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Tax Policy Design in the Presence of Social Preferences:
Some Experimental Evidence

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Abstract: This paper reports the results of an experiment designed to examine whether a taste for fairness affects people's preferred tax structure. Using the Fehr and Schmidt (1999) model we devise a simple test for the presence of social preferences in voting for alternative tax structures. The experimental results show that individuals demonstrate concern for their own payoff and inequality aversion in choosing between alternative tax structures. However, concern for redistribution decreases as the deadweight losses from progressive taxation increase. Our findings have important implications for tax policy design. (JEL C92, D63, H21, H23; tax policy, social preferences, fairness.)

This paper examines whether a taste for fairness influences people's preferences among alternative tax structures. Using an experimental approach, we devise a simple test for social preferences in voting for alternative tax structures. We find that accounting for social preferences helps explain the choices among alternative tax structures of *some* individuals. However, we find that the willingness to accept a smaller payoff for greater distributional equity decreases as the deadweight loss from progressive taxation increases.

These findings have important implications for tax policy design. If, for example, individuals are averse to income inequality, tax structures that increase inequality may reduce individual utility. According to Alm (1998) and Andreoni, Erard, and Feinstein (1998) among others, the perceived fairness of a tax system also may influence voluntary tax compliance. Thus, a fundamental issue in

optimal tax theory is whether individual utility is based exclusively on one's own payoff.¹

Outside the context of tax policy design the existence of social preferences is now well established [Frohlich and Oppenheimer (1992), Ledyard (1995), Camerer (1997), Bolton and Ockenfels (2000), and Charness and Rabin (2002)].² Within the context of tax policy design, the most direct evidence bearing on the fundamental issue examined in this paper comes from the path breaking work of Frohlich and Oppenheimer (1992) and Engelmann and Strobel (2004).³ Frohlich and Oppenheimer (FO) use laboratory experiments to investigate which principle of distributive justice people choose, absent knowledge of their position in the income distribution. More specifically, they ask subjects to express a preference for a principle of distributive justice among several stylized principles, such as the maximin principle [Rawls (1971)] and the efficiency principle

¹ Optimal tax policy prescriptions are derived from the maximization of a social welfare function of individual utilities subject to a tax revenue constraint [see, for example, Ramsey (1927), Mirrlees (1971), Diamond and Mirrlees (1971), Martinez-Vazquez and Rider (1996), and McCaffery and Slemrod (2004)].

² There is substantial evidence suggesting that social preferences influence behavior in a variety of environments including firm pricing policies [Kahneman, Knetsch, and Thaler (1986)], ultimatum bargaining games [Güth, Schmittenger, and Tietz (1990)], and public goods experiments [Bagnoli and McKee (1991)] to name just a few. To be fair, there is some evidence suggesting that fairness considerations may not be important in other environments [e.g., Roth, Parnikar, Okuno-Fujiwara, and Zamir (1991) and Fehr and Schmidt (1999)].

³ Lewis (1978) and Gemmell, Morrissey, and Pinar (2004) use survey data to uncover individual preferences for progressive tax structures. Other studies use experiments to investigate preferences for the degree of tax progressivity [Hite and Roberts (1991)] and framing effects on preferences for progressive taxation [McCaffery and Baron (2004)]. Sheffrin (1994) provides a comprehensive review of papers in this area.

[Harsanyi (1953, 1955)] among others.⁴ FO find that most groups choose a mixed principle: they prefer to maximize average income as suggested by Harsanyi constrained by an income floor for the worst-off individual as suggested by Rawls.

FO do not directly address the choice of tax structure; rather their results imply that people care about both the efficiency and distributional consequences of tax policy. Though very instructive, their experiments do not provide quantitative evidence on the nature of the trade offs among the potentially conflicting goals of maximizing one's own payoff, maximizing the sum of individual payoffs, and maximizing the payoff of the worst off individual. In a related paper, Engelmann and Strobel (ES) use one-shot, distribution experiments to compare the performance of several behavioral models, including theories reflecting social preferences. Consistent with FO, ES conclude that theories of inequality aversion have no additional explanatory power in their data beyond what can be explained by the mixed rule of efficiency and maximin. They also conclude that theories of inequality aversion perform poorly in cases where a distribution with less inequality is Pareto dominated by another distribution.

The focus of the current paper is on the relevance of fairness motives in the choice of tax structures. We use Fehr and Schmidt's (1999) model of inequality aversion to test whether people are willing to choose a distribution with a smaller own payoff to achieve a more equitable distribution of after-tax payoffs. In the experiments reported below, we present participants with a simple task.

⁴ By efficiency we mean maximizing the sum of individual payoffs, rather than Pareto efficiency.

We randomly assign nine participants in each experimental session with a payoff that is uniformly distributed between \$10 and \$50, in increments of \$5. Then, the participants are asked to vote for either a uniform head tax or a progressive tax. The vote of the majority determines the tax structure and consequently the distribution of after-tax payoffs to the subjects. Thus, the laboratory experiments are designed to elicit individually held social preferences for redistributive taxation. Our central finding is that some people are willing to accept a smaller own after-tax payoff to reduce payoff inequality, but this apparent demand for fairness decreases as the cost of reducing inequality increases.

Our experiments differ from those of FO and ES in a number of ways. In particular, our experimental design allows us to gauge whether participants are willing to sacrifice in terms of a smaller after-tax payoff to reduce payoff inequality. ES's canonical experiments do not.⁵ This is a crucial difference because, in our view, a decision reflects a taste for fairness when the preferred outcome requires the decision-maker to sacrifice by reducing their own after-tax payoff to achieve a more equitable distribution of payoffs.

The paper proceeds as follows. In the next section, we describe a model that formalizes our notion of fairness. Section Three presents a summary of voting behavior, and Section Four discusses an econometric model and presents additional results. Section Five offers concluding comments.

⁵ Engelman and Strobel provide some evidence based on sessions in which the subjects know their position in the income distribution and are required to sacrifice to achieve greater equity and/or efficiency. However, these experiments are mentioned only in passing to test the robustness of results obtained from their canonical experiments with role uncertainty and the decision maker's own payoff is unaffected by the choice of distribution.

I. A Model of Social Preferences

As reviewed in the previous section, there is growing empirical evidence of social preferences. Although theoretical work continues on the form of such preferences, from our perspective, three recent models are particularly relevant in framing the way people vote for different tax structures. First, Charness and Rabin (2002) develop a model that combines social preferences, efficiency concerns, and reciprocity to predict behavior in economic experiments. In their two-person formulation, a player's utility is the weighted average of one's own payoff and the other player's payoff where the weight of the other player depends on payoff inequality and whether the other player has behaved fairly. Second, Bolton and Ockenfels (2000) argue that own payoff and relative standing can explain observed behavior in many economic games. In their formulation, however, players do not care about inequality among other players or efficiency. Finally, Fehr and Schmidt (1999) develop a model where own payoff and inequality aversion play key roles.

Several considerations are pertinent in choosing a theoretical framework to explain individual preferences among alternative tax structures. First, we are not concerned here with reciprocity because voting processes are often anonymous, which prevents participants from observing one another's voting behavior and rules out the ability to punish unfair play. Second, the choice of tax structure may be affected not only by income inequality aversion but also concerns about efficiency. Third, social preferences may take varied and complex forms in the population. An approach that allows some people to be

concerned with their own payoff and income inequality aversion is, we believe, sufficiently flexible to account for different possible voting behaviors. Fourth, people may be more apt to show social preferences in “low cost” voting environments when their decisions tend to matter less, as compared to “high cost” private choice environments, with the latter being the context of the three models summarized above.⁶

Fehr and Schmidt’s model provides a basis for our examination of social preferences. In their model, a player’s utility is a linear sum of one’s own payoff and the losses from disadvantageous and advantageous inequality.⁷ In Fehr and Schmidt’s model with n players, an individual’s utility depends on one’s own payoff and inequity aversion as follows:

$$U_i(\pi_1, \pi_2, \dots, \pi_n) = \pi_i - \delta_1 \left(\frac{1}{n-1} \right) \sum_{i \neq j} \max\{\pi_j - \pi_i, 0\} - \delta_2 \left(\frac{1}{n-1} \right) \sum_{i \neq j} \max\{\pi_i - \pi_j, 0\}.$$

In this model π_i is the monetary payoff to player i ; δ_1 is a parameter measuring the degree of disadvantageous inequality aversion; and δ_2 is a parameter measuring the degree of advantageous inequality aversion. The first term on the right-hand-side reflects player i ’s concern for his own payoff. The next two terms reflect i ’s utility loss from disadvantageous and advantageous inequality, respectively. Following Fehr and Schmidt, we assume that a player’s inequity aversion is self-centered, so that he does not care about inequities among other

⁶ Eichenberger and Oberholzer-Gee (1998) provide experimental evidence that is consistent with the argument that behavior is fairer in a political sphere because social norms can be observed through voting behavior, which is nearly costless to observe.

⁷ Inequality is disadvantageous when a reference individual earns more than the person evaluating the outcome. Advantageous inequality arises when the reference individual makes less than the evaluator.

players. Finally, we normalize the disutility from inequity aversion by $n - 1$ in order to ensure that the effect of inequity aversion is independent of the number of players. As discussed below, this model of individual utility can be adapted to predict an individual's vote between two tax structures and our experimental design allows us to test for social motives.⁸

II. Experimental Design and Method

The choice of tax structure in a democratic country is a complex process, but ultimately it can be reduced to voters supporting different political platforms with tax proposals that differ in terms of the associated efficiency losses and the distribution of tax burdens. In our experimental setting individuals may express a preference for distributional equity by voting for a progressive tax, some of which involve efficiency losses.

There are nine experimental sessions, each consisting of a series of five trials or rounds.⁹ Nine university students participate in each session, which are completed in approximately 30 minutes.¹⁰ The average age of participants is 22.0

⁸ One feature of this model is the potential for high correlation among the three terms. We discuss the empirical implications of this correlation below.

⁹ Since learning may occur, we perform a statistical test for 'round' effects. As discussed below, the test rejects round effects at conventional levels of significance. However, repeated game effects, such as "cooperative" choices, should not be of concern because the nine participants in each of our experimental sessions vote anonymously. Further to ensure that cooperation is not a determinant of behavior we include Treatment 3, as described subsequently. In this treatment participants do not know the outcome of the majority vote each round until the conclusion of the session.

¹⁰ To establish a consistent pre-tax payoff distribution across the nine sessions, it is necessary to choose a fixed number of participants for each session. We eliminate the possibility of a split majority or 50-50 vote by requiring an odd

years, and no participant takes part in more than one session.¹¹ After they arrive for the experiment, participants receive a set of instructions and follow along as an experimenter reads aloud. The instructions are provided in Appendix 1. The experimental design is summarized in Table 1 and described below.

In all sessions, participants are endowed with an income or pre-tax payoff that is theirs to keep for participating in the study except that they must pay a tax. Each participant's pre-tax payoff or experimental income is determined by drawing a card from a set of nine cards. The following incomes are recorded on the cards: \$10, \$15, \$20, \$25, \$30, \$35, \$40, \$45 and \$50. They also are reminded that because each income is equally likely, the average pre-tax payoff across participants is \$30.¹² Within each period income cards are drawn without replacement and participants are instructed that their income is private information that should not be revealed at any time during the session.

In all treatments the tax is one of two types. As Table 2 reports, Tax 1 is a lump-sum head tax of \$7.50, and Tax 2 is a progressive income tax. Notice from Panel A of Table 2 that the sum of after-tax payoffs in Treatments 1 through 3 is

number of subjects. We settled on nine participants in order to provide anonymity among the participants on the one hand and to limit the total cost of the experiments on the other.

¹¹ Some critics question the relevance of insight provided by experiments with student subject pools. If the average person behaves differently from university students, we should be cautious in drawing inferences about the general population. However, there is some evidence that the behavior of subjects drawn from other populations is not markedly different from the behavior of student subjects [Davis and Holt (1993, page 17)].

¹² Of course, people may behave differently when their income is earned rather than endowed. In particular, people may be more predisposed to fairness when endowed. Hoffman, McCabe, Shachat, and Smith (1994) provide experimental evidence that first movers in dictator and ultimatum games offer less when the first mover is decided by the higher score on a general knowledge test.

\$202.50 for both the head tax and the progressive tax. In other words, the two tax regimes are revenue neutral.¹³ For Treatments 1 through 3, four of the nine participants receive a higher after-tax payoff under the head tax, as noted in Table 1. Four (4) low-income participants receive a higher after-tax payoff under the progressive tax; whereas, the median participant with a pre-tax payoff of \$30 receives the same after-tax payoff under both tax structures.¹⁴ If the participants care only about their after-tax payoff, high-income participants (i.e., pre-tax payoff > \$30) prefer the head tax; low-income participants (i.e., pre-tax payoff < \$30) prefer the progressive tax; and the median income participant (i.e., pre-tax payoff = \$30) is indifferent between the two.

The subjects' after-tax payoffs are determined by the vote of the majority in the choice between the head tax (Tax 1) and the progressive tax (Tax 2).¹⁵ Participants are given 5 minutes to indicate the preferred tax. In Treatments 1 and 2, the votes are tallied; the chosen tax structure is publicly announced after each round of voting; and participants are reminded that their after-tax payoff is private information that should not be disclosed at any time. In Treatment 3 the result of each round of voting is withheld from the participants until the conclusion

¹³ In Treatments 4 and 5, we simulate the excess burden of progressive taxation by using two tax regimes that are not revenue neutral, as discussed below.

¹⁴ During the experiments, we used the more neutral terms Tax 1 and Tax 2 rather than lump-sum tax and progressive tax to refer to the two tax regimes in order to avoid unintentionally biasing the responses.

¹⁵ Clearly, we are concerned in this paper with outcome fairness. There is a related literature that examines process fairness. By using majority voting to decide the outcome, we have invoked a process, which we believe to be widely accepted as process fair. However, it would be interesting in future research to examine the sensitivity of the results reported here to alternative decision rules such as super majority or weighted voting schemes.

of all five rounds to control for the potential influence on individual voting of observing whether others are ‘cooperating’ by voting for the progressive tax.

These procedures are repeated five times in each session. As the instructions indicate, participants are told at the outset that they will be paid according to the results of only one of the trials, and this trial will be chosen by a card draw at random by one of the participants from a set of cards labeled 1-5. Since *ex ante* the students have no way of knowing which trial is the payout trial, it is in their interest to treat all trials with equal seriousness.¹⁶

Treatment 1 differs from Treatments 2 and 3 in that participants in Treatment 1 vote for a tax structure *before* they draw an income card. In other words, participants in Treatment 1 vote before they know their pre-tax payoff. Since they do not know their place in the income distribution when they vote, they have no way of knowing whether the chosen tax regime increases or decreases their after-tax payoff, and, in that sense, they vote ‘as if behind a veil of ignorance,’ to use a phrase coined by Rawls (1971). In Treatments 2 and 3 participants vote after drawing an income card; so they know their standing in the income distribution when they vote. These treatments allow us to see if knowledge of one’s position in the income distribution, and thus the effect of the vote on their own after-tax payoff, changes the outcome of the vote.

At the conclusion of each session, participants complete a post-experiment questionnaire, designed to collect demographic information. Consideration of demographic information does not indicate any notable

¹⁶ As part of the empirical analysis described subsequently, we include dummy variables for the rounds and find no evidence of a round effect.

differences between participants across all sessions (1-10), as expected given that all subjects are recruited from the same pool. In addition, their responses on the questionnaire indicate that they found the experiment interesting and the monetary incentives motivating. Participants respond on an eleven-point scale as to how interesting they found the experiment, where 1 = not very interesting and 11 = very interesting. The mean response across all treatments is 9.0. Participants also respond on an 11-point scale as to how they would characterize the amount of money earned for taking part in the experiment, where 1 = nominal amount and 11 = considerable amount. The mean response across all treatments is 10.0.¹⁷

III. Summary of Voting Behavior

A. Revenue Neutral Treatments

Table 3 summarizes the preferences of participants as revealed by their voting behavior. In Treatment 1, the majority vote for Tax 2 (the progressive tax) in 11 of 15 trials. In these sessions, participants do not know their income level when they cast their votes, and the majority chooses the progressive tax. This result supports the view that people care about the distributional consequences of taxation and not simply their own payoff. Since the participants do not know their pre-tax payoff, however, some may be attempting to pool payoff risk by

¹⁷ Participants are paid \$2 for timely arrival to the experimental session and \$2 for completion of a post-experiment questionnaire. Thus, while the average after-tax earnings in the revenue-neutral treatments are \$22.50, the average compensation is \$26.50 with a range of \$6.50 to \$46.50.

voting for the progressive tax, reflecting risk aversion rather than payoff inequality aversion.

To control for this potential confound, we allow participants to observe their pre-tax payoff before voting.¹⁸ This experimental design also allows us to observe whether participants are willing to sacrifice income to satisfy a taste for fairness; if in fact a taste for fairness drives the previous finding. In Treatment 2, the majority votes for the progressive tax (Tax 2) in 10 of 15 trials. As we will see, it is not necessarily the median participant with a pre-tax payoff of \$30 who is decisive. Thus, some participants vote for the progressive tax even though they suffer in terms of their own after-tax payoff. In other words, it appears that some participants are prepared to pay or sacrifice income in order to satisfy a taste for fairness. In contrast, some median income participants vote against the progressive tax even though their after-tax payoff is unaffected by their choice of distribution. This evidence of heterogeneity in the population may play an important role in distribution experiments and may explain ES's conclusion that theories of inequality aversion do not provide additional explanatory power beyond that afforded by the efficiency and maximin principles.

An additional concern about the experimental design described above may stem from participants attempting to play cooperative strategies. When participants know their income before they vote and the majority vote is revealed

¹⁸ In a strict sense, removing the “veil of ignorance” only removes the uncertainty regarding one’s income. Uncertainty remains as to how others will vote, and we cannot rule out an effect on voting from this type of uncertainty. However, uncertainty regarding how others will vote is equally present behind the “veil of ignorance.”

after each round, high-income participants in a given round may attempt to 'signal' a cooperative strategy by voting for the progressive tax, even though doing so reduces their after-tax payoff in that round. In this manner, they may try to elicit cooperation to 'insure' against the risk of being low income in subsequent rounds. A solution to this potential confound is a treatment in which participants receive no information about the majority vote in each round until the end of the session. Thus, in Treatment 3 we withhold the result of each voting round until the conclusion of the experiment to control for the potential influence of 'cooperation' on individual voting. The results of Treatment 3 are reported in Table 3 and indicate that the majority votes for the progressive tax (Tax 2) in 11 of 15 trials. Since these results are very similar to those obtained with Treatment 2 (10 of 15), votes by high income participants for the progressive tax do not appear to reflect attempts to signal a cooperative strategy.

Table 4 provides additional insight into the behavior of our experimental participants. As one might expect, in Treatment 1 where the participants vote 'behind a veil of ignorance,' voting for either Tax 1 or Tax 2 shows no pattern across payoffs. Interestingly, however, we observe approximately the same proportion of total votes for the progressive tax (Tax 2) in Treatment 2 (56 percent or 76/135), in which pre-tax payoffs are known before voting as when they are not, as in Treatment 1 (60 percent or 81/135).

In Treatment 2, we observe a clear pattern in voting behavior with low (high) payoff individuals showing a strong preference for the progressive tax (head tax). It is impossible to infer whether low payoff individuals are voting for

the progressive tax out of concern for their own payoff and/or due to disadvantageous inequality aversion because the two motives are congruent. However, we also see that the median-income participant whose pre-tax payoff is \$30 votes only slightly more often for Tax 2 (53 percent or 8/15).¹⁹ Significantly in terms of our hypothesis, some individuals vote for the progressive tax (Tax 2) even when it is clearly not in their monetary self-interest to do so. In fact, we observe more votes for Tax 2 by high-income participants (15.0 percent or 9/60 votes) as compared to votes for Tax 1 (head tax) by low-income participants (1.6 percent or 1/60 votes).

For Treatment 3 in which participants do not know the results of previous rounds of voting, the results are very similar. As in Treatment 2, we observe a strong preference for the progressive tax (56 percent or 75/135) and more high-income participants vote for Tax 2 (15.0 percent or 9/60 votes) as compared to votes for Tax 1 by low-income participants (0 percent or 0/60 votes).

Table 4 also provides further insight into voting behavior. For each of Treatments 2 and 3 (pre-tax income is known before voting), nine out of a possible 60 votes cast are for the progressive tax by participants with a pre-tax payoff greater than the median and therefore risk a smaller own payoff by voting

¹⁹ The median voter hypothesis predicts that the decisive voter should be the median payoff participant as long as preferences are monotonic over the issue in question. In Treatment 2, the median payoff participant is decisive in 80 percent of the trials and only 55 percent of the trials in Treatment 3. Since tastes for redistribution may be heterogeneous, preferences are not necessarily monotonic. Therefore, the median voter hypothesis may not be applicable in redistribution settings. This observation may have important implications for ES's experimental design in which the median payoff participant is the sole decision maker. We would like to thank an anonymous referee for bringing this issue to our attention.

for the progressive tax (Tax 2). Eight participants cast these eighteen votes and no more than two participate in any session. We refer to these subjects as fairness-minded. Six of the eight always vote for the progressive tax (Tax 2), regardless of their income level.

Demographic information indicates the following. The median participant is in the third year of university study. Three fairness-minded participants are in their first year at the university, two are in their third year, and three are in their fourth year. Four fairness-minded participants are men, and four are women. The median participant in our experiment and the median fairness-minded participant both report income in the \$25,001 to \$50,000 range. Thus, there is no apparent demographic link across participants who are fairness-minded. Overall the results support the hypothesis that *some* subjects exhibit social preferences and prefer a more equitable distribution of after-tax payoffs, even if it is personally costly to do so.

B. *Treatments with a Distortionary Progressive Tax*

Because previous experimental evidence suggests that some people care about efficiency or maximizing the sum of payoffs, we conduct two additional treatments. In Treatments 4 and 5, we require participants to choose between two tax regimes that are not revenue neutral. In other words, the sum of the after-tax payoffs from the 'distortionary' progressive tax is less than that of the lump-sum head tax. Notice from Panel B of Table 2 that in Treatment 4, the sum of the after-tax payoffs is \$247.50 with the head tax; whereas the sum of the after-tax payoff with the progressive tax is \$202.50. With the exception of the two lowest

pre-tax payoff participants (\$10 and \$15), all the subjects have an economic incentive – absent inequality aversion - to vote for the head tax (Tax 1).

Turning to Treatment 5, the after-tax payoffs are summarized in Panel C of Table 2. Again, the tax regimes are not revenue neutral. The sum of after-tax payoffs with the lump-sum head tax is \$225.00; whereas the sum of after-tax payoffs with the distortionary progressive tax is \$202.50. Now, five of nine participants have an economic incentive – again absent inequality aversion – to vote for the head tax (Tax 1). In short, the tax regimes in Treatments 4 and 5 are not revenue neutral; otherwise the procedures are identical to those described above for Treatment 2.

Table 3 reports the voting behavior of the majority in Treatments 4 and 5. In contrast to the behavior observed in Treatments 2 and 3, the majority of participants votes against the progressive tax. In Treatment 4, the majority votes for the lump-sum head tax in every trial. In Treatment 5, the majority votes for the lump-sum head tax in eight out of ten trials. In other words, when the tax is not revenue neutral, fewer people are willing to sacrifice in terms of a smaller own after-tax payoff to reduce payoff inequality. This result is suggestive: Individuals care about distributional equity, but they also care about the total size of the pie or efficiency.²⁰

Votes by income level, reported in Table 4, support this interpretation. Few people with high incomes (i.e., pre-tax payoff > \$30) vote for the progressive tax.

²⁰ As the excess burden of the distortionary progressive tax increases, we observe the number of people voting for the progressive tax (Tax 2) decreases. This suggests that there may be a downward sloping demand for fairness. This is an interesting issue for further study.

In Treatment 4, all participants with incomes greater than or equal to \$20 have an economic incentive – absent distributional considerations - to vote for the head tax (Tax 1). We observe only 2.0 percent (1/50 votes) of high-income votes for the progressive tax (Tax 2) in Treatment 4. Similarly, in Treatment 5 only 4.0 percent (2/50) of high-income voters (pre-tax payoff \geq \$30) choose the progressive tax (Tax 2).

However, the votes of participants with pre-tax payoffs less than \$20 in Treatment 4 are particularly suggestive regarding social preferences assuming the form of a taste for efficiency. Low-income participants (pre-tax payoffs $<$ \$20) in Treatment 4 benefit in terms of own payoff from the progressive tax (Tax 2). Yet, as Table 4 shows, 20 percent (5/20) of these participants vote for the lump-sum tax, which results in a smaller own payoff.

Turning to a similar analysis of Treatment 5, those with pre-tax payoffs less than \$30 benefit in terms of own payoff under the progressive tax (Tax 2). In contrast to Treatment 4, now, only 13 percent (2/15) vote for the lump-sum tax and against their self interest in terms of own payoff. As Table 4 shows, however, the benefit in terms of the increase in the sum of total payoffs of voting for the lump-sum tax is smaller, and the cost in terms of own payoff is larger for these low-income participants in Treatment 5 than in Treatment 4. In other words, the price of fairness has increased for these low-income participants in Treatment 5 relative to the price facing them in Treatment 4. The increasing price of fairness may account for the smaller percentage of low-income participants willing to vote for the efficient, lump-sum tax in Treatment 5.

In short, there is evidence that *some* participants exhibit social preferences among tax structures in the form of inequality aversion and concern for efficiency. This is evidenced by those who vote for tax structures that impose a sacrifice in terms of a smaller own payoff with the only apparent compensating benefits being reduced payoff inequality in the case of high-income participants and a more efficient tax in the case of some low income participants in Treatments 4 and 5. Finally, the percentage of fairness-minded votes decreases as the cost of achieving these social goals increase in terms of a reduced own payoff. This suggests a downward sloping demand for fairness.

IV. Statistical Model and Results

The foregoing discussion provides a number of interesting insights. Generally speaking, the results are consistent with the predictions of the Fehr-Schmidt model of inequality aversion. Yet, we recognize that preferences may take varied and complex forms. An approach that allows some people to be concerned with their own payoff, efficiency (e.g, the sum of total after-tax payoffs), and income inequality aversion provides flexibility to account for a variety of possible voting behaviors.²¹ However, an examination of these three motives in a single empirical model is problematic because, as shown in Appendix 2, there is an exact linear relationship between the sum of total payoffs, own payoff, disadvantageous inequality, and advantageous inequality.²² Thus, to

²¹ As described previously, efficiency is measured by the sum of the individual payoffs.

²² Appendix 2 includes a proof of the proposition that the change in the sum of total payoffs is a linear combination of changes in own payoff, disadvantageous inequality, and advantageous inequality.

perform a more rigorous test of the observations reported in the previous sections, we evaluate two models relative to the Fehr-Schmidt model, as described subsequently.

An empirical analog of the Fehr-Schmidt model, which predicts voting behavior in our experimental environment, is given as follows:

$$y_i^* = (B_0 + \alpha_i^*) + B_1 \Delta \pi_i + B_2 \Delta \left(\frac{1}{n-1} \right) DI_i + B_3 \Delta \left(\frac{1}{n-1} \right) AI_i + \varepsilon_i.$$

In this model, y_i^* is a latent-continuous-random-variable representing the difference in utility from a head tax (Tax 1) relative to a progressive tax (Tax 2). The right-hand-side variables include the difference in own-payoff, the difference in disadvantageous inequality, and the difference in advantageous inequality.²³ The difference in utility of a particular choice cannot be observed, but we can observe the individual's vote. Since voting for a binary choice is incentive compatible, we assume that an individual votes in favor of the tax structure that maximizes own utility, and

$$Vote_i = \begin{cases} 1 & \text{if } y_i^* > 0. \\ 0 & \text{if } y_i^* \leq 0. \end{cases}$$

If individual i prefers a head tax, then $y_i^* > 0$ and $Vote_i$ assumes a value of 1. On the other hand, if i prefers a progressive tax as evidenced by a vote for a progressive tax, then $y_i^* \leq 0$ and $Vote_i$ assumes a value of 0.

The analysis of voting behavior by income suggests that there may be heterogeneous tastes for fairness in our sample. To control for such unobserved heterogeneity, we include α_i^* , which is a latent random variable that reflects

²³ DI_i and AI_i are as defined previously in Section 1.

unobserved idiosyncratic tastes for fairness. Finally, the random error of the structural model is given by ε_i .

To provide insight into the relative ability of the Fehr-Schmidt model to predict observed voting behavior in our experiments, we estimate two additional models. The conventional model of purely selfish preferences which is given as follows:

$$y_i^* = (B_0 + \alpha_i^*) + B_1 \Delta \pi_i + \varepsilon_i.$$

In this model the difference in own after-tax payoffs is the only explanatory variable. To account for social preferences assuming the form of a concern for efficiency, irrespective of distributional concerns, we estimate a model that accounts for the difference in own payoff and the difference in the sum of after-tax payoffs or the S/E model which is given as follows:

$$y_i^* = (B_0 + \alpha_i^*) + B_1 \Delta \pi_i + B_2 \Delta E_i + \varepsilon_i.$$

This model reflects concern for own payoff and efficiency, where ΔE_i is the difference in the sum of after-tax payoffs in the two tax regimes.²⁴ We test whether the purely selfish or the S/E model provides a superior fit to the experimental data than the Fehr-Schmidt model.

In estimating the three models, we are confident that α_i^* is statistically independent of the other regressors, as required by the random effects probit specification. Our confidence is based on the fact that the regressors for each observation depend on pre-tax income, which is randomly assigned to

²⁴ We also used as an alternative measure of efficiency a dummy variable taking the value of 1 for Treatments 4 and 5, which are not revenue neutral. Inferences are similar to those reported subsequently.

participants by the draw of a card. We also include a constant in each regression; therefore, α_i^* measures deviations from a common mean. Lacking any information about the distribution of α_i^* , it seems reasonable to assume that deviations from a common mean are normally distributed in the student population from which we draw our sample.

Accordingly, we assume $\alpha_i^* \sim N(0, \sigma_\alpha^2)$ and $E[\alpha_i^* | \pi_t, E_i, DI_i, AI_i, \varepsilon_i] = E[\alpha_i^*]$, where DI_i and AI_i are the indices of disadvantageous and advantageous inequality, respectively. By further assuming the error term ε_i has a standard normal distribution, it follows that our specification is a multivariate probit model.²⁵ Therefore, we estimate the three empirical models using random effects probit.²⁶

Our experimental design elicits repeated binary choices by 90 subjects. The resulting data include 450 votes from Treatments 2, 3, 4 and 5.²⁷ Our results are very similar to those reported subsequently whether we estimate random-

²⁵ The assumption of unit variance is an innocent normalization [see, for example, Greene (2002)]. Also, Hsiao (2003) provides an excellent discussion of discrete choice as well as fixed and random effects models.

²⁶ For the random-effects model, the likelihood (for an independent unit i) is expressed as an integral, which is computed in STATA using Gauss-Hermite quadrature. STATA recommends that the fitted model be evaluated for sensitivity to the chosen number of quadrature points. As a rule of thumb, if the coefficients do not change by more than a relative difference of 0.01 percent, then the choice of quadrature points does not significantly affect the outcome and the results may be confidently interpreted. When we change the number of quadrature points by ± 4 points, our estimates do not change by more than the indicated 0.01 percent.

²⁷ The results of Treatment 1 are not used to estimate these models. To calculate the regressors in these models, we need a reference payoff. Since the pre-tax income is unknown to the subjects before voting in Treatment 1, the choice of reference income for the calculations of the regressors is not obvious. In any event, the regressors would be identical for any choice of reference income. For example, the expected payoff is an obvious candidate to serve as the reference income. However, the regressors would be identical for every observation for this or any other choice of reference income in Treatment 1.

effects or fixed-effects linear probability (logit) models. In contrast to ES's one shot design, our experimental design allows us to control for individual effects, which Charness and Rabin (2002) and the analysis of voting behavior in the previous section suggest may be important in experiments designed to elicit tastes for distributional equity.

Table 5 reports estimated coefficients, t-statistics in parentheses, and marginal effects in brackets for the three specifications. Recall that the estimated coefficients of a probit equation indicate the direction of change due to a vote for a head tax (Tax 1); whereas the marginal effects show the change in the probability of a vote for the head tax due to a unit change in the corresponding independent variable.²⁸ Below the number of observations, the table reports a χ^2 statistic for a test of the linear restriction among the coefficients implied by the model with respect to the Fehr-Schmidt model. The implied linear restrictions are described in Appendix 2. Finally, the table reports goodness of fit measures, including Wald's χ^2 test statistic for the significance of the regression, the estimated value of the log-likelihood function, and McFadden's Adjusted Pseudo- R^2 .²⁹

²⁸ To test for learning or order effects, we include individual dummy variables for each round, dropping the dummy variable for round 5. This vector of estimated coefficients is statistically insignificant, and the other results are virtually identical to those in Table 5. We also estimate the regressions in Table 5 with a dummy variable for gender (female = 1). This estimated coefficient is statistically insignificant at conventional levels. These results are available from the authors upon request.

²⁹ Like a conventional R^2 in the context of ordinary least squares, McFadden's Pseudo- R^2 increases as the number of regressors in the model increases. Accordingly, we adjust the likelihood ratio index by subtracting K, the number of

In general, the estimated coefficients of the three models take the expected signs and are statistically significant at conventional levels, except for the measure of disadvantageous inequality, which is statistically insignificant (p-value = 0.115).³⁰ In the purely selfish model, a vote for a head tax is positively related to the change in own payoff. In the S/E model, a subject is more likely to vote for a head tax as the deadweight loss from the progressive tax increases. Finally, as predicted by the FS-model of social preferences, subjects are averse to payoff inequality as evidenced by the negative and statistically significant, estimated coefficient of the difference in the index of advantageous inequality.³¹ The likelihood ratio tests reject the linear restrictions implied by the purely selfish and S/E models in favor of the unrestricted FS-model; both models are rejected with a p-value less than 0.01. The values of the χ^2 test statistics (degrees of freedom = 2) are reported in Table 5.

regressors in the model, from the numerator of the index. See McFadden (1984) and note number 4 of Table 5 for further details.

³⁰ We estimate variants of the regressions reported in Table 4 by including a dummy variable for Treatment 3. The estimated coefficient of this dummy variable is statistically insignificant at conventional levels. We also estimate all three models on the 315 observations from Treatments 2, 4 and 5. Again, these estimates are nearly identical to those reported in Table 5. This supports our conclusion in the previous section that there does not appear to be any attempt to signal cooperation in Treatments 2, 4 and 5 when the majority vote is announced after each trial. These results are available from the authors upon request.

³¹ As previously discussed, there is nearly exact multicollinearity among the regressors of the FS model. A recommended remedy for near exact multicollinearity is to increase the sample size. Tellingly, the significance level of the estimated coefficient of DI increases when the sample size increases from 315 observations (p-value = 0.312) to 450 observations (p-value = 0.115). This suggests that a further increase in the sample size may result in a statistically significant estimate. These results are available from the authors upon request.

In summary, our evidence suggests that the FS model of social preferences best explains the decisions made by our experimental participants. Though we cannot separate the impacts of own payoff, efficiency, and inequity aversion, the empirical analysis supports the assertion that social preferences matter.

V. Conclusion

We report results of a simple experiment designed to examine individual preferences among tax structures. We find that *some* individuals may possess social preferences or concern for one's own payoff as well as the payoff of others. This finding may have important implications for tax policy design. For example, ours is a cautionary message for advocates of substituting a consumption tax for the current federal income tax. Proponents of this policy ostensibly believe that the potential benefit of efficiency gains from substituting a consumption tax for the current federal income tax more than compensate for the potential cost of greater after-tax income inequality. Our experimental evidence suggests that at least some individuals may be far more concerned with the distributional consequences of tax policy design than generally recognized by some economists and policymakers. In short, economic models that do not account for social preferences may provide a faulty guide to tax policy design.

TABLE 1—EXPERIMENTAL DESIGN

Treatment	Sessions	Vote before or after income revealed?	Results of vote reported after each round?	Amount of head tax	Number with higher after-tax payoff with head tax
1	1-3	Before	Yes	\$7.50	4 out of 9
2	4-6	After	Yes	\$7.50	4 out of 9
3	7-9	After	No ¹	\$7.50	4 out of 9
4	10-11	After	Yes	\$2.50	7 out of 9
5	12-13	After	Yes	\$5.00	5 out of 9

¹ The result of each round of voting is withheld from the participants until the conclusion of all five rounds to control for the potential influence on individual voting of observing whether others are 'cooperating' by voting for the progressive tax.

TABLE 2—TAX STRUCTURES

Panel A: Treatments 1 through 3 (Sessions 1-9)

Pre-Tax Income	Tax 1: Head tax		Tax 2: Progressive tax	
	Tax	After-tax payoff	Tax	After-tax payoff
\$10.00	\$7.50	\$2.50	\$0	\$10.00
\$15.00	\$7.50	\$7.50	\$2.00	\$13.00
\$20.00	\$7.50	\$12.50	\$3.00	\$17.00
\$25.00	\$7.50	\$17.50	\$4.00	\$21.00
\$30.00	\$7.50	\$22.50	\$7.50	\$22.50
\$35.00	\$7.50	\$27.50	\$11.00	\$24.00
\$40.00	\$7.50	\$32.50	\$12.00	\$28.00
\$45.00	\$7.50	\$37.50	\$13.00	\$32.00
\$50.00	\$7.50	\$42.50	\$15.00	\$35.00
Total	\$67.50	\$202.50	\$67.50	\$202.50

Panel B: Treatment 4 (Sessions 10-11)

Pre-Tax Income	Tax 1: Head tax		Tax 2: Progressive tax	
	Tax	After-tax payoff	Tax	After-tax payoff
\$10.00	\$2.50	\$7.50	\$0	\$10.00
\$15.00	\$2.50	\$12.50	\$2.00	\$13.00
\$20.00	\$2.50	\$17.50	\$3.00	\$17.00
\$25.00	\$2.50	\$22.50	\$4.00	\$21.00
\$30.00	\$2.50	\$27.50	\$7.50	\$22.50
\$35.00	\$2.50	\$32.50	\$11.00	\$24.00
\$40.00	\$2.50	\$37.50	\$12.00	\$28.00
\$45.00	\$2.50	\$42.50	\$13.00	\$32.00
\$50.00	\$2.50	\$47.50	\$15.00	\$35.00
Total	\$22.50	\$247.50	\$67.50	\$202.50

Panel C: Treatment 5 (Sessions 12-13)

Pre-Tax Income	Tax 1: Head tax		Tax 2: Progressive tax	
	Tax	After-tax payoff	Tax	After-tax payoff
\$10.00	\$5.00	\$5.00	\$0	\$10.00
\$15.00	\$5.00	\$10.00	\$2.00	\$13.00
\$20.00	\$5.00	\$15.00	\$3.00	\$17.00
\$25.00	\$5.00	\$20.00	\$4.00	\$21.00
\$30.00	\$5.00	\$25.00	\$7.50	\$22.50
\$35.00	\$5.00	\$30.00	\$11.00	\$24.00
\$40.00	\$5.00	\$35.00	\$12.00	\$28.00
\$45.00	\$5.00	\$40.00	\$13.00	\$32.00
\$50.00	\$5.00	\$45.00	\$15.00	\$35.00
Total	\$45.00	\$225.00	\$67.50	\$202.50

TABLE 3 — MAJORITY VOTING

Treatment	Sessions	Number of sessions with majority voting for		Percentage of majority vote	Percentage of votes for Tax 1
		Tax 1 (Head tax)	Tax 2 (Progressive tax)		
1	1-3	4	11	0.64	0.40
2	4-6	5	10	0.56	0.44
3	7-9	4	11	0.55	0.44
4	10-11	10	0	0.81	0.81
5	12-13	8	2	0.54	0.56

TABLE 4—VOTING BY INCOME

Income	Treatment 1		Treatment 2		Treatment 3		Treatment 4		Treatment 5	
	Sessions 1-3		Sessions 4-6		Sessions 7-9		Sessions 10-11		Sessions 12-13	
	Tax 1	Tax 2	Tax 1	Tax 2	Tax 1	Tax 2	Tax 1	Tax 2	Tax 1	Tax 2
10	9	6	1	14	0	15	2	8	0	10
15	4	11	0	15	0	15	3	7	0	10
20	6	9	0	15	0	15	9	1	0	10
25	7	8	0	15	0	15	10	0	2	8
30	4	11	7	8	10	6	10	0	9	1
35	7	8	13	2	13	1	10	0	10	0
40	2	13	13	2	13	2	10	0	10	0
45	7	8	11	4	12	3	9	1	9	1
50	8	7	14	1	12	3	10	0	10	0
Total	54	81	59	76	60	75	73	17	50	40

Note: Tax 1 is a head tax; Tax 2 is a progressive tax.

TABLE 5—RANDOM EFFECTS PROBIT REGRESSIONS OF VOTING

Independent Variable	Purely Selfish Model	SE Model	Fehr-Schmidt Model
Constant	-0.3504 (-1.85)*	-0.6153 (2.57)***	1.9988 (2.24)**
Change in own after-tax payoff	0.4804 (9.99)*** [0.1783]	0.4814 (9.78)*** [0.1791]	0.7006 (6.05)*** [0.2722]
Change in the sum of total after-tax payoffs	-	0.0191* (1.85) [0.0071]	-
Change in disadvantageous inequality	-	-	-0.2450 (-1.58) [-0.0952]
Change in advantageous inequality	-	-	-0.6172 (-3.50)*** [-0.2398]
Number of Observations	450	450	450
χ^2 test of the significance of the model against the FS-model	12.69***	9.20***	-
χ^2 test of the significance of the regression	99.74***	95.91***	96.25***
Estimated value of the log-likelihood function	-118.15	-116.41	-111.81
McFadden's Adjusted Pseudo-R ²	0.62042	0.6167	0.6313

Notes:

1. In the upper panel, estimated coefficients are reported at the top of each cell; below which t-statistics for the estimated coefficients are reported in parentheses (); and below that the corresponding marginal effects are reported in brackets [].
2. Although we do not report t-statistics for the marginal effects, there is no change in significance for any of the variables.
3. A single asterisk (*) indicates that the estimate is significant at the 10-percent level, a double asterisk (**) indicates significance at the 5-percent level, and a triple asterisk (***) indicates significance at the 1-percent level.

4. McFadden's Adjusted Pseudo-R² = $1 - \left(\frac{\ln \hat{L} - K}{\ln L_0} \right)$; where $\ln[L_0]$ is the maximized value of the log-likelihood

function computed with only a constant term; $\ln[\hat{L}]$ is the maximized value of the log-likelihood function for the model, and K is the number of regressors (see footnote 26 for further details).

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APPENDIX 1: EXPERIMENTAL INSTRUCTIONS

The experimental instructions for Treatment 2 follow. Changes in the instructions for other treatments are noted in brackets.

General Instructions

This experiment is concerned with the economics of decision-making. The instructions are simple, and if you follow them carefully and make good decisions, you might earn a considerable amount of money that will be paid to you in cash.

In this experiment you will be given an endowment of cash. This endowment is your income for participating in the experiment today except that you must pay a tax.

A Record Sheet is included with these instructions. You will keep track of your decisions on this sheet.

Specific Instructions

Your income, before taxes, is determined by drawing a card from a set of nine cards. The incomes recorded on the nine cards are as follows: \$10, \$15, \$20, \$25, \$30, \$35, \$40, \$45, and \$50. Notice that because each income level is equally likely, the average income is \$30 *before taxes are paid*.

The tax you pay on income will be one of two types. With Tax 1, all participants in this room will pay a tax of \$7.50. With Tax 2, the tax paid varies across income levels. The following table summarizes the tax that is paid for each income level.

Pre-Tax Income	Tax 1		Tax 2	
	Tax	After-tax payoff	Tax	After-tax payoff
\$10.00	\$7.50	\$2.50	\$0	\$10.00
\$15.00	\$7.50	\$7.50	\$2.00	\$13.00
\$20.00	\$7.50	\$12.50	\$3.00	\$17.00
\$25.00	\$7.50	\$17.50	\$4.00	\$21.00
\$30.00	\$7.50	\$22.50	\$7.50	\$22.50
\$35.00	\$7.50	\$27.50	\$11.00	\$24.00
\$40.00	\$7.50	\$32.50	\$12.00	\$28.00
\$45.00	\$7.50	\$37.50	\$13.00	\$32.00
\$50.00	\$7.50	\$42.50	\$15.00	\$35.00

[Note: The amount paid with Tax 2 varies in Treatments 4 and 5. The tax structures are reported in Table 2 of the paper.]

Whether the tax you pay is Tax 1 or Tax 2 will be determined by majority vote. After the experimenter distributes the income cards, you will be given 5 minutes to indicate which tax you prefer. When all participants have recorded their votes on their Record Sheets, the experimenter will tally the votes and report on the outcome. Please record the tax paid and your after-tax payoff on your Record Sheet. Your income is your private information and should not be disclosed to other participants at any time.

[Note: In Treatment 1 income cards are distributed after votes are recorded.]

We will repeat these steps 5 times. At the end of each trial, the tax chosen by the majority vote is announced. However, only one of the five trials will be binding. A number from one to five will be randomly selected to determine the binding trial. Your after-tax payoff from the binding trial is yours to keep and will be paid to you in cash.

[Note: In Treatment 3 the outcome of voting each round is not revealed until the conclusion of the session.]

Please do not confer with other participants in making your decisions at any time. Please remember that once you record your vote in each trial, you cannot change it.

Record Sheet

Trial	Your vote (Tax 1 or Tax 2)	Majority vote (Tax 1 or Tax 2)	Your pre-tax payoff (from your income card)	Your after-tax payoff
1				
2				
3				
4				
5				

APPENDIX 2: PROOF OF LINEAR DEPENDENCE

This appendix shows that an exact linear relationship exists between changes in own payoff, disadvantageous inequality, advantageous inequality, and the sum of total payouts.

Proposition 1: The change in the sum of total payoffs (ΔE) is an exact linear function of the sum of a change in own payoff, a change in the index of disadvantageous inequality, and a change in the index of advantageous inequality under any two tax regimes, or $\Delta E = n\Delta\pi_i + (n-1)^{-1}\Delta DI_i - (n-1)^{-1}\Delta AI_i$ for

all i . Where $\Delta E = \sum_{i=1}^n (\pi_{1i} - \pi_{2i})$ is the difference in the sum of the after-tax payoffs under tax regimes 1 and 2.

Before proceeding with the proof of this proposition, it is helpful to establish our notation and re-write some familiar expressions. Let π_{ki} be the after-tax payoff of individual $i = 1, \dots, n$ under tax regime $k = 1, 2$. Further, assume that the after-tax payoffs are ranked in ascending order such that $\pi_{k1} \leq \pi_{k2} \leq \dots \leq \pi_{ki} \leq \dots \leq \pi_{kn}$.

This allows us to re-write DI_{ki} and AI_{ki} as follows: $DI_{ki} = (n-1)^{-1} \sum_{\forall j>i}^n (\pi_{kj} - \pi_{ki})$ and

$AI_{ki} = (n-1)^{-1} \sum_{j=1}^{j<i} (\pi_{ki} - \pi_{kj})$. With these definitions in hand, we proceed to a proof of our proposition.

Proof:

We begin by writing the FS-model as follows:

$$nS_{ki} + (n-1)^{-1} DI_{ki} - (n-1)^{-1} AI_{ki}.$$

Now, we re-write the model and re-arrange terms.

$$\begin{aligned} nS_{ki} + (n-1)^{-1} DI_{ki} - (n-1)^{-1} AI_{ki} &= n\pi_{ki} + \sum_{j>i}^n (\pi_{kj} - \pi_{ki}) - \sum_{j=1}^{j<i} (\pi_{ki} - \pi_{kj}) \\ &= (n-1)\pi_{ki} + \left[\sum_{j=1}^{j\leq i} (\pi_{kj}) + \sum_{j>i}^n (\pi_{kj}) \right] - \left[\sum_{j=1}^{j<i} (\pi_{ki}) + \sum_{j>i}^n (\pi_{ki}) \right] = \sum_{i=1}^N \pi_{ki} = E_k, \forall i. \end{aligned}$$

Since Δ is a linear operator, this completes the proof.

This result is extremely useful. It shows that the Fehr-Schmidt model is flexible enough to represent a variety of theories of individual choice with different linear restrictions on the coefficients of the FS-model.

$$(1) \text{Vote}_i = a_1 + a_2\Delta S_i + a_3\Delta DI_i + a_4\Delta AI_i + e_i;$$

$$(2) \text{Vote}_i = a_1 + a_2\Delta S_i + e_i; \text{ and}$$

$$(3) \text{Vote}_i = a_1 + a_2\Delta S_i + a_3\Delta E + e_i,$$

Model (1) is the Fehr-Schmidt model of income inequality aversion.

Model (2) is the model of purely selfish preferences and is equivalent to (1) with two linear restrictions on the coefficients: $a_3 = a_4 = 0$.

Model (3) is Harsanyi's model of social preferences and is equivalent to (1) with two linear restrictions on the coefficients: $a_3 = a_4 = a_3'$.

These relationships allow us to use a likelihood ratio test to evaluate the linear restrictions implied by (2) and (3) with respect to the unrestricted model of (1). In each case, the likelihood ratio test statistic is distributed χ^2 , with degrees of freedom (df) equal to the number of restrictions (df = 2).