects to Room B subjects is

$0
$1
$2
$3
$4
$5
$6
$7
$8
$9
$10

If you have a question, please raise your hand, and I will approach you to answer your question. Please continue to maintain silence throughout the experiment. You may begin.
Time is up! Please make sure you place your Decision 2 form in your Decision 2 envelope and seal the envelope. Return this envelope to your packet.

Please now remove the Decision 3 envelope from your packet. Leave the other envelopes in the packet. Take the form out of the Decision 3 envelope. I will now go over the instructions, which you may read along with me.

Decision 3

This is the final decision of the experiment. Your earnings from this decision will be added to your net earnings from previous decisions. In this round, each person here in Room A will be randomly paired with a different person in a different room, which we will call Room C. The people in Room C are a different group from the Room B subjects in the first round of this experiment. You will never know the identity of your counterpart in Room C, nor will your counterpart ever know who you are. Your earnings depend on the actions you and your Room C counterpart choose. You and your counterpart will separately and independently choose an action, X or Y. Your combined actions will jointly determine your earnings in the following way:

<table>
<thead>
<tr>
<th>You choose X and your counterpart chooses X</th>
<th>Your earn</th>
<th>Your counterpart earns</th>
</tr>
</thead>
<tbody>
<tr>
<td>You choose X and your counterpart chooses Y</td>
<td>$8</td>
<td>$8</td>
</tr>
<tr>
<td>You choose Y and your counterpart chooses X</td>
<td>$10</td>
<td>$0</td>
</tr>
<tr>
<td>You choose Y and your counterpart chooses Y</td>
<td>$4</td>
<td>$4</td>
</tr>
</tbody>
</table>

Please circle your choice of action X or action Y below. You will have five minutes for this decision. When you are finished, please put this form back in envelope 3 and seal it.

I choose action:  X  Y

If you have a question, please raise your hand, and I will approach you to answer your question. Please continue to maintain silence throughout the experiment. You may begin.

Time is up! Please make sure you place your Decision 3 form in your Decision 3 envelope and seal the envelope. Return this envelope to your packet.

Please now remove the envelope labeled “Questionnaire” from your packet. Leave the other envelopes in the packet. Take the form out of the Questionnaire envelope. Please take the time to consider and answer all of the questions on the Questionnaire as thoroughly as possible. You will have ten minutes to complete this form. In particular, please take care in answering the questions on the final page regarding your service activities. When you reach that page, please read the instructions carefully, and if you have a question, please raise your hand, and I will approach you to answer your question.

When you are finished, please put the form back in the Questionnaire envelope and seal it.

Now you may proceed individually to the box behind the study carrel at the back of the classroom. Deposit your packet anywhere in that box. Please take your belongings with you, and you may leave immediately after depositing your packet.

Thank you for your participation.
Evaluation of Guest Lecturer

Please read and respond briefly to the questions below to help evaluate guest lecturers such as this one. Your answers are completely private.

The first two questions are about the lecture and its content.

1. What do you think was the most important point of the lecture today?

2. Please state whether you think such lectures are useful and why or why not.

The following questions are about the lecturer himself. Please evaluate the lecturer on the five-point scale provided:

3. How would you rate the lecturer’s overall speaking skills?
   5  Very high   4  High   3  Average   2  Low   1  Very low

4. How would you rate the lecturer in terms of personal likeability?
   5  Very high   4  High   3  Average   2  Low   1  Very low

5. How would you rate the lecturer in terms of enthusiasm?
   5  Very high   4  High   3  Average   2  Low   1  Very low

6. How would you rate the lecturer’s knowledge of the subject matter of the lecture?
   5  Very high   4  High   3  Average   2  Low   1  Very low
Questionnaire

1. For Decision 1 of the experiment, you chose how much, if any, of your $10 to give to your counterpart in Room B. Why did you choose the amount that you did for this decision?

2. For Decision 3 of the experiment, you chose action X or action Y. As a reminder, your decision combined with that of your counterpart in Room C jointly determine your earnings in the following way:

<table>
<thead>
<tr>
<th>Your choice</th>
<th>Your earn</th>
<th>Your counterpart earns</th>
</tr>
</thead>
<tbody>
<tr>
<td>X and X</td>
<td>$8</td>
<td>$8</td>
</tr>
<tr>
<td>X and Y</td>
<td>$0</td>
<td>$10</td>
</tr>
<tr>
<td>Y and X</td>
<td>$10</td>
<td>$0</td>
</tr>
<tr>
<td>Y and Y</td>
<td>$4</td>
<td>$4</td>
</tr>
</tbody>
</table>

Why did you choose the action that you did for this decision?
Please answer all questions below, indicating just one answer per question, as we cannot use forms with incomplete or multiple answers.

3. What is your college?
   1 Business                3 Liberal Arts
   2 Communications and Fine Arts  4 Science and Engineering

4. What is your first major (if undeclared, write UD)?

5. What year in college are you?
   1 Freshman          3 Junior
   2 Sophomore          4 Senior
   5 Graduate

6. What is your age?
   _______ years

7. What is your gender?
   1 Male                2 Female

8. What is your ethnicity (if several apply, please choose the one that you consider most accurate)?
   1 Asian/Pacific-Islander  4 Latino/Hispanic
   2 Black/African-American  5 Middle-Eastern
   3 Caucasian              6 Native-American/American Indian

9. What is your best estimate of your total expenditures this school year (September through May)? Please consider all expenses including tuition, housing, food, clothing, transportation, entertainment, etc., even if some are covered by financial aid or grants.
   $______________ for the current school year (September through May)

10. What is the total (gross) income last year of your parents or guardians (or spouse, if married)? Exclude your own earnings. Please choose a single response, even if it is a guess.
    1 $0 to less than $25,000    5 $100,000 to less than $125,000
    2 $25,000 to less than $50,000   6 $125,000 to less than $150,000
    3 $50,000 to less than $75,000    7 $150,000 or more
    4 $75,000 to less than $100,000

11. Approximately how much money have you earned total through your work over the past year (the past twelve months)?
    $__________

12. Have you ever taken a University level ethics course - at LMU this is the upper division Philosophy core requirement (circle appropriate answer)?
    1 No          2 Yes, in the current semester    3 Yes, in a past semester

13. On the following scale, how religious would you say you are?
    1 Not at all religious    2 Slightly religious    3 Moderately religious    4 Very religious