

# What Motivates Political Preferences? Self-Interest, Ideology, and Fairness in a Laboratory Democracy\*

Justin Esarey<sup>†</sup>  
Emory University

Timothy C. Salmon<sup>‡</sup>  
Florida State University

Charles Barrilleaux<sup>§</sup>  
Florida State University

March 29, 2009

## Abstract

Many prior experiments have shown that individuals will sacrifice their own well-being to benefit others in a variety of situations. This behavior can be easily explained if people possess a preference for fairness or an aversion to inequality. On a larger scale, such preferences might explain the existence of macroeconomic redistributive income tax programs. This paper presents an experiment designed to determine the degree to which such preferences might drive voting behavior regarding redistributive taxation. In the experiment, subjects earn money in a real effort production task and are allowed to vote on a tax rate that determines the scope of redistribution. The subjects also complete a questionnaire about their economic ideology, allowing a test of whether this ideology is linked to the willingness to vote for redistributive taxes. We find that a subject's relative earnings primarily determines her voting behavior, with our subjects showing little in the way of preferences for fairness or inequality aversion.

**Key Words:** Redistributive taxation; preferences for fairness

**JEL Classification:** C90, D63

## 1 Introduction

While classical economic theory supposes that individuals possess purely self-interested preferences, countless demonstrations both in the lab and in the field suggest that individual preferences may possess some other-regarding components. This phenomenon has been shown to be quite robust in the bargaining literature with dictator and ultimatum games (see Camerer (2003) for an in-depth survey) as well as other contexts such as public goods (see Ledyard (1995)) and contracting

---

\*We gratefully acknowledge the financial support of the DeVoe Moore Center at Florida State University, which made this experiment possible. Many valuable comments and suggestions were offered by TK Ahn, Tom Carsey, Eric Dickson, Rachel Croson, Mark Rom, Jackie Rubin, David Macpherson and participants in our presentations at the 2006 Annual Meeting of the Southern Economic Association, the 2006 Annual Meeting of the Southern Political Science Association, the 2006 Annual Meeting of the Midwest Political Science Association, and the colloquium series at the University of Vermont. All errors remain our own.

<sup>†</sup>Department of Political Science, Emory University, 1555 Dickey Drive, 327 Tarbutton Hall, Atlanta, GA 30306. jesarey@emory.edu.

<sup>‡</sup>Department of Economics, Florida State University, Tallahassee, FL, 32306-2180. tsalmon@fsu.edu

<sup>§</sup>Department of Political Science, 172 Bellamy Building, Florida State University, Tallahassee, FL, 32306. cbar-rile@fsu.edu

(Fehr and Falk (2002) and Fehr and Gächter (2002)). The preferences demonstrated in laboratory experiments for small stakes have also been shown to translate into larger stakes environments and environments outside of the laboratory;<sup>1</sup> for example, actions in experimental dictator games have been linked to generosity in contributions to charitable groups (Kam, Crammer, and Fowler (2007)).

There is a very large-scale and important observable phenomenon that might be driven by these other-regarding preferences: government programs aimed at redistributing income. If people in general possess preferences for a fair distribution of income, these preferences should lead to the income redistribution programs that we observe at virtually all levels of government. So one might take the existence of these programs as strong evidence for the macro-level impact of other-regarding preferences. Alternatively, these programs could be explained with classical, self-interested preferences if politicians advocate them to gain favor with the low-income voters who will financially benefit from such programs. This explanation allows for the existence of income redistribution programs without requiring any other-regarding nature to preferences. There is, however, a reason to believe that at least some support for these programs also comes from individuals with high incomes who will not financially benefit from income redistribution (see for example Wilson and Banfield (1964)). This might be taken as evidence supporting the claim that income redistribution programs are based more on other-regarding preferences of the rich rather than self-interest of the poor.

There is also some experimental evidence corroborating this point, showing that subjects do appear to favor more equitable income distributions. Scott, Matland, Michelbach, and Bornstein (2001) and Michelbach, Scott, Matland, and Bornstein (2003) present experiments in which subjects are asked to choose between various hypothetical income distributions for a hypothetical country given multiple options. The authors find that people generally choose more equitable distributions. Frohlich, Oppenheimer, and Eavey (1987) investigate what sort of redistribution principles subjects will adopt when they are introduced to various possibilities, finding that subjects prefer to set a minimum income floor below which people cannot fall. While these studies suggest that people make political choices according to principles of fairness, the evidence provided is gathered in a setting that is (by design and intentionally) not directly analogous to how taxing and redistribution decisions are made. The Scott, Matland, Michelbach, and Bornstein (2001) and Michelbach, Scott,

---

<sup>1</sup>Again the Camerer (2003) chapter on bargaining surveys much of this literature, but more specifically see Roth, Prasnikar, Okuno-Fujiwara, and Zamir (1991), Forsythe, Horowitz, Savin, and Sefton (1994), Hoffman, McCabe, and Smith (1996), Straub and Murningham (1995), Slonim and Roth (1998), Cameron (1999), and List and Cherry (2000).

Matland, and Bornstein (2003) experiments involved hypothetical choices about potential income distributions, and so making more equitable choices cost the subjects nothing. The Frohlich, Oppenheimer, and Eavey (1987) study required participants to select from among a list of specified redistributive principles about which they were extensively educated, and also required extensive small-group discussion with an emphasis on unanimity when the group selected a redistribution plan. Furthermore, subjects' choices regarding redistribution policies are made behind a Rawlsian "veil of ignorance" that rarely exists when actual political decisions are made.

Our goal in this study is to attain more direct evidence on the degree to which people will support income redistribution policies in an environment similar to the one that people face in the outside world. If there is support for these policies, we also wish to determine the main causes of that support. Toward that end, we have designed a laboratory experiment intended to simulate a live voting and redistribution environment more closely than past experiments. In particular, our subjects earn income through a real effort task and vote on redistributive taxes that will have a direct impact on their well-being and that of other subjects in the experiment. To determine whether the subjects' stated preference for equality corresponds to their choices in the experiment, we survey the subjects to identify their demographic and ideological characteristics. In light of prior evidence from Hoffman, McCabe, Shachat, and Smith (1994) showing that the generosity of subjects in bargaining games is very sensitive to whether or not they earned the superior bargaining position, our experiments are also designed to vary the degree of entitlement subjects might feel toward their earnings to determine if this has an impact on preferences for redistribution.

We find that many subjects vote in favor of high tax rates for income redistribution, but most of those votes can be explained by the relative earnings position of the voters. Those subjects earning above-average incomes in the experimental effort task vote for low taxes, and those subjects earning below-average incomes from the task vote in favor of high tax rates. By contrast, stated ideological preferences for income redistribution have no direct impact on vote choice, though there are some important and interesting indirect effects: conservatives' votes tend to be more closely tied to their self-interest compared to their liberal counterparts. These findings suggests support of redistribution policies, at least in this environment, may not be explainable by preferences for fairness or income inequality.

The remainder of the paper proceeds as follows. Section 2 presents our experiment design. Section 3 presents our results. Section 4 return to the issue of preferences for fairness and inequality, particularly focusing on future work to investigate alternative origins of support for income

redistribution.

## 2 Experiment Design

Our experiment is designed to evaluate the impact of an individual's self-interest and ideology on his/her preference for income redistribution. The subjects earn money by exerting effort in a production task. This task is a twenty question multiple-choice spelling test that the subjects have 90 seconds to complete. All spelling questions were the same for every subject in a given period across sessions. This task mirrors aspects of the real-world work environment that we want to parallel: the task is difficult, uninteresting, performed under deadline pressure and some people are intrinsically more skilled at it than others.<sup>2</sup> There are three distinct phases in each session of our experiment: production without a redistributive tax, production under an initial tax rate chosen by the subjects, and production under a new tax rate chosen by the subjects. The first phase (without tax) lasts for 5 periods of production, while the second and third phases (with an endogenously chosen tax rate) last for three periods of production each.

We conducted our experiment in the xs/fs Computer Laboratory at Florida State University. We recruited subjects using a web-based announcement system that allowed a pre-existing pool to volunteer for the experiment a few days in advance. The pool consisted of undergraduate students who opted to receive experiment announcements after initial recruitment in introductory social science classes. The subjects did not know the details of our experiment and had not previously participated in sessions of this experiment (though some had experience with other, unrelated experiments). All participants received a \$10 show-up fee plus the money they earned during the experiment, which averaged an additional \$10 to \$15. At the appointed time, either 9 or 11 subjects sat at individual computer terminals with carrels that prevented interaction among participants. After administering a standard consent form, we distributed instruction forms and asked the subjects not to talk with others for the remainder of the experiment. The entire experiment was conducted using the zTree software package for conducting experiments Fischbacher (2007).

In the experiment, subjects use the median voting rule to select the tax rate that will apply: each subject casts a vote for the tax rate he/she prefers (in the range of 0 to 100 percent) and the median vote becomes the tax rate. Under the median voting rule, sincere voting is a weakly dominant strategy; the subjects have no strategic incentive to hide their most-preferred tax rate.

---

<sup>2</sup>The spelling test is also similar to the production task used in Frohlich and Oppenheimer (1990), which makes comparisons with their results more valid.

Once a rate is chosen, our experimental program collects taxes and divides the proceeds evenly among all subjects for the three production periods following the choice. Because all subjects receive an equal share, the tax scheme redistributes income from above-average earners (who pay more than an equal share of taxes) to below-average earners (who pay less). Those with incomes near the average gain or lose little from redistribution.

We systematically vary the relationship between merit and pay across sessions of our experiment to activate entitlement (or merit fairness) norms possessed by our subjects. In the Equal Pay treatment, each subject receives an equal pay rate of \$0.16 per correctly spelled word and a penalty of -\$0.053 per incorrectly spelled word.<sup>3</sup> In the Random Unequal Pay treatment, the program randomly assigns subjects in a session to either a high pay rate (\$0.20 per correct word, -\$0.067 per incorrect word) or a low pay rate (\$0.12 per correct word, -\$0.04 per incorrect word) at the beginning of the experiment, with 50% of subjects in each pay rate. Since the number of subjects is odd, we place one extra subject in the low pay rate. In an Earned Unequal Pay treatment, we place every subject at the low pay rate (\$0.12 per correct word, -\$0.04 per incorrect word) for the first two periods. The program awards the 50% of subjects with the highest number of correctly answered questions a high pay rate (\$0.20 per correct word, -\$0.067 per incorrect word) at the end of the second period; this new pay rate lasts for the remainder of the experiment. Again, one extra person remains in the low pay rate due to the odd number of subjects. Each session used only one pay treatment condition. At any pay rate and in any treatment, we neither reward nor penalize answers left blank.

The instructions for the first phase of our experiment,<sup>4</sup> which we distribute in written form to the subjects and read aloud, include a trial period of production that the subjects work through on their computer terminals to familiarize themselves with the software. We also show and explain to the subjects a sample payoff screen, which the subjects see after every 90-second period of production. The payoff screen displays how many questions an individual subject answered correctly and incorrectly, his/her pay and penalty associated with each, and his/her total earnings. The screen also shows the overall average net earnings for the period of other participants in that session, so that each subject determines how well he/she is doing relative to others in that session. In the Earned Unequal Pay treatment, subjects are told about the tournament aspect of the experiment at this time, before the first two periods begin.

---

<sup>3</sup>The penalty for incorrectly spelled words is designed to prevent random guessing from being a winning strategy. The expected value of a random guess is \$0 in every treatment.

<sup>4</sup>The instructions scripts are in an Appendix to the paper.

After the first phase of production (5 periods), we deliver written and oral instructions for the second phase of the experiment, which introduces the taxation and redistribution system that will exist in the remaining portion of the experiment. We familiarize the subjects with the tax system, teach them the median voting rule under which they select their own tax rate, and show them sample voting screens. The voting screen includes the subject's own average per-period earnings in the previous three periods as well as the overall per-period average of all subjects in the session. This allows subjects to compare their production performance to the average of the other subjects' production. The subjects are also shown a sample payoff screen that displays how much money is taken out in taxes and how much is received from redistribution at the end of each production period. We explicitly tell the subjects that the tax system is redistributive and show them an example of how income redistribution occurs under the tax plan.

After three rounds of production under the tax regime they voted for, the subjects are verbally instructed that they have another opportunity to choose a tax rate under the same voting process as before. After choosing a new tax rate via the median voter rule, the subjects produce for three rounds under this tax. All voting and payoff display screens are the same as in the previous phase.

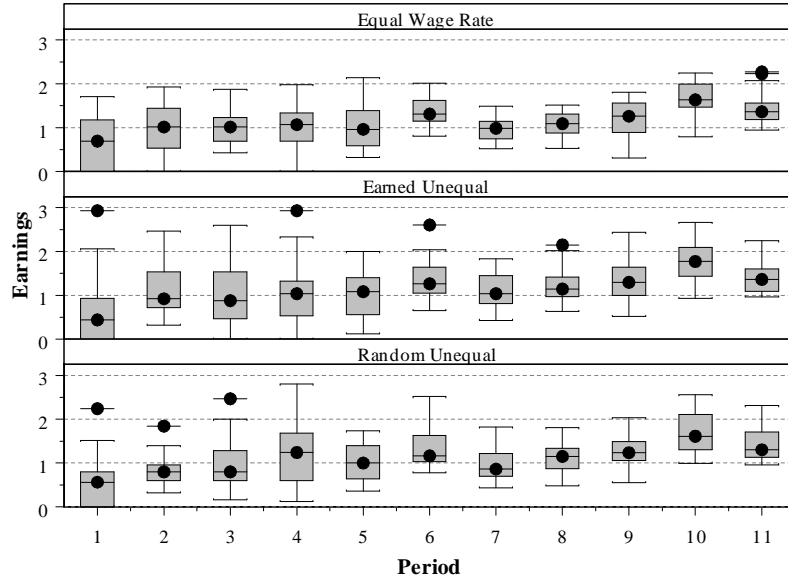
At the end of three more rounds of production, the experiment ends. Subjects fill out a short questionnaire about their demographic characteristics and political beliefs. The questionnaire, which is included as an appendix to this paper, asks 14 questions drawn from the General Social Survey. Some questions are designed to measure the subjects' identification with conservative economic ideology, such as:

“Please react to the following statement: In a free society, it is all right if a few people accumulate a lot of wealth and property while many others live in poverty.”

“Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?”

Other questions ask about the upbringing of the subjects, such as the education level of their parents, their city of primary residence (before college), and their family's income. Our standard demographic questions included race, religion and major area of study. Once every subject completes the questionnaire, we pay each subject their individual earnings privately before releasing them from the experiment.

We ran three sessions of the experiment in each treatment, for a total of nine sessions. Two of



**Figure 1:** Box Plot of Earnings Across Sessions and Over Time

the nine sessions included eleven subjects; the rest had nine subjects<sup>5</sup> for a total of 85 subjects. We observed a total of 170 votes, 85 first votes and 85 second votes.

### 3 Results

Since each subject produced for 11 periods in a session, we observed 935 rounds of production in all. Across all treatments, subjects answered an average of 8.41 spelling questions correctly in each round, with a standard deviation of 3.66 questions. Figure 1 displays box plots of the earnings distributions in the experiments. There is substantial earnings variation in the pre-tax portion of the experiment (periods 1-5) with many subjects earning at or near 0. After the tax is introduced in period 6, income inequality is decreased. Furthermore, inequality is greatest in the Earned Unequal Pay treatment.

<sup>5</sup>Both of the two sessions with 11 subjects are in Treatment 1. While the session with 9 subjects is not substantially different in any respect, it would be extremely difficult to statistically detect a difference between a treatment effect and a group size effect in our sample—if one exists. Given that prior experience in public goods experiments suggests a negligible group size effect for a difference this small (Ledyard 1995), we find this possibility unlikely.

### 3.1 Voting Behavior

Over the course of the sessions, the median tax rate chosen ranged from 35% to 50% in the first round of voting, and from 15% to 60% in the second round of voting. These tax rates indicate that some subjects were voting for substantial redistributive income taxes. Our first goal is to explain what influenced the voting behavior that underlies the tax rates the subjects chose.

A simple self-interest model would suggest that there should only be a single determinant of a subject's vote: their expectation of making earnings above or below the average in the production phase. In our experiment, tax rates are chosen after subjects have experience with production and have observed whether they are likely to be above or below average earners. However, the chosen tax rate applies to future earnings, not past earnings, meaning that subjects may be uncertain where they will rank in the future as they vote. It is reasonable to presume that the best predictor of future relative earnings is past relative earnings, and so subjects who have earned above or below average in the past should expect that they will continue to do so. Subjects who earn just around the average will reasonably expect that they have a greater likelihood of switching from above or below average than those farther from the average. Thus, a reasonable base model of how a subject will vote is a probabilistic choice model: that the farther above (below) average are their past earnings, the more likely they are to vote for lower (higher) tax rates. Those earning approximately at the average will be largely indifferent to all possible taxes, and should vote with nearly a uniform distribution across the entire range.

Because the dependent voting variable is bounded between 0 and 100 percent, we cannot use an OLS estimator for two reasons. First, OLS can predict votes that lie outside of the [0,100] boundaries. Second, we expect covariates to have a non-linear relationship with voting behavior due to the boundaries: increasing values of the covariates can only increase (or decrease) votes so far, implying that fitted regression line must be *s*-shaped with *y*-asymptotes of 0 and 1. We therefore employ a logistic regression model that takes the following form:

$$y = \frac{1}{1 + \exp(-X\beta)}$$

where  $X$  contains a vector of explanatory variables. The list of  $X$  variables must include a measure of the difference between a subject's earnings and the average earnings of all the subjects in that session in the three periods leading up to the vote, or their *Distance from Earnings Average*; recall that subjects themselves know this value prior to voting. We also want to use elements

from the ideology and demography survey to determine whether any of these variables can help to explain voting behavior. To allow for a parsimonious regression, we have constructed an overall ideology measure that incorporates all the relevant opinions and attitudes about income inequality gathered on the post-experiment questionnaire. We do this using a principal component analysis (PCA) to construct a variable (called *Conservatism*) for each subject that measures the economic component of their ideology, leaving out social issues. We extract a principal component from responses to questions 1 through 5 of the questionnaire to form this variable, as these questions are direct measures of ideology and/or attitude toward income redistribution.<sup>6</sup> This first principal component has a singular value twice as large as the next nearest component and loads on the observable variables as expected. The resulting index ranges from -2.19 to 3.46, with higher values indicating greater economic conservatism.

We designed our experiment with different treatments allowing for the possibility that different entitlement norms may emerge and effect voting behavior. Hence, we must also include dummy variables corresponding to our treatment conditions of *Random Unequal Pay* and *Earned Unequal Pay* with the *Equal Pay* treatment serving as the baseline. Our statistical model of voting behavior includes all the variables mentioned above along with certain interaction terms between them. A product term between *Conservatism* and *Distance from Earnings Average* tests whether ideology amplifies or mitigates the impact of self-interest on preference for redistribution. Product terms between *Distance from Earnings Average* and the treatment variables (*Random Unequal Pay Treatment* and *Earned Unequal Pay Treatment*) test whether the fairness of the treatment changes our subjects' responsiveness to their self-interest.

This logistic model, which is fitted using non-linear least squares (NLS) regression, accounts for the non-linear relationship implied by our bounded variable structure by fitting an *s*-shaped voting response function to the data, with asymptotes at 0 and 1. The result of fitting this model to subjects' vote choices is depicted in table 1.

The model indicates strong and statistically significant evidence for the power of self-interest in determining preference for redistribution. The higher one's earnings in the three periods prior to the voting decision, the lower one's vote will be (conversely, lower earnings lead to votes for higher

---

<sup>6</sup>Several readers asked specifically about the distinction between items 5a and 5b. The underlying question for this item is whether respondents think of themselves as conservative, middle of the road, liberal, or in none of these terms. The first three responses to this item are clearly ordinal, but the fourth is qualitatively different from the others. We therefore break the responses down twice: first, whether respondents think ideologically or not, and second, if they think ideologically which ideology do they identify with.

	Vote 1		Vote 2	
	Coefficient	p-value	Coefficient	p-value
<b>Distance from Overall Earnings Avg.</b>	-3.20	< 0.001	-4.03	0.003
<b>Conservatism</b>	-0.0340	0.710	-0.0116	0.924
<b>Random Unequal Pay Treatment</b>	-0.205	0.466	0.216	0.554
<b>Earned Unequal Pay Treatment</b>	-0.290	0.353	-0.242	0.651
<b>Distance*Random Pay</b>	1.42	0.111	1.52	0.319
<b>Distance*Earned Pay</b>	0.0570	0.954	-2.89	0.288
<b>Distance*Conservatism</b>	-0.914	0.014	-1.05	0.105
<b>Constant</b>	-0.249	0.214	-0.464	0.077
<i>n</i> = 85	$R^2 = 0.530$		$R^2 = 0.412$	

\*Coefficients are unstandardized. Two-tailed p-values are reported.

**Table 1:** NLS Logistic Model of Influences on Vote Choice

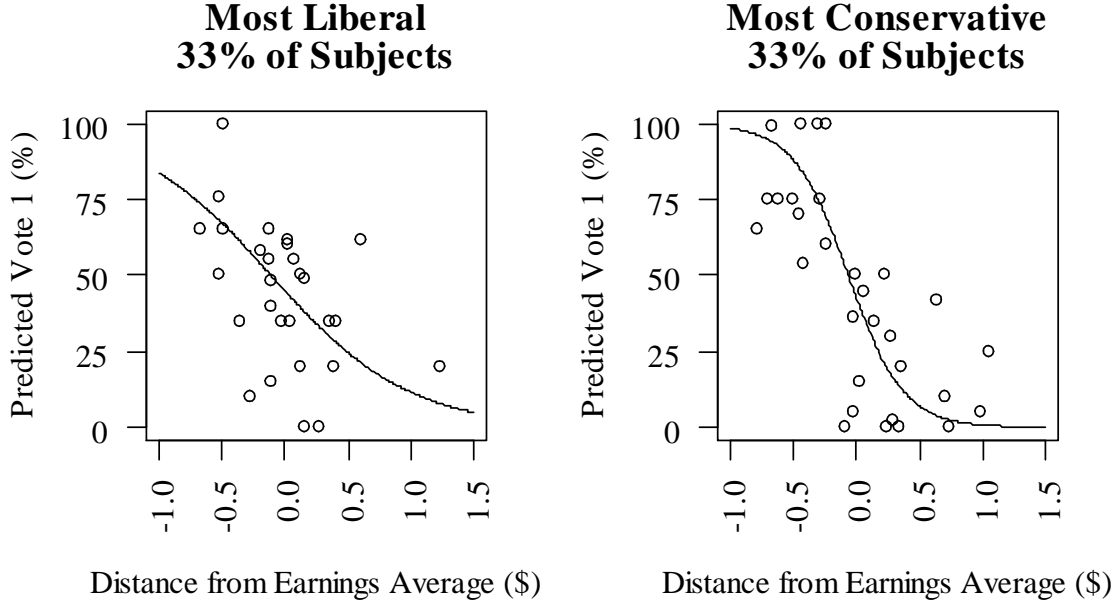
taxes). Ideology is not related to voting behavior at any level of statistical significance.<sup>7</sup> No other non-interaction variable in our model exerts an independent influence on voting behavior, giving strong support to the idea that self-interest is an overwhelmingly important consideration in the formation of preferences for redistribution and in subsequent voting behavior.

Though the model reveals no (statistically significant) *direct* relationship between ideology and preference for redistribution, there is some evidence of an *indirect* relationship. A negative, statistically significant interaction term between *Distance from Earnings Average* and *Conservatism* indicates that economic conservatives are more responsive than liberals to their self-interest when they choose their vote for redistribution, though self-interest is a determinative factor for both. Figure 2, a plot of the predicted first vote against distance from earnings average for the most conservative and most liberal subjects in our sample, tells the story: though subjects across the ideological spectrum respond to their self-interest, stronger conservatives tend to be more strongly influenced.<sup>8</sup> It is interesting to note that conservatives do not just vote for lower taxes than liberals when they are relatively wealthier. Counter to their stated ideology, they also vote for higher taxes than liberals when they are relatively poorer.

The results in table 1 suggest that subjects made more self-interested choices in the second

<sup>7</sup>Due to the problematic issues associated with such a compound *Conservatism* measure we repeated our analysis including all variables from the questionnaire separately in place of the ideology index. All variables from the survey were statistically insignificant in the model, including the questions that we did not incorporate into our index. We chose to present the more parsimonious analysis but the full results are available upon request.

<sup>8</sup>Though there are three outliers on the *distance\*conservatism* interaction term for the first vote regression and one outlier for the second vote regression, removing these outliers does not change the statistical significance of the interaction variable.



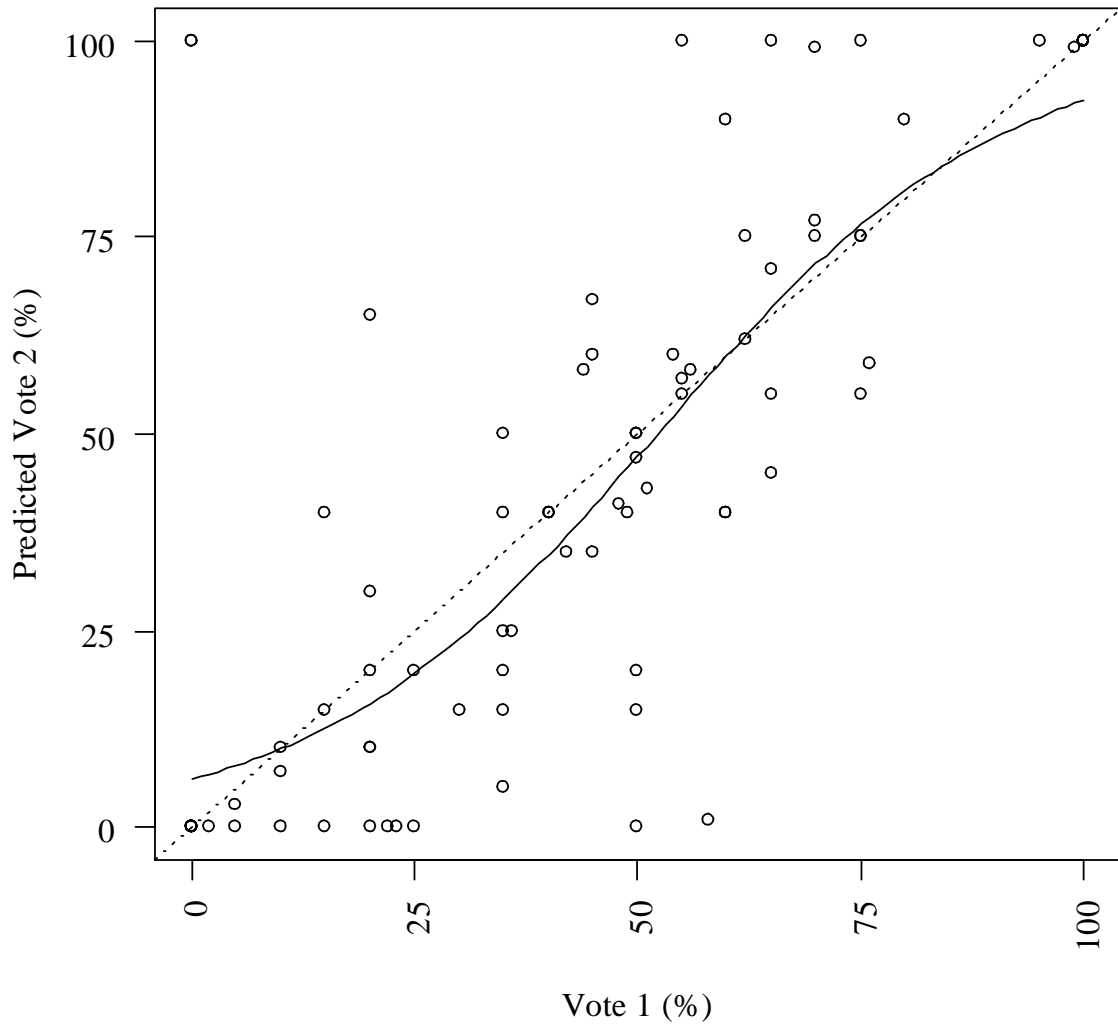
**Figure 2:** Scatter points are observations in the indicated quantile of *Conservatism* scores. Fitted line is predicted Vote 1 from the NLS logistic model in table 1, with *Conservatism* set at its 16.5th and 83.5th percentile (respectively) in the Equal Pay treatment.

vote than in the first. A logistic model of percentage vote change<sup>9</sup> using the independent variables from table 1 confirms that an increase (decrease) in preference for redistributive taxes tends to be associated with lower (higher) income, with the same significant and negative interaction between conservatism and self-interest as in table 1. Figure 3 illustrates that a subject’s second vote tends to be in the same direction as his or her first vote, but more extreme in magnitude. That is, subjects tend to drift toward the extremes of 0% and 100% votes over time.<sup>10</sup> There are multiple reasonable explanations for this result: subjects may become more self-interested over time, they may become more certain of whether they will be above or below the average earnings in a period, or they may become more informed about the relationship between the tax system and their preferences as they gain experience under its rules.

A naive deterministic choice model in this environment would predict that all tax choices should be either 0% (if the subject believes his earnings would be above average) or 100% (if the subject

<sup>9</sup>In this model, the dependent variable was the proportion of possible change in vote that a subject actually made. For example, if a subject voted for 50% taxes the first time and 60% taxes the second, the dependent variable would have been  $\frac{.6-.5}{1-.5}$ . This transformation allows for the fact that those who start with votes closer to the endpoints of the voting scale have less distance to move than those with votes closer to the middle. To facilitate a logistic regression, we also linearly transformed this variable (whose domain was -1 to 1) to lie between 0 and 1. The results of this regression are omitted, but are available in the supplemental packet of materials online at <http://mailer.fsu.edu/~jee03c>.

<sup>10</sup>In fact, the relationship between these two quantities is statistically significant ( $p < .001$ ), and 60% of the variance in second vote choice can be explained by a logistic model on first vote choice (and a constant) alone.

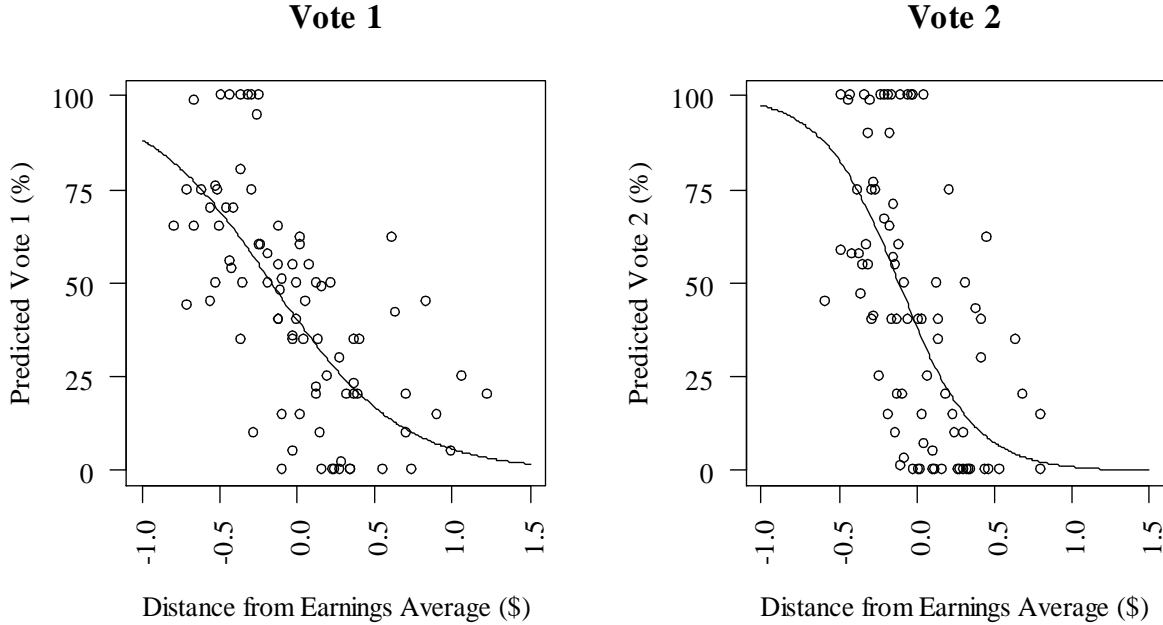


**Figure 3:** Scatter points are observations of subjects' first and second votes. The solid prediction line was generated from a logistic model using only Vote 1 and a constant term as predictors. The dotted 45 degree line shows whether Vote 2 was higher or lower than Vote 1.

believes his earnings would be below average). This prediction ignores any expected disincentive effects of high tax rates causing diminished productivity. If one takes into account an effect of high tax rates decreasing productivity, then the 100% rate would be replaced by the rate that would generate the maximum tax receipts. For now we will use the naive model as our benchmark model, ignoring these incentive effects as they have no real effect on the present analysis. In section 3.3 we will return to this issue of the incentive effects of taxes to determine if they do lead to decreased productivity.

Figure 4 demonstrates clearly that the stark prediction is not observed: most of the votes range between these extremes. We offer two explanations for this deviation from the simple prediction. The best explanation is that which formed the basis for our statistical model. While voters at the extremes of the earning distribution are likely to have strong incentives to place extreme votes, those in the middle of the earnings distribution are practically indifferent between all tax rates because they expect to receive in the redistribution almost exactly what they pay into the system. Thus a standard probabilistic choice model (wherein a subject chooses the tax rate maximizing his/her expected income only with higher probability) would predict that subjects in this intermediate range would have virtually equal probability of choosing tax rates along the entire range. This would produce exactly the pattern of votes we observe. Further, voters whose earnings are near the average earnings of all subjects are uncertain about what their social position will be in the future because there is a stochastic component to production (one may score poorly on the future spelling tests because they include words one does not know, for example.) If they are risk-averse, these voters will be more likely to vote for some level of redistribution as a form of insurance against future poor performance. The exact level of redistribution desired will vary depending on the subject's (unobservable) beliefs about where he/she will be relative to the average in future rounds of production as well as the degree of the subject's risk aversion. So although our findings do not match with a simple deterministic model of self-interested choice, either of these two empirically reasonable alterations to the model would predict exactly the relationship found in the data between votes and relative earnings.

It is also possible to interpret the deviations we find from the deterministic choice model as evidence for the existence of heterogeneous norms of fairness or equality in our subject pool. If subjects possess such heterogeneous preferences for fairness or income equality, then these preferences could pull the subjects away from the self-interest predictions of 0% and 100%, with the distance determined by the strength of their conviction. This prediction can be found by applying any



**Figure 4:** Scatter points are observations for the relevant vote. Fitted line is predicted Vote from the NLS logistic model in table 1, with *Conservatism* set at its 50th percentile and all other variables at their mean.

standard models of inequity aversion, such as Konow (1996), Fehr and Schmidt (1999), Charness and Rabin (2002), and Frohlich and Oppenheimer (2005). If all subjects were strongly inequity averse, they would all vote for a 100% tax because this eliminates inequality.<sup>11</sup> Votes for tax rates less than 100% would be the result of a trade-off between the self-interest of a subject and the strength of their aversion to inequality. Alternatively, one could view the noise of observations above or below the fitted lines as the result of heterogeneous preferences for equality. Thus observed voting behavior could be taken as a way of measuring a subject's inequity aversion. Due to the lack of correlation between votes and the ideology measure, though, the evidence to support this interpretation is not strong.

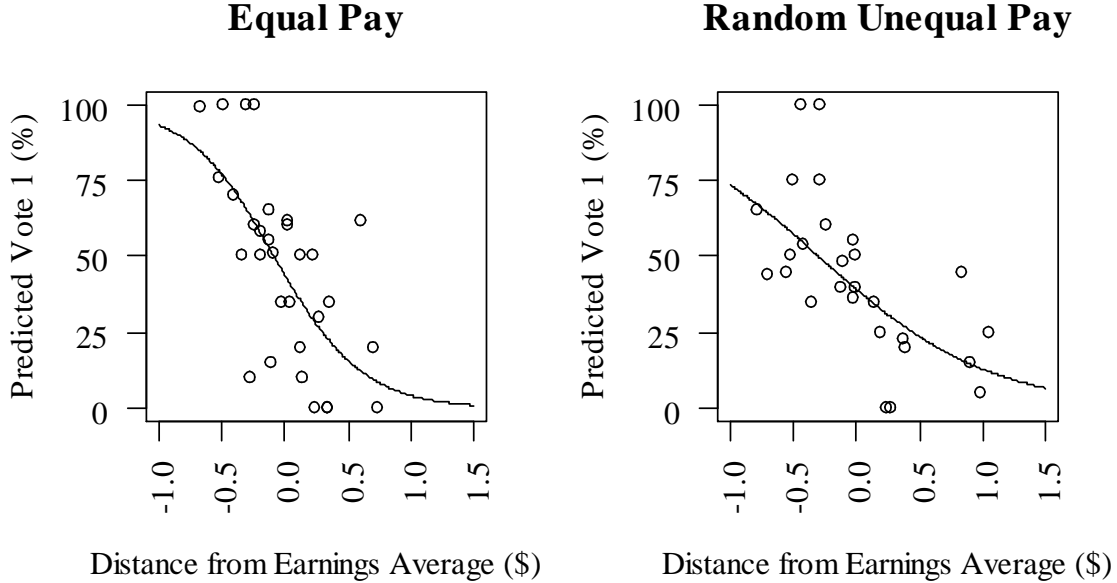
In addition to measuring demographic details of the subjects and their political ideology, our survey was also designed to determine if subjects were averse to income inequality. This was done by asking the subjects in several different ways regarding the degree to which they agreed or disagreed that income inequality is acceptable to them. Subjects who repeatedly responded that they found income inequality unacceptable should be classifiable as inequity averse and this is what

<sup>11</sup>Again, there is the potential work disincentive at tax rates so high suggesting that there might be a lower tax rate that would lead to both higher overall income along with relatively low inequality that people might prefer. Such a correction to the argument does not change the fundamental point.

the Conservatism variable in the regression measures. If the deviations from self-interest found in the data were due to inequity aversion, then a subject's Conservatism score or more broadly his answers to the survey questions regarding income inequality should have some explanatory power in the regression. Table 1 and the figures for this section should demonstrate quite clearly that the votes are strongly correlated with self-interest but not with their responses on the survey. Thus it appears unlikely that a satisfying case can be built to argue that the deviations from self-interest are due to inequity averse preferences. A reasonable explanation for the lack of a relationship between our two inequity aversion measures (i.e. the survey responses and the voting behavior) is that while some subjects may express inequity aversion when it is costless as in the survey, their voting behavior demonstrates that these concerns are dominated by self-interest in the environment of the experiment and perhaps in particular when it is costly to express inequity aversion. This is an issue we will discuss in more detail in the conclusion.

### 3.2 Entitlement Norms

We designed our three treatments based upon prior results in Hoffman, McCabe, Shachat, and Smith (1994) and Scott, Matland, Michelbach, and Bornstein (2001) showing that the degree to which someone feels entitled to earnings or position will affect his or her behavior. Table 1 shows us that manipulating the entitlement (or merit fairness) of the treatment exerted no direct effect on the subjects' preference for income redistribution. There is, however, tenuous evidence for a second order treatment effect through the interaction of *Distance from Earnings Average* with *Randomly Unequal Pay Treatment* as it possesses a borderline statistically significant positive coefficient in the first vote (though not the second). Substantively, this finding means that people encountering randomly unequal pay rates—those with the weakest entitlement to their earnings—tend to be less responsive to their self-interest in making vote decisions than subjects under the equal pay treatment. In our earned unequal pay treatment, the interaction effect was in the same direction as the randomly unequal pay treatment for the first vote but statistically insignificant. In the second vote, the earned unequal pay treatment interaction reversed sign, but was statistically indistinguishable from the interaction coefficient on the randomly unequal pay treatment (and both were indistinguishable from zero.) This relationship between fairness and responsiveness to self-interest is depicted visually in figure 5: there is less-extreme separation of voting patterns by self-interest in the pay structure that most strongly entitles subjects to enjoy income advantages. Our finding tentatively supports the notion that subjects become less sensitive to their self-interest



**Figure 5:** Scatter points are observations from the treatment in the graph. Fitted line is predicted Vote 1 from the NLS logistic model in table 1, with *Conservatism* set at its 50th percentile.

as the pay structure becomes less related to merit, and consequently as it becomes more unjust. The finding would be consistent, for example, with subjects' having a greater preference for income equality under conditions where merit and reward were less-closely linked, as has been found in previous experiments. The potential earnings inequality generated by the wage differential in the Random Unequal treatment is large: those at the low pay rate would have to work nearly twice as hard to generate the same earnings as those with the high wage. The relatively small size of the treatment effect on behavior, though, suggests that the subjects did not perceive the inequity introduced by the randomized wage to be substantial enough to offset the fundamental fairness of an earnings distribution achieved through real effort.

It is possible that the degree of entitlement embodied in a pay system creates an entirely different regime, changing the effect of ideology, self-interest, and the interaction between them. For example, economic conservatives might interpret the earned unequal pay treatment as being fair while liberals do not, which in turn could cause a systematic ideological difference in their voting patterns. To explore this possibility, we run a separate logistic regression for each treatment whose results are displayed in table 2. Note that this procedure is equivalent to interacting each independent variable with the treatment condition, which creates a large number of new independent variables in the regression and reduces degrees of freedom. This analysis confirms our initial conclusions, with an

Variable	Vote 1		Vote 2	
	Coefficient	p-value	Coefficient	p-value
<b>EQUAL PAY TREATMENT</b> ( $n = 31$ )				
Distance from Average Earnings	-3.66	0.002	-5.20	0.041
Conservatism	0.141	0.491	0.225	0.266
Distance*Conservatism	-1.44	0.081	-2.48	0.168
Constant	-0.0818	0.769	-0.357	0.243
<b>RANDOM PAY TREATMENT</b> ( $n = 27$ )				
Distance from Average Earnings	-1.55	0.003	-2.38	0.016
Conservatism	0.159	0.313	-0.0723	0.752
Distance*Conservatism	-0.438	0.370	-0.354	0.668
Constant	-0.405	0.041	-0.214	0.429
<b>EARNED PAY TREATMENT</b> ( $n = 27$ )				
Distance from Average Earnings	-2.78	0.001	-7.31	0.009
Conservatism	-0.258	0.108	-0.443	0.086
Distance*Conservatism	-0.454	0.513	-3.32	0.106
Constant	-0.486	0.051	-0.623	0.122

\*Coefficients are unstandardized. Two-tailed p-values are reported.

**Table 2:** NLS Logistic Model of Influences on Vote Choice, by Treatment

additional twist. Conservatism has a borderline statistically significant and negative direct effect on preference for redistribution in the earned unequal pay treatment; that is, conservatives vote for lower tax rates (compared to liberals) when higher pay rates are assigned according to productivity. This finding suggests that conservatives interpreted the treatment to be fair (and therefore voted for low taxes to reward merit) while liberals interpreted the treatment to be unfair (and therefore voted for high taxes to equalize income). This kind of disagreement is frequently reflected in real-life political debates, where liberals and conservatives often disagree about the extent to which the rich are entitled to their earnings.

### 3.3 Productivity, Pay and Tax Incentives

Do redistributive taxes lower productivity by weakening the incentive to work? We answer this question using a linear regression model, where the dependent variable is the number of spelling questions correctly answered by an individual subject in a period. Our *Pay Rate* and *Tax Rate* variables measure the marginal impact of incentives on productivity. Each model employs the *Conservatism* index and treatment dummies that were used and explained in the earlier voting models to capture any possible influence that ideology or pay fairness might exert on productivity. It is possible, for example, that our subjects produce less when they feel that their pay is unfair.

It is important to note that we must exclude the *Earned Unequal Pay* treatment from our model, because this treatment creates correlation between pay rate and productivity by design.

As our productivity data are time-series cross-sectional, we must be concerned about spatial and contemporaneous correlation in the errors biasing our inferences, particularly because certain rounds of production seem to have easier or harder questions than others and because there is a strong upward trend in productivity over time (probably due to experience with the task). Our modeling approach accounts for these features of our data to prevent them from affecting our estimates. First, we use an ordinary least squares model with panel-corrected standard errors (PCSE) that are designed to correct for spatial correlation Beck and Katz (1995). We also report Huber-White standard errors clustered on the individual for comparison to the PCSE technique. Second, we include a *Period* variable to remove the trend from the data.

Table 3 shows the results of three different modeling approaches. The first modeling approach uses OLS, as described above. The second model adds a panel-specific autoregressive error specification to correct for possible temporal dependence in productivity within subjects. The final model, generalized least squares with random-effects correction, attempts to eliminate any potential bias in coefficient estimates resulting from unmodeled individual-specific intercept shifts in productivity that would result if, for example, certain individuals are inherently better spellers (or harder workers) than others under any conditions. In all models, all coefficients stay at roughly the same magnitude and statistical significance save two (*Conservatism* and *Randomly Unequal Pay*).<sup>12</sup>

Our regression results indicate that higher pay is associated significantly with greater productivity: a one cent increase in pay rate per correctly answered question is associated with an additional one-fourth of a question per period being answered correctly on average. Interestingly, a higher tax rate was not associated with lower productivity, a puzzling result given that the practical impact of pay rate and tax rate on incentives to produce are identical. Further any explanations for why the tax rate should not have slowed production related to the design of the experiment, e.g. subjects had nothing else to do than produce, should also have led subjects to not change their behavior due to differences in wages. Despite this symmetry of effect, subjects apparently interpreted the effects differently and did not slow their production in response to higher taxes.

Relating this result to results on labor supply elasticity is somewhat complicated. While standard theory suggests that labor supply should increase with pay rate, most elasticity estimates are

---

<sup>12</sup>These two exceptions become statistically insignificant in the GLS model, probably due to their close correlation with individual-specific traits being captured by the random effects.

	OLS			OLS with PCSE AR(1)		GLS with Random Effects	
	Coeff	H-W p-value	PCSE p-value	Coeff	p-value	Coeff	p-value
<b>Tax Rate</b>	-0.031	0.969	0.985	0.963	0.577	-0.307	0.643
<b>Pay Rate</b>	25.6	0.025	< 0.001	27.3	< 0.001	25.6	0.041
<b>Conservatism</b>	0.112	0.620	0.043	0.169	0.006	0.111	0.659
<b>Period</b>	0.428	< 0.001	0.001	0.445	0.003	0.443	< 0.001
<b>Random Unequal</b>	0.460	0.499	< 0.001	0.396	0.037	0.466	0.505
<b>Constant</b>	1.66	0.362	0.037	1.17	0.188	1.63	0.429
	$R^2 = 0.168$			$R^2 = 0.600$		$R^2 = 0.168$	

\*Coefficients are unstandardized. Two-tailed p-values are reported.

**Table 3:** Analysis of Productivity

quite small. Blundell and MaCurdy (1999) for example show that a 10 percent increase in male wage rates would increase the amount of labor supplied by 1 percent in the median (though the response rate for women was higher). Blundell, Duncan, and Meghir (1998) find a similarly moderate response to changes in taxes. One standing concern about interpreting such studies though is that most of the population does not have much of an ability to adjust their labor supply except in large discrete blocks as wages change. This tends to diminish the ability to find much responsiveness. In our experiment, though, subjects could smoothly respond to changes in the wage rate allowing for the stronger observed response. Fehr and Goette (2007) conducts a field experiment by unexpectedly changing the wages of bike messengers who can freely choose working hours and observed their response. Consistent with our results, they found a substantial increase in the labor supply measured as the number of shifts worked per week, though they observed a simultaneous decrease in effort per shift. They could not observe such changes in response to changes in tax rates. Consequently it is difficult to determine the degree to which our results match up with existing results on labor supply.

The regression results also picked up an upward trend in productivity over the course of the experiment, which amounted to an average increase of two-fifths of a question correctly answered per period elapsed. Interestingly, the regression detects a slight association between economic conservatism and productivity; the most extreme conservatives in our data set produced one-half a question more per period on average than the most extreme liberals in our set. This relationship is possibly an artifact of correlation between conservatism and family schooling in the data: in an

unreported analysis, we find a statistically significant relationship between self-reported father's schooling and the conservatism index.

## 4 Summary and Discussion

Substantial prior literature has established that subjects in laboratory experiments are typically willing to sacrifice their own well-being to make financial allocations more equal among participants. We tested the applicability of this result in an environment that contained some of the key contextual issues that are usually excluded from more abstract games but which might be important in situations involving income redistribution. Our general finding is that votes for a redistributive tax were almost entirely in accordance with self-interest: above-average earners vote for low tax rates and below-average earners vote for high tax rates. A measure of subjects' preferences for fairness or equality, their self-reported economic ideology, was unrelated to their behavior in this experiment. Because our ideology measure should have been correlated with any intrinsic preferences regarding inequality aversion, we conclude that any preferences for fairness or inequality that our subjects possessed were not strong enough to overcome self-interest in this context.

Because our results seem to run counter to prior experimental evidence, it is important to interpret these results carefully. While there is evidence that people will sacrifice their own well being for others in abstract ultimatum and dictator games, there is also evidence that this willingness decreases substantially in cases where the subjects have earned their position. Our results should be taken as further confirmation of that result. Our results strongly suggest that when money to be re-distributed is earned through real effort, subjects are much less willing to sacrifice their well-being for that of others.

The failure of the ideology variable to explain much about the data (at least using direct effects) is more problematic and more interesting. There can be little doubt that there is substantial support for income redistribution outside of the lab, often by people who do not receive a financial benefit from redistribution. There is, however, little evidence of such behavior in our experimental society. What is missing from our design that generates support for redistribution by wealthy individuals outside the lab?

One initial answer is that some field evidence corroborates our result as externally valid. Smith (1975) presents data from a Washington state ballot initiative on school funding equalization, providing a figure that looks almost identical to our figure 4. Counties that stood to lose money

from the initiative had a low percentage voting in favor of equalization, while those that stood to gain had a high percentage voting for the initiative.<sup>13</sup> Other field research on education funding (see Berkman and Plutzer (2004)) which seeks to address the self-interest question directly using individual-level data with suitable controls for ideology and other confounding factors, shows that preferences for funding local schools are driven by self-interest as well as the extent to which citizens feel tied to the community. We also note that Verba and Schlozman (1977) find a link between income and preference for redistribution in field survey data.

There are, however, other important elements missing from our design that are relevant to income redistribution plans outside the laboratory. The efficiency of the government transfer program is one such element: programs thought to possess low efficiency will generally enjoy lower popular support. Our program possessed 100% efficiency though, so this omission may cause our experiment to overestimate the nature of the support for such programs. Another plausible concern is that income variability among our subjects is too small to induce a desire for redistribution, introducing a bias that favors our finding evidence for self-interest. A large pool of prior laboratory evidence (see chapter 2 in Camerer (2003)) strongly suggests otherwise: subjects seem to care about inequity when even more trivial amounts of money are concerned (i.e., on the level of one subject receiving \$3 while another receives \$7.) The fact that subjects in prior experiments consistently act in opposition to their own self-interest suggests that the strongly self-interested behavior in our experiment is not due to a lack of variation in income among the subjects.

A similar argument can be made with regard to the overall size of the stakes. Our subjects were voting on tax plans that amounted (at most) to marginal gains or losses of five to ten dollars, while actual redistributive tax policies lead to redistribution on a scale many orders of magnitude larger. To make the issue clear: could our subjects have acted more egoistically in the lab than they would have in the voting booth because they knew no one would starve or go homeless as a result of their behavior? Again, data from previous experimental studies suggest not. Several studies of dictator and ultimatum games raise the stakes to hundreds of dollars with US undergraduates and even a month's wages or more in countries where researchers can afford to conduct such experiments (see cites from footnote 1). Where raising the stakes has any effect (and often there is none), increased stakes induce more self-interested behavior. This implies again that our experiment may actually be too conservative in estimating the influence of self-interest on behavior compared to the field.

---

<sup>13</sup>The Washington state study contains neither a measure of ideology for each school district nor individual voting data, as would be necessary to confirm that the self-interest of a county's residents is not very highly correlated with its ideology to verify that the conclusion is not spurious.

Another important potential factor has to do with the attribution of merit or fault to those with lower incomes. In our experiment, the only reason a subject would receive income redistribution payments is due to some combination of effort and ability in the production task. Recipients of income transfers outside the lab are often in their position due to circumstances beyond their control. Residents of New Orleans and Mississippi who were displaced due to Hurricane Katrina, for example, certainly did nothing to deserve their negative wealth shock except perhaps choosing to live in an area with a higher than average risk of such an event. Similarly, many recipients of welfare payments are unemployed due to cyclical downturns in the economy or the profitability of their prior employer. Both situations may well be beyond the control of the unemployed person. It is very likely that people who are not willing to redistribute income to people who are responsible for their own poverty would be likely to transfer income to people who had poverty thrust upon them for reasons beyond their control. This point also raises the social insurance aspect of many redistribution plans: people who are financially secure may support such income redistribution to insure themselves against poverty in the future if they suffer a random negative wealth shock. Our current results are insufficient for evaluating these alternative explanations for the widespread support that income redistribution enjoys. Evaluating these possibilities is left for future research. What our results do appear to establish is that, absent these other possible triggers, people seem to vote largely according to their self-interest and not their ideology or intrinsic preference for equitable income distributions.

## References

- Beck, N. and J. Katz (1995). What to do (and not to do) with Time-Series Cross-Section Data. *American Political Science Review* 89(3), 634–647.
- Berkman, M. and E. Plutzer (2004). Grey Peril or Loyal Support: The Elderly and Expenditures on American Education. *Social Science Quarterly* 85(5), 1178–1192.
- Blundell, R., A. Duncan, and C. Meghir (1998). Estimating Labor Supply Responses Using Tax Reforms. *Econometrica* 66(4), 827–861.
- Blundell, R. and T. MaCurdy (1999). Labor Supply: A Review of Alternative Approaches. In O. Ashenfelter and D. Card (Eds.), *Handbook of Labor Economics, Volume 3A*, pp. 1559–1695. Amsterdam: Elsevier.
- Camerer, C. F. (2003). *Behavioral Game Theory: Experiments in Strategic Interaction*. Princeton University Press and The Russell Sage Foundation.
- Cameron, L. A. (1999). Raising the Stakes in the Ultimatum Game: Experimental Evidence from Indonesia. *Economic Inquiry* 27(1), 47–59.

- Charness, G. and M. Rabin (2002). Understanding Social Preferences With Simple Tests. *Quarterly Journal of Economics* 117(3), 817–869.
- Fehr, E. and A. Falk (2002). Psychological Foundations of Incentives. *European Economic Review* 46(4-5), 687–724.
- Fehr, E. and S. Gächter (2002). Do Incentive Contracts Undermine Voluntary Cooperation? Working Paper No. 34, IERE University of Zurich.
- Fehr, E. and L. Goette (2007). Do Workers Work More if Wages are High? Evidence from a Randomized Field Experiment. *American Economic Review* 97(1), 298–317.
- Fehr, E. and K. Schmidt (1999). A Theory of Fairness, Competition and Cooperation. *Quarterly Journal of Economics* 114(3), 817–868.
- Fischbacher, U. (2007). z-Tree: Zurich Toolbox For Readymade Economic Experiments. *Experimental Economics* 10(2), 171–178.
- Forsythe, R., J. Horowitz, N. Savin, and M. Sefton (1994). Fairness in Simple Bargaining Experiments. *Games and Economic Behavior* 6, 347–369.
- Frohlich, N. and J. Oppenheimer (1990). Choosing Justice in Experimental Democracies with Production. *American Political Science Review* 84(2), 461–477.
- Frohlich, N. and J. Oppenheimer (2005). Skating on Thin Ice: Cracks in the Public Choice Foundation. Working Paper University of Maryland.
- Frohlich, N., J. Oppenheimer, and C. Eavey (1987). Laboratory Results on Rawls’s Distributive Justice. *British Journal of Political Science* 17(1), 1–21.
- Hoffman, E., K. McCabe, K. Shachat, and V. Smith (1994). Preferences, Property Rights and Anonymity in Bargaining Games. *Games and Economic Behavior* 7, 346–380.
- Hoffman, E., K. McCabe, and V. Smith (1996). On Expectations and Monetary Stakes in Ultimatum Games. *International Journal of Game Theory* 25(3), 298–301.
- Kam, C., S. Cranmer, and J. Fowler (2007). When it’s not all about me: Altruism, participation, and political context. Social Science Research Network Working Paper Series.
- Konow, J. (1996). A Positive Theory of Economic Fairness. *Journal of Economic Behavior & Organization* 31(1), 13–35.
- Ledyard, J. O. (1995). Public Goods: A Survey of Experimental Research. In J. H. Kagel and A. E. Roth (Eds.), *The Handbook of Experimental Economics*, pp. 111–194. Princeton, New Jersey: Princeton University Press.
- List, J. and T. Cherry (2000). Learning to Accept in Ultimatum Games: Evidence from an Experimental Design that Generates Low Offers. *Experimental Economics* 3(1), 11–31.
- Michelbach, P., J. Scott, R. Matland, and B. Bornstein (2003). Doing Rawls Justice: An Experimental Study of Income Distribution Norms. *American Journal of Political Science* 47(3), 523–539.

- Roth, A. E., V. Prasnikar, M. Okuno-Fujiwara, and S. Zamir (1991). Bargaining and Market Behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo: An Experimental Study. *American Economic Review* 81(5), 1068–1095.
- Scott, J., R. Matland, P. Michelbach, and B. Bornstein (2001). Just Deserts: An Experimental Study of Distributive Justice Norms. *American Journal of Political Science* 45(4), 749–767.
- Slonim, R. and A. Roth (1998). Learning in High Stakes Ultimatum Games: An Experiment in the Slovak Republic. *Econometrica* 66(3), 569–96.
- Smith, J. (1975). A Clear Test of Rational Voting. *Public Choice* 23(1), 55–68.
- Straub, P. and K. Murningham (1995). An Experimental Investigation of Ultimatum Games: Information, Fairness, Expectations, and Lowest Acceptable Offers. *Journal of Economic Behavior and Organization* 27(3), 345–364.
- Verba, S. and K. Schlozman (1977). Unemployment, Class Consciousness, and Radical Politics: What Didn't Happen in the Thirties. *Journal of Politics* 39(2), 291–323.
- Wilson, J. Q. and E. C. Banfield (1964). Public-Regardingness as a Value Premise in Voting Behavior. *The American Political Science Review* 58(4), 876–887.