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Can Economic Ethics Be Taught?

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

Abstract

Recent business scandals and economic upheavals have prompted calls for ethics education. This paper reports the results of three studies that explore possible effects of different types of *economic ethics*, i.e., ethics instruction in an economic context. Study 1 examines possible effects on fairness views of mandatory justice instruction. Study 2 measures generosity and cooperation among students in economics classes following lectures on professional ethics. Study 3 examines whether distributive and reciprocal preferences are correlated with current or past volunteering activities. The findings are mixed, tentatively suggesting that the existence and type of effect depend on the method employed.

Keywords: ethics, fairness, cooperation, reciprocity, experiments
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INTRODUCTION

“Old truths have been relearned; untruths have been unlearned. We have always known that heedless self-interest was bad morals; we know now that it is bad economics.” Second Inaugural Address of Franklin Roosevelt, January 20, 1937.

There is a growing discourse on returning economics to its origins as a “moral science” and on strengthening the emphasis on ethics in economics teaching and research, e.g., see Bruni and Sugden (2013), Sandel (2013), and Schiller and Schiller (2011). The rise in support for ethics education has cut across many fields, as evidenced by the addition of initiatives such as the UNESCO Ethics Education Programme and by the rapid growth of college-level ethics requirements and of service learning activities valued in the billions of US dollars. But special concerns have been raised about the ethical training of those who, though their participation in or influence on the economy, have often been seen as complicit for such events as the 2001 accounting scandals at Enron and WorldCom, the 2007-08 financial crisis and subsequent Great Recession, and, more recently, the Libor rate-fixing scandal. As a result, the percentage of business schools that report requiring an ethics course for their MBA programs has more than doubled from 34% in 2001 to 79% in 2011 (Beyond Grey Pinstripes). There are related concerns about the repercussions of economics training on parties to such events as well as evidence of broad types of unethical conduct among professional economists (e.g., List et al., 2001), and economists have begun to call for the establishment and instruction of ethical guidelines in their profession, e.g., Atkinson (2011) and DeMartino (2011).

There are multiple ways in which economics training might affect society and the economy through its moral content (or lack thereof). Although many economists maintain that they should specify means rather than ends (note this is itself a normative claim), their education stresses certain goals (mostly efficiency-based ones) and centres on a view of human nature as self-interested. These factors can insinuate themselves into policy recommendations, including as they pertain to taxation, regulation, education and public health. The private sector relies on economists in various functions, including for consulting on financial markets and economic forecasting. The undergraduate economics degree is a leading major for many private and public sector positions and, in some countries, for post-graduate studies in law, finance, management, marketing and accounting. Moreover, the potential impact of economics instruction extends far wider, as it is required for virtually all undergraduate and graduate business degrees, including the influential MBA degree. Indeed, a voluminous literature that began with Marwell and Ames (1981) and Carter and Irons (1991) asks “does studying economics lead to more self-interested behaviour?” The weight of evidence suggests those trained in economics do act more selfishly than others, although the results are mixed on the culpability of economics training per se.¹

¹ Other studies on this question include Bauman and Rose (2011), Beil and Laband (1996), Frank, Gilovich and

This paper turns the above question on its head and asks “does studying ethics in an economic context affect moral attitudes or behaviour?” Most economists are familiar with *economics and ethics*, which signifies the study of normative economics, including but not limited to welfare economics. The focus of this paper, on the other hand, is what I will call *economic ethics*, which seeks to modify moral attitudes or intrinsically motivated moral behaviour. Specifically, this refers to ethics training in an economic context, such as in an economics class, in the treatment of an economic topic (e.g., economic justice), or in an economic activity (e.g., working or volunteering). That is, economics and ethics is a subfield of economics that conveys knowledge or tools, akin to labour economics or industrial organization, whereas economic ethics aims to transform students morally in some way.

To address the above question, this paper reports the results of three studies conducted at a comprehensive US university. Each study employs a different sample of college students and differs in other important ways from the others. There are two general reasons for this multi-study approach. First, both the methods and the goals of ethics training vary considerably, and an aim here is to represent this breadth. Second, every empirical method has its pros and cons, so a multi-method approach allows us to avail ourselves of the strengths of different methods while avoiding reliance on a single empirical strategy and its attendant weaknesses. I elaborate these points and describe the three studies briefly below.

Study 1 is designed in light of the fact that the traditional and oldest form of ethics instruction takes place in philosophy classes. Although some of these instructors might wish to affect student behaviour, a more modest and widely shared goal is to influence moral reasoning. This study, therefore, examines possible effects on fairness views of exposure to five weeks of readings and lectures in a mandatory philosophy class on an ethical topic that is important to both economics and philosophy, viz., distributive justice. Study 2 is motivated by the exponential rise, over the past few decades, in the types and numbers of ethics courses that have a professional focus, including legal ethics, medical ethics, engineering ethics, accounting ethics, communication ethics, and, as noted in the figures cited above, business ethics. Professional ethics seeks chiefly to effect behavioural, rather than attitudinal, changes, most frequently by shifting the focus of concern from self to others and/or by motivating cooperative behaviour. Thus, in this study, students in economics classes hear lectures on business ethics and then participate in classroom economics experiments that measure

Regan (1993, 1996), Frank and Schulze (2000), Frey and Meier (2003), Frey, Pommerehne and Gygi (1993), Haucap and Just (2010), Haucap and Müller (2014), Hu and Liu (2003), Laband and Beil (1999), Meier and Frey (2004), Ockenfels and Weimann (1999), Selten and Ockenfels (1998), Spraggon and Oxoby (2009), Stanley and Tran (1998), Yezer, Goldfarb and Poppen (1996), and Zsolnai (2003). Other research finds that those who study economics care less about fairness and/or more about efficiency than others, e.g., see Faravelli (2007), Fehr, Naef and Schmidt (2006), and Hole (2013). An important question is whether laboratory findings on social preferences extend to behaviour in the field. The results are mixed: Benz and Meier (2008) and Stoop (2014) find that social preference experiments are predictive of behaviour outside the laboratory, whereas Voors et al. (2012) do not. Levitt and List (2007) argue this depends on variations in context. Indeed, context is a possible constraint on extrapolation from one experiment to another or from one observational study to another.

generosity and cooperation. Study 3 addresses the most recent, and rapidly growing, development in moral education, which is experiential rather than formal. Specifically, service learning involves civic engagement linked to academic institutions and is increasingly promoted for various putative personal and social benefits, including for its morally transformative effects on participants. According to Campus Compact (2012), service learning at its member college campuses has grown steadily and was valued at \$9.7 billion in 2012, 70% above the level in 2008. More generally, 65% of Americans reported volunteering in the previous year (Gallup, 2013), and worldwide volunteer work has been valued at 2.4% of the global economy (Salamon, Sokolowski and Haddock, 2011). The two moral preferences that have been most frequently identified as economically important in both laboratory and field studies are distributive and reciprocity preferences. Study 3, therefore, examines whether current distributive and reciprocal preferences in a laboratory economics experiment are correlated with current or past volunteering activities outside the laboratory. Thus, between these three studies, we consider various important types of morality, including generosity (Croson and Konow, 2009), fairness (Faravelli, 2007), cooperation (Dannenberg et al., 2014) and reciprocity (e.g., Becker, Messer and Wolter, 2013).

One aspect of different types of ethics instruction is the expected duration of the particular effects. Although long-term changes would always seem preferable, many interventions actually target the short- or intermediate-term in apparent recognition of diminishing effects over time. Indeed, the possibility of long-run effects of professional ethics seem in doubt (e.g., see Schwitzgebel, 2013, and papers cited therein), hence some policies to promote compliance with moral and legal norms in the workplace are implemented repeatedly and at regular intervals, e.g., California law AB1825 mandates sexual harassment training for most people in supervisory capacities at least every two years. Other ethics measures with short-run goals include the common practices of charitable organizations to call door-to-door or to employ mailed solicitations, telethons, and televised advertisements that exhort potential donors to immediate action. Thus, there is variation in the studies here regarding measured durations. Study 2 focuses on the short-run, whereas studies 1 and 3 also include evidence on the intermediate- and long-run, respectively.

With respect to empirical methodology, all studies involve some degree of control, since the interest here is in intrinsic motivation, which is difficult to isolate among the confluence of motives typically at work in observational data. Otherwise, however, the studies employ diverse empirical methods, which are designed both to represent a breadth of empirical tools and to fit the particular types of ethics training at hand. One important distinction is whether one seeks to identify the impartial moral views of third parties (so-called “spectators”) or the willingness of interested parties (so-called “stakeholders”) to act on those views. Study 1 concerns the former, so it employs questionnaires and avoids personal stakes that might bias reports. Studies 2 and 3, on the other hand, address the latter and are incentivised, therefore,

with monetary stakes. Study 3 also considers spectator decisions using monetary payments. Study 1 uses vignettes, or hypothetical scenarios, instead of payments to elicit spectator views for various reasons. For one, Study 1 concerns numerous and very different concepts of justice, and experiments limit the contextual richness that is often helpful for exploring a complex fairness concept, let alone a whole set of them. Moreover, vignettes have proven better at aiding reasoning about complex concepts than presentation of problems in abstract form (Goldstein and Weber, 1995). This paper, therefore, employs both questionnaires and incentivized experiments for reasons similar to those of Gaechter and Riedl (2006).

Another important methodological choice is whether ethics training is mandatory or voluntary, and there are arguments for and against each. On the one hand, if subjects can avoid ethics training, selection biases that weaken inferences about causality can result. On the other hand, pro-sociality can be affected by exogenously imposed rules, as suggested by the study of Dannenberg, Lange and Sturm (2014). Specifically, compulsory participation involves extrinsic incentives, which can have distortionary effects on social norms, according to Gneezy, Meier and Rey-Biel (2011). These effects can be favourable, e.g., by helping to establish a norm, or unfavourable, e.g., by crowding out intrinsic motivation. But, in any case, these represent potential confounds, which are distinct from the possible influence of the training itself on the intrinsically motivated morality that is the focus of this study.

Thus, the studies reported here occupy different positions on this spectrum. In Study 1, all of the students at this university were required by the core to take an ethics class listed in the philosophy department, so there should be no selection effect. The students in Study 2 chose to take the economics course or a major that required it. Thus, these findings might not generalize to other groups, but that fact is of no concern here, since it is precisely this self-selected group that we wish to examine in Study 2. Participation in Studies 1 and 2 was, in a strict sense, not mandatory, since students were advised of their right not to take part; nevertheless, all did, in fact, participate, so neither should exhibit selection bias. Subjects in Study 3 chose to participate in the economics experiment, and their behaviour in it is related to their volunteering activities outside the laboratory. The volitional nature of the latter, in particular, could call into question inferences about causality between volunteer activities and moral preferences. One approach would be to assign students randomly to a group that is required to do volunteer work and to one that is not. This, however, raises several other problems, which are discussed later in the section on Study 3 (e.g., the concern about intrinsic motivation discussed above, and the challenges to making service truly mandatory and to defining the appropriate control). Study 3 approaches this question, therefore, as it is usually done in observational studies and considers the degree to which the correlational evidence is consistent or at variance with reasonable competing hypotheses.

There are interesting related studies on how moral attitudes or behaviour at times change, e.g., Cappelen et al. (2011), Ellingsen and Johannesson (2005), Stutzer et al. (2011), Almås et

al. (2010) and Fan (2000). Nevertheless, there has been no work, to my knowledge, on the questions raised here, despite the rising interest in and potential importance of economic ethics. The current paper, therefore, is an exploratory effort focused chiefly on the possible existence of such effects and not on their magnitude or mechanisms. These particular moral preferences were targeted because of numerous and long-standing findings pointing to their importance in the laboratory and the field. Given the richness of moral preferences and in the interests of brevity, however, some important issues, such as those concerning honesty (e.g., Gneezy, 2005), are left for future work. The results are sometimes surprising and suggest that the existence and type of effect depend on the particular ethics training employed. Sections I to III correspond to Studies 1 to 3, respectively, and section IV concludes.

I. PHILOSOPHICAL ETHICS: VIEWS OF DISTRIBUTIVE JUSTICE

We start with the longest established form of ethics instruction and with the effect that seems easiest to achieve. Ethics education has historically been the domain of philosophers, and the traditional, and still main, approach to the teaching of moral philosophy involves presenting and critically analysing dominant ethical theories. Although ethicists might hope that such instruction will favourably impact behaviour, a more modest, and presumably widely shared, objective is that such instruction will alter and perhaps improve moral reasoning. Since this type of ethics training is conceptual, it also seems more fitting to examine its possible effects on attitudes or beliefs than on behaviour. Most of the loosely related empirical research has focused on possible effects of professional ethics instruction, such as in business and the sciences, and has been subject to various methodological shortcomings.² There appear to be no empirical studies of the effects of traditional ethics courses in philosophy, let alone as they apply to an economic question. Distributive justice is a moral preference that has proven to be one of the most important economic forces in the laboratory, labour markets, taxation, education policy, and the provision of health care (e.g., see Konow, 2003). Thus, Study 1 examines how views of economic fairness might be affected by exposure to in-depth treatment of theories of distributive justice in an ethics course taught by a philosopher.

Design and Procedures

A potential problem, of course, is the existence of a selection bias: those who choose to enrol in an ethics course might differ from those who do not in ways related to their fairness views. This study exploits a natural experiment to address this challenge. All students at the university where the study was conducted were required by the university core to take an

² For surveys of such indirect empirical evidence, see Bloodgood, Turnley and Mudrack (2008), Mayhew and Murphy (2008), and Schwitzgebel (2013). These find little or no evidence of professional ethics instruction based mostly on self-reported behaviour. Another type of evidence, which is indirect but more closely related to philosophical (as opposed to professional) ethics, comes from a series of studies by Schwitzgebel and his collaborators indicating that ethics instructors behave no better and often worse than other philosophy professors, e.g., see Schwitzgebel (2009), Schwitzgebel and Rust (2009, 2010), and Schwitzgebel et al. (2012).

ethics course listed in the philosophy department. These students provide a treated group without selection bias, but a question arises about the appropriate control. One possibility was to conduct the study *within subjects*, i.e., to take before and after measures of attitudes. This has the advantage of involving mostly the same subjects. On the other hand, some differences could still be expected because of students adding, dropping or being absent from the class, and using a within subjects method increases concerns about *experimenter demand effects* (see Zizzo, 2010), e.g., it is likely that some respondents would be prompted to change their responses to the identical survey that is repeated after covering course material on the very topic of the survey. Alternately, the survey could be conducted *between subjects*, and the natural experiment delivered this means, as well: students were not permitted to take the ethics course until they reached Junior (i.e., third year) status. This created a treated group free of selection biases to set beside a comparable control group drawn from a wide variety of first and second year courses whose only systematically observable difference was age. Of course, views might differ because of age, but several arguments, including those based on regression analysis, count against this.³ Most questionnaires were administered in class and elicited fairness views about circumstances described in vignettes, i.e., hypothetical scenarios. The goal of Study 1 is not to test knowledge of ethical theories, which would be expected to change even if there were no change in views, nor is it to examine behavioural effects. Rather, the aim is to explore possible repercussions of ethics instruction for impartial moral reasoning, and, as discussed in the introduction, vignettes eliminate material incentives that might bias views while providing concrete and contextually rich situations. Moreover, the questionnaire was administered according to good design principles (see Appendix A).

At least four aspects of the design should make it easier for differences between the two respondent groups to manifest. First, ethics classes were selected that included a particularly intensive treatment of distributive justice: the first five weeks were dedicated to readings and in-class discussions of this topic that included concepts of equality, desert, libertarianism, consequentialism, Kantianism, Rawlsianism, and natural law. Second, most of the respondents (98 of 151) completed the survey at the start of the class meeting immediately following completion of the section on justice, so that the material would still have been fresh in their minds. Other students, who had taken courses with the same five week section on distributive justice in one of three previous semesters and responded to mailed surveys, were included to boost sample size and to examine the duration of any effects. Nevertheless, the responses of the two groups of ethics students did not differ significantly, save on one

³ The treated group includes numerous respondents whose ages overlap with those of lower class rank, and regressions of responses on age reveal no significant relationship. This non-effect is also consistent with the similarity of views between this student sample and an older general population in other surveys, e.g., see the discussion in Appendix A and Question 8A in Konow (2003). Moreover, age might be more of a concern, if we found systematic differences between the treatment and control, but, as we will see, there are systematic similarities. Such a finding requires some (unidentified) force to offset the effect of age in just the right magnitude across the wide range of different fairness contexts examined here, which seems highly unlikely.

question that will be discussed later. Third, the effect that is investigated is a possible change in moral views elicited in an attitudinal survey, which only requires expressing a different belief and not some potentially costly change in behaviour. Fourth, it is well established that fairness views expressed in surveys can be sensitive to even slight changes in wording or presentation, so it would not be surprising, if they were also affected by course material that specifically espouses positions about what is fair (see the discussion in Appendix A).

Results and Analysis

The questions were selected to represent major schools of thought about distributive justice, specifically, they are organized into the four families of theories that structure the treatment of justice in Konow (2003). Indeed, the results for the non-ethics sample are drawn from that

Table 1

Fairness Views of Ethics and Non-ethics Students

<u>Question</u>	<u>Theoretical Family/Concept or Topic</u>	<u>Ethics sample % (N)</u>	<u>Non-ethics sample % (N)</u>	<u>Difference %</u>
<i>Consequentialism & Welfare Economics</i>				
1	Pareto Principle	35 (91)	43 (132)	-8
2	Compensation Principle	34 (58)	41 (123)	-7
3	Absence of envy	9 (147)	10 (260)	-1
<i>Need & Equality</i>				
4	Basic needs	87 (142)	89 (122)	-2
5	Difference Principle	19 (146)	20 (177)	-1
6	Original position	15 (149)	14 (142)	1
7	Nature as the cause of inequality	3 (149)	1 (150)	2
<i>Equity & Desert</i>				
8	Proportionality	89 (147)	85 (295)	4
9	Responsibility	83 (148)	81 (78)	2
10	Conflicting responsibility concerns	54 (146)	48 (131)	6
<i>Context & Framing Effects</i>				
11	Wage cut if market wage falls	10 (148)	17 (258)	-7*
12	Wage cut if falling sales	54 (100)	59 (191)	-5
13	Wage cut if new lower profit business	81 (94)	67 (220)	14**
14	Price cut with cost-plus pricing	23 (103)	32 (158)	-9
15	Low price but small share of sales	55 (91)	65 (85)	-10

Notes: Percentages are the fraction of respondents judging the justice concept or action as fair. Sample sizes (N) are in parentheses and (apart from non-responses) differ for the non-ethics groups, since results were collected in various waves; in the ethics sample, some questions have fewer observations given the aim to avoid presenting similar questions or contrasting versions of the same question (e.g., questions 1 and 2) to the same respondents. Difference is the percentage of ethics students minus the percentage of non-ethics students. Results of two-tail tests of differences in proportions are reported as *p<.10, **p<.05.

publication and others of the author, which were based on surveys conducted in a wide range of introductory college classes. Table 1 summarizes the results for the 15 questions ordered into one of the four families of theories for the ethics and non-ethics samples and includes percentage responses and, in parentheses, sample sizes. The percentages represent those respondents expressing a fairness view consistent with endorsement of the theory or approval of the action described in the scenario. The difference in the percent of fairness judgments between the two groups is indicated in the final column as well as the significance of any differences according to two-tail tests of differences in proportions. As is apparent from this final column, the magnitude of these differences is small and rarely exceeds single digits, and only one difference (for question 13) is significant at conventional levels. Thus, despite setting the bar low, we find little evidence of an effect of this type of ethics instruction.

Discussion of Questions

To give an idea of the breadth of fairness concepts represented, let us review the topics very briefly. Note that justice scholars might disagree about whether specific questions accurately or fully capture a theory, especially given the complexity and subtlety of many theories. That point, however valid, is not relevant for the purpose at hand: the goal here is not to test the theories, which merely serve as inspiration for the questions, but rather to compare fairness judgments of the two samples in a variety of contexts. Those interested in the fairness concepts themselves are referred to Appendix A for the complete statement of questions and to the original sources referenced there for a more extensive discussion of the concepts.

Most of normative economics, including welfare economics, is consequentialist, i.e., it judges the rightness of acts or states based on their consequences or outcomes. The central concept in economics is, of course, the *Pareto Principle*, which endorses gains, as long as no one loses. Question 1 asks which is fairer: an equal allocation between two parties or one that makes one party better off and the other the same. Most respondents in both samples go against the Pareto Principle and find the equal allocation fairer. The results to question 2 reveal a similar level of opposition to the related *Compensation Principle*, which allows for losers, as long as winners could theoretically compensate losers (even if they do not). In welfare economics, fairness per se is usually associated with *Absence of envy*, which defines an allocation as fair, if no agent prefers (i.e., envies) the bundle of another. Only 9-10% of respondents judge an envy-free allocation as fair in question 3.

Other theories of justice emphasize equality and/or basic needs. Question 4 reveals that large majorities of both groups of respondents support allocating enough of a grant to satisfy the *basic needs* of some people temporarily, even if doing so reduces the ability to raise the living standard of others permanently. Probably the most influential justice theory of the twentieth century was due to John Rawls (1971). Rawls's full blown theory is too complex to tackle in short questions, so a simplified and piecemeal approach was adopted: questions 5 to 7 seek to represent in stylized form assumptions or conclusions of his theory. Large

majorities of respondents go against Rawls's *Difference Principle* (question 5), a distributive rule that only allows inequalities that benefit the poorest, his central thought experiment (the *Original position* in question 6), and the insignificant role he seems to allow for non-random factors in life (*Nature* in question 7).

Another school of thought, based on equity and desert, associates justice with inequality rather than equality. Inspired by Aristotle (1925) and formulated and refined in social psychology, sociology and economics, equity theory calls for *Proportionality* of outputs to inputs. That is, fair allocations are proportional to individual contributions, which finds strong support in question 8. Theories of desert are most frequently based on the putative *Responsibility* of agents for the benefits or burdens that have occurred. Differing allocations are fair, if agents are responsible for underlying differences, but are equal, if they are not, consistent with the majority view in question 9. Additional survey questions and economics experiments (e.g., see Konow, 2000, 2001) corroborate desert and support the accountability principle, which combines proportionality and responsibility: fair allocations are in proportion to the contributions individuals control but ignore differences which they do not. Despite evidence of broad support for the accountability principle, views are sometimes closely divided, as when there are multiple and *Conflicting responsibility concerns*. That is the case in question 10, where there is tension between a fair earnings from a single transaction versus fair overall earnings between two parties. The approximate equal split among both samples to this question and several others in this questionnaire help dispel possible suspicions that the similarities between the two samples is due to the absence of controversy in the scenarios: even in knife-edge cases, both groups respond similarly.

Much justice research has focused on the effects on fairness judgments of context, including the variable allocated, the set of individuals involved and the presentation of facts (or framing effects), which questions 11 to 15 address. It is seen as unfair for an employer to cut employee wages merely because of a fall in the market wage (question 11), although it is more acceptable, if the employer's sales are falling (12), and even fair, according to most respondents, if the employer switches to a new business where its profits fall (13). Most respondents do not think fairness requires a factory to cut its price with its costs by the amount called for by cost-plus pricing (14). Finally, a small majority accepts an unfairly low price on one product, if the product comprises a small fraction of sales and the seller can show a profit on other items (15).

With question 13, we see the first and only difference in responses between ethics and non-ethics students that is statistically significant at conventional levels. If this were the only judgment affected by ethics instruction, the overall conclusion would remain that there is little evidence of an effect. On the other hand, there are other reasons to suspect even this result. First, one significant difference out of 15 is roughly what is to be expected by chance and in the absence of any difference in the populations. Second, if ethics instruction caused

this difference, one would expect the difference to be larger for students currently enrolled in the course. Yet, there are no significant differences between the responses of current and former ethics students on any questions save question 13, and this one case goes opposite the predicted direction. If we break down the sample of ethics students for this question into those currently enrolled in ethics, students who took ethics in a past semester, and those who took ethics but have subsequently graduated, only this last group differs significantly from the non-ethics sample: 13 of 14 graduates find it fair for the employer to cut wages in its new, low profit business (perhaps an idiosyncratic consequence of this group's work experience?).

In sum, we observe little evidence that traditional instruction in philosophical ethics affects moral judgments about concrete, contextually rich situations, which is especially striking given the various aforementioned considerations that suggest this is a modest expectation. This finding could be unique to these contexts and/or this course. On the other hand, it is consistent with studies that have looked for other types of effects of ethics classes and found little to no supportive evidence (e.g., see, Schwitzgebel, 2013). Nevertheless, the absence of such effects does not necessary mean this method is ineffective in a broader sense, a point to which I will return in the concluding section of this paper.

II. PROFESSIONAL ETHICS: GENEROSITY AND COOPERATION

One can contrast the philosophical ethics just discussed with what is often called “professional ethics,” such as business ethics and medical ethics. The latter seeks explicitly to modify behaviour, whether through imparting knowledge, molding thoughts or motivating action. This seemingly sets the bar higher in terms of outcomes than with the changes in moral reasoning considered in the prior section. Study 2 employs lectures that exhort students enrolled in economics classes to moral conduct and examines possible effects on their behaviour in classroom economics experiments. Specifically, I focus on possible short-run effects of ethics training on levels of generosity and willingness to cooperate, given the economic significance of such motives and measures in the workplace (discussed in the introduction) and in general (e.g., see Andreoni, 2006, Gintis et al., 2004, and Frank, 1987).

Design and Procedures

This study involved three sections of introductory microeconomics taught by the same instructor, and all sections participated on the same day in the twelfth week of instruction. The students, 59% of whom majored in business or economics, were informed beforehand that there would be guests on that date but were not told any further details. Each class began with a lecture by a different guest professor, who was introduced by the regular instructor. The only procedural difference between the three sections was the identity of the guest lecturer, and each constituted, therefore, a different treatment.⁴ Since there are alternative

⁴ Although not perfect, the assignment of students to sections was quasi-random: students were, in large part, randomly assigned registration dates while caps on sections were gradually raised, which effectively forced

approaches to teaching professional ethics, two different ethics professors were invited and employed different methods. Treatment A involved what I will call “enlightened self-interest”: the professor argued that those who act in their narrow self-interest do not fare as well, in the long run, as those who behave in the general interest. Treatment B propounded what I will call “moral duty”: this professor focused on a method for choosing the right course of action and exhorted students to follow that action. These two treatments mirror the main division between schools of thought in ethics. “Enlightened self-interest” can be seen as an example of the school that focuses on achieving some outcome (the “Good”) and “moral duty” as an example of the other school, which stresses moral rules or virtues (the “Right”). The third section, C, was a control: a statistics professor discussed applications of statistics to microeconomics. After about thirty minutes, the lecturer finished and departed, and the instructor asked the students to complete an evaluation of the guest lecturer. After collecting the evaluations, the regular instructor then introduced the experimenter without explicitly making or disavowing any connection to the previous lecture.

Given the classroom setting, the experiment was not computerized. Students were given a \$3 show-up fee, chiefly to reassure them that monetary payments were real, and they were informed that participation was voluntary but all took part. 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. Pains were taken to ensure so-called double-blind anonymity: subjects collected their materials and deposited them confidentially and one at a time, and the use of subject IDs, blank slips to ensure equal thickness of payment envelopes and a randomly chosen student to distribute payments outside the presence of the experimenter ensured individual earnings were known only to the subject (the Instructions can be found in Appendix B).

Results and Analysis

We begin by summarizing the results of the dictator and prisoner dilemma decisions, which are illustrated in Figures 1 and 2, before turning to a more detailed analysis. The median dictator transfer across all treatments was \$4 (the mean was \$3.91), and numerous subjects gave more than one-half, in apparent contradiction to fairness: 9% of dictators across all

many students into one section or the other.

conditions and 14% in the Moral duty treatment gave more than \$5. Such transfers are high relative to most prior dictator experiments, so I refer to giving in this first decision as generosity. Mean dictator gifts are higher in the ethics treatments than the Control, indeed, giving is highest in the Moral duty condition followed by the Enlightened self-interest treatment. The average cooperation rate across all conditions is 42.4%, and, from Figure 2, we see cooperation is also higher in the ethics treatments than the Control, although the order is different: cooperation is highest in the Enlightened self-interest treatment followed by the Moral duty treatment.

Figure 1
Mean Transfers in the Dictator Game

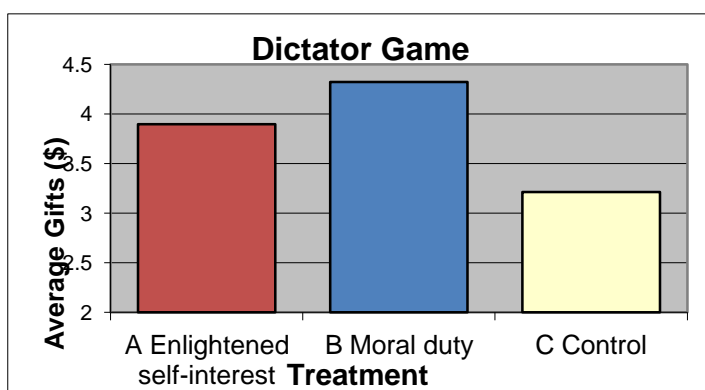
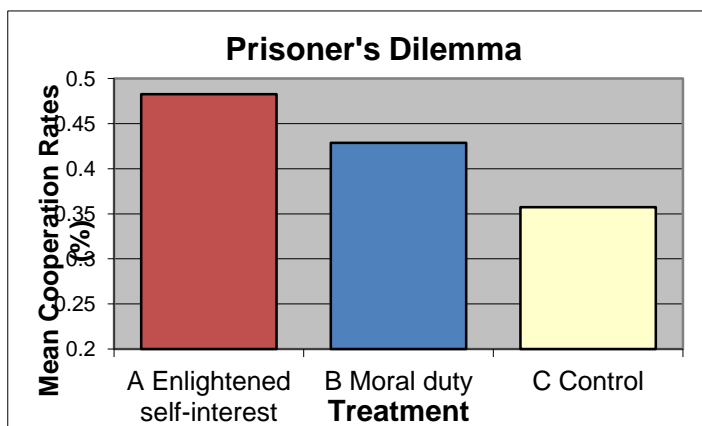


Figure 2
Cooperation Rates in the Prisoner's Dilemma



The results are summarized in Table 2 and suggest that different types of ethics instruction trigger different behavioural effects: appealing to moral duty more strongly impacts generosity whereas highlighting enlightened self-interest is better at promoting cooperation. This seems plausible: moral duty arguments encourage unconditional moral behaviour as with dictator generosity, whereas enlightened self-interest stresses mutual dependence, which relates to cooperation. Of these two claims, however, only the former is statistically significant: one-tail tests of the hypothesis that ethics instruction favourably

affects behaviour compared to the Control are significant for the mean dictator gift in the Moral duty treatment ($t=1.88$, $p=.032$) but not for the proportion cooperating in the Enlightened self-interest treatment ($z=.96$, $p=.168$) (or the other two comparisons, i.e., A vs. C for the dictator gift and B vs. C for the prisoner's dilemma). Although the 12.6% point difference between cooperation rates in A and C is not statistically significant with the sample sizes these small classes permitted, if it proved robust, it would be economically significant: with randomly matched players, mutual cooperation would occur, on average, 83% more frequently in the Enlightened self-interest treatment than in the Control (in 23.3% of cases vs. 12.7% of cases).

Table 2

Effects of Ethics Lectures on Generosity and Cooperation

	<i>Generosity in dictator game (dollars given)</i>		<i>Cooperation in prisoner's dilemma</i>
	<i>mean</i>	<i>(standard deviation)</i>	<i>percent</i>
A. Enlightened self-interest	3.90	(2.43)	48.3
B. Moral duty	4.32	(2.37)	42.9
C. Control	3.21	(2.01)	35.7

Note: *N* equals 29 for treatment A and 28 for B and C.

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 3. First, we note that all three lecturers received high average ratings with all but one of the nine scores exceeding 4. Comparing the Control to A and B, respectively, on the four questions, only three of the eight comparisons are significant, and all three of these indicate the lecturer in the Control was rated somewhat more favourably than the ethics professors. This fact casts doubt on the conjecture that the more favourable patterns of generosity and cooperation after the ethics lectures were due to some desirable personal characteristics of the lecturers.⁵

⁵ It would be interesting to include these ratings in regressions, which would presumably strengthen treatment effects, 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.

Table 3
Ratings of Lecturers

	<i>Speaking skills</i>	<i>Personal likability</i>	<i>Enthusiasm</i>	<i>Knowledge</i>
Mean scores				
A. Enlightened self-interest	4.23	4.27	3.87	4.70
B. Moral duty	4.55	4.48	4.52	4.86
C. Control	4.36	4.79	4.68	4.71
Two-tail t-tests of differences in means (p-values)				
A vs. C	.453	.001	.000	.174
B vs. C	.225	.038	.281	.380

Note: Responses on five point Likert scale where 5=Very high and 1=Very low.

We have considered patterns of generosity and cooperation across treatments, but it is interesting to examine possible within subject correlation in decisions. Dictator transfers are highly positively correlated with their estimates of the average transfer of the group ($.75 > r > .56$ and $p < .001$ in all three conditions), and, although the average transfer of subjects in treatments A and B (but not C) is greater than their estimates of the group average, none of these differences is significant ($.76 > p > .21$). Thus, participant estimates resemble their actions, but their estimates are not, on average, biased, possibly reflecting a consensus effect, i.e., overestimating the similarity of one's actions to those of others.⁶ Is generosity related to cooperation? The results of this study suggest a resounding no: dictator gifts are uncorrelated with cooperation in the prisoner's dilemma ($.22 > r > .05$ and $.79 > p > .25$), and the dictator gifts of co-operators did not differ significantly from those of defectors in any treatment according to tests of differences in means ($.93 > p > .26$). Thus, the dictator and prisoner's dilemma decisions appear to have tapped into distinct motives, consistent with the evidence that they can be primed independently. Overall, Study 2 suggests that professional ethics lectures can produce short-run behavioural effects and that the particular effect depends qualitatively on the type of moral argument, although the result for cooperation is tentative.

III. VOLUNTEERING: DISTRIBUTIVE AND RECIPROCAL PREFERENCES

The most recent and rapidly growing approach to ethics training is experiential, rather than formal. Service learning engages students in volunteer work and is touted not only for its social benefits but also for its putative contributions to building the moral character of volunteers. As explained in the introduction, volunteering is not only an important factor at

⁶ Note, as Engelmann and Strobel (2012) demonstrate, that a consensus effect need not be *false*; i.e., it may 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. Also note the pattern here might be a case of norm-compliance, i.e., a desire to match one's behaviour 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 ($.26 < p < .76$). I thank Colin Camerer and Alexander Koch for comments that contributed to this discussion.

many college campuses but also represents a significant component of economic activity, in general. There has been little experimental economic research on volunteering (as opposed to charitable donations), and existing work has focused on factors that affect the rate of volunteering (Al-Ubaydli and Lee, 2011, Conrads, et al., 2013, Lacetera et al., 2014) or the performance of volunteers (Gneezy and Rustichini, 2000). The only study relating volunteering and moral motivation, to the author's knowledge, is Xiao and Houser (2014), whose main result is that college students who are properly incentivized to volunteer are subsequently more likely to express an interest in future volunteering. Study 3 considers, more generally, whether current or past volunteering is related to distributive or reciprocal preferences, the two moral preferences most frequently cited as economically important in both laboratory and field studies.

Background

Volunteering is chosen by volunteers, so there is always the possibility of selection bias. One approach would be to assign students randomly to a group that is required to do, say, community service or to one that is not. It would be interesting to study such mandatory service, but then other concerns arise. First, it would not be a study of volunteering, which is, by definition, volitional. Second, as explained in the introduction, a requirement, by being extrinsically incentivized, can crowd-in or crowd-out intrinsic moral motivation and, therefore, confound inferences about it. This may be seen as a specific instance of the more general point that experiments are weaker than observational studies on *external validity*, i.e., generalizability to behaviour in natural environments. Third, it is unclear how to make service truly mandatory among a college population, since students usually enjoy some degree of latitude regarding such participation: often they may take or drop professors, courses, majors, colleges or even institutions of higher learning, and these choices might depend, at least in part, on such requirements. Fourth, although experiments are typically stronger on causality, causal inference is always a matter of degree: even a perfectly randomized study does not give certainty about causality, e.g., because one in twenty tests of a non-relationship will be positive by chance and because one can never be completely sure that the conditions that differentiate treatment and control do not also hide some unintended and unnoticed causal factor. For example, in the experiment proposed above, students in the control group should presumably be assigned a different "non-volunteer" activity, but that activity might have its own favourable or unfavourable impact on moral preferences.

Thus, there is no single ideal empirical method for investigating moral preferences, as there are trade-offs between the strengths and weaknesses of each. This gives credence to the approach suggested by Levitt and List (2007) of employing multiple empirical strategies in a series of studies. Since this is an initial study of volunteering and moral preferences, the field is open to any point on this spectrum. Study 3 combines controlled laboratory measurement of intrinsic moral motivation with uncontrolled volunteering in the field. It is stronger,

therefore, on identifying intrinsic moral preferences but weaker on causal claims between those preferences and volunteering. Ideally, it will be followed by other studies that occupy other points on the spectrum, including a randomized study along the lines described above. Study 3, however, approaches this question as do econometric studies that use correlational evidence. Of course, correlation does not imply causation, but the absence of correlation does suggest the lack of causation, so a positive result would at least clear this modest hurdle. Moreover, the analysis proceeds as is usually the case with observational studies and considers the degree to which the correlational results are consistent or at variance with reasonable competing hypotheses. I will argue that the particular pattern of findings here seems more consistent with a causal relationship between volunteering and moral preferences than the most obvious alternative explanations.

Whereas Study 2 explored short-run behavioural effects, Study 3 considers both short-run and long-run relationships between ethics training and behaviour. In addition, Study 3 includes consideration of reciprocal preferences. Becker et al. (2013) report the results of an interesting field experiment of reciprocal preferences over the long-run, but their study is of gift-giving and not of effects of volunteering on subsequent behaviour. Study 3 can be seen as relevant to the influential movement in the social sciences and biology advocating “strong reciprocity” (see Fehr and Fischbacher, 2004, Gintis et al., 2004). More specifically, we consider here the claim in that literature that people are motivated to punish others, even in situations where they have no personal stakes or personal relationships in the matter at hand. Many explanations for this phenomenon are based on the malleability of such preferences, over varying periods of time, in response to experience. Here we examine the relationship of reciprocal preferences, including of uninvolved third parties, to recent and past volunteering activities. Whereas previous work has focused on third party punishment, the experiment on which the present analysis is based also includes consideration of third party reward.

Design and Procedures

Study 3 employs previously unanalysed data on volunteering that was collected for the experiment on distributive and reciprocal preferences reported in Croson and Konow (2009). I will now describe the features of that study, which are relevant to the analysis at hand (note this includes only the X Decision treatments). Subjects were recruited online from among the student population, and, after preliminary procedures, including the payment of a \$5 show-up fee, there followed two dictator decisions. First, each subject in Group X decided how much, if any, of a \$10 endowment to share with an anonymous counterpart in Group Y in six different \$2 increments (i.e., 0, 2, 4, 6, 8 or 10 dollars). Second, there followed a previously unannounced dictator decision in which each subject from a group different from X chose how much of a \$20 endowment to give to a Group X subject and how much to X’s paired Group Y counterpart. This decision employed the “strategy method”: second stage dictators (or “allocators”) made six decisions about how to divide the \$20, one for each of the six

possible first stage decisions, without knowing X's choice. There were two treatments, which differed with respect to the identity of the second stage allocator. In the so-called "stakeholder" treatment, this was the same Group Y subject with whom the Group X subject was paired for the first dictator decision. In the so-called "spectator" treatment, it was someone from a third set of subjects, Group Z. Z subjects were paid a fixed \$20 fee unrelated to their decision to allocate the separate \$20 between the X/Y pair assigned to them. After the second dictator allocation, all subjects completed questionnaires that elicited information on types and hours of community service activities in the current semester and in the previous four years. Since demographic variables have sometimes proven important to behaviour in the laboratory (e.g., see Almås et al., 2014), the questionnaire also collected information about the subjects including their majors, which provides new evidence relevant to previous findings regarding business and economics students. Then actual Group X decisions and their corresponding Y or Z allocations, respectively, were made known, and subjects were paid. Further details of the experiment can be found in Croson and Konow (2009).

This experiment was designed to eliminate strategic motives and to enable association of each of these decisions with a distinct and different set of motives. The first Group X decision combines self-interest and distributive preferences. The Group Z allocation should reflect distributive and reciprocal preferences, since Z, apart from distributive concerns, might additionally wish to reward or punish X for high or low first stage transfers, respectively. The Group Y allocation potentially results from the same motives as those of Group Z except for the added effect of self-interest, since the Y subjects have a stake in the second allocation. Here I use the Group X decisions to analyse the willingness to depart from self-interest and to act on distributive preferences, and I analyse the Group Y and Z decisions for identifying reciprocal motives (as well as the effect of self-interest on such motivation in the case of Group Y).

Distributive Preferences: Results and Analysis

Table 4 presents results of regression analyses of the Group X gifts to Y. Regression (1) reveals that these gifts are positively correlated with total hours of volunteering: dictator transfers increase by 21 cents for every 100 hours of volunteering. This is equivalent to 55 cents more given by a volunteer who worked the average number of hours in the past (227) compared to someone who did no volunteer work (the average gift overall was \$3.07). Regression (2) explores this further by separating current volunteering from volunteer activities over the past four years, and it finds this relationship is associated solely with past volunteering. Additional regressions that included demographic variables, such age, race, gender and year in college, found no significant relationships with the exception of major, which is included here: in Table 4 we see that business or economics majors gave about \$1.14 less than other majors, consistent with the aforementioned literature suggesting these majors tend to be less pro-social.

Table 4

Distributive Preferences: Regressions for Group X Gifts

	(1)	(2)
Volunteer work (100s of hours)		
Total current and past	0.21** (0.09)	
Current semester		-0.09 (3.62)
Past four years		0.21** (0.10)
Business/economics major	-1.13** (0.53)	-1.14** (0.55)
R-squared	.14	.14

Note: Level of significance: * $p < .10$, ** $p < .05$; standard errors in parentheses; $N = 60$.

The effects of the timing of the service activities can help discriminate alternative hypotheses about the relationship between generosity and service. If both volunteering and dictator giving are caused by a tertiary variable like personality (i.e., stable individual traits), then both current and past volunteering should be related to gifts. If volunteering causes dictator giving, however, either current or past activities might be relevant. On the one hand, recent events are often more salient. On the other hand, one alternative hypothesis is that repeated acts of generosity increase long-run psychological well-being, which, in turn, produces a disposition toward subsequent dictator giving (see Konow and Earley, 2008). Thus, results here showing a relationship between generosity and past service are more consistent with the psychological well-being hypothesis than with the alternative causal hypothesis due to recency or with the personality hypothesis, which excludes direct causality between generosity and volunteering.

Reciprocal Preferences: Results and Analysis

Turning now to reciprocal preferences, recall that second stage allocators (i.e., Y and Z dictators) could condition their allocations to X on how much X gave in the first stage. Indeed, second stage allocators gave more, on average, to X for higher levels of X giving. To examine reciprocal preferences, we focus on this response, i.e., how much more did the second dictator allocate to X, on average, for each dollar increase in the amount transferred by X to Y in the first stage? For simplicity, consider first the Z allocators, and note that if they cared only about the distribution of final earnings between X and Y, this response should equal 1: for each additional dollar X gave to Y in the first stage, Z should increase the amount to X by one dollar in the second stage in order to maintain the distribution of earnings

between X and Y at whatever levels Z desires. But a Z response greater than 1 suggests reciprocal motives, specifically, reciprocity can be positive, i.e., to reward X for generosity, or negative, i.e., to punish X for stinginess.

Table 5

Reciprocal Preferences: Regressions for Y and Z Responses

Response of second dictator to:	(1) all X gifts	(2) low X gifts	(3) high X gifts
Constant	1.31*** (0.09)	1.28*** (0.15)	1.25*** (0.18)
Group Y allocator	-0.40** (0.13)	-0.24 (0.20)	-0.51*** (0.24)
Current volunteer work (10s of hours)	-0.08*** (0.02)	-0.02 (0.04)	-0.19*** (0.04)
Past volunteer work (100s of hours)	0.04* (0.02)	0.08** (0.03)	0.02 (0.03)
R-squared	.26	.14	.31

Note: Level of significance: * $p < .10$, ** $p < .05$, *** $p < .01$; standard errors in parentheses; $N=60$.

Table 5 presents the results of regression analyses of the response of second stage allocators. Regression (1) takes as the dependent variable the increase in second stage allocations to X averaged over all levels of X gifts (from \$0 to \$10). A dummy variable equals 1, if the second stage dictator was a Group Y allocator, meaning that the constant captures the response of Group Z allocators. This constant shows that Z allocators exhibit reciprocal motives: they increase the amount given to X subjects by \$1.31 for each dollar X increased its gift to Y, which significantly exceeds 1 ($F=10.23$, $p < .01$). We see that this response is significantly lower by 40 cents, when the second stage allocator is a Group Y subject, indicating a more muted response by Ys to X transfers.⁷ Turning to volunteering, we break this down by current and past volunteer work, in light of the prior results indicating distinct effects (the coefficients are calibrated to reflect 10s of hours for current and 100s of hours for past given the large difference in periods covered by each, viz., one semester for current and four years for past). The coefficients indicate that the response of second stage allocators decrease with the amount of current volunteering and increases with amount of past volunteering, whereby the latter effect is marginally significant.

In light of previous findings of an asymmetry in positive and negative reciprocity,

⁷ Unlike the case with Z allocators, however, a Y response less than or equal to 1 does not necessarily signal the absence of reciprocal motives, since Y decisions are also affected by self-interest, which might reduce responses. In fact, the results of Croson and Konow (2009) suggest reciprocal preferences do play a role in Y allocations despite a net response of less than 1 in the case at hand.

including in the study from which these results draw (see Croson and Konow, 2009, and references cited therein), I break down this response further. It is natural in this experiment to consider negative reciprocity as the average response to low X gifts (i.e., less than one-half of first stage stakes) in regression (2) and positive reciprocity as the average response to high X gifts (i.e., more than one-half) in regression (3). Compared to (1), these reveal similar Y and Z responses, although (2) indicates Y punishment of low X gifts no longer differs significantly from that of Zs, and the constant in (3) exceeds 1 at marginal significance ($p < .09$). On the question of interest, the second and third regressions reveal an asymmetry regarding volunteering. The negative effect related to current volunteering is due entirely to reduced reward for high X gifts, i.e., the reward for X generosity is lower. The positive effect related to past volunteering is due entirely to increased punishment for low X gifts, i.e., a sharper increase for increasing the X transfer or, since this is in the punishment range, a larger decrease in the allocation to X, if X gives less to Y. Further regression analysis finds no significant differences in response effects of volunteering between Y and Z allocators or of demographic variables, including major, age, race, gender and year in college.

What explanations offer themselves for the relationships between reciprocal preferences and volunteering? Suppose some tertiary variable, like personality, causes both volunteering and affects reciprocal preferences. It is not clear, however, what this force is, and, as in the case of X gifts, why it would differ for current and past volunteering. Alternately, a reasonable interpretation of strong reciprocity applied to this context is that those with greater past service will punish more forcefully, not only as stakeholders (Ys) but even as third party spectators (Zs). Strong reciprocity focuses on punishment, but I am unaware of any evidence or hypotheses pertaining to positive reciprocity and volunteering, so we are left to speculate. Perhaps allocators with high current service are more inclined to view generosity as a duty that does not require reward, although this is merely conjecture, given the limited work on positive reciprocity, let alone as it bears on volunteering.

IV. DISCUSSION AND CONCLUSIONS

The studies reported in this paper have yielded differing results about economic ethics, suggesting both possible effects and non-effects. Moreover, the effects appear to vary qualitatively with the approach adopted. As a general point, this seems not only plausible but also the most interesting result. If there were no significant effects, it would be inauspicious (even if it were not the final word). If, on the other hand, every method produced an effect, it would not only seem odd but would also present no challenge to economic ethics. I conclude with a discussion of the findings and suggestions for further research.

Study 1 failed to find systematic effects of the philosophical approach that focuses on teaching theories of ethics on everyday judgments, specifically, in the context of a topic of importance for both economics and philosophy, viz., distributive justice. Of course, future work could examine the robustness of this finding to other philosophical approaches and

other topics, but it does seem generally consistent with related research on non-effects of business ethics. It is also in line with the following thought: if moral intuitions are deeply ingrained and embedded in familiar contexts, then they might not be easily swayed by abstract arguments, even if they do respond to contextual variables and framing. In addition, if moral intuitions are not only deep but plural (as much empirical work arguably suggests), then multiple moral theories might well appeal to different aspects of those intuitions without convincing the student of the need to declare one theory or intuition as the victor. This stability might not only be what we should expect, but, if we hold that actual values (or at least some of them) are normatively valid, also a reassuring finding for normative work.

There is another way in which some types of ethics instruction should not necessarily be expected to generate behavioural or attitudinal changes. Philosophical ethics chiefly involves providing the means to reflect on normative questions. When applied to disciplines, for example, in the form of bioethics, political theory, and economics and ethics, the primary goal is typically to deliver tools that can inform such reflection and, perhaps, the formation and evaluation of policies. Thus, philosophical treatment of topics such as distributive justice should perhaps be judged not by the standards of economic ethics, which looks for changes in attitudes or behaviour. Rather, it should be evaluated as one would economics and ethics, i.e., by its success or failure in imparting knowledge and/or skills. That is, students of economics and ethics should not be expected to become more moral, at least no more than students of health economics should be expected to become healthier. In both cases, the principal targeted effect is indirect: students should acquire tools that can aid them in formulating or evaluating measures that achieve ethical ends or promote greater health, respectively. Thus, the absence of a direct effect on student attitudes or behaviour does not necessarily mean a type of instruction is ineffective: we must also consider other goals and indirect effects.

Study 2 examines professional ethics and its behavioural effects on students in economics classes, and it underscores the importance of distinguishing different methods and different effects. The evidence indicates that dictator generosity and cooperation in a prisoner's dilemma involve distinct motives and that appeals to moral duty increase the former; it is also suggestive that emphasizing enlightened self-interest promotes the latter. The results are consistent with the existence of distinct moral motivations and behaviours and seem plausible: encouraging unconditional regard for others increases giving, whereas stressing mutual interdependence and self-interested reasons for caring about others is favourable to cooperation. Future research might examine other pedagogical approaches and other effects, such as on reciprocity and honesty.

Study 3 involved a two stage dictator experiment and found that volunteering activities are correlated with distributive and reciprocal preferences, although the exact effects depend on whether the volunteering is current or past. Dictator generosity is positively related to hours of volunteering in the past but not the present, and business and economics students are

significantly less generous than other majors. The former effect seems consistent with a theory based on psychological well-being, which posits that repeated acts of generosity contribute to subsequent dictator giving. Among second stage dictators, punishment (negative reciprocity) was significantly greater among those who have volunteered more in the past, whereas reward (positive reciprocity) was significantly lower among those volunteering more currently. The punishment result seems consistent with a claim of strong reciprocity, viz., that cumulative social interactions reinforce negative reciprocity. The alternative conjecture that a personality trait accounts for these effects on generosity and reciprocity does not seem consistent with the differing effects of current versus past volunteering. On the other hand, volunteering is, by definition, volitional, so one cannot rule out a selection bias. It would be interesting in future work to examine the effects of mandatory service: although this use of extrinsic incentives risks obscuring inferences about intrinsic motivation, if properly implemented, it should reduce concerns about selection bias.

Calls for economists to revive the standing of their discipline as a moral science have recently increased against a backdrop of significant economic disruptions that many attribute, at least in part, to the personal failings of economic actors. This paper represents a first attempt to respond to these calls for the teaching of ethics in economic contexts, or “economic ethics.” More research is needed to examine the robustness of these results and to explore additional methods and effects. But the ultimate goal, in my view, is the establishment of pedagogical methods that motivate economists and economics students to act in accordance with shared moral standards in their personal and professional capacities. This may be seen as a complement to the more traditional “economics and ethics,” which provides the foundation and tools for incorporating ethics into economic policy and analysis.

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APPENDIX – NOT FOR PUBLICATION

Appendix A – Study 1

Discussion of Sensitivity of Fairness Views to Presentation (Framing Effects)

As an example of framing effects, consider version A of Question 4 from Kahneman, Knetsch and Thaler (1986):

4A. A company is making a small profit. It is located in a community experiencing recession with substantial unemployment *but no inflation*. There are many workers anxious to work at the company. The company decides to *decrease salaries by 7%* this year. Please rate this as:

Fair 38% Unfair 62% N=125

Version B of this question is identical, except that the italicized text is replaced by the following:

4B. ... *and inflation of 12%* ... *increase salaries only 5%* ...

Fair 78% Unfair 22% N=129

These questions reveal a framing effect, viz., money illusion: most respondents find it unfair to cut nominal pay in the absence of inflation, but a large majority finds roughly the same real pay cut fair, if accomplished by inflation. This shift in proportions is highly significant. The non-student sample used by Kahneman et al. might differ somehow from the student subject pool used here, but available evidence suggests otherwise: the survey of ethics students included question 4B, and 84% of the 147 students surveyed responded Fair, an insignificant 6 percentage point difference from the Kahneman, et al. result.

Fairness Questionnaire

The questionnaire was administered according to good design principles and so as to encourage thoughtful and candid responses while minimizing extraneous effects.

Specifically, respondents were given up to twenty minutes in-class (or as long as they wished for mailed surveys), responses were anonymous, contrasting or similar versions of questions were never presented to the same subject, the questionnaire was brief so as not to overtax respondents' attention, and questions were sequenced according to a randomized Latin-square design to address order effects.

The questions appear below. See the notes at the bottom of this section for an explanation of the key, which is included in parentheses below each question. Note that original response format for most questions was binary (e.g., Fair or Unfair), but some had multiple response categories, which were converted into a binary form for ease of presentation and of analysis in Table 1.

1. A small newly independent island nation is considering how to allocate its one banana plantation and its one sugar plantation. There are only two farmers on the island interested in these plantations. The government chooses among the following two plans either of which would result in the same total production of both bananas and sugar.

Plan X. Both farmers receive one-half of each plantation. Each farmer earns an average profit of \$100 per day from bananas and sugar combined. Therefore, the total of both farmers' profits is \$200 per day.

Plan Y. One farmer receives the banana plantation and the other farmer receives the sugar plantation. The farmers' profits are unequal since the sugar plantation is more profitable than

the banana plantation: average daily profit of the banana farmer is \$100 and that of the sugar farmer is \$200. At \$300 per day, combined profits are greater under this plan because specialization reduces production costs.

Please circle the plan which you consider more fair:

Plan X

Plan Y

(2: 5C; Plan Y)

2. A small newly independent island nation is considering how to allocate its one banana plantation and its one sugar plantation. There are only two farmers on the island interested in these plantations. The government chooses among the following two plans either of which would result in the same total production of both bananas and sugar.

Plan X. Both farmers receive one-half of each plantation. Each farmer earns an average profit of \$100 per day from bananas and sugar combined. Therefore, the total of both farmers' profits is \$200 per day.

Plan Y. One farmer receives the banana plantation and the other farmer receives the sugar plantation. The farmers' profits are unequal since the sugar plantation is more profitable than the banana plantation: average daily profit of the banana farmer is \$90 and that of the sugar farmer is \$160. At \$250 per day, combined profits are greater under this plan because specialization reduces production costs.

Please circle the plan which you consider more fair:

Plan X

Plan Y

(2: 5B; Plan Y)

3. Chris, who is blind, does not like TV and Pat, who is a vegetarian, does not like hamburger. Suppose that Chris and Pat work for the same company in the same capacity and earn the same base salary. The time comes for the end of the year bonus. Chris, who works much harder than Pat, receives a \$2 coupon for a hamburger. The less productive Pat, on the other hand, receives as a bonus a \$2000 wide screen television. Please rate this as:

Fair

Unfair

(1: 5; Fair)

4. Parador is an underdeveloped country whose people live at subsistence level: only their basic needs for food, shelter and clothing are satisfied. The only assistance available is a one time grant of \$100 million which the government of Parador has received. It can distribute this grant as it sees fit between two projects.

Project X. In eastern Parador there is malnutrition due to a drought. To prevent the starvation of the 500,000 people affected and to return them to subsistence level would require \$100 per person, or \$50 million.

Project Y. In western Parador there is an agricultural development program awaiting funding which would permanently raise its participants from subsistence level to a moderate standard of living. Its cost is also \$100 per person.

What do you think is the most fair distribution of the \$100 million between Projects X and Y (express in millions of dollars and make sure the total is \$100 million)?

Project X: _____ million

Project Y: _____ million

(2: 6A; % giving Project X \geq \$50M)

5. Suppose Mike and Bill begin working for a computer software company at the same time and in the same capacity. Initially they both earn a salary of \$50,000 per year. After a trial period Mike demonstrates that he is hard working, productive and performs far beyond initial expectations. Bill, on the other hand, is lazy, unproductive and performs far below

initial expectations. Their supervisor decides to give Mike a \$10,000 per year raise and to cut Bill's salary by \$1000. Please rate the supervisor's decision to raise Mike's salary and to cut Bill's as:

Fair Unfair

(1: 1B; Fair)

6. The owner of a small office supply store has two employees, Mike and Bill. They are equally productive and hardworking and are both currently earning \$7 per hour. The owner decides to move his store to a new location nearby where he knows business will be better. He lets his workers know that if they wish to continue at the new location he will be able to raise their wage. He explains that they will continue to have the same responsibilities but that one worker will earn \$8 per hour and the other \$12 per hour. He also explains that which worker gets the higher wage will be determined later on the basis of a coin toss. The workers can choose to go with the owner to the new location under these terms or to find similar work elsewhere for their current \$7 per hour. They both choose to go with the owner. Please rate the store owner's terms for the new wages as:

Fair Unfair

(1: 1A; Fair)

7. Mike and Bill are identical twins who were reared in an identical family and educational environment. They are the same in terms of physical and mental abilities, but Mike is more industrious than Bill. For that reason, after they begin their careers Mike ends up earning more than Bill. Please indicate whether you view such a difference in their earnings as:

Fair Unfair

(1: 1C; Fair)

8. Bill and Sam manage a small grocery store at different times and on different days. The manager's duties are always the same and the days and times which each work vary pretty much randomly, but Bill works 40 hours per week while Sam works 20 hours per week. Suppose the manager's salary for a 60 hour week is \$1200. Which of the following is the fairest division of this salary?

- A. Bill gets \$600 and Sam gets \$600.
- B. Bill gets \$700 and Sam gets \$500.
- C. Bill gets \$800 and Sam gets \$400.

(3: 5; C)

9. Bob and John become shipwrecked on an uninhabited island where the only food is bananas. They can collect as many bananas as they want by climbing up a tree, picking them before they fall into the ocean and throwing them into a pile. Bob and John are identical in terms of physical and mental abilities except that Bob was born with one hand and John with two. Together they pick a total of 20 bananas per day, but because of his condition Bob picks fewer bananas per day than John. John takes 12 bananas from the pile leaving 8 for Bob. Please rate this as:

Fair Unfair

(3: 1B; Unfair)

10. Davis and Thompson have restaurants in a shopping mall. Davis owns a video game machine with which he breaks even: it costs \$40 per week to maintain and, in Davis' restaurant, generates \$40 per week in revenue. In Thompson's restaurant maintenance costs

would still be \$40 but, because of the younger clientele there, weekly revenue would be \$80. Davis decides to rent the video game machine to Thompson and continues to pay for the \$40 weekly maintenance costs. Because Davis is related to the owner of the mall, he got a much more favorable location, and solely for that reason Davis runs a highly profitable business whereas Thompson operates on a very small profit. Please circle the weekly rent you consider fair for Davis to charge Thompson.

- A. \$50
- B. \$60
- C. \$70

(2: 2B; A)

11. A small photocopying shop has one employee who has worked in the shop for six months and earns \$9 per hour. Business continues to be satisfactory, but a factory in the area has closed and unemployment has increased. Other small shops have now hired reliable workers a \$7 an hour to perform jobs similar to those done by the photocopy shop employee. The owner of the photocopying shop reduces the employee's wage to \$7. Please rate this as:

Fair Unfair

(2: 11A; Fair)

12. A moderate sized company in a small community is the major local employer. The workers of the company are represented by their own independent local labor union. Sales of the company's product fall significantly, so the company cuts pay by 10%. Please rate this as:

Fair Unfair

(2: 10A; Fair)

13. A house painter employs two assistants and pays them \$9 per hour. The painter decides to quit house painting and go into the business of providing landscape services. With about the same time and effort, the former house painter's profits fall significantly in his new business. In landscape services the going wage is lower so he reduces the workers' wages to \$7 per hour for the landscaping work. Please rate this as:

Fair Unfair

(1: 9B; Fair)

14. Suppose a factory produces a particular table which it sells to wholesalers. The factory has been selling all the tables it can produce for \$150 each. Suppose that the factory has now found a supplier who charges \$20 less for the materials needed to make each table. What price is now fair to the factory and to the wholesalers?:

- A. \$150
- B. \$140
- C. \$130

(2: 7B; C)

15. Suppose a furniture manufacturer is the single supplier of chairs to a retail store, and both firms have similar sales volume and profits. Suppose that both firms would agree that \$100 is a fair price for the retail store to pay the furniture manufacturer for each chair: this price gives a fair return to the furniture manufacturer on its investment of time and money. Nevertheless, through government price controls the price is set very much lower. This leaves the furniture manufacturer with a very small profit on the chairs. Nevertheless, chair sales represent a small fraction of the furniture manufacturer's business since it produces many other profitable goods. Please rate this price as:

Very Fair
(2: 8G; Fair or Very Fair)

Fair

Unfair

Very Unfair

Notes: The information in parentheses following each question indicates the publication in which the question originally appeared (1=Konow, 2003; 2=Konow, 2001; 3=Konow, 1996), the original question number, and the response category/categories reflected in the percentages in Table 1. Thus, for example, under question 14, (1: 9B; Fair) means this was Question 9B in Konow (2003), and the percentages in Table 1 are those choosing Fair to this question.

Appendix B

Study 2 – Experimental Instructions

[Introduction of guest lecturers by regular professor]

Treatments A and B:

Most of the economics we study in this course is based on the assumption that people always act to promote their self-interest, so I thought it would be interesting to bring a different perspective from guest lecturer who specializes in ethics. Dr. _____ has kindly agreed to talk about ethics in an economic context.

Treatment C:

Most of the economics we study in this course deals with microeconomic theories, so I thought it would be interesting to bring a different perspective from guest lecturer who specializes in statistical applications of economics. Dr. _____ has kindly agreed to talk about an applied microeconomic topic.

[Economics experiments by experimenter]

I am now handing out \$3 to every person who showed up today. Please complete one of the Show-up Fee Receipt forms that are circulating to acknowledge your receipt of this fee. Make sure to complete this receipt using the pen that is also circulating. There are two copies of these forms – please enter your information on only one of the forms, not both.

This is an experiment about economic decision-making. If you follow the instructions carefully, you can earn a considerable amount of money in addition to the \$3 you have already received for showing up today. You will receive these additional payments privately, in cash, after the experiment.

Now that the experiment has begun, we ask that you do not talk or communicate with one another in any way. If you have a question after we finish the instructions or at any time during the experiment, please raise your hand, and the experimenter will approach you and answer your question in private.

You may be familiar with psychology experiments. Psychologists sometimes make use of deception in their experiments whereas economists do not. Everything that you are told during this experiment about the procedures, decisions and outcomes is completely accurate and truthful.

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

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 the two contents of the envelope: one is a sheet that states “Allocation Decision” at the top, and the other is 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 “Allocation Decision” instructions, which you may read along with me.

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.

Time is up! Please make sure you place your Allocation Decision form in your envelope and seal the envelope.

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

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 labeled “Decision 2” from your packet. Leave the other envelopes in the packet. Put the other materials in your packet, including the 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:

	You earn	Your counterpart earns
You choose X and your counterpart chooses X	\$8	\$8
You choose X and your counterpart chooses Y	\$0	\$10
You choose Y and your counterpart chooses X	\$10	\$0
You choose Y and your counterpart chooses Y	\$4	\$4

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.