Exposure to Ideology and Distributional Preferences*

Raymond Fisman† Columbia University

Shachar Kariv‡ UC Berkeley

Daniel Markovits§ Yale University

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Abstract

We study the impact of exposure to ideology on *distributional preferences* in the context of modified Dictator Games that vary the price of giving. We exploiting a natural experiment in education – random assignment to first-term instructors at the Yale Law School – in order to distinguish the self-selection into a discipline from the learning that education in this discipline provides. We find that subjects exposed to instructors that stress traditional economic ideas display a greater emphasis on efficiency (increasing total payoffs) relative to those exposed to instructors that stress ideas from the humanities, who emphasize equity (reducing differences in payoffs). Subjects exposed to economics instructors also display greater levels of indexical

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†Graduate School of Business, Columbia University, Uris 823, New York, NY 10027 (E-mail: rf250@columbia.edu, URL: http://www-1.gsb.columbia.edu/faculty/rfisman/).

‡Department of Economics, University of California, Berkeley, 508-1 Evans Hall # 3880, Berkeley, CA 94720 (E-mail: kariv@berkeley.edu, URL: http://emlab.berkeley.edu/~kariv/).

§Yale Law School, P.O. Box 208215, New Haven, CT 06520 (E-mail: daniel.markovits@yale.edu, URL: http://www.law.yale.edu/faculty/DMarkovits.htm).

selfishness (greater weight on own payoff) relative to those exposed to humanist instructors.

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1 Introduction

Many social and economic behaviors invoke other-regarding, or distributional, preferences. Moreover, these behaviors are highly heterogeneous across individuals and across societies. This has led economists to analyze the role of personal and social attributes in determining the distributional preferences that govern these behaviors. The relationship between these attributes and distributional preferences is important for both economic theory and practice, including, for example, for understanding how preferences for income redistribution and other policies differ across societies, as in Alesina and Glaeser (2004), or across individuals.

In this paper, we study the impact of exposure to ideology on distributional preferences by exploiting a *natural* experiment in education. Our subjects are J.D. students at Yale Law School (hereafter, YLS), who are all required to take a mandatory first-term curriculum but are randomly assigned to classes taught by different instructors. Although the courses taught in the first term are fixed by the institution, there is no designated syllabus, and the several instructors are left free (and indeed encouraged) to design their courses as they see fit. This produces substantial variation in the contents of what different students are taught both generally and, in particular, with respect to distributive questions.

More specifically, some instructors emphasize traditional economic ideas and champion an aggregative approach that evaluates laws and policies according to the incentives that they give people to deploy resources productively and efficiently. Other instructors emphasize ideas from the humanities – philosophy, history, and critical theory – that, as we explain in the next section, favor a more egalitarian view of the role of law. Still other instructors are eclectic in their views and so fall in between these poles. The practical effects of these differences on teaching can be substantial.

These differences, moreover, may be objectively identified by looking to instructors' educational backgrounds and professional affiliations. Faculty

with Ph.D. degrees in economics or humanistic disciplines may be assigned accordingly, and those with only J.D. degrees receive a neutral assignment or an assignment based on professional affiliations. Thus, YLS's first-term instructional program generates a natural experiment which isolates the influence of learning from any selection effect associated with choosing to study economics or humanities in the first place.

In order to study the effects of this exposure to ideology on distributional preferences, we conducted *laboratory* experiments with YLS students employing modified dictator games that vary endowments and the relative prices of giving that they face (Andreoni and Miller, 2002). These decision problems are presented using a graphical experimental interface that allows for the collection of a rich data set, as in Fisman, Kariv, and Markovits (2007) (hereafter, FKM). This methodology allows us to identify individual distributional preferences that may be related to individual characteristics.

Specifically, we study a dictator game in which a person $self$ allocates some endowment across $self$ and an *anonymous other* in any way he wishes such that $p_s\pi_s + p_o\pi_o = 1$ (without essential loss of generality, the endowment is normalized to 1) where π_s and π_o correspond to the payoffs to persons $self$ and *other*, respectively, and $p = p_o/p_s$ is the *relative price of giving*. This configuration creates *budget sets* over π_s and π_o that allow us clearly to identify the equity-efficiency tradeoffs that subjects make in their distributional preferences: *decreasing* the *expenditure share* (or budget share) of giving $p_o\pi_o$ when the relative price of giving p *increases* indicates distributional preferences weighted towards efficiency (increasing total payoffs), whereas *increasing* $p_o\pi_o$ when p *increases* indicates distributional preferences weighted towards equity (reducing differences in payoffs).

We consider two experimental treatments. The first treatment is identical to the (linear) two-person experiment of FKM. With a view to testing the robustness of our results, the second treatment was identical to the first with the exception that on each budget line the computer identified and labeled three allocations consistent with maximizing *prototypical* distributional preferences that accord equal weight to payoffs of $self$ and *other*: utilitarian preferences, the utility function $\log \pi_s + \log \pi_o$, and Rawlsian preferences. After taking part in the experiment, we had our subjects fill out a survey that had subjects provide information on such attributes as gender, political affiliation, and volunteering activities.

Our main results can be summarized as follows.

- In the first treatment, subjects exposed to economics instructors place a greater emphasis on efficiency relative to those exposed to humanist or neutral instructors, who emphasize equity. Subjects exposed to economics instructors also display greater levels of *indexical selfishness* (greater weight on the payoff for $self$) relative to those exposed to humanist instructors; those exposed to neutral instructors exhibit intermediate selfishness.

- In the second treatment, the labeled (prototypical) allocations were chosen more often, but the correlation between economics exposure and distributional preferences was unaffected (labeling also had no effect on the number of subjects whose full set of choices corresponded to prototypical distributional preferences). Additionally, we find that labeling allocations prompts subjects to apply the various views of fairness in a self-serving and selective manner.

We therefore conclude that exposure to ideology matters for distributional decisions, a finding that is all the more remarkable given the heterogeneity in our experimental outcomes, the comparative maturity of our subject pool, and the relatively brief imposed exposure (one semester) to different ideologies. Given the random assignment to classes we exploit (and the lack of correlation of assignment with any observables), the results cannot be driven by individual attributes such as educational background, gender, religion, political affiliation, among others.

The rest of this paper is organized as follows. Section 2 provides a discussion of closely related literature. Section 3 describes the experimental design. Section 4 provides the results from the first treatment and Section 5 from the second treatment. Section 6 contains some concluding remarks. In the interests of space, the paper also uses three data and technical online appendices for the interested reader. Full experimental instructions, including the computer program dialog windows and post-experiment surveys are reproduced in Appendix I. Appendix II provides individual-level information about our experimental subjects. Appendix III examines individual subject-level decision-making by replicating the nonparametric and parametric analyses of FKM.

2 Related literature

The task of identifying the origins of distributional preferences is an old problem, though we argue that this study represents a substantial advance on earlier work in several ways. Prior studies have been especially concerned with the specific effect of studying economics on other-regarding behaviors. However, this line of research has faced two primary obstacles, which our experiments are designed to overcome:

- In field studies, it has proved difficult to distinguish clearly between selection and treatment, that is, between the impact of students' self-selection into a discipline on the one hand, and on the other of the learning that education in this discipline provides.

- In laboratory settings, researchers have not confronted subjects with sufficiently rich choice scenarios to allow for a clear demarcation between distributive behaviors motivated by pure self-interest and by distributional preferences concerning efficiency versus equity.

Several studies try to address the selection versus treatment concern by looking at a difference in differences between freshmen and seniors across disciplines. However, due to the lack of random assignment, these studies cannot distinguish between different time trends in other-regarding behaviors between the two groups. Furthermore, by definition, these studies cannot measure the effect that education in one discipline would have had on those who elected to study other disciplines. One further difficulty with previous studies is that they are based on experiments that engage strategic concerns and reciprocity motivations that cloud the interpretation of behaviors as reflecting distributional preferences.

Perhaps due to such confounding factors, findings in this literature have been mixed. Rubinstein (2006) argues that economics students lean more towards profit maximization than students in other disciplines. Carter and Irons (1991) report that the effects of economics on behavior are present among freshman and do not increase among seniors and therefore conclude that "economists are born, not made." Other studies that broadly support this view include Frey and Meier (2003), and Gandal et al. (2006). By contrast, Frank et al. (1993, 1996) report that students "generally show a pronounced tendency toward more cooperative behavior with movement toward graduation, but this trend is conspicuously absent for economics majors" and

therefore concluded that a learning effect exists. Kagel et al. (1996) find no difference in the behavior of students recruited from economics and psychology courses, casting doubt on the selection effect (and also, of course, on any learning effect). We are aware of only a single other study that assesses the relationship between economics exposure and efficiency-equity tradeoffs – that of Fehr et al. (2006). However, this paper also conflates selection and treatment by comparing economics majors to others.

Our use of a natural experiment allows our results to be interpreted more directly as the effect of exposure to ideology. Further, in the laboratory, we restrict attention to dictator games and ignore the complications of strategic behavior and reciprocity motivations in response games in order to focus on behavior motivated by purely distributional preferences. More generally, our experimental methodology allows us to collect richer data about other-regarding behaviors than has heretofore been possible and thus to identify more convincingly individual distributional preferences that may then be related to individual characteristics.

Finally, this paper also contributes the large and growing body of work on distributional preferences. Experimental research has been very fruitful in both establishing the empirical reliability of distributional preferences and directing theoretical attention to such preferences. The vast body of research includes Loewenstein et al. (1989), Bolton (1991), Rabin (1993), Levine (1998), Fehr and Schmidt (1999), Bolton and Ockenfels (1998, 2000), Charness and Rabin (2002), Andreoni and Miller (2002), and Cox et al. (2008) among others. Camerer (2003) provides a comprehensive discussion of experimental and theoretical work in economics focusing on dictator, ultimatum and trust games.

3 Experimental designs and procedures

3.1 The field environment

We study the effects of exposure to different ideology using the *random* assignment to courses at YLS. YLS employs a mandatory first-term curriculum requiring that all incoming student take the same four courses: constitutional law, contracts, torts, and civil procedure. Each course is taught in several versions, by different instructors. Although the courses in the first term curriculum are fixed by the institution, there is no designated syllabus, and the

several instructors are free (and indeed encouraged) to design their syllabus as they see fit. This produces substantial variation in the contents of what different students are taught, both generally and, in particular, with respect to conceptions of the economic role of the law.

3.1.1 YLS instructors

We conducted all the experimental sessions in April 2007, with first, second, and third year students participating. During 2004-2006, when these students took their first-term courses, a total of 16 instructors taught contracts and torts (all but one of these taught students in our sample), the courses on which we focus on below. These are the courses with more substantive economic content, as reflected in the fact that there are no instructors with an economic background or professional affiliation teaching in the remaining elements of the first term curriculum: constitutional law and civil procedure.

In addition to the commonly recognized concern that economists emphasize self-interestedness, there are substantive differences across disciplines in emphasizing efficiency versus equity. For example, in a contracts course, economists will tend to emphasize commercial relations among firms and efficiency of contract design. By contrast, philosophers or critical theorists will emphasize contractual relations that involve individuals, such as consumer contracts or even employment and marriage and, moreover, focus more consistently on equity among contract participants. Similarly, economists will emphasize the role of torts in making possible the efficiency gains of the marketplace, measuring costs and benefits by reference exclusively to the narrow self-interests of the persons whom the policies being assessed will affect. Philosophers, on the other hand, are more likely to treat torts as elaborating an individual ethic of care and responsibility, while historians might approach tort law through the lens of the distributive conflict between capital and labor during and after the industrial revolution. Again, economists emphasize efficiency whereas humanists in various ways emphasize equity.

To classify individual instructors, we make use of objective criteria gathered from descriptions on the faculty information pages on the web sites of their respective law schools. Most instructors were affiliated only with YLS although several also had other affiliations. For those with multiple affiliations, all listings were consulted. The primary classificatory standard looks to instructors' graduate educations. Those whose only graduate degree is a J.D. are recorded as *neutral*, while those who hold a graduate degree in

either economics or a humanity are assigned to the category corresponding to their highest non-law degree. Table 1 classifies the contracts and torts instructors that taught the subjects in our sample, and lists the total enrollment in 2004-2006 academic years and the number of first, second, and third year subjects in their classes. The bimodal distribution in class size is simply a reflection of YLS's policy of having each student take three of the four first-term courses as a large lecture (with enrollments of roughly 60) and one as a small seminar (with an enrollment of roughly 16).

[Table 1 here]

Of the instructors listed in Table 1, there are four exceptions to classificatory rule above. Alan Schwartz holds a J.D. only but his faculty listing reports that he has been President of the American Law and Economics Association and editor of The Journal of Law, Economics and Organization. Accordingly, he is assigned to economics. Guido Calabresi also holds a J.D. only and his faculty listing reports no professional affiliations, but he is widely thought of as one of the founders of Law and Economics. Accordingly, he is assigned to economics. Robert Gordon holds a J.D. only but his faculty listing reports that he holds a chair in legal history. Accordingly, he is assigned to humanities. Finally, Carol Rose holds a Ph.D. in history but her faculty listing reports that she is a member of the American Law and Economics Association. Accordingly, she is assigned to neutral.

3.1.2 YLS students

The 67 subjects in the experiment were recruited from the entire YLS student body. Only a single subject did not list his first term instructors. Throughout the paper, whenever we list the number and percentages of subjects with particular properties, we will be considering the remaining 66 subjects. Of those, 30 subjects are first-year students (45.5 percent), 23 subjects are second-year students (43.9 percent), and the remaining subjects are third-year students.

For each experimental subject, we use the information on contracts and torts instructors summarized Table 1 above to generate an aggregate measure of his relative exposure to economic and humanist ideologies. To this end, for each subject, we define the variable ***econ*** (economics) as follows:

> ***econ*** $= 1$ *(resp.* ***econ*** $= 0$*) if the subject was taught by at least one economist and no humanist (resp. at least one humanist and no*

economist), and $\boldsymbol{econ} = 0.5$ *if the subject was taught by neither an economist nor a humanist, or by one of each.*

Defining the measure ***econ*** to take on these values will facilitate the interpretation of regression coefficients, and in later robustness tests, we will explore in greater depth the validity of our functional form assumptions. Intuitively, the variable ***econ*** captures whether a subject has exposure to humanist ideology with no counteracting economics exposure, exposure to neither ideology or counteracting exposure to both, or exposure to economics with no counteracting humanist exposure.

The variable ***econ*** is fairly evenly distributed across its three values: 19 subjects (28.7 percent) had at least one humanist and no economist ($\boldsymbol{econ} = 0$), 21 subjects (31.8 percent) had least one economist and no humanist ($\boldsymbol{econ} = 1$), and 15 subjects (22.7 percent) had two neutral instructors and 11 subjects (16.7 percent) had one each from humanistic disciplines and economics ($\boldsymbol{econ} = 0.5$). Throughout the rest of the paper, we refer to economics, humanist, and neutral subjects according to their ***econ*** classification.[1]

Finally, while the assignment to first-term courses is random by construction, we nonetheless wish to assess the validity of our treatment by testing whether it is correlated with other individual attributes that could affect distributional preferences. To this end, we had our subjects fill out a survey (see Appendix I) after taking part in the experiment that provided information on such attributes as gender, political affiliation, volunteering activities, among others. Table 2 below reports the results of a set of binary regressions that examine the extent to which the variable ***econ*** is correlated with subjects' individual characteristics. The last column of Table 2 provides summary statistics on the subject level characteristics.

[Table 2 here]

The results, reported in Table 2, show that in no case was the correlation with the variable ***econ*** significant. We therefore conclude that the first-term instructional program at YLS generates a *natural* experiment concerning the influence of exposure to economic ideology on other-regarding behaviors in

[1]The distribution of our subjects' exposure to economics closely mirror overall enrollment at YLS. For contracts, we oversampled those taught by humanities instructors relative to economics instructors. This is reversed for torts.

the laboratory.[2] This obviates the familiar and otherwise intractable problem of self-selection into economics (and other courses of study): random assignment effectively requires some students, but not others, to study economics or a humanity, without concern for their prior sympathies.

3.2 The lab environment

We conducted four experimental sessions at one of the computer labs at YLS, using experimental procedures identical to those used by FKM.[3] After subjects read the instructions (reproduced in Appendix I), the instructions were read aloud by an experimenter. No subject reported any difficulty understanding the procedures or using the computer program. Throughout the experiment we ensured anonymity and effective isolation of subjects in order to minimize any interpersonal influences that could stimulate other-regarding behavior. Each experimental session lasted for about one and a half hours. A $10 participation fee and subsequent earnings, which averaged about $34, were paid in private at the end of the session.

The experiment consisted of two treatments. Each experimental treatment consisted of fifty independent decision problems. In each decision problem, each subject was asked to allocate tokens between $self$ (π_s) and an unknown *other* (π_o), chosen at random from the group of subjects in the experiment. Each choice involved choosing a point on a graph representing a budget line over possible token allocations. Each decision problem started by having the computer select a budget line randomly from the set of budget lines that intersect with at least one of the axes at 50 or more tokens, but with no intercept exceeding 100 tokens. Payoffs were calculated in terms of tokens and then translated at the end of the experiment into dollars. Each token was worth 25 cents.

Choices were made by using the computer mouse or the keyboard arrows to move the pointer on the computer screen to the desired allocation (π_s, π_o) and then clicking or hitting the enter key (see FKM for an extended description of the experimental interface). The approach has a number of

[2] When these variables are included as covariates in the regressions below, our results are unchanged.

[3] It is of course possible that presenting choice problems graphically biases choice behavior in some particular way – and that is a useful topic for further research – but there is no evidence that this is the case: behavior elicited graphically by FKM is quite consistent with behavior elicited by other means (Camerer, 2003).

advantages over earlier methodologies. First, a choice made on a budget constraint provides more information about preferences than a typical binary choice. Second, because the interface is extremely user-friendly, it is possible to present each subject with *many* choices in the course of a single experimental session, yielding a much larger data set.

The first treatment in each experimental session was identical to the (linear) two-person experiment in FKM, with the exception that choices were restricted to allocations on the budget constraint.[4] The second treatment was identical to the first with the exception that on each budget line the computer identified three allocations that reflect alternative prototypical conceptions of putting *equal* weight on the payout to $self$ and $other$. These allocations were labeled and described in the experimental instructions as follows:

□ This allocation always lies at the endpoint of the line segment that is farthest from the origin. This maximizes the sum of payouts.

\+ This allocation always lies at the midpoint of the line segment. The allocation gives you and the other person each half of your maximum feasible payout.

O This allocation always lies on the 45 degree line. The payouts are the same to yourself and to the other person.

These allocations are consistent with maximizing utilitarian preferences (with respect to money), the utility function $\log \pi_s + \log \pi_o$, and Rawlsian preferences (with respect to money), respectively. Subjects received this set of instructions only at the end of the first part of the experiment, and there was no feedback between the two parts of the experiment.

At the end of the experiment, payoffs were determined as follows. The experimental program first randomly selected one decision round from each treatment to carry out. Each round had an equal probability of being chosen. Each subject then received the tokens that he held in that round π_s, and the subject with whom he was matched received the tokens that he passed π_o. Thus, as in Andreoni and Miller (2002), in each treatment, every subject received two groups of tokens, one based on his own decision to hold tokens and one based on the decision of another random subject to pass tokens.

[4] In FKM, choices were *not* restricted to allocations on the budget constraint, but very few subjects chose interior allocations $p_s\pi_s + p_o\pi_o < 1$.

The computer program ensured that the same two subjects were not paired twice. That is, for any pair of subjects i and j, if i passed tokens to j, then i did not also receive tokens from j so the same two subjects were not paired as $self$-$other$ and $other$-$self$.

4 Treatment I

4.1 Data description

We first provide an overview of some important features of the experimental data as a prelude to our econometric analysis. The data generated by each choice are $(\pi_s, \pi_o, \bar{\pi}_s, \bar{\pi}_o)$, where (π_s, π_o) are the coordinates of the choice made by the subject and $(\bar{\pi}_s, \bar{\pi}_o)$ are the endpoints of the budget line (so we can calculate the relative price of giving $p = \bar{\pi}_s/\bar{\pi}_o$ for each observation). Figure 1 reports the distributions of the expenditure on tokens given to *other*, $p_o\pi_o$ (the subject's endowment is normalized to 1 so $p_o = 1/\bar{\pi}_o$) for economics ($\boldsymbol{econ} = 1$), neutral ($\boldsymbol{econ} = 0.5$), and humanist ($\boldsymbol{econ} = 0$) subjects, and compare them with the analogous distribution reported in FKM. The horizontal axis identifies the fractions for different intervals and the vertical axis reports the percentage of decisions corresponding to each interval. Note that we divide the bottom and top deciles in half because of the very sharp difference in the number of allocations within these deciles. The distribution of allocations for economics subjects is skewed to the left, indicating greater self-interestedness. However, economics subjects also allocated all their tokens to *other* ($p_o\pi_o \geq 0.95$) with relatively high frequency, hinting at the presence of efficiency concerns.

[Figure 1 here]

Figure 2 below shows the decision-level distributions in Figure 1 disaggregated into three price-ratio terciles: intermediate prices of around 1 ($0.79 \leq p \leq 1.26$), steep prices ($p > 1.26$) and symmetric flat prices ($p < 0.79$). Focusing in particular on the purely selfish allocations ($p_o\pi_o \leq 0.05$) by price tercile, the difference between the fraction of selfish allocations selected on flat versus steep budget lines is relatively small for humanist subjects (0.35 versus 0.42), as compared to the difference for economics subjects (0.54 versus 0.75). For neutral subjects, this difference is closer to that of humanist subjects (0.50 versus 0.56). At the other extreme, economics subjects more

often allocated all their tokens to *other* ($p_o\pi_o \geq 0.95$) when confronted with flat budget lines than neutral or humanist subjects (0.11 versus 0.03 and 0.08, respectively). Perhaps as expected, there are almost no such allocations on intermediate or steep budget lines. Finally, the large fraction of allocation with near-equal expenditures on tokens kept and given ($0.45 \leq p_o\pi_o \leq 0.55$) by humanist subjects relative to neutral and economics subjects (0.12 versus 0.07 and 0.05, respectively) over all price-ratios hints at the emphasis on equality.

[Figure 2 here]

The decision-level distributions in Figure 1 potentially obscure the presence of individual concerns *on average* for others. For example, a subject who gives everything to *other* half of the time and keeps everything for $self$ the other half would generate extreme giving values, when in fact such a person keeps an intermediate fraction on average. Hence, Figure 3 reports the distributions presented in Figure 1 aggregated to the subject level. The horizontal axis identifies the fractions for different intervals and the vertical axis reports the percentage of subjects corresponding to each interval. Perhaps as expected, the distributions all have a larger mode around the midpoint and no observations below the midpoint. More interestingly, the distribution is more skewed to the left for economics subjects, indicating a greater number of purely selfish and near-selfish subjects. By contrast, the distribution of humanist subjects has a larger mode around the midpoint of 0.5, indicating that $self$ and *other* are treated more symmetrically by humanist subjects.

[Figure 3 here]

Table 3 reports the mean and standard deviation of the expenditure on tokens given to *other* $p_o\pi_o$ for economics, neutral and humanist subjects. We present the statistics for *all* allocations, as well as for each price tercile. Table 3A presents the data for the full sample and Table 3B presents the statistics after screening the data for selfish subjects (those who chose selfish allocations $p_o\pi_o \leq 0.05$ in 45 or more decision rounds). Overall, YLS subjects allocated approximately 19 percent of tokens to *other*, accounting for 19 percent of total expenditure, which is very similar to 19 percent and 21 percent, respectively, in the two-person budget sets experiment reported of FKM. (The distinction between token share $\pi_o/(\pi_s + \pi_o)$ and expenditure

share $p_o\pi_o$ is only relevant in the presence of price changes.) In the studies of standard split-the-pie two-person dictator games reported in Camerer (2003), the typical mean allocations are of about 20 percent. More interestingly, over all price terciles, the mean expenditure on tokens given to *other* is smaller for economics and neutral subjects than for humanist subjects.

[Table 3 here]

Finally, the aggregate statistics above tell us little about the particular allocations chosen by individual subjects. In select cases, it is possible readily to identify subjects whose choices correspond to prototypical distributional preferences. Of our 66 subjects, 22 (33.3 percent) behaved perfectly selfishly. There are relatively modest differences in the prevalence of purely selfish preferences across economics, humanist and neutral subjects – nine (42.9 percent), seven (26.9 percent), and six (31.6 percent) subjects, respectively. By comparison, FKM report that, 20 subjects (26.3 percent) behaved perfectly selfishly. A single subject made nearly equal allocations $\pi_s = \pi_o$ indicating Rawlsian preferences. No subject allocated all his tokens to $self$ when $p_s < p_o$ and to *other* when $p_s > p_o$ (utilitarian preferences), and no subject made equal expenditures on $self$ and *other* $p_s\pi_s = p_o\pi_o$ (logarithmic preferences). FKM similarly report that very few subjects made allocations that fit with non-selfish prototypical distributional preferences. These are of course special cases, where the regularities in the data are very clear. In Appendix III, we depict, for each subject, the relationship between the log-price ratio $\ln(p)$ and the token share $\pi_o/(\pi_s + \pi_o)$ as points in a scatterplot. These graphs provide information about the particular allocations chosen by an individual subject and how they vary with changes in relative prices.

4.2 Econometric analyses

We next turn to regression analyses that examine the patterns in the data more systematically. Let $\left\{(p^t_{s,i}, p^t_{o,i}, \pi^t_{s,i}, \pi^t_{o,i})\right\}_{t=1}^{50}$ be the data generated by subject i's choices, where $(p^t_{s,i}, p^t_{o,i})$ denotes the t-th observation of the price vector and $(\pi^t_{s,i}, \pi^t_{o,i})$ denotes the associated allocation. Our main econometric specification has an expenditure function of the form:

$$p^t_{o,i}\pi^t_{o,i} = \beta_1 + \beta_2 \boldsymbol{econ}_i + [\gamma_1 + \gamma_2 \boldsymbol{econ}_i]\log(p^t_i) + \epsilon^t_i \qquad (1)$$

where $p^t_i = p^t_{o,i}/p^t_{s,i}$ and ϵ^t_i is assumed to be distributed normally with mean zero and variance σ^2_n. Note that expenditure is estimated as budget shares,

which are bounded between zero and one, with an $i.i.d.$ error term. We generate estimates of the β and γ coefficients using a Tobit model that accounts for the censored distribution, and use robust standard errors that allow for clustering at the level of the individual subject i.[5]

The β coefficients represent the *indexical* weight on $self$ versus *other* payoffs, whereas the γ coefficients parameterize attitudes towards the efficiency-equity tradeoff between $self$ and *other*. That is, $\gamma < 0$ indicates distributional preferences weighted towards efficiency (increasing total payoffs), whereas $\gamma > 0$ indicates distributional preferences weighted towards equity (reducing differences in payoffs). So, when $\gamma = 0$, expenditure shares are insensitive to the price of giving. For any $\gamma < 0$, the fraction of expenditure $p_o\pi_o$ *decreases* as $\log(p)$ increases (negative price responsiveness of giving), indicating concerns for increasing total payoffs rather than reducing differences in payoffs. In contrast, for any $\gamma > 0$, the fraction of expenditure $p_o\pi_o$ *increases* as $\log(p)$ increases (positive price responsiveness of giving), indicating concerns for reducing differences in payoffs rather than increasing total payoffs.

Since the price ratio is a log expression, we can interpret γ_1 as the percentage point change in $p_o\pi_o$ that results from a one percent increase in p for humanist subjects, and γ_2 captures how this sensitivity differs by economics exposure. Hence, the configuration of budget sets over π_s and π_o allows us to capture *simultaneously* both differences in indexical selfishness (as captured by β_2) emphasized in earlier research, as well as differences in efficiency-equity tradeoffs (as captured by γ_2). This is a crucial distinction that has eluded earlier work, and one that we are able to address using our experimental design and the rich dataset generated as a result. Table 4 presents the estimation results.

[Table 4 here]

In column (1), we omit the interaction term $\boldsymbol{econ}_i \log(p_i^t)$ to focus on the impact of exposure to economics on indexical selfishness. The coefficient

[5] There is plausibly some non-independence of observations across subjects exposed to the same instructor. Ideally, we would allow for two-way clustering on contract instructor and torts instructor separately, employing a method such as that of Cameron et al. (2008). However, given the relatively small number of instructors of each type, their approach is infeasible. When we cluster on professor-pairs (yielding a total of 21 clusters), the standard errors in our analyses are virtually unchanged.

$\hat{\beta}_2$ is equal to -0.212 and is significant at the 5 percent level implying that, on average, economics subjects ($\boldsymbol{econ} = 1$) spent about 21 percent less on the tokens given to *other* as compared to humanist subjects ($\boldsymbol{econ} = 0$). The coefficient $\hat{\gamma}_1$ is equal to -0.105 and is significant at the 1 percent level, implying that, overall, YLS subjects are concerned with increasing aggregate payoffs rather than reducing differences in payoffs. This is consistent with the results of FKM and Charness and Rabin (2002) that also lean overall toward a social welfare conception of distributional preferences.

In column (2), we add the interaction term $\boldsymbol{econ}_i \log(p_i^t)$. Its coefficient $\hat{\gamma}_2$ is equal to -0.143 and is significant at the 5 percent level, implying a greater weight on efficiency relative to equity among economics subjects relative to humanist subjects. The magnitude is very large, implying that moving from the 25-th percentile of the relative price of giving p to the 75-th percentile (a shift in relative prices from approximately 0.68 to 1.47) results in a differential increase in the fraction of expenditure on tokens given to *other* of just over 10 percent for economics relative to humanist subjects. This is roughly consistent with the effect implied by the summary statistics presented in Table 3 above. We also observe that $\hat{\gamma}_1$, which measures the direct effect of the log price ratio is indistinguishable from zero, implying that the expenditure share is largely invariant to the relative price of giving for humanist subjects.

In columns (3) and (4), we repeat the estimations reported in columns (1) and (2), after screening out the 22 subjects (33.3 percent) with purely selfish allocations. The choices of these subjects are of course insensitive to the relative price of giving. To the extent that being purely selfish is correlated with exposure to economics, as suggested by the results in column (1), this may bias our estimates of the effect of economics on the efficiency-equity tradeoff, $\hat{\gamma}_1$ and $\hat{\gamma}_2$. We find that on this reduced sample, the corresponding estimates are of comparable magnitude and in both cases still significant at the 5 percent level. We therefore conclude that, even after screening the data for selfish subjects, economics subjects place more indexical weight on $self$ versus $others$ payoff and also display a more pronounced emphasis on increasing aggregate payoffs of $self$ and *other* rather than reducing the differences in payoffs between $self$ and *other*.

We also allow for greater flexibility in functional form, including indicator variables $\chi^{\boldsymbol{n}}$ and $\chi^{\boldsymbol{e}}$ for neutral ($\boldsymbol{econ} = 0.5$) and economics ($\boldsymbol{econ} = 1$) subjects, respectively. This generates an econometric specification that has

an expenditure function of the form:

$$p_{o,i}^t \pi_{o,i}^t = \beta_1 + \beta_2 \chi_i^{\boldsymbol{n}} + \beta_3 \chi_i^{\boldsymbol{e}} + [\gamma_1 + \gamma_2 \chi_i^{\boldsymbol{n}} + \gamma_3 \chi_i^{\boldsymbol{e}}] \log(p_i^t) + \epsilon_i^t. \qquad (2)$$

where ϵ_i^t is assumed to be distributed normally with mean zero and variance σ_n^2. We a again generate estimates of the β and γ coefficients using a Tobit model. Table 5 below reports the estimation results. In column (1), we omit the interaction terms $[\chi_i^{\boldsymbol{n}} + \chi_i^{\boldsymbol{e}}] \log(p_i^t)$. The results indicate that there is a monotonic, approximately linear relationship between exposure to economics and the expenditure on the tokens given to *other*. This suggests that there is both a "positive" effect of economics instructor, and also a "negating" effect of humanist instructor. In column (2), we add the interaction terms and find that the coefficient $\hat{\gamma}_2$ is negative, but indistinguishable from zero. so the effect on price responsiveness comes primarily from economics subjects. In columns (3) and (4), we repeat the estimations reported in columns (1) and (2) and report similar results on the reduced sample of non-selfish subjects.

[Table 5 here]

The data generated by the experiments can also be used to address a variety of important and interesting questions about *individual* behavior. One aspect of the rich data sets generated by the experiments is that they allow us to analyze behavior at the level of individual subjects, testing for the consistency of their choices and identifying their underlying structure. To economize on space, the individual level analysis is provided in Appendix III, where we report the estimates of individual-level constant elasticity of substitution (CES) demand functions for giving.[6]

5 Treatment II

With a view to testing the robustness of our results from the first treatment, the second treatment was identical to the first with the exception that on each budget line the three allocations consistent with utilitarian, logarithmic, and Rawlsian preferences were identified and labeled. The descriptions for each of these labeled allocations in the experimental instructions are given in the experimental design section. The second treatment allows us to probe the

[6] Andreoni and Miller (2002), Cox et al. (2007), and FKM among others successfully utilized the CES form to recover distributional preferences.

robustness of our results to framing effects. For example, it may be that only a small push is needed to have subjects *learn* their preference for efficiency (or equity), and that a brief explication of the fairness implications of each allocation could erase the reported differences among subjects based on their exposure to economics instructors. The second treatment may also help to allay concerns that students taught by economics-oriented instructors are better able to recognize the allocative decisions that reflect efficiency rather than having a greater underlying preference for efficiency.

One obvious effect of labeling such allocations is that they are potentially chosen with greater frequency. For each treatment, Table 6 below lists the fraction of decisions corresponding to each of these labelled allocations over economics ($\boldsymbol{econ} = 1$), neutral ($\boldsymbol{econ} = 0.5$) and humanist ($\boldsymbol{econ} = 0$) subjects. Further, in each case we present the data for all allocations, as well as the data by price tercile. Perhaps as expected, allocations that are consistent with utilitarian, logarithmic, and Rawlsian preferences were chosen more in the second treatment – 53.2 percent of all allocations compared to only 39.2 percent in the first treatment. Nevertheless, this had no effect on the fraction of subjects whose choices correspond to prototypical distributional preferences. In both treatments, 22 subjects (33.3 percent) behaved perfectly selfishly. Of those, 20 subjects behaved perfectly selfishly in both treatments. One subject made nearly equal allocations in both treatments indicating Rawlsian preferences. No subject fit with utilitarian or logarithmic preferences in either treatment.

[Table 6 here]

More interestingly, there are large differences between flat and steep budget lines in the change in the frequency with which labeled allocations were chosen. The frequency of allocations consistent with logarithmic preferences (the midpoint of the budget line) increased on both flat and steep budget lines. For allocations consistent with utilitarian preferences, the increase is concentrated on steep budget lines, when this choice coincides with the purely selfish allocation – possibly the labeling of this allocation allows subjects to apply the utilitarian view of fairness in a self-serving and selective manner. On the other hand, the increase in equal allocations consistent with Rawlsian preferences is entirely concentrated on flat budget lines where such choices are relatively less costly - again, this may be a self-serving application of the Rawlsian view of fairness. Finally, we note that there is a greater effect of labeling on utilitarian allocations for economics and neutral subjects;

by contrast, the effect of labeling on the Rawlsian allocation is primarily found in humanist subjects. This is consistent with the utilitarian label having greater resonance with economics subjects, and the Rawlsian label being more compelling for humanities subjects. But in both cases there is selective and self-serving application of these notions of fairness.

Table 7 report the regression analog to Table 6 in a set of specifications that pool the data from the two treatments. We define an indicator variable λ that indicates choices that were made in the second treatment where the allocations were labeled. In column (1) we present a Logit specification with an indicator variable for allocations consistent with utilitarian preferences as the outcome. We include the interaction term $\lambda \boldsymbol{econ}_i$ to capture the extent to which economics subjects differentially increased their choices of utilitarian allocations as a result of labeling. This interaction term is positive, but not statistically significant. In columns (2) and (3) we divide the sample into choices on budget lines where the log-price ratio is bigger and smaller than zero, respectively. We find that $\lambda \boldsymbol{econ}_i$ is positive and significant at the 5 percent level for $\log(p) > 0$, but very close to zero (and slightly negative) for $\log(p) < 0$. In columns (4), (5), and (6) we report analogous results with an indictor variable for equal allocations consistent with Rawlsian preferences as the outcome. In this case, we do not find that the coefficient on $\lambda \boldsymbol{econ}_i$ is significant in any specification. However, the signs of the coefficients on this interaction term are consistent with an increase in Rawlsian allocations by humanist subjects, but only on budget lines where $\log(p) < 0$.

[Table 7 here]

Finally, Table 8 assesses whether our main estimation results reported in Table 4 above are affected by labeling allocations. In column (1) we include just the direct effect of labeling λ and also the interaction term $\lambda \log(p_i^t)$. The direct effect is significant at the 5 percent level, and implies a three percentage point decrease in $p_o \pi_o$ in the second treatment. Additionally, the interaction term is borderline significant. Both reflect the pattern described above, that on budget lines where $\log(p) > 0$ subjects are more likely to choose the selfish allocation in the labeled treatment, which is also the allocation consistent with utilitarian preferences. In column (2) we include the interaction term $\lambda \boldsymbol{econ}_i$. Its coefficient is positive and significant at the 10 percent level, reflecting that the increase in selfish allocations on budget lines where $\log(p) > 0$ was concentrated among economics subjects. Finally,

in column (3) we add the three-way interaction term $\lambda \boldsymbol{econ}_i \log(p_i^t)$ but its coefficient is indistinguishable from zero.

[Table 8 here]

6 Conclusion

We conclude by re-emphasizing the main findings from our experiment. First, our experiments provide stronger evidence than any previous work that distributional preferences are *learned*. They are not fixed by deep and immutable background factors but rather evolve, including in mature adults, in response to more recent and superficial stimuli. Moreover this evolution can be characterized not just causally but normatively, in the sense that the stimuli to which students' distributional preferences respond include (among other things, to be sure) normative arguments in favor of the preferences that the persons eventually adopt.

Second, and equally important, our experiments yield insights into what students learn from instructors who emphasize different ideological content. Crucially, the difference between economic and humanist ideology is not just that economics leads students to become more selfish, although this is one of our findings. Rather than identifying a simple difference between immoral economics and moral humanities, our experiments reveal that an important part of the difference between these disciplines is that they instill competing conceptions of impartiality in those who study them: students exposed to economics are relatively more likely to adopt distributive preferences that emphasize efficiency, whereas students exposed to the humanities are relatively more likely to adopt distributive preferences that emphasize equity.

Finally, our results also suggest a number of potential directions for future work. Our experiments document a relatively short-run impact of ideological exposure. One line of future research should therefore investigate the longevity of the effects that we have measured. (Although our subject pool included third- and second- and well as first-year law students, our sample size was too small to examine whether the effects of first-term instruction fade as students progress through law school.) Another line of research should investigate the internal dynamics of ideological learning. That is, to what extent does the transmission of ideological content from instructor to student depend on the instructor's simply stating ideas, and to what extent does it depend on her expressly approving of or promoting them.

Our study has practical as well as theoretical implications. Students graduate into a world that allows them opportunities to implement their distributional preferences. This is especially true for the students in our sample, many of whom will assume positions of substantial power in national and indeed global economic and political affairs. (Both the President and his main contender for the Democratic Presidential nomination, now Secretary of State, attended elite law schools, and the latter attended YLS.) Insofar as what students are taught influences how students will deploy their power, both individual instructors and the institutions where they work have good reason to reconsider what, and how, they teach.

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Table 1. Instructors' information

	Instructor	Classification	Highest degree	Enrollment			# of obs.			Total
				04	05	06	04	05	06	# of obs.
Torts	G. Calabresi	Economics	J.D., M.A. (Politics, Philosophy and Economics)	66	87	70	7	7	8	22
	J. Coleman	Philosophy	Ph.D. (Philosophy)	--	--	64	--	--	13	13
	J. Donohue	Economics	J.D., Ph.D. (Economics)	63	16	16	1	2	2	6
	D. Kysar	Neutral	J.D.	--	84	--	--	13	--	13
	P. Schuck	Neutral	J.D.	63	17	48	5	1	6	12
Contracts	I. Ayres	Economics	J.D., Ph.D. (Economics)	15	17	16	0	1	2	3
	L. Brillmayer	Neutral	J.D.	--	50	52	--	4	6	10
	R. Brooks	Economics	J.D., Ph.D. (Economics)	65	--	--	1	--	--	1
	S. Carter	Neutral	J.D.	16	53	16	2	7	1	10
	A. Chua	Neutral	J.D.	--	--	16	0	4	5	9
	R. Gordon	History	J.D.	16	51	16	0	1	3	4
	H. Hansmann	Economics	J.D., Ph.D. (Economics)	--	--	16	--	--	3	3
	D. Markovits	Philosophy	J.D., Ph.D. (Philosophy)	16	17	16	6	6	7	19
	C. Rose	Neutral	J.D., Ph.D. (History)	63	--	--	4	--	--	4
	A. Schwartz	Economics	J.D.	--	--	33	--	--	3	3

For more information, see YLS faculty web page at http://www.law.yale.edu/faculty/faculty.htm. Subjects' individual characteristics are based on self reporting in post-experiment surveys. The surveys are reproduced in Appendix I. Appendix II provides individual-level information about our experimental subjects.

Table 2. The correlation between ***econ*** and subjects' individual characteristics

	Coefficient	Mean
Economics	0.100	0.152
	(0.135)	(0.361)
Only child	0.075	0.091
	(0.169)	(0.290)
Religious	-0.072	0.614
	(0.100)	(0.487)
Male	-0.122	0.520
	(0.096)	(0.510)
Log(Age)	-0.0070	3.217
	(0.100)	(0.087)

In the first column, each coefficient is the result of a binary regression, where ***econ*** is the dependent variable. The second column presents the mean and standard deviation of each individual attribute. Economics is an indicator variable denoting undergraduate major in economics or finance. Religious is an indicator variable denoting being raised in a religious home. Subjects' individual characteristics are based on self reporting in post-experiment surveys. Standard errors in parentheses. *, **, *** indicate 10, 5, 1 percent significance levels, respectively. Appendix II provides more refined information about our experimental subjects.

Table 3. The mean expenditure share on the tokens given to *other*

A. Full sample

		Economics (***econ*** =1)	Neutral (***econ*** =0.5)	Humanist (***econ*** =0)
Price-ratios	All	0.143 (0.252)	0.265 (0.270)	0.167 (0.232)
	Steep	0.089 (0.180)	0.244 (0.257)	0.145 (0.211)
	Intermediate	0.112 (0.185)	0.246 (0.213)	0.152 (0.193)
	Flat	0.227 (0.335)	0.301 (0.322)	0.207 (0.282)
	# of obs.	1050	1300	950

B. Non-selfish subjects

		Economics (***econ*** =1)	Neutral (***econ*** =0.5)	Humanist (***econ*** =0)
Price-ratios	All	0.376 (0.239)	0.228 (0.244)	0.241 (0.283)
	Steep	0.355 (0.238)	0.197 (0.224)	0.153 (0.215)
	Intermediate	0.355 (0.163)	0.208 (0.198)	0.193 (0.210)
	Flat	0.414 (0.291)	0.286 (0.295)	0.378 (0.352)
	# of obs.	650	950	600

Standard deviation in parentheses. Selfish subjects are those who chose selfish allocations in 45 or more decision rounds.

Table 4. The impact of exposure to economic and humanist ideologies on the expenditure on tokens given to *other* (Eq. 1)

Dep variable: $p_o \pi_o$

	(1)	(2)	(3)	(4)
econ	-0.212**	-0.221**	-0.184***	-0.194***
	(0.103)	(0.103)	(0.069)	(0.071)
log(p)	-0.105***	-0.033	-0.107***	-0.019
	(0.028)	(0.045)	(0.032)	(0.053)
econ log(p)		-0.143**		-0.178**
		(0.071)		(0.083)
Selfish subjects	Yes	Yes	No	No
# of obs.	3300	3300	2200	2200

Tobit regressions of expenditure function (1) using robust standard errors that allow for clustering at the level of the individual subject. Selfish subjects chose selfish allocations in 45 or more decision rounds. Standard errors in parentheses. *, **, *** indicate 10, 5, 1 percent significance levels, respectively.

Table 5. The impact of exposure to economic and humanist ideologies on the expenditure on tokens given to *other* (Eq. 2)

Dep. variable: $p_o \pi_o$

	(1)	(2)	(3)	(4)
χ^n	-0.124	-0.126	-0.165***	-0.167***
	(0.096)	(0.095)	(0.0607)	(0.0599)
χ^e	-0.212**	-0.222**	-0.179***	-0.189***
	(0.102)	(0.103)	(0.0677)	(0.0699)
log(p)	-0.104***	-0.059	-0.105***	-0.0405
	(0.027)	(0.052)	(0.0311)	(0.0607)
χ^n log(p)		-0.007		-0.0366
		(0.061)		(0.0717)
χ^e log(p)		-0.143**		-0.176**
		(0.071)		(0.0820)
Selfish subjects	Yes	Yes	No	No
# of obs.	3300	3300	2200	2200

Tobit regressions of expenditure function (2) using robust standard errors that allow for clustering at the level of the individual subject. Selfish subjects chose selfish allocations in 45 or more decision rounds. Standard errors in parentheses. *, **, *** indicate 10, 5, 1 percent significance levels, respectively.

Table 6. The fraction of prototypical allocations, Treatment I vs. Treatment II

A. All subjects

	Treatment I			Treatment II		
	Utilitarian	Rawlsian	Logarithmic	Utilitarian	Rawlsian	Logarithmic
All prices	0.280	0.074	0.037	0.326	0.111	0.096
	(0.449)	(0.262)	(0.189)	(0.469)	(0.314)	(0.294)
$p<1$	0.044	0.090	0.029	0.032	0.163	0.095
	(0.205)	(0.287)	(0.167)	(0.176)	(0.370)	(0.293)
$p>1$	0.514	0.058	0.045	0.609	0.060	0.097
	(0.500)	(0.234)	(0.208)	(0.488)	(0.237)	(0.296)

B. Economics subjects

	Treatment I			Treatment II		
	Utilitarian	Rawlsian	Logarithmic	Utilitarian	Rawlsian	Logarithmic
All prices	0.377	0.050	0.017	0.445	0.052	0.082
	(0.485)	(0.217)	(0.130)	(0.497)	(0.223)	(0.274)
$p<1$	0.065	0.069	0.019	0.042	0.088	0.096
	(0.246)	(0.253)	(0.135)	(0.200)	(0.283)	(0.294)
$p>1$	0.709	0.030	0.016	0.816	0.018	0.068
	(0.455)	(0.169)	(0.125)	(0.388)	(0.134)	(0.252)

C. Neutral subjects

	Treatment I			Treatment II		
	Utilitarian	Rawlsian	Logarithmic	Utilitarian	Rawlsian	Logarithmic
All prices	0.259	0.049	0.038	0.315	0.097	0.072
	(0.438)	(0.216)	(0.192)	(0.465)	(0.296)	(0.258)
$p<1$	0.018	0.064	0.031	0.023	0.127	0.083
	(0.133)	(0.244)	(0.174)	(0.150)	(0.333)	(0.275)
$p>1$	0.473	0.036	0.045	0.611	0.067	0.060
	(0.500)	(0.187)	(0.208)	(0.488)	(0.249)	(0.238)

D. Humanist subjects

	Treatment I			Treatment II		
	Utilitarian	Rawlsian	Logarithmic	Utilitarian	Rawlsian	Logarithmic
All prices	0.203	0.136	0.058	0.209	0.194	0.144
	(0.403)	(0.343)	(0.234)	(0.407)	(0.395)	(0.351)
$p<1$	0.054	0.148	0.037	0.035	0.296	0.110
	(0.225)	(0.356)	(0.189)	(0.183)	(0.457)	(0.313)
$p>1$	0.360	0.121	0.078	0.376	0.097	0.177
	(0.480)	(0.326)	(0.268)	(0.485)	(0.296)	(0.382)

Standard deviation in parentheses.

Table 7. Effect of instructor type on prototypical allocations, Treatment II

	(1)	(2)	(3)	(4)	(5)	(6)
econ	0.666	0.942	0.365	-1.054	-2.064	-0.342
	(0.459)	(0.697)	(1.158)	(1.366)	(1.468)	(1.338)
λ	0.0969	0.173	-0.294	0.527	-0.0616	0.924**
	(0.093)	(0.120)	(0.227)	(0.372)	(0.356)	(0.414)
λ***econ***	0.220	0.525**	-0.0569	-0.223	0.295	-0.627
	(0.170)	(0.245)	(0.485)	(0.647)	(0.687)	(0.710)
Price-ratios	All	$p>1$	$p<1$	All	$p>1$	$p<1$
Dep. Var	Utilitarian	Utilitarian	Utilitarian	Rawlsian	Rawlsian	Rawlsian
Pseudo R^2	0.03	0.08	0.00	0.04	0.04	0.05
# of obs.	6592	3336	3256	6600	3336	3256

Logit regressions using robust standard errors that allow for clustering at the level of the individual subject. The indicator variable λ indicates choices that were made in the second treatment where the allocations were labeled. Standard errors in parentheses. *, **, *** indicate 10, 5, 1 percent significance levels, respectively.

Table 8. The effect of labeling prototypical allocations on the expenditure on tokens given to *other*

Dep variable: $p_o \pi_o$

	(1)	(2)	(3)
econ	-0.253**	-0.144	-0.143
	(0.102)	(0.119)	(0.120)
log(p)	-0.076**	0.001	(0.003)
	(0.036)	(0.050)	(0.052)
λ	-0.032*	0.004	0.005
	(0.017)	(0.023)	(0.023)
λ log(p)	-0.029	-0.0333*	-0.0305
	(0.019)	(0.019)	(0.0261)
λ***econ***		-0.079*	-0.079*
		(0.043)	(0.043)
econ log(p)		-0.146**	-0.138
		(0.069)	(0.095)
λ***econ*** log(p)			-0.006
			(0.049)
# of obs.	6600	6600	6600

Tobit regressions using robust standard errors that allow for clustering at the level of the individual subject. The indicator variable λ indicates choices that were made in the second treatment where the allocations were labeled. Standard errors in parentheses. *, **, *** indicate 10, 5, 1 percent significance levels, respectively.

Figure 1. Decision-level distributions of expenditure on tokens given to *other*

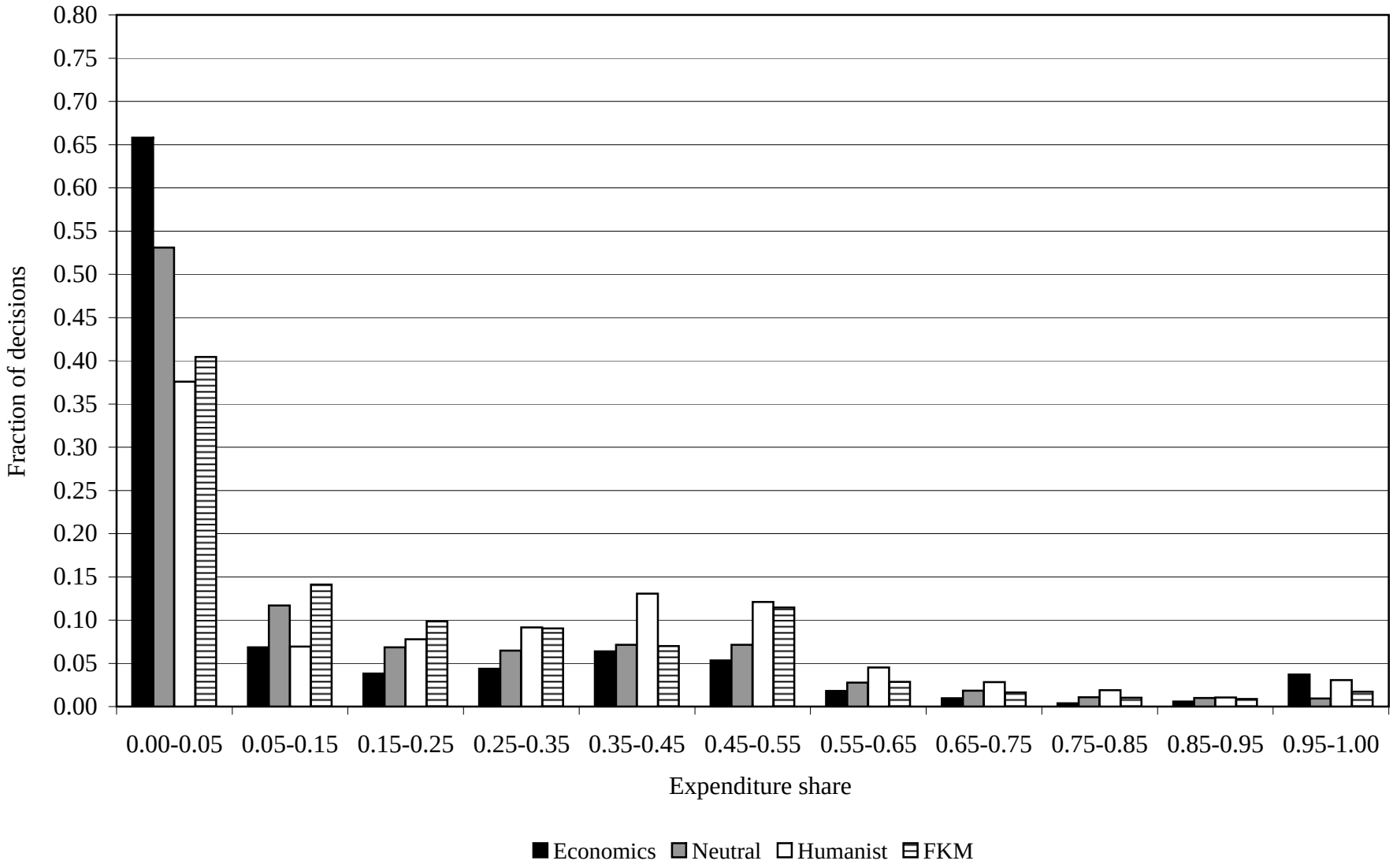

Figure 2A. Decision-level distributions of expenditure on tokens given to *other* by price-ratio tercile
Economics subjects

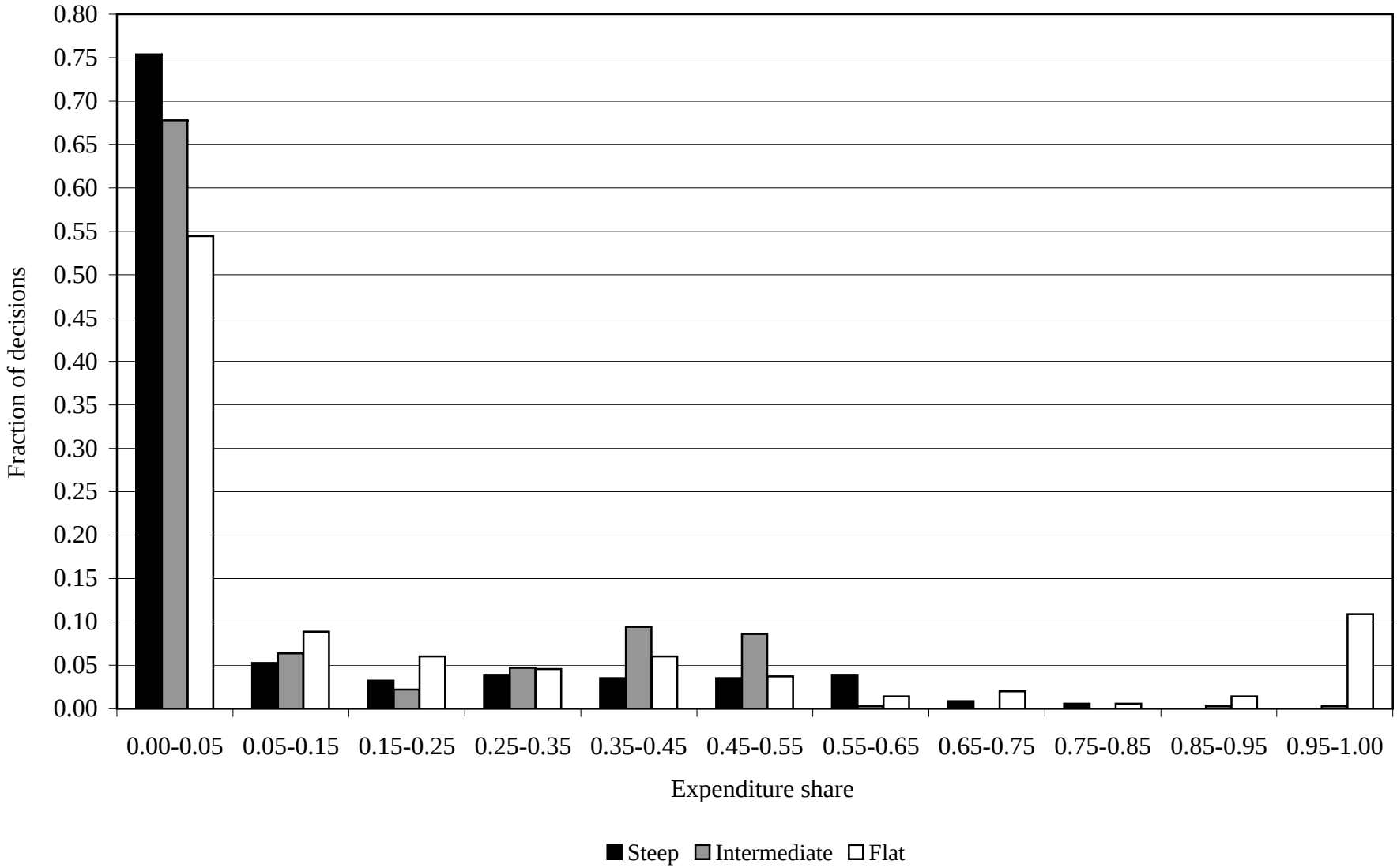

Figure 2B. Decision-level distributions of expenditure on tokens given to *other* by price-ratio tercile
Neutral subjects

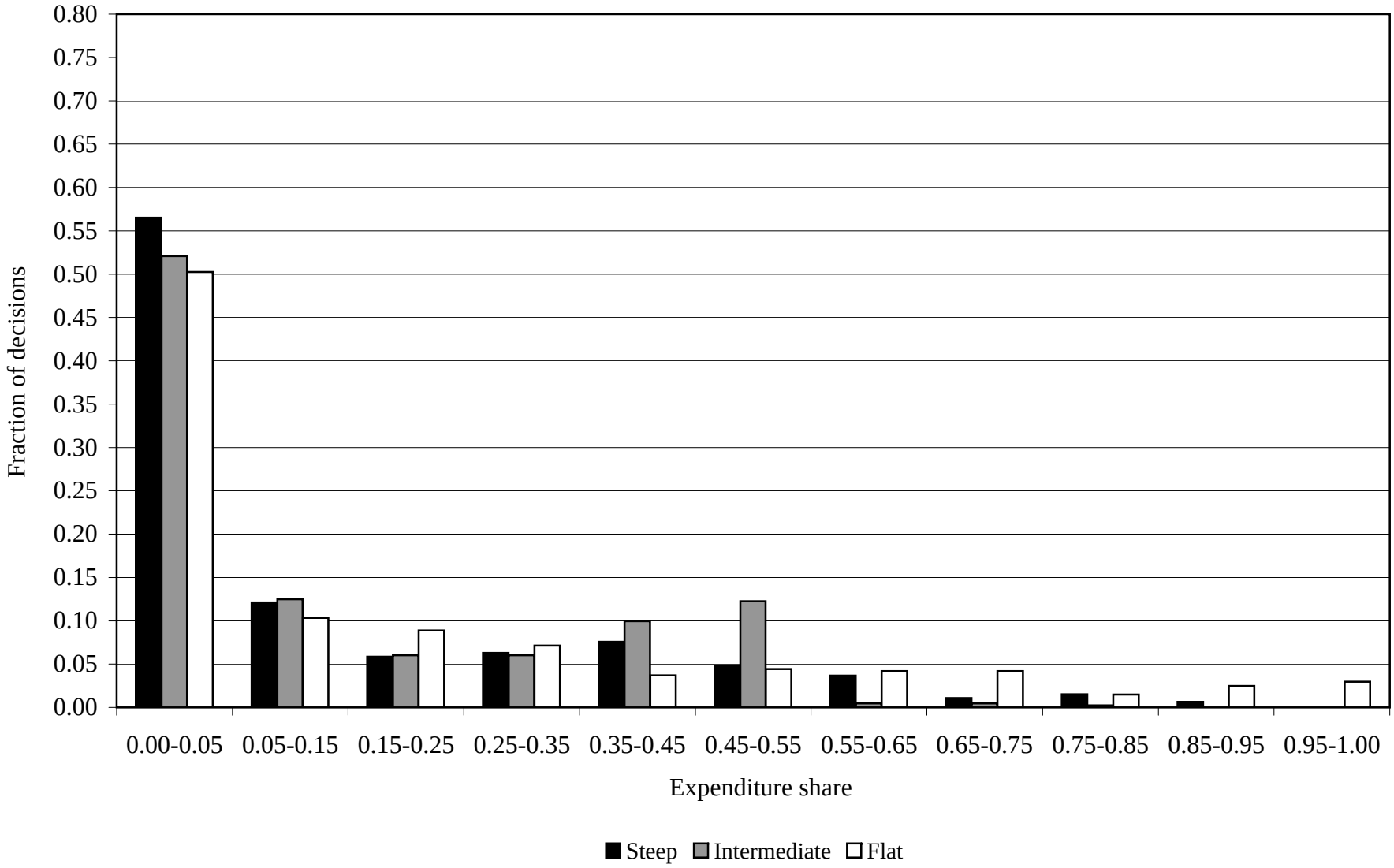

Figure 2C. Decision-level distributions of expenditure on tokens given to *other* by price-ratio tercile
Humanist subjects

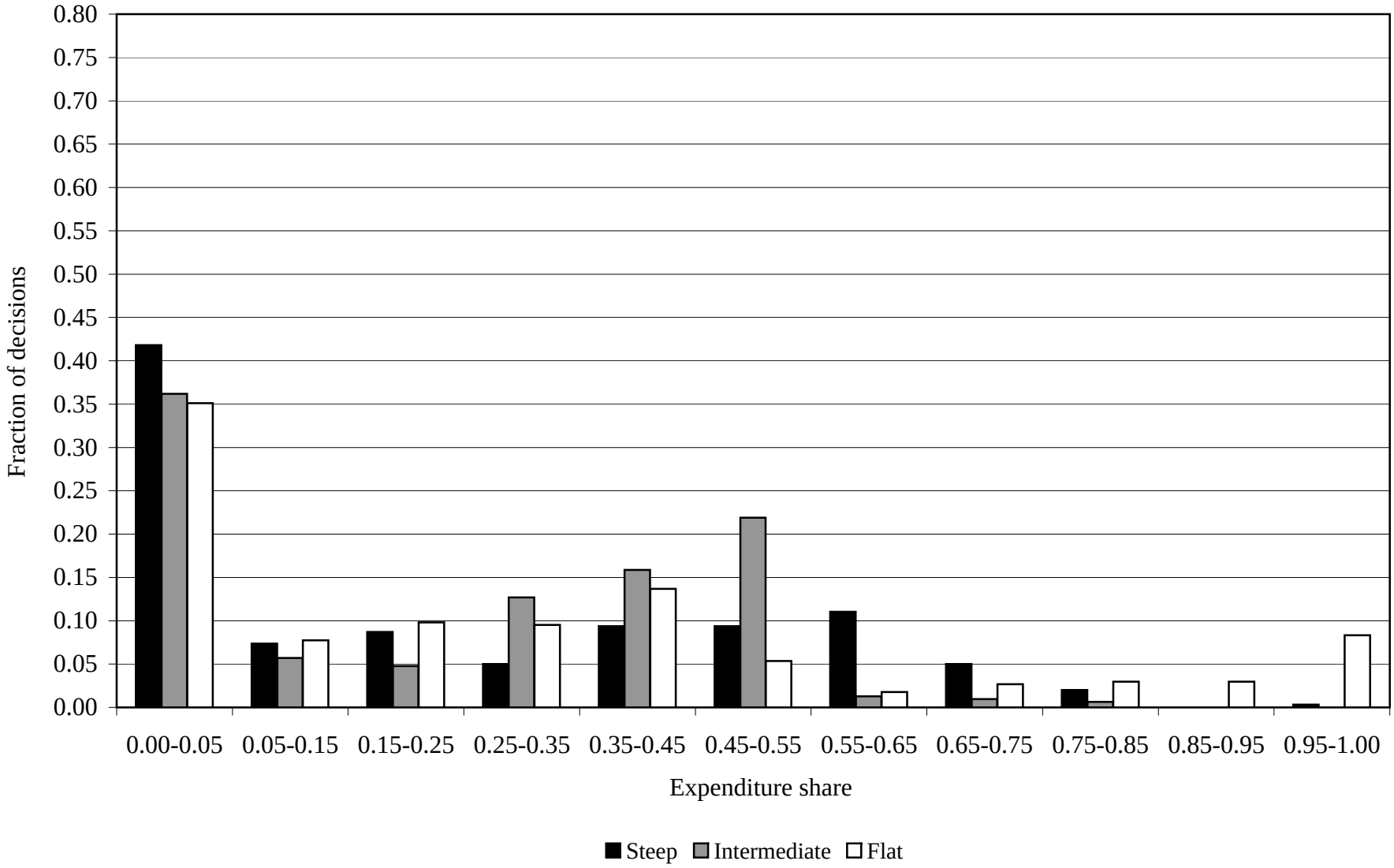

Figure 3. The distributions of the expenditure on tokens given to *other* aggregated to the subject level