

Do Ethics Classes Influence Student Behavior?

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Abstract:

Does university-level ethics instruction influence students' practical behavior? Little direct empirical work has been done on this question. I briefly review the existing data: two studies on honesty in laboratory settings and two re-analyzable economics studies on donation to student charities. Although these data suggest no overall effect of ethics instruction on moral behavior, the research is so limited that it is difficult to draw any conclusion. I then turn to the somewhat larger literature that examines the influence of ethics classes on students' self-reported moral attitudes. This literature is so flawed that, again, it is difficult to draw any conclusion, but the most reasonable interpretation appears to be that ethics instruction has at most a small influence on student attitudes.

Do Ethics Classes Influence Student Behavior?

1. Introduction.

Real-world moral behavior is hard to study. Maybe this partly explains the otherwise surprising paucity of scientific research on the question that forms the title of this essay. I will describe what research there is, and then I will describe some related research on the effects of university-level ethics instruction on students' verbally expressed attitudes. Generally, the research methodology in this area is poor and the effects weak and inconsistent. I will conclude that we have no compelling scientific justification to believe that ethics classes have any influence on students' real-world moral behavior. Perhaps, as ethicists, this should concern us.

2. Two Laboratory Dishonesty Studies.

I have found only two empirical studies that aim to directly examine the effects of university-level ethics instruction on students' moral behavior. These studies are not, as I will explain, very compelling evidence.

Bloodgood, Turnley, and Mudrack (2008) asked 230 business students to complete four timed word-search tasks. The students were told that they were being randomly assigned to groups of six or seven, that the person reporting the highest score in each group would receive \$10, and that every member of the group reporting the highest total score would receive \$5. Students then discarded their word-search sheets in a provided trashcan and self-reported the number of words

they had found in each task. Bloodgood and colleagues retrieved the discarded word-searches from the trashcan and compared the number of words students said they had found to the number of actually circled words. In all, only 16% of participants reported having found more words than they had circled. Bloodgood and colleagues compared the behavior of students who had already completed a required course in business ethics with the behavior of those who had not yet taken that course. They did not find that students overall were less likely to cheat if they had taken the course. However, they did find statistical interaction effects suggesting the possibility of less cheating among high ACT-score students and low-religiosity students if they had taken business ethics classes than if they had not. Bloodgood and colleagues interpreted this pattern as suggesting that business ethics classes do reduce student cheating, but only among less religious students who have “more room to grow” and among intelligent students who are “better at learning and applying the ethical lessons” (p. 565-566).

Mayhew and Murphy (2009) used a similar design. They gave business students an opportunity to anonymously report how well they had done on a trivia quiz. Students were paid depending on how many correct and incorrect answers they said they had, although they were also told that under certain conditions their answers would be checked and their payment reduced if they were found to have overreported the number correct. Mayhew and Murphy found no difference between a cohort of students who had been required to take a business ethics course and an earlier cohort who had not been offered that course. However, they did find that students who had taken business ethics were less likely to inflate their scores in a *non*-anonymous condition, a result Mayhew and Murphy interpreted as suggesting that ethics instruction might have “established expectations of ethical behavior” which were not internalized (p. 407).

These studies have serious limitations, if our aim is to assess the effects of ethics instruction on student behavior. In neither study were students randomly assigned into ethics vs. non-ethics courses. In the Bloodgood and colleagues study, this opens up the possibility of uncontrolled confounding factors such as interest in ethics, maturity, proximity to graduation, and ethics instruction from other sources. In the Mayhew and Murphy study, the lack of random assignment opens up the possibility of age and cohort differences. In both studies, the students knew they were being observed by professors at their university, in a laboratory setting, and so might have behaved very differently from how they would have behaved in a more typical practical setting. And it's not clear that either study accurately measures immoral behavior: The Mayhew and Murphy has a beat-the-experimenter game-like character that might make strategic misreporting ethically neutral; and it's possible that a non-negligible minority of the participants in Bloodgood et al., knowing their word-find sheets were being discarded, might not have bothered to circle every word they found, which if true could largely account for the 16% of students found "cheating". Even taking such misreporting at face value as a measure of dishonesty, it's not clear how closely such dishonesty resembles or correlates with the more serious types of misconduct that are presumably the instructional focus of most business ethics classes.

Thus, I doubt we should conclude much from these studies.

3. Re-Analyses of Two Economics Studies.

A small literature, launched by Carter and Irons (1991) and Frank, Gilovich, and Regan (1993), examines whether studying economics makes people more selfish. This literature is potentially

interesting as indirect evidence: If studying economics makes people more selfish, that seems to imply that university instruction can influence people's moral behavior for the worse – and if in some cases for the worse, maybe in other cases for the better?

Unfortunately the results of this economics literature are inconclusive. The evidence does seem to suggest that economics students tend to make more selfish, money-maximizing choices in settings that are explicitly structured as economic games (e.g., both offering and accepting small amounts in “ultimatum games”). But it's not clear that such selfishness generalizes to “real-world” (i.e., non-laboratory, non-“game”) settings, nor that studying economics causes whatever differences do exist. None of the three existing studies in this literature that use real-world, non-self-report measures finds an effect. Laband and Beil (1999) find economists no less likely than other social scientists to underreport income when paying income-based membership dues to academic societies. Frey and Meier (2003) and Bauman and Rose (2011), examining data from two universities in which all students were given the option of donating small fixed amounts of money to selected charities when registering for classes, found that although students majoring in business or economics were less charitable on average than students with other majors, over the course of their education their rates of charitable giving did not decrease relative to students of other majors. From this, both groups of researchers concluded that teaching economics did not reduce students' charitable giving.

Unfortunately, neither Frey and Meier nor Bauman and Rose collected data on ethics instruction in particular. However, both groups did collect data on student major. Thus, I was able to re-analyze the original raw data to see if philosophy majors, who normally take several ethics classes, increased their rates of charitable giving over the course of their studies. What I found

was the mirror-image of the data on business and economics majors: Philosophy students were more likely than other students to donate to charity, but their rates of charitable giving did not detectably change over time or relative to other students. Thus, exposure to the philosophy ethics curriculum did not appear to influence philosophy majors' rates of giving to these charities. For more detailed analysis, see the Appendix.

Of course, the most sophisticated charitable givers might choose to save their charitable dollars for charities other than those selected by the Universities of Zurich and Washington in this period. And since random assignment is not possible, the data are likely to be confounded, for example with socio-economic status, which tends to be higher for humanities and social science majors than for other majors, at least in the U.S. (Leppel, Williams, and Waldauer 2001; Goyette and Mullen 2006). Furthermore, philosophy students might join and leave the major and the university at different times than do other students and for different reasons, introducing year-by-year subject-pool differences.

To summarize Sections 2 and 3: Direct empirical evidence of the behavioral effects of university ethics education are meager and problematic. However, for whatever they are worth, the data do not point toward an overall effect. To date, there is no compelling evidence that ethics instruction has any effect on students' real-world moral behavior.

4. The Influence of Ethics Instruction on Self-Reported Attitudes.

There are a few dozen published studies on the influence of university ethics instruction on students' self-reported moral attitudes and behavior. Conveniently, two meta-analyses from 2009 address the bulk of the research.

Waples, Antes, Murphy, Connelly, and Mumford (2009) analyze the literature on business ethics instruction, looking at a variety of attitudinal and self-report variables across 25 studies (some studies of university students, but also some studies of ethics instruction for professional businesspeople). They find an overall weighted average effect size of $d = 0.29$; for studies focusing on university students, $d = 0.28$. (Cohen's d is the difference in means divided by the standard deviation; $d = 0.20$ is conventionally considered a "small" effect size and 0.50 a "medium" effect size.) Waples and colleagues conclude that "business ethics instruction, as reported in the literature, is at best minimally effective in enhancing ethics among students and business people" (p. 146). Similarly, Antes et al. (2009) analyze 20 studies of ethics instruction in the sciences (e.g., in biomedical ethics and in the responsible conduct of research), finding a weighted mean effect size of $d = 0.42$ and concluding that "ethics instruction is at best moderately effective as currently conducted" (p. 397).

It is, however, I think, difficult to interpret such analyses without a sense of the methods and results of the studies included. Examining the target studies in detail, the results are, in my view, more disappointing than suggested by the modest language of Waples, Antes, and collaborators quoted above. To give readers a feel for the literature, I will briefly discuss three sample studies. I selected the three studies by starting with the two studies in the meta-analyses that had the most citations since 2009 in Google Scholar as of July 31, 2013. Since both were studies of business ethics, I added a third study from the other meta-analysis – the study most-cited since 2009, excluding one study that examined the medical school curriculum in general without any direct measure of ethics instruction in particular. In my view, these three studies are representative of the literature.

Ritter (2006) examined students from two versions of an Organizational Theory and Behavior class, a class required of all Management majors at a mid-sized university in the southern U.S. Students in one class received additional instruction in an ethics curriculum that students in the other class did not receive. Ritter used two measures of the effectiveness of instruction. One was a “moral awareness” measure adapted from Smith and Oakley (1997), which asked students to indicate the extent to which they found various situations, like padding one’s expense account or bribing foreign officials, “ethically acceptable”. Looking at 77 students total, Ritter found no statistically significant difference on this measure between students exposed to the ethics curriculum and those not exposed.^[1] A second measure was a “moral reasoning” measure adapted from Clarkeburn (2002) which presented students with ethical vignettes, asked how they would behave, and then asked them to describe five issues they considered in making their decision. Responses were coded qualitatively on a 0-3 scale from non-ethical to the highest level of ethicality. Students in both versions of the class were given this moral reasoning measure both at the beginning at the end of the semester. Ritter presents raw data rather than inferential statistics for this measure, but compiling the data reveals a change in the predicted direction, but not approaching statistical significance.^[2] In a post-hoc analysis, Ritter reports that women appeared to have shifted more than men on both measures, including at an uncorrected p value of $\leq .01$ on one version of the first measure. She concludes that “the positive effects of an ethics training program were witnessed only in women” (p. 161).

Eynon, Hill, and Stephen (1997) mailed a survey to a sample of 1092 certified public accountants from across the United States, achieving a 16% response rate. The survey asked

recipients to report some demographic information including whether they had completed a college ethics course, to answer some opinion questions about ethics instruction, and to complete the three-scenario version of the Defining Issues Test. The Defining Issues Test (Rest 1979) presents respondents with paragraph-long moral dilemmas, such as whether to steal a highly-priced drug to save one's spouse, and then asks respondents to rate the importance of different possible factors one might consider in deciding how to act. The DIT designers regard some of the factors as characteristic of mature "postconventional" moral thinking (e.g., "Whether the law in this case is getting in the way of the most basic claim of any member of society") and others as characteristic of less mature thinking (e.g., "Whether the druggist deserves to be robbed for being so greedy and cruel"). In a multiple regression analysis, Eynon and colleagues found that CPAs who reported having taken a college ethics course tended to score about 8 points higher on the DIT, on a scale from 0 to 90. The authors interpret this result as "support for the effectiveness of ethics interventions" (p. 1304).

Myyry and Helkama (2002) report a study of 50 social psychology students who participated in a 20-hour ethics education curriculum in Finland, compared to a control group of six education students who participated in a qualitative research methods course. Participants read a 500-word story both at the beginning and at the end of the instruction. The story was a complex scenario involving child abuse, and participants were asked to "single out the elements that should be considered" in deciding whether the child should be placed in foster care. Answers were scored by the investigator, bearing in mind the extent to which the answers revealed "sensitivity to the special characteristics of the patient" and "awareness of what actions serve the rights and welfare of others". Possible scores ranged from 0 to 34. Students who had completed the ethics

curriculum showed a statistically marginal increase in score, from 8.9 to 10.0.^[3] However, since the small control group's mean score declined, the results of a statistical analysis comparing the two groups showed a greater positive shift for the ethics students than for the controls.^[4]

The reader with a critical eye will find many causes for concern about these studies, including small sample sizes or low response rates, lack of proper controls, possible experimenter bias in coding, dubious use of statistics, participants possibly being influenced by guesses about the nature of the study, experimenters "teaching to the test", and the validity of the measures. Not every study in the literature is subject to all of these concerns, but in the extent to which they invite considerable skepticism, the three studies reported above are typical of the state of the literature. More recent studies and older studies not included in the Waples and Antes meta-analyses are approximately similar in methods and quality (e.g., Jones 2009; Antes et al. 2010; Bosco, Melchar, Beauvais, and Desplaces 2010; Lau 2010; Warnell 2010; Lawrence, Reed, and Locander 2011; Burns 2012; Harkrider et al. 2012; Johl, Jackling, and Wong 2012; May and Luth 2013).

Several important sources of distortion will tend to favor finding an effect where none exists. Experimenters generally want positive results, perhaps especially if they are examining the effectiveness of curricula they support. Participants tend to want to please experimenters by confirming their suspected hypotheses (Orne 1962; Rosnow and Rosenthal 1997). Positive findings are generally more likely to be pursued and published (the "file drawer" problem; Rosenthal 1979). And positively-valenced measures – presumably including various types of

educational attainment and various measures of moral sensitivity – often correlate for a variety of reasons other than a direct causal relationship among the correlates (Meehl 1990). Under such conditions, standard quantitative meta-analyses like those of Waples and Antes might tend to reveal a positive relationship even if none exists (John, Loewenstein, and Prelec 2012; Schwitzgebel 2013).

Perhaps the most striking feature of this literature is not that when one combines the results in meta-analysis a modest relationship shows up. Rather, it's that that despite the likely existence of several sources of bias toward positive results, studies do not report larger and more consistent relationships between what students are taught and what they later say, often to those same teachers.

5. Conclusion.

In sum, we do not yet know what influence university-level ethics instruction has on students' moral behavior. The empirical literature is too weak to justify any confident conclusion.

Many ethics instructors hope to influence students' moral attitudes and behavior, and presumably some succeed. However, I see three reasons to suspect that the overall positive influence of the typical ethics class will tend to be very small. First, it seems reasonable to suppose that classroom instruction will generally have its largest effect on verbally espoused attitudes in a university context and diminishing effects on practical behavior outside the classroom in the long term; and, as discussed in Section 4, researchers have struggled to show even short-term effects of classroom instruction on verbally espoused attitudes. Second, recent evidence suggests that ethics instructors do not behave any morally better than do other professors (Rust and

Schwitzgebel forthcoming; Schwitzgebel and Rust forthcoming), and it is somewhat odd to suppose that ethics instruction would have a substantial effect on student behavior if it has no corresponding effect on instructor behavior. Third, any positive influences classroom ethics instruction might be partly or entirely counteracted by negative influences. Potential negative influences include: enhancing students' tendency to concoct superficial rationalizations that license attractive misconduct (Rust and Schwitzgebel forthcoming; Schwitzgebel and Rust forthcoming); undercutting students' intuitive sense of what is right, leaving them cynical or at sea (Baier 1985; Williams 1985); or giving students the impression that severe ethical infractions often go unpunished, making the moderate infractions to which they are tempted seem excusable or even their due.

I leave it as an open question what justifies teaching ethics at the university level, for example to business students. However, *if* part of the justification is an assumed positive influence on students' moral behavior after graduation, we should be aware that that assumption is dubious and without good empirical support. ^[5]

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Appendix: Charitable giving of philosophy majors at University of Zurich and University of Washington.

In the Frey and Meier data from University of Zurich 1999-2004, philosophy majors were statistically more likely than other students to donate to at least one charity in their first semester:

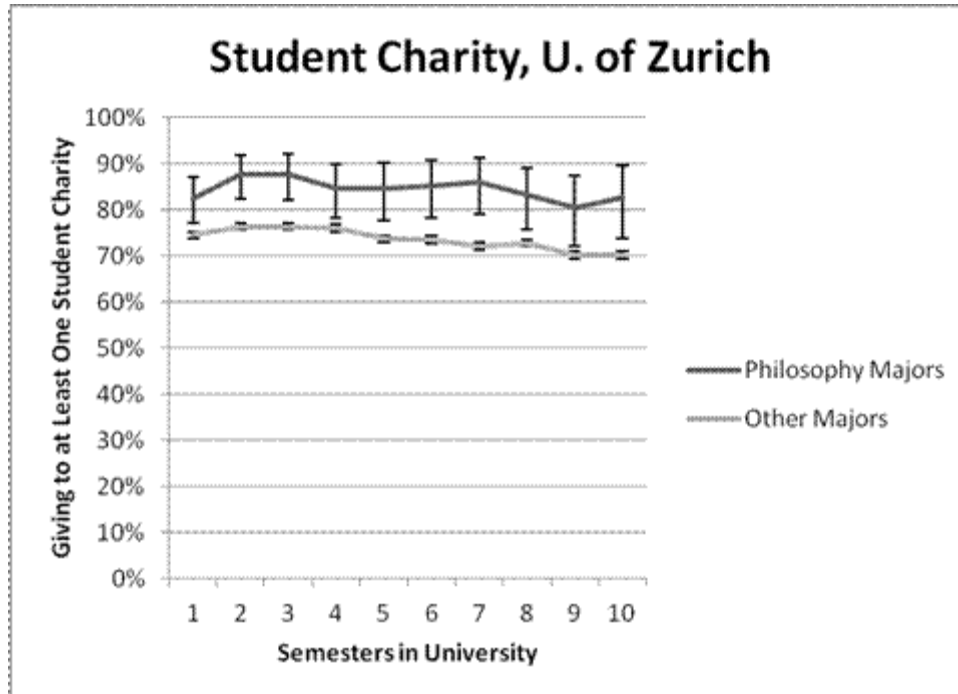
82% vs. 75%.^[6] That difference continued throughout schooling. For example, by the eighth

semester, it was 83% vs. 73%.^[7] Given the huge dataset, the non-philosophy majors' 2%

decline in donation rate from 75% in the 1st semester to 73% in the 8th semester was statistically significant. Although no such decline was detectable among philosophy majors, a 2% decline is well within the 95% confidence interval of the difference between philosophy majors' 1st and 8th

semester charity rates.^[8] This lack of a statistically detectable difference in rate of change is confirmed by a logistic regression predicting donation from number of semesters of schooling (excluding students with over 10 semesters), philosophy major, and the interaction of semesters*philosophy.^[9] (An interaction effect would indicate a different relationship between amount of schooling and donation rates among philosophy majors than among non-philosophy majors.) See Figure 1.

Figure 1: Percent of students giving to at least one University of Zurich student charity, by semesters of university education, philosophy majors vs. all other majors. Error bars indicate one-proportion 95% confidence intervals.

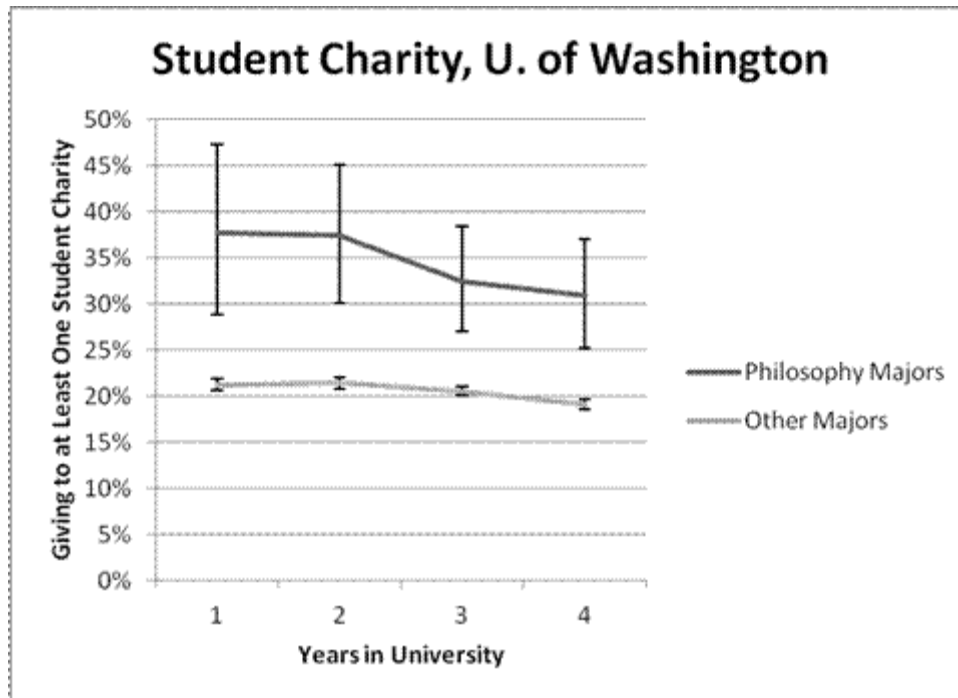


The Bauman and Rose dataset, from University of Washington 1999-2002, confirms the Frey and Meier findings on a different student population. In their first year of university, philosophy

majors were statistically more likely than other students to give to at least one charity: 38% vs. 21%.^[10] And again, the difference continued through senior year, when the donation rate was 31% for the philosophy majors vs. 19% for the rest.^[11] As in the Frey and Meier, the dataset was sufficiently large that the 2% decline among non-majors was statistically significant.^[12] Also as in the Frey and Meyer, although there was no statistically detectable change in philosophy majors' donation rates, the 2% non-majors' decline was well within the 95% confidence interval of the difference in donation rates between philosophers' 1st and 4th years.^[13] Finally, just as in the Frey and Meyer, a logistic regression predicting donation to at least one charity from number of years in school, philosophy major, and the interaction of years*philosophy finds no evidence of an interaction effect.^[14] See Figure 2.

Thanks to both research groups for sharing their raw data with me.

Figure 2: Percent of students giving to at least one University of Washington student charity, by years of university education, philosophy majors vs. all other majors. Error bars indicate one-proportion 95% confidence intervals.



[1] Dividing the measure into two factors, she reports $t = 0.95$, $p = .35$, and $t = -0.04$, $p = .97$.

[2] For example, the control group had 37/56 (66%) low scores (0 or 1 on a scale of 0-3) both at the beginning and at the end of the term, while the treatment group went from 35/58 (60%) low scores to 29/58 (50%; $Z = 1.1$, $p = .26$, 95% CI for diff -7.7% to 28.3%). Alternatively, the data can be considered non-dichotomously via a general linear model, predicting score from dummy variables for group, time, and interaction of group*time. In this analysis, no predictor is statistically significant. The interaction predictor coefficient = 0.31 with a 95% CI from -0.29 to + 0.92.

[3] $t = 1.85$, $p = .07$.

[4] ANOVA, $F = 7.7$, $p < .01$.

[5] For helpful comments and criticism, thanks to Jon Haidt.

[6] 196/238 vs. 12594/16875, $Z = 3.1$, $p = .002$, 95% CI for diff +2.8 to + 12.6%. This dataset includes seven additional semesters' worth of data not reported in Frey and Meier 2003.

[7] 110/132 vs. 9065/12451, $Z = 3.2$, $p = .001$, 95% CI for diff +4.1% to +17.0%.

[8] $Z = -3.5$, $p < .001$, 95% for diff -0.8% to -2.8%; $Z = 0.2$, $p = .81$, 95% CI for diff -7.0% to +9.0%.

[9] Number of semesters: OR = .97, $Z = -16.1$, $p < .001$; philosophy major: OR = 1.83, $Z = 4.3$, $p < .001$; interaction: OR = 1.01, $Z = 0.5$, $p = .64$, 95% CI for interaction OR 0.96 to 1.06. Excluding students with more than 10 semesters, the correlation of semesters of schooling with a dummy of 1 for having donated is $r = -.02$ ($p = .36$) for philosophy majors and $r = -.04$ ($p < .001$) for other majors. (Students with many semesters of schooling are socially different from typical undergraduates and their time-charity relationship looks non-linear.)

[10] 43/114 vs. 3290/15497, $Z = 3.6$, $p < .001$, 95% CI for diff +7.6% to +25.4%.

[11] 77/249 vs. 4682/24574, $Z = 4.0$, $p < .001$, 95% CI + 6.1% to +17.6%.

[12] $Z = 5.3$, $p < .001$, 95% CI -1.4% to -3.0%.

[13] $Z = 1.3$, $p = .28$, 95% CI -17.4% to +2.8%. The correlation of years of schooling with a dummy of 1 for having donated is $r = -.06$ ($p = .11$) for philosophy majors and $r = -.02$ ($p < .001$) for other majors.

[14] Number of years: OR = .95, $Z = -6.1$, $p < .001$; philosophy major: OR = 2.43, $Z = 4.1$, $p < .001$; interaction: OR = .93, $Z = -0.9$, $p = .35$, 95% CI for OR 0.81 to 1.08.