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Commitment versus Discretion in the Peasant–Dictator Game

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We report an experiment designed to test the influence of commitment versus discretion in a simple bargaining game. Game theory predicts and the public policy literature emphasizes that the ability to make commitments promotes efficiency. We find that commitment does significantly increase efficiency in the experiment. Finally, we relate our findings to the extant literature on extensive form bargaining experiments by examining fairness and trust as explanations for some observed anomalies. *Journal of Economic Literature* Classification Numbers: C720, C790, C920, E610. © 1995 Academic Press, Inc.

I. INTRODUCTION

The ability to bind one's future actions has long been recognized as an advantage when bargaining over a given surplus. A strategic analysis predicts that the party with the ability to make a commitment will receive all of the surplus. This outcome is often judged as unfair. However, in many situations a strategic analysis also predicts that in order to create an efficient surplus one of the parties must have an ability to make commitments.

The public policy games literature emphasizes the role commitment plays in promoting efficiency. For example, Kydland and Prescott (1977) describe numerous areas of public policy in which public commitments promote economic efficiency, including patents, flood plain projects, and investment tax credits. Barro and Gordon (1983) emphasize the role commitments play in promoting efficient monetary policy. Other examples include debt repudiation and capital income taxation.[1]

[1] See Persson and Tabellini (1990) for an introduction and references to the public policy literature. See Veitch (1986) on repudiations and confiscations by the medieval state and Eichengreen (1989) on capital levies during the interwar period. See Schelling (1960, 1980) on the role commitment plays in bargaining.

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For concreteness consider the following version of the capital income taxation problem: A peasant endowed with beans in the spring can either eat them or plant them in a field. If he plants them, he earns a gross rate of return greater than one. Efficiency requires that he plant the beans. However, the land is ruled by a dictator. If the peasant plants the beans, the dictator can tax his harvest in the fall. The dictator chooses the tax rate to maximize his tax revenue.[2] What tax rate does the dictator pick? How many beans does the peasant plant? How do the answers to these questions depend on whether the dictator can make a commitment?

We attempt to answer these questions by formalizing the peasant–dictator parable into a well-defined game, solving the game for the predicted outcomes, and studying the accuracy of these predictions in the laboratory. We find that commitment does significantly increase efficiency as predicted by the public policy games literature. Finally, we relate our findings to the extant literature on extensive-form bargaining experiments by examining fairness and trust as explanations for some observed anomalies.

II. Analytical Framework

Let W denote the peasant's endowment of beans, c_1 denote the number of beans consumed in the spring, k denote the number of beans planted in the spring, and c_2 denote the number of beans consumed in the fall; then the peasant's decision problem is

$$\begin{aligned} &\max_k U = c_1 + c_2 \\ &\text{s.t. } c_1 = W - k, \qquad c_1, k \geq 0, \\ &\qquad c_2 = (1 - \tau^e)(1 + r)k, \qquad r > 0, 0 \leq \tau^e \leq 1, \end{aligned} \tag{1}$$

where r denotes the rate of return on beans planted in the spring, U denotes the peasant's utility, and τ^e denotes the tax rate expected by the peasant. The crucial variable in the peasant's decision problem is the expected after-tax rate of return, $(1 - \tau^e)(1 + r)$. To see this, rewrite the peasant's decision problem by substituting the constraints c_1 and c_2 into the objective function, which gives

[2] Both benevolent and proprietary dictators have an incentive to tax capital income. Consider a benevolent dictator seeking to minimize welfare reducing distortions introduced by taxes used to finance a given level of useful public expenditures. Taxing labor introduces a distortion that lowers work effort and, hence, welfare, while taxing accumulated capital, since it is already installed, does not introduce any distortions and, hence, does not lower welfare. The proprietary dictator assumption is used in the parable for simplicity.

$$\max_{k \in [0, W]} U = W + ((1 - \tau^e)(1 + r) - 1)k. \tag{2}$$

Since the peasant's utility function is linear in consumption, we get the following solution:

$$k^* = \begin{cases} W & \text{if } (1 - \tau^e)(1 + r) > 1; \\ [0, W] & \text{if } (1 - \tau^e)(1 + r) = 1; \\ 0 & \text{if } (1 - \tau^e)(1 + r) < 1. \end{cases} \tag{3}$$

This is the peasant's best response correspondence to τ^e. The peasant should invest all of his endowment if the expected after-tax rate of return exceeds 1 and none of his endowment if the expected after-tax rate of return is below 1. The essential analytical problem for the peasant is forming some expectation about the dictator's tax rate. A strategic peasant would formulate this expectation based on some model of the dictator's decision problem. We consider two possibilities below, commitment and discretion.

A. *Commitment*

Suppose that the dictator could irrevocably commit himself before the spring planting season to a tax rate τ^* in the fall. Since τ^* is chosen before k, the peasant can set $\tau^e = \tau^*$ in Eq. 3. The dictator's decision problem is to choose a tax rate τ that maximizes tax revenue, R, collected from the peasant's harvest, $k(\tau^e)$, given $\tau^e = \tau^*$; that is,

$$\max_{\tau \in [0, 1]} R = \tau(1 + r)k(\tau). \tag{4}$$

If investment is zero, then the dictator collects zero tax revenues. The peasant has a strict incentive to invest if $(1 - \tau^e)(1 + r) - 1 > \varepsilon > 0$. Letting ε go to zero, the revenue maximizing tax rate τ^* under commitment is

$$\tau^* = \frac{r}{1 + r}. \tag{5}$$

The peasant's weak best response to τ^* is to set k^* equal to W. The strategy combination (k^*, τ^*) is a weak Nash equilibrium of the peasant–dictator game when the dictator commits to a tax rate before the spring planting. Moreover, it is the unique subgame perfect equilibrium of the game. The peasant's utility is equal to W and the dictator's revenue is equal to rW. Under commitment, the peasant chooses the efficient level of investment, but the dictator is able to collect all of the surplus from investment.[3]

B. *Discretion*

The previous section modeled commitment as a situation where the dictator moves before the peasant. Realistically, dictators cannot irrevocably commit themselves in the spring to a specific tax rate in the fall: that is what it means to be a dictator. In the fall, the harvest will be $(1 + r)k$ regardless of any strategems intended to influence τ^e and the dictator can set τ at his discretion.

The dictator chooses τ to maximize his revenue, taking $(1 + r)k$ as given. Hence, the dictator's decision problem under discretion is

$$\max_{\tau \in [0,1]} R = \tau(1 + r)k \qquad \text{given } k. \tag{6}$$

This is a linear objective and the best response correspondence is

$$\tau' = \begin{Bmatrix} 1 & \text{if } (1 + r)k > 0 \\ [0, 1] & \text{if } (1 + r)k = 0 \end{Bmatrix}. \tag{7}$$

Hence, τ' equal to 1 is a weakly dominant strategy.[4]

The peasant will recognize this and use Eq. (7) to form his expectation of τ. Substituting τ^e equal to 1 into Eq. (3) implies that $k' = 0$, that is, the peasant chooses to eat his endowment in the spring and there is no surplus from investment. The strategy combination (k', τ') is the unique Nash equilibrium to the peasant–dictator game under discretion and can be derived using iterative weak dominance.

This solution implies that the peasant's utility is W, which is the same as the value under commitment, but under discretion the dictator's tax revenue is now 0. Hence, the discretionary solution is weakly Pareto inferior to the commitment solution.

[3] The solution to the peasant–dictator game under commitment is similar to the subgame perfect equilibrium of the posted price bilateral monopoly; see Fouraker and Siegel (1963). It differs from the widely studied ultimatum bargaining game in that the peasant's response is not all or nothing; see Davis and Holt (1993) for references.

[4] The dictator's problem under discretion, if there is positive investment, is similar to the decision problem studied by Foresythe *et al.* (1994) and Hoffman *et al.* (1994). The size of the pie to be divided is $(1 + r)k$ and the shares are determined by τ.

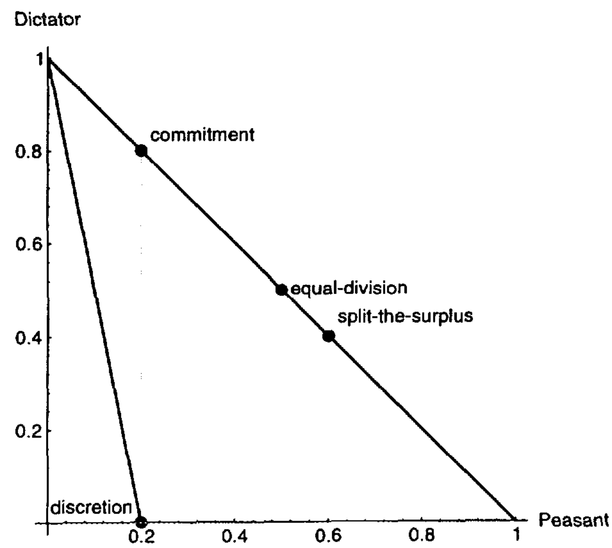

FIG. 1. Solution concepts illustrated in the payoff space; $W = 0.2$ and $r = 4.0$.

Figure 1 illustrates the two solutions for the case with $W = 0.2$ and $r = 4.0$. The set of feasible payoffs is given by the triangle formed by (0.2, 0), (1, 0), and (0, 1). The dotted line connecting the commitment and the discretion solutions divides the space of feasible outcomes in two. The peasant's utility in an outcome left of the dotted line is lower than the peasant can achieve by eating his endowment in the spring. The line connecting (1, 0) and (0, 1) represents the set of efficient outcomes. The commitment solution is contained in this set, but the discretion solution is not.

C. *Cooperative Bargaining Solutions*

Previous research suggests that the accuracy of a strategic analysis based on the abstraction assumption of self-interested money-motivated subjects can depend on how far the predicted solution is from cooperative bargaining solutions derived under the money-motivated abstraction assumption.[5] So, Fig. 1 also reports two cooperative bargaining solutions to the peasant–dictator game.

[5] An alternative approach not pursued here is to maintain the abstraction assumptions of individual rationality and mutual consistency that are fundamental to a strategic analysis but reject the abstraction assumption of money-motivated behavior that is fundamental to our experimental approach.

The equal-division solution requires an efficient level of investment and simply divides the fall harvest equally between the peasant and the dictator. The presumption is that subjects simply ignore their bargaining power. This naive solution conflicts with individual rationality for the peasant when the rate of return, r, is less than one.

The Nash bargaining solution is a cooperative concept that selects outcomes that are individually rational for both players. Abstracting from the sequence of moves, assuming efficient investment, and assuming that the relevant "conflict" point is $(W, 0)$ gives the following formulation of the Nash bargaining solution.

The set of feasible solutions is denoted $F = \{(U, R) \in \mathbb{R}_+^2 | U + R \leq (1 + r)W\}$. The Nash bargaining solution is found by solving the maximization problem

$$\max_{(U,R)\in F} (U - W)(R - 0), \tag{8}$$

which gives

$$\begin{aligned} U^{\mathrm{STS}} &= W + \frac{1}{2} rW, \\ R^{\mathrm{STS}} &= \frac{1}{2} rW. \end{aligned} \tag{9}$$

We call this solution split-the-surplus (STS), where the surplus to be split is rW. Let τ^{STS} denote the tax rate that implements the split-the-surplus

$$\tau^{\mathrm{STS}} = \frac{r}{2(1 + r)} = \frac{1}{2}\tau^*.$$

solution. Note that the split-the-surplus tax rate is half the commitment tax rate.

There is no possibility for preplay negotiation or contracting in our experiment. Hence, if either equal-division or split-the-surplus accurately predicts behavior, this would imply that the subjects are not exploiting their bargaining power. It is, however, possible to use the cooperative solutions to select an equilibrium derived by a strategic analysis.

While (k^*, τ^*) is the unique subgame perfect equilibrium under commitment, there are other Nash equilibria and one of them corresponds to the Nash bargaining solution.[6] It is constructed by assigning the following

[6] Camerer *et al.* (1993) provide evidence that subjects do not retrieve the information necessary to backward induct in sequential bargaining games. Hence, one might expect our subjects to violate subgame perfection as well.

strategy to the peasant: let $k = W$ if $\tau = \tau^{STS}$, but if $\tau \neq \tau^{STS}$, then $k = 0$. The dictator's best response to this strategy is to set the $\tau = \tau^{STS}$. Of course, this equilibrium violates subgame perfection under commitment, since it requires the peasant to forgo profitable investments when $(1 - \tau)(1 + r) > 1$ but $\tau \neq \tau^{STS}$. When $r \geq 1$, the equal-division solution can also be used to select a Nash equilibrium, but, except for $r = 1$, this would also violate subgame perfection.

Under discretion there is a unique Nash equilibrium. Hence, the unique prediction of a strategic analysis, whether it requires subgame perfection or not, is (k', τ'): no investment. There is no equilibrium selection role for cooperative solution concepts under discretion.

III. Research Hypotheses

Before turning to our experiment, it is useful to summarize the predictions of the strategic analysis, the equal-division solution, and the split-the-surplus solution under the constraint $(1 + r)W = \$1$. Given this constraint, subjects bargain over \$1 and the surplus rW equals $(1 - W)$. Let $\mathbf{T}$ equal 0 if the treatment is discretion and 1 if the treatment is commitment. Let $\mathbf{U}$ denote the peasant's money earnings in the period game and $\mathbf{R}$ denote the dictator's money earnings. A little algebra shows that the predictions of the three approaches can be nested in one set of basic equations,

$$\mathbf{U} = \alpha_i + \beta_{i1} W + \beta_{i2} \mathbf{T} (1 - W), \tag{10.a}$$

$$\mathbf{R} = \alpha_c + \beta_{c1} W + \beta_{c2} \mathbf{T} (1 - W), \tag{10.b}$$

where α_j, β_{jl} are parameters and $j \in \{i, c\}$ and $l \in \{1, 2\}$. The strategic analysis restricts the parameters to be

$$\beta_{i1} = 1 \quad \text{and} \quad \alpha_i = \beta_{i2} = 0,$$
$$\beta_{c2} = 1 \quad \text{and} \quad \alpha_c = \beta_{c1} = 0.$$

The naive equal-division solution restricts the parameters to be

$$\alpha_i = 0.5 \text{ and } \beta_{il} = 0 \quad \text{for all } l,$$
$$\alpha_c = 0.5 \text{ and } \beta_{cl} = 0 \quad \text{for all } l.$$

The naive split-the-surplus solution restricts the parameters to be

$$\alpha_i = 0.5 \text{ and } \beta_{il} = 0.5 \text{ and } \beta_{i2} = 0,$$

$$\alpha_c = 0.5 \text{ and } \beta_{cl} = -0.5 \text{ and } \beta_{c2} = 0.$$

Note that only the strategic analysis predicts the treatment variable will have a significant influence on earnings and that the naive equal-division solution predicts the endowment will not influence earnings.

IV. Experimental Design

The treatment variable in our experiments was the order of moves: specifically, whether the dictator chose τ before or after the peasant chose k. In order to insure own-subject control, each session consisted of a sequence of three treatments, either commitment–discretion–commitment or discretion–commitment–discretion. Each treatment consists of 20 periods and this was announced in the instructions. Hence, a session consisting of three treatments lasts a total of 60 periods.

Previous research suggests that the accuracy of a strategic analysis based on the abstraction assumption of self-interested money-motivated subjects can depend on how far the predicted solution is from cooperative concepts of fairness. Hence, we blocked the parameter space by imposing the constraint that $(1 + r)W = \$1$ on the parameters and selecting from five values of the endowment $W \in \{\$0.20, \$0.40, \$0.50, \$0.60, \$0.80\}$. The efficient frontier for each pair was the line connecting (1, 0) and (0, 1) in Fig. 1, but the set of feasible outcomes and the predicted solutions changed with the blocking variables. Ignoring the strategic aspects of the endowment and the order of moves, all pairs bargained over $1.00 each period.

Previous research also suggests that subject experience can influence the accuracy of strategic predictions. Hence, a second blocking variable is subject experience. Five sessions were conducted with subjects who had participated once in a previous peasant–dictator session. The 2 treatment variables, 5 values of the endowment, and 2 levels of experience give an experimental design with 20 cells, see Table I. The sessions are numbered according to the sequence in which they were conducted.

Ten subjects participated in each session. The dictator was called "collector" and the peasant was called "investor" in the instructions. The subjects were randomly assigned a type, either collector or investor, and randomly rematched with the other subjects each period. (This is a one-population two-type random pairwise matching protocol.)

The subjects played the game using a computer-assisted graphical user interface available in the Texas A&M University (TAMU) economics laboratory. The instructions were read aloud to insure that the description of the game was common information. Appendix A reports the instructions

TABLE I
EXPERIMENTAL DESIGN CELLS BY SESSION NUMBERS

	$C_{[1-20]} - D_{[21-40]} - C_{[41-60]}$		$D_{[1-20]} - C_{[21-40]} - D_{[41-60]}$	
W	Inexp.	Exp.	Inexp.	Exp.
0.2	2,11		15	
0.4	3,4	20	13,14	18
0.5	9			
0.6	1,6	19	7,12	8,17
0.8	5,10		16	

Note. W denotes endowment, C denotes commitment, D denotes discretion, Inexp. denotes inexperienced, and Exp. denotes experienced.

text file used by the graphical user interface. No preplay negotiation was allowed. After each repetition of the period game, the subjects' earnings were calculated for that period. There was no common historical outcome data available to the subjects.

The subjects were undergraduate economics students attending Texas A&M University in the 1992 spring and summer semesters. A total of 152 subjects participated in the 20 sessions reported below and of this total 48 subjects participated twice, once as inexperienced subjects and once as experienced. After reading the instructions, but before the session began, the students filled out a questionnaire to determine that they understood how to compute payoffs for both player types. If any subject made a mistake on the questionnaire, the relevant instructions were read again.

The sessions take about three hours to conduct. Consequently, subjects could earn significantly more than the minimum wage. For example, if subjects coordinate on the equal-division outcome, then each subject would earn $30.

V. EXPERIMENTAL RESULTS

Figure 2 reports a scatter plot of the mean earnings in the last two treatments of each session. Squares denote discretion and diamonds denote commitment. The most striking feature of Fig. 2 is the perfect separation of the observations into two disjoint clusters, one of squares and one of diamonds. The treatment appears to have had a significant effect on behavior. First, we focus on commitment's influence on efficiency and then we turn to commitment's influence on bargaining. Second, we report an econometric analysis of the data in Fig. 2. Finally, we relate our findings to the extant

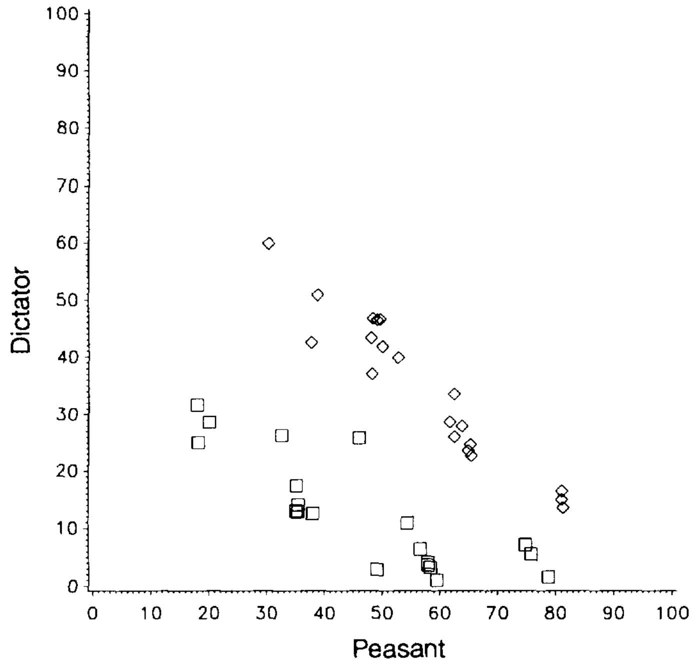

FIG. 2. Mean earnings plotted in the payoff space (denoted in cents). Square denotes discretion and diamond denotes commitment.

literature on extensive-form bargaining experiments by examining fairness and trust as explanations for some observed anomalies.

A. *Efficiency*

Each session contributes a repeated measure on the effect of the treatment variable, that is, each session contributes one square and one diamond to Fig. 2. Let subscript C denote commitment and D denote discretion. When $(U, R)_C > (U, R)_D$, commitment has improved efficiency. This occurs in all 20 sessions.[7] Commitment-improved efficiency as predicted by the strategic analysis.[8]

In order to measure the economic significance of this effect, Table II reports the mean efficiency by treatment and endowment. Here efficiency is being measured as the percent of the feasible surplus $(1 - W)$ actually

[7] Appendix B reports the mean earnings of all treatments.

[8] A null hypothesis that $\text{Prob}[(U, R)_C > (U, R)_D] \leq \text{Prob}[(U, R)_C \not> (U, R)_D]$ is easily rejected, since $(1/2)^{20}$ is much smaller than conventional significance levels.

TABLE II
Mean Efficiency by Treatment and Endowment

	Percent efficient (k/W)		
W	Discretion	Commitment	Gain
0.2	34	83	49
0.4	19	88	69
0.5	4	85	81
0.6	9	76	67
0.8	6	81	75

Note. Units are realized efficiency as a percent of feasible efficiency.

achieved on average. (A little algebra reveals that this measure of efficiency is equal to the share of the endowment invested, k/W.) For all blocking variables, efficiency under commitment is more than twice that under discretion: for $W = 0.4$ it is 4 times as large, for $W = 0.5$ it is 20 times as large, for $W = 0.6$ it is 8 times as large, and for $W = 0.8$ it is 12 times as large.

While the data reveal both a statistically and economically significant effect of the treatment on efficiency as predicted, there are some important discrepancies with the strategic analysis. Table II reveals that under discretion subjects realized an economically significant share of the feasible surplus for some of the blocking variables even though the strategic analysis predicts zero realized surplus. For example, under $W = 0.2$ ($r = 4$) subjects earned 34% of the feasible surplus and for $W = 0.4$ ($r = 1.5$) subjects earned 19% of the feasible surplus. It is interesting to note that these values occur when r is greater than 1. Naive players who best respond to a uniform prior over the strategy space of their opponents would invest when r is greater than 1.[9]

Conversely, subjects fail to capture all the feasible surplus under commitment. The measure of efficiency varies from a low of 76% for $W = 0.6$ ($r = 0.67$) to a high of 88% for $W = 0.4$ ($r = 1.5$). It is interesting to note that the endowment blocking variables associated with the highest levels of efficiency are the ones that include equal division in the set of Nash equilibria. The lowest value occurs at 0.6, which is the first value that excludes equal division. These discrepancies with the strategic analysis, which will be developed in what follows, should not distract us from the fact that commitment increases efficiency.

[9] Of course, the behavior we observe persists even with experienced subjects. Alternatively, it may be that when it does not cost much to be nice more people are nice.

TABLE III
MEAN EARNINGS BY ENDOWMENT

W	Discretion (U, R)	Commitment (U, R)
0.2	(19, 28)	(36, 51)
0.4	(35, 16)	(49, 44)
0.5	(49, 3)	(53, 40)
0.6	(56, 8)	(64, 27)
0.8	(76, 5)	(81, 15)

Note. Units are in cents.

B. *Bargaining*

The strategic analysis also makes predictions about how the surplus will be divided. Table III reports mean earnings by endowment. Recall that the strategic analysis predicts $(W, 0)$ under discretion and $(W, 1 - W)$ under commitment. Except for the cells with $W = 0.2$, the peasant earns more than the dictator. While the strategic analysis of discretion predicts this ordering of shares, it is a remarkable fact that in all discretion cells mean peasant earnings are less than W. This is true for 19 of 20 individual sessions. Mean peasant earnings for the exception, Session 15, are equal to W. If we only use the last five periods of the last two treatments, it is still true for 16 of 20 sessions and, again, mean peasant earnings for the four exceptions equal W. Only two of the four exceptions involved experienced subjects. On average, the dictator takes more than 100% of the realized surplus under discretion.

Conversely, mean peasant earnings are above W in all of the commitment cells. This is also true for all 20 sessions individually. Hence, on average the dictator takes less than 100% of the realized surplus under commitment. For W equal to 0.2 and 0.4, the peasant is earning 80% (16/20) and 22% (9/40) more than predicted. The peasant receives 24, 17, 7, 10, and 6% of the realized surplus by W. The treatment appears to influence the peasant's bargaining position. While there is evidence for surplus sharing, the observed peasant share is much less than half for all values of W. In order to judge the economic and statistical significance of these discrepancies, we estimate Eq. (10).

C. *Estimating an Empirical Model*

The empirical model is derived from Eq. (10) by adding an error term to each equation. Inspecting Fig. 2 leads one to reject the hypothesis of homoscedastic residuals. Not surprisingly, the variance of the errors is increasing in the feasible surplus. Hence, we estimate two versions of Eq. (10) using weighted least squares, where the feasible surplus was used as the weighting variable.

The first model included dummy variables for time and experience. Equation (11) reports the estimated equation and standard errors:

$$\begin{array}{l} U = -5 + 101\,W + 22\,T(1 - W) + 0\,D\_\text{TIME} + 1\,D\_\text{EXPERIENCE}. \\ \quad (2.6) \quad (3.4) \quad (2.9) \qquad\qquad (1.0) \qquad\qquad (1.0) \end{array} \tag{11.a}$$

$$\begin{array}{l} R = 28 - 28\,W + 38\,T(1 - W) + 3\,D\_\text{TIME} - 1\,D\_\text{EXPERIENCE}. \\ \quad (5.0) \quad (6.6) \quad (5.7) \qquad\qquad (2.0) \qquad\qquad (2.5) \end{array} \tag{11.b}$$

Earnings and parameters are denoted in cents. The P-values for the dummy variables for time and experience are .13, .69, .94, and .63 respectively. Moreover, they are not economically significant. The largest estimate is that the dictator earns 3 cents more in Periods 41–60 than in 21–40 and the standard error is 2 cents. Hence, we focus on the estimates for the empirical model without dummy variables for time and experience.

Equation (12) reports the estimated equation, standard errors, and diagnostic statistics:

$$\begin{array}{l} U = -4 + 101\,W + 21\,T(1 - W), \qquad R^2 = 0.99, F = 11347.2 \\ \quad (1.8) \quad (2.5) \quad (2.0) \end{array} \tag{12.a}$$

$$\begin{array}{l} R = 24 - 24\,W + 46\,T(1 - W), \qquad R^2 = 0.94, F = 228.1 \\ \quad (3.7) \quad (5.0) \quad (4.0) \end{array} \tag{12.b}$$

All of the parameters differ significantly from 0 at the usual 5% level and all but the intercept parameter in the peasant earnings equation differ at the 1% level. (Recall the sample size is 40). We tested the restrictions on the parameters implied by the three research hypotheses using an F-test. The smallest F statistic was 43 and all of the theoretical models can be rejected at the 1% significance level.[10]

However, the parameter estimates are interesting in themselves. The average peasant earnings vary almost exactly one-for-one with the endowment W, as predicted by the strategic analysis. However, the regression line for average peasant earnings is shifted below 0 by 4 cents and the average peasant's share of the *feasible* surplus under commitment is 21%, see (12.a). Hence, the parameter estimates indicate both a giveaway of 4 cents and some surplus sharing under commitment.

[10] Restricting the sample to sessions with $\omega \geq 0.5$ changes the parameter estimates a few pennies, but not the story. The data still reject all three hypotheses.

The dictator-earnings equation (12.b) is also useful in diagnosing why the statistical tests reject the strategic analysis. The parameter on $T(1 - W)$ is 46, which is much less than the predicted value of 100. This discrepancy arises both because the actual surplus is less than the predicted surplus under commitment and because of surplus sharing behavior. Recall that $T = 1$ under commitment and $T = 0$ under discretion. Hence, Eq. (12.b) implies that the dictator is getting 70% of the *feasible* surplus under commitment and 24% of the *feasible* surplus under discretion.

The sensitivity of the results to the order of moves provides strong evidence against the cooperative bargaining models. Moreover, the observed surplus-sharing behavior is neither equal division nor split-the-surplus. While the strategic analysis fails to predict the observed inefficiency under commitment, the surplus sharing behavior, or the giveaway under discretion, it does accurately predict the significant improvement observed in mean dictator earnings.

D. *Individual Behavior: Fairness and Trust*

Our results stand in marked contrast to the early sequential bargaining experiments. The usual reported finding was that the key strategic variables were uncorrelated with the data.[11] One reason why this may be so is that subjects are not purely money motivated. Ochs and Roth (1989) emphasize that subjects are rejecting offers with higher money earnings than they can earn in the subgame resulting from their rejection. These disadvantageous counterproposals suggest that the subject's revealed preference in sequential bargaining experiments violates the abstraction assumption of money-motivated behavior.

Forsythe *et al.* (1994) test whether a "taste-for-fairness" alone can explain the positive offers made in ultimatum bargaining experiments. The experiment compared the standard ultimatum game in which one subject proposes a division of the surplus and the other accepts or rejects the division with a dictator game in which one subject simply divides the surplus. The fairness hypothesis states that the distribution of proposals in the ultimatum and dictator games are identical. Forsythe *et al.* were able to reject this hypothesis formally using the Epps/Singleton (1986) characteristic function statistic (CF statistic hereafter). However, they did observe more than 60% of the dictators giving away positive amounts of their endowment, which is still inconsistent with the abstraction assumption of money-motivated behavior.

Hoffman *et al.* (1994) note that the usual experimental protocol insures only intersubject anonymity and investigate whether the observed violations are an artifact of the lack of anonymity with respect to the experimenter.

[11] See for example Ochs and Roth (1989, p. 362) and references cited therein.

TABLE IV
CHARACTERISTIC FUNCTION TEST OF THE FAIRNESS HYPOTHESIS

W	CF statistic	P-value
0.2	415.18	0.0
0.4	1057.09	0.0
0.5	85.75	0.0
0.6	1309.76	0.0
0.8	665.34	0.0

They conduct a dictator game using a novel double-blind matching protocol that guarantees subjects anonymity with respect to other subjects and the experimenter. The protocol reduced the percent of dictators giving away positive amounts of their endowment to about 40%.

Given positive investment the peasant–dictator game enters a subgame that is strategically equivalent to the dictator game studied in the literature. The *fairness hypothesis* for dictator behavior in the peasant–dictator game is that, for a given endowment, the distribution of tax rates under commitment and discretion when $k > 0$ are identical. Using the CF statistic we reject the fairness hypothesis for all endowments, see Table IV.[12] It is useful to examine the empirical distribution functions directly to see why the fairness hypothesis fails.

Figures 3a to 3e report the empirical distribution function (EDF) for τ under discretion given $k > 0$, $d[W]$, and under commitment, $c[W]$, by endowment. The sample sizes for commitment are {300, 600, 100, 700, 300} and for discretion given $k > 0$ are {202, 262, 16, 254, 87} respectively. Units on the horizontal axis are in percent. The vertical dashed line in the figure indicates the shape of the theoretical distribution function for commitment in the subgame perfect equilibrium, which is a step function with a single step at $(1 - W) \times 100$. The theoretical distribution function for discretion given positive investment has a single step at 100: the dictator is predicted to take it all.

Our results in the dictator subgame are qualitatively similar to those reported by Forsythe *et al.* (1994) and Hoffman *et al.* (1994), but reveal fewer violations of the money-motivated abstraction assumption. We never observed dictators giving away more than 40% of the gross return from investment and we observed as little as 26% violations in $d[0.8]$; see Fig.

[12] The CF statistic is distributed chi-square with four degrees of freedom. See Epps and Singleton (1986), Binmore *et al.* (1991), and Forsythe *et al.* (1994) for Monte Carlo studies of its power against various alternative hypotheses.

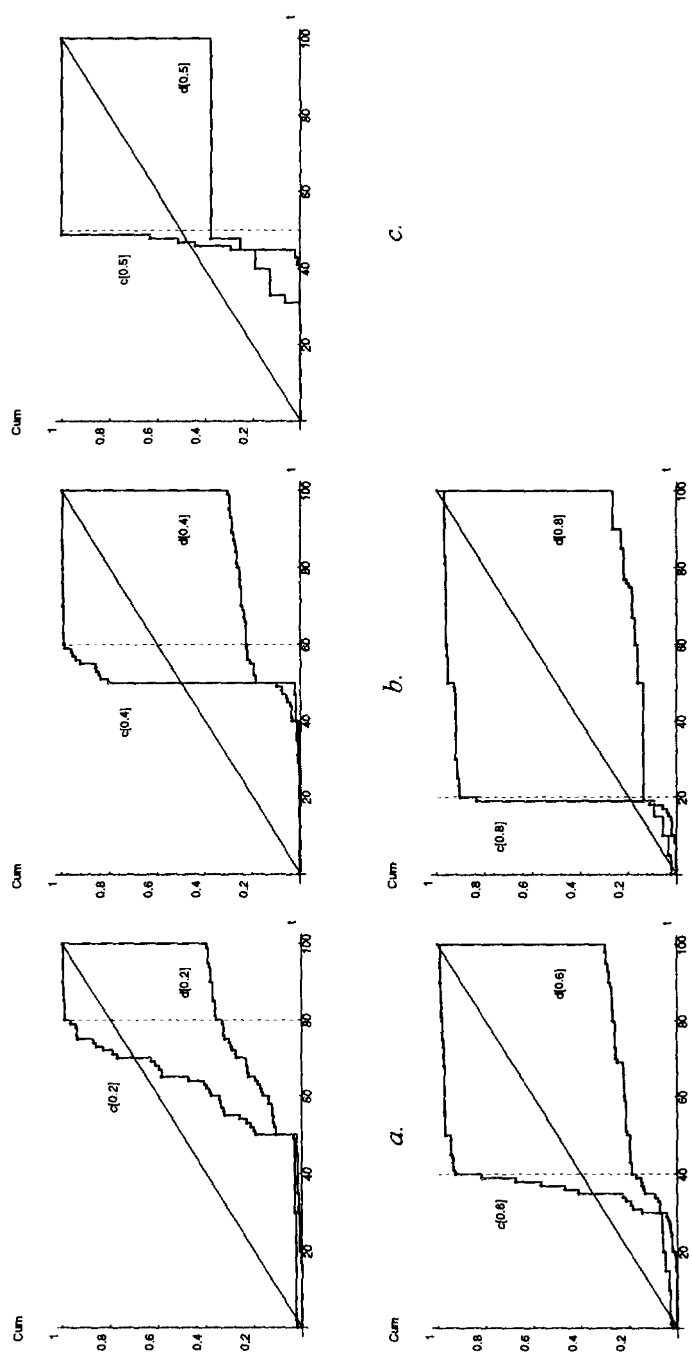

FIG. 3. Empirical distribution functions of tax rates under commitment, $c[W]$, and under discretion when $k > 0$, $d[W]$, by endowment. The dashed line indicates the theoretical distribution function under commitment.

3. The modal tax rate is always 100% as predicted by a strategic analysis. The fact that $d[i] \neq d[j]$ for $i \neq j$ is evidence that this other-regarding behavior is influenced by endowment. Specifically, for the dictator subgame of a discretion treatment we observe that the fewer dictators violating the abstraction assumption of money-motivated behavior, the more peasants are favored by strategic considerations, which suggests that subjects are concerned about relative earnings.

Although the sample is small, it is interesting to note that dictators either took all of the surplus or left the peasant with a strict profit from investment in $d[0.5]$. This phenomena is also present in $d[0.2]$ and $d[0.4]$, both of which have less than 10% of the observations (4 and 8%, respectively) between a tax rate that leaves the peasant with a profit and a tax rate that takes everything. Clearly, those dictators who are not conforming to a strategic analysis based on the money-motivated abstraction assumption are not simply behaving randomly. Rather they appear concerned that the peasant make a profit on his investment under discretion.

Under commitment it is the dictator that is uncertain about the profitability of his choices. Figure 3a reports the commitment tax rate EDF for endowment 0.2, $c[0.2]$. The equal-division tax rate accounts for 17% of the observations in $c[0.2]$ and it is the modal tax rate. Only 2% of the observed tax rates are equal to τ^*, while 96% of the observed tax rates are less than τ^* in $c[0.2]$.

Figure 3b reports the commitment tax rate EDF for endowment 0.4, $c[0.4]$. The equal division tax rate is the modal tax rate in $c[0.4]$ and accounts for 70% of the observations. Only one-half of 1% of the observations are equal to τ^*, while 98% of the observations are less than τ^*.

Figure 3c reports the commitment tax rate EDF for endowment 0.5, $c[0.5]$. The equal-division tax rate is no longer the modal tax rate in $c[0.5]$. In fact, it was not observed at all. Instead, the modal tax rate is 49% and it accounts for 37% of the observations. A 49% tax rate gives the peasant a strict, but small, incentive to invest. Since the equal-division tax rate coincides with τ^* in $c[0.5]$, no subject played τ^* and 100% of the observations are less than τ^*.

Figure 3d reports the commitment tax rate EDF for endowment 0.6, $c[0.6]$. The modal tax rate in $c[0.6]$ is 35% and it accounts for 18% of the observations. The modal tax rate gives the peasant a strict incentive to invest of five cents. The equal-division tax rate accounts for 2% of the observations even though it does not give the peasant any incentive to invest. About 10% of the observations are equal to τ^*.

Figure 3e reports the commitment tax rate EDF for endowment 0.8, $c[0.8]$. The modal tax rate in $c[0.8]$ is 19% and it accounts for 72% of the observations. The modal tax rate gives the peasant a strict incentive to invest of one cent. The equal-division tax rate accounts for only 3% of the observations. Only 7% of the observations are equal to τ^* and 84% of the observations are less than τ^*.

Inspecting Figs. 3a to 3e suggests subjects deviated from τ^* in order to insure that the peasant had a strict incentive to invest under commitment. Hence, it is interesting to estimate the optimal tax rate for a given endowment, $t^*(W)$, from the empirical revenue function. Figures 4a to 4e report mean dictator earnings measured in cents by tax rate measured in percent. The radius of the circle around a mean observation indicates sample size. The curve in each figure is the SAS cubic spline interpolation fitted to the sample data rather than the mean data reported in the figures.

The figures reveal that τ^* is never optimal against actual peasant behavior. Indifference is not systematically resolved in favor of the dictator, as assumed in the strategic analysis. Moreover, the optimal size of the strict incentive to invest, e, is not independent of the endowment. Let $e^*(W) = t^*(W) - \tau^*(W)$ denote the optimal incentive given average peasant behavior, then $e^*(0.2)$ is about 15 cents, $e^*(0.4)$ is about 10 cents, $e^*(0.5)$ is about 5 cents, $e^*(0.6)$ is also about 5 cents, and $e^*(0.8)$ is about 2 cents. Hence, the average peasant is not just responding to the absolute incentive to invest but also to the relative incentive to invest. When the peasant is endowed with more than half the dollar, he is less likely to refuse to invest when confronted with tax rates approaching the theoretical prediction τ^*. So, nonpecuniary considerations are less distorting under commitment the more the initial endowment favors the peasant.

The peasant–dictator game allows one to examine a hypothesis for peasant behavior analogous to the fairness hypothesis for dictator behavior, which we call the naive trust hypothesis. The *naive trust hypothesis* is that, for a given endowment, the distribution of investment under commitment when $\tau < \tau^*$ and under discretion are identical. Not surprisingly, the CF statistic easily rejects the naive trust hypothesis for all endowments; see Table V. As with the fairness hypothesis our interest is in understanding why the naive trust hypothesis fails.

Figures 5a to 5e report the investment EDFs under commitment given $\tau < \tau^*$, $c[W]$, and under discretion, $d[W]$, by endowment. The sample sizes for commitment given $\tau < \tau^*$ are {290, 591, 100, 573, 251} and for discretion are {300, 600, 100, 700, 300} respectively. Units on the horizontal axis are percent of endowment invested. Given $\tau < \tau^*$ under commitment, the peasant has a strict incentive to invest and is predicted to invest all of his endowment. So the theoretical distribution function for k under commitment has a single step at 100. Conversely, the theoretical distribution function for k under discretion has a single step at 0.

The investment under commitment EDFs in Fig. 5 reveal more violations of the abstraction assumption of strictly money-motivated behavior. Given $\tau < \tau^*$, as much as 27% of the peasants refuse to fully exploit their positive

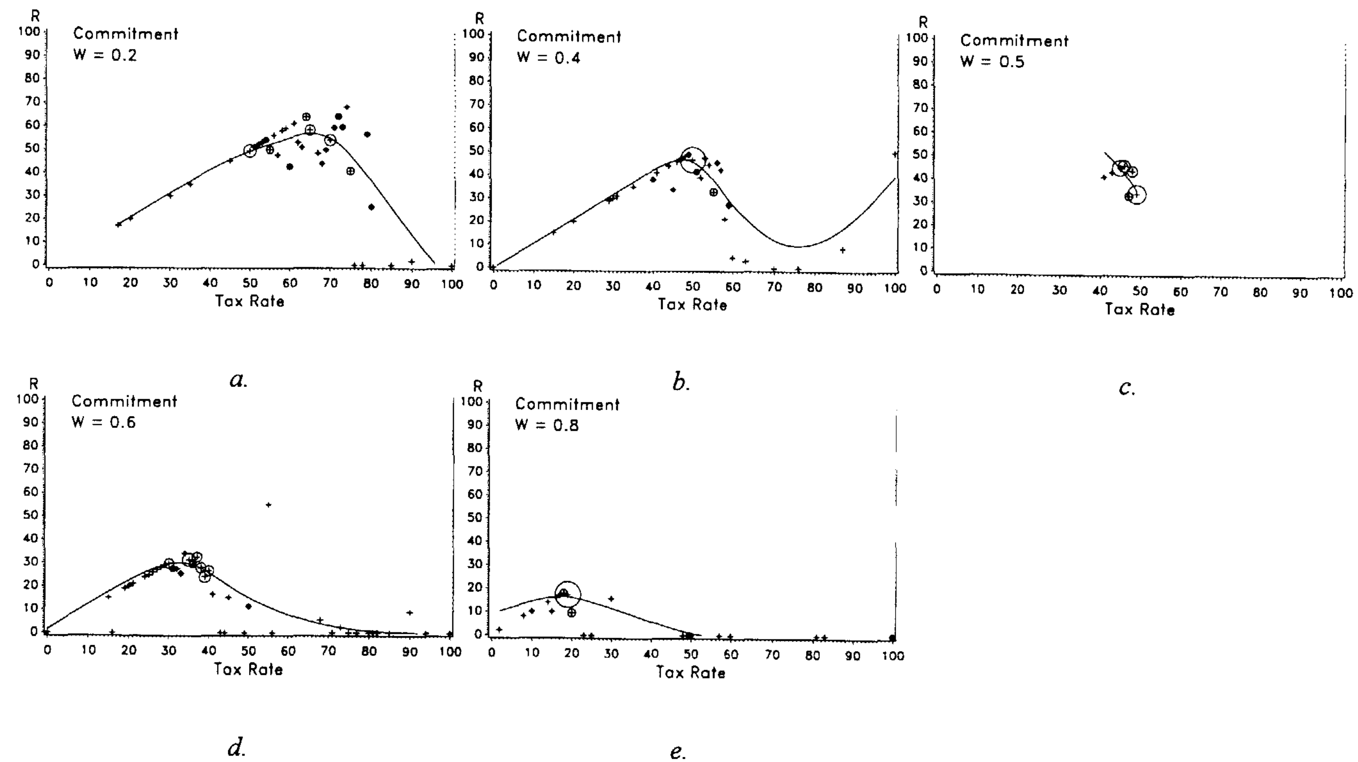

FIG. 4. Mean dictator earnings (R) under commitment measured in cents by tax rate measured in percent. The radius of the circle around a mean observation indicates sample size. The curve in each figure is the SAS cubic spline interpolation fitted to the sample data.

TABLE V
CHARACTERISTIC FUNCTION TEST OF THE NAIVE TRUST HYPOTHESIS

W	CF statistic	P-value
0.2	470.28	0.0
0.4	1994.08	0.0
0.5	564.87	0.0
0.6	2136.36	0.0
0.8	2460.13	0.0

investment opportunities; see $c[0.2]$ in Fig. 5a. These observed violations fall to a low of 9% as the endowment goes to 0.8; see $c[0.8]$ in Fig. 5e. Interestingly, when $\tau < \tau^*$ the peasant has a strict incentive to invest. Hence, when they do not they are engaged in the kind of behavior emphasized by Ochs and Roth (1989), that is, they are foregoing profit in order to preserve dignity or achieve some other nonpecuniary motive.

The strategic analysis predicts zero investment under discretion. The peasant should not trust the dictator. But inspecting Fig. 5 reveals that when the endowment is low and, hence, the rate of return is high the prediction is not particularly accurate. Only 33% of the peasants conform in $d[0.2]$; see Fig. 5a. As the endowment increases theory becomes more accurate: 71% conform in $d[0.8]$.

Is it profitable to naively trust the dictator in the commitment treatment? Figure 6 reports mean peasant earnings, U, under discretion measured in cents by share invested, k, measured in percent. The radius of the circle around a mean observation indicates sample size. The curve in each figure is the SAS cubic spline interpolation fitted to the sample data rather than the mean data reported in the figures.

The figures reveal that k' is always optimal against actual dictator behavior. When the endowment is low the pecuniary incentive to play k' is low, see Fig. 6a, and as the endowment increases the losses from trusting the dictator increase, see Fig. 6e. It is not profitable to naively trust the dictator in the commitment treatment.

Berg *et al.* (1993) investigate whether dictator giving depends on social norms concerning appropriate behavior using a game very similar to our peasant–dictator game under discretion with $W = \$10$ and $r = 2$. They use the double-blind matching protocol and conduct a "one-shot" game. Their *social norms trust hypothesis* is that there exist some levels of investment, which demonstrate confidence in the trustworthiness of the dictator, that result in net positive earnings for the peasant. They report that investing half or all of the endowment induced the dictator in the subgame to return

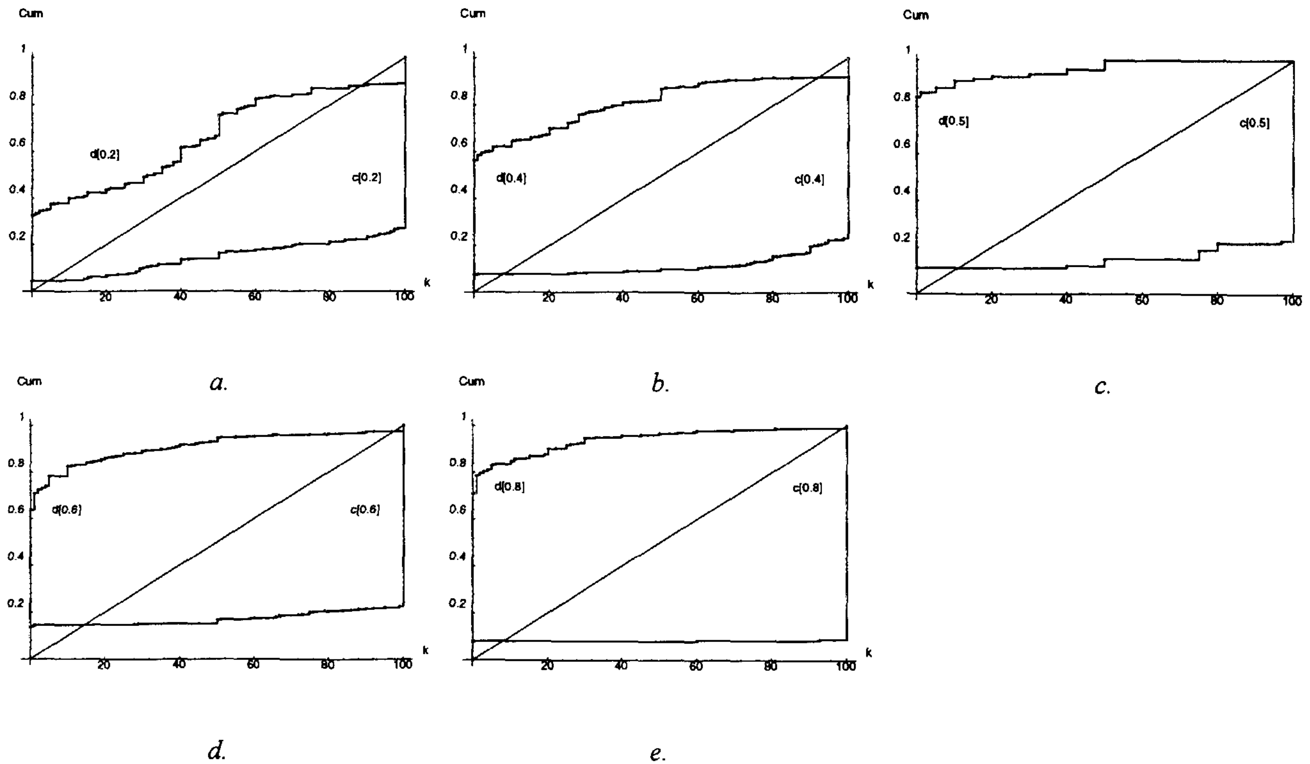

FIG. 5. Empirical distribution functions of investor share, k, under commitment when $\tau < \tau^*$, $c[W]$, and under discretion, $d[W]$, by endowment.

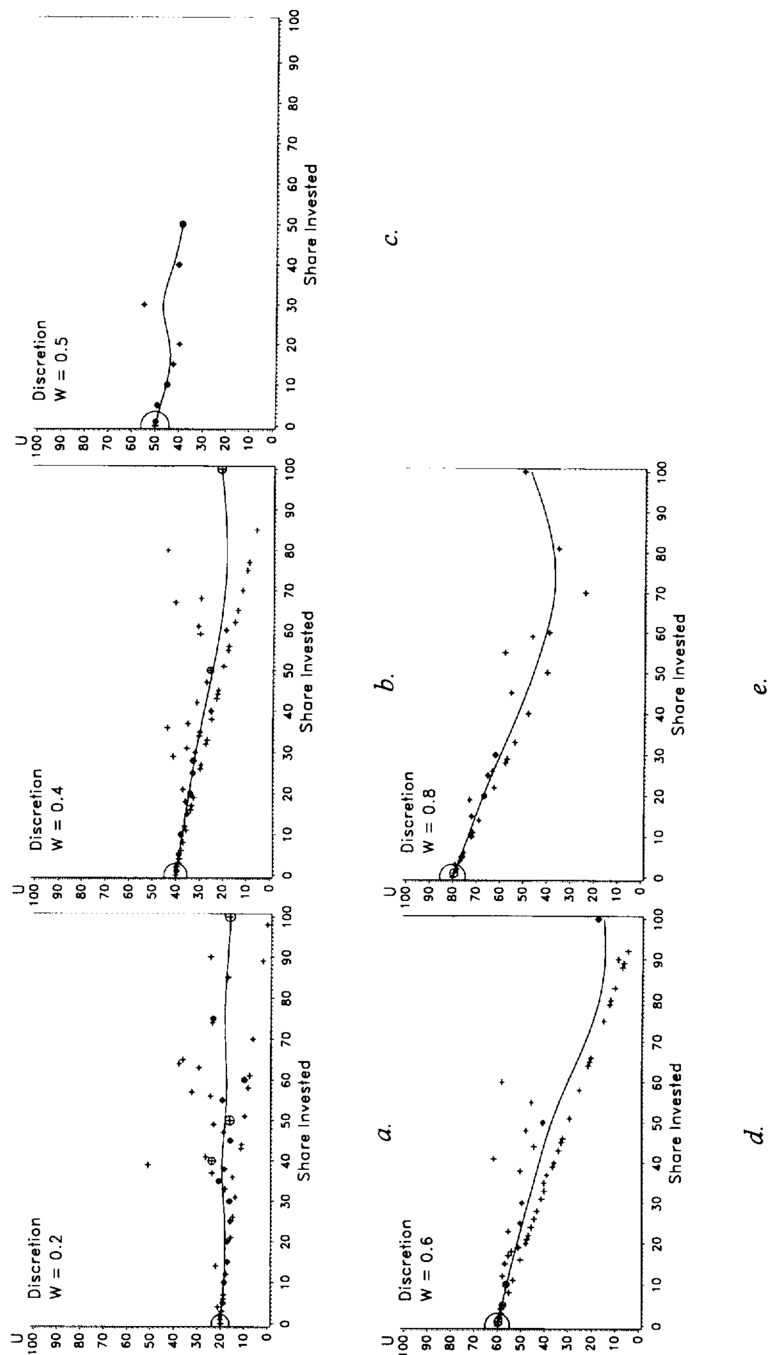

FIG. 6. Mean peasant earnings (U) under discretion measured in cents by share invested, k, measured in percent. The radius of the circle around a mean observation indicates sample size. The curve in each figure is the SAS cubic spline interpolation fitted to the sample data.

enough to the surplus to give the peasant net positive earnings, but that other levels of investment did not.

Figure 6a reveals that there is no evidence for the social norms trust hypothesis in our data. While mean earnings do spike up at 50 and 100% for some endowments, neither investing half nor all of the endowment yields positive net returns for any endowment.

VI. Summary and Conclusions

Our experiment reveals both a statistically and economically significant increase in efficiency under commitment. This efficiency gain from commitment was inversely related to the rate of return on investment varying from a low of 49% to a high of 81%. Hence, the emphasis on commitment in the public policy games literature seems well placed.

Our results stand in marked contrast to the early sequential bargaining experiments. The usual reported finding was that the key strategic variables were uncorrelated with the data. Here we find that the strategic variables are highly correlated with the data. Nevertheless, our data exhibit some of the same anomalies emphasized in the sequential bargaining literature and both the strategic and cooperative bargaining models can be rejected at the usual levels of statistical significance. The data on individual behavior reveal that a small number of tax rate observations violate the abstraction assumption of money-motivated behavior under discretion and an even smaller number of investment observations violate the abstraction assumption under commitment.

These violations made it profitable for the dictator to choose a tax rate less than that predicted by a strategic analysis of the commitment treatment. The empirically optimal deviation was decreasing in the endowment. So, the average peasant is not just responding to the absolute incentive to invest but also to the relative incentive to invest.

While many peasants did trust the dictator under discretion, this was not profitable on average. The theoretically optimal level of investment was the empirically optimal level. However, the pecuniary incentive to invest nothing was small when the endowment was small, which may explain why even experienced subjects exhibit this anomaly.

The fairness hypothesis, which implies that dictators do not exploit their bargaining power, and the trust hypothesis, which implies that peasants do not expect dictators to exploit their bargaining power, can both be rejected by the data. Given our limited ability, we conclude that a strategic analysis based on the abstraction assumptions of individual rationality, mutual consistency, and money-motivated behavior provides the best predictions for the peasant–dictator game. Given the discovered anomalies, it should be possible to invent abstraction assumptions that lead to more accurate theories.

Appendix A: Instructions Text File for Graphical User Interface—Commitment Treatment

@015WELCOME!@007

This is an experiment in the economics of strategic decision making. Various research foundations have provided funds for this research. If you follow the instructions and make good decisions, you may earn a considerable amount of money, which will be paid to you in cash.

@015THE LOGITECH MOUSE@007

You will be making choices using a Logitech mouse, which should be on the mouse pad in the middle of your table. If you cannot find the mouse, please raise your hand. Hold the mouse in a relaxed manner with your thumb and little finger on either side of the mouse. Rest your wrist naturally on the table surface. Slide the mouse on the surface of the mouse pad to move the cursor on your screen. Let your hand pivot from the wrist. Use a light touch.

In order to participate in this experiment, you will need to be able to POINT, move the cursor on to an object by sliding the mouse, and CLICK, push any one of the mouse buttons. We will call pointing at an object and then clicking your mouse CLICKING ON an object displayed on the screen.

In order to display the next page, slide your mouse so that the pointer is on @031PAGE DOWN@007, located on the blue bar below, and click any button. To review a page, CLICK ON @031PAGE UP@007.

@015NO TALKING@007

As part of the scientific method in this session it is important that you remain silent and do not look at other peoples' work. If you have any questions or need assistance of any kind, please raise your hand and an experimenter will come to you. If you talk, laugh, exclaim out loud, etc., you will be asked to leave the experiment and you will not be paid. We expect and appreciate your cooperation.

@015PERIOD EARNINGS@007

At the beginning of each period investors will receive an endowment of \$0.60, which the investor can divide between their balance and an investment opportunity. The investment opportunity earns a gross return equal to the amount invested times 1.6667. Hence, if the investor were to invest the entire endowment of \$0.60 the gross return would be \$1.00.

The collector chooses a share of the gross return, which influences the investor's period earnings and the collector's period earnings. The investor's earnings from investment equals the gross return minus the collector's share. The investor's period earnings equals the investor's earnings from investment plus the amount of the endowment saved to their balance. The collector's earnings equal the collector's share of the gross return from investment.

The sequence of events is as follows: first, the collector chooses a share of the gross return and then the investor chooses a share of the endowment to invest.

@015WHAT IF... SCREEN@007

In a moment you will fill out a questionnaire to insure that everyone understands how choices influence period earnings. A What If... screen is available to help you fill out the questionnaire and to help you make your choices during the session.

There will be two blue choice bars marked from 0 to 100 on the What If... screen, which you can use to evaluate hypothetical choices. If you CLICK ON a choice bar, a green line will appear, which will move along the choice bar as you move your mouse. CLICK the mouse a second time to make a choice and to restore normal operation of the mouse. The column of information under the heading Hypothetical on the right side of the screen shows earnings for each player under the hypothetical choices.

Please CLICK ON @048What If...@007, while we pass out the questionnaire.

@015MATCHING PROTOCOL@007

There will be twenty periods in this part of the session. You will be randomly assigned a type: either investor or collector. You will then be randomly matched with a participant of the other type whose decisions will influence your earnings in the current period. The pairings will be anonymous: No participant will be able to identify you. You will be randomly re-assigned and re-matched with another participant at the beginning of each period.

@015MAKING CHOICES@007

You make a choice by CLICKING ON the choice bar, moving the green line to the desired value, CLICKING a second time, CLICKING ON the @047ACCEPT@007 box that appears, and then CLICKING ON the @047YES@007 box to confirm your choice.

This confirmation step lets you catch any mistakes you make. If you want to change your choice, CLICK ON @079NO@007 and then make a different choice. Only after the confirmation step has been completed is your choice final.

@015THE CHOICE SCREENS@007

We will now view the two choice screens used in the experiment. The screens are labeled by the blue bar across the top of the screen: either Collector Main or Investor Main. Each of the screens will be active so that you may see how you can access other screens from the choice screens.

Notice that during the experiment you will be able to switch to the @048Instructions@007 screen, the @048Record@007 screen, or the @048WHAT IF...@007 screen from any choice screen by CLICKING ON the desired screen. This gives you the ability to review the history of play and study alternative choices from any choice screen.

We will view the Collector Main screen first. On the Collector Main screen, your choice for that period will appear in white on the left side of the screen. The investor's hypothetical choices appear on the right side of the screen.

The first blue bar is used to choose the Collector's Share. The second blue bar is used to evaluate hypothetical values of the share of the Investor's endowment invested. However, the actual share invested will be made by the investor.

To return to the instructions properly make a choice for the Collector, that is, CLICK ON the choice bar, move the green line to the desired value, CLICK a second time, CLICK ON the @047ACCEPT@007 box that appears, and then CLICK ON the @047YES@007 box to confirm your choice.

CLICK ON @048COL1@007 now.

If you have returned to the instructions by CLICKING ON @048INSTRUCTIONS@007, you must return to Collector Main and make a choice before you can continue with the next step. You return to Collector Main by CLICKING ON @047RETURN@007.

On the Investor Main screen, the collector's choice will appear on the right side of the screen in yellow, which indicates that this is the actual value for the period. The investor's choice is made using the second blue bar, which works the same way as the Collector's choice bar.

We will now view the Investor Main choice screen. To return to the instructions properly make a choice for the Investor, that is, CLICK ON the choice bar, move the green line to the desired value, CLICK a second time, CLICK ON the @047ACCEPT@007 box that appears, and then CLICK ON the @047YES@007 box to confirm your choice.

CLICK ON @048INV2@007 now.

@015A TIME OUT EXAMPLE@007

You will have one minute to make a choice. If no choice has been made, you will be timed out. When a collector or an investor times out, the computer will use a value of zero for the collector's share or the investor's share respectively when calculating period earnings for the pair.

For the first forty seconds, Collector Main and Investor Main will operate normally. After the first forty seconds, the bar giving you access to other screens will disappear and the top bar will turn green. No matter what screen you are currently viewing, the computer will return you to the choice screen. When only ten seconds remain the top bar will switch from green to red. If you are timed out, the computer will use a value of zero to determine period earnings.

We will now demonstrate how the time out feature works using the Collector Main screen. Let the screen time out and note how the top bar changes color as time passes. This is the amount of time you will have to make a choice during the session.

CLICK ON @048TIMER@007 now.

@015WAITING SCREEN@007

While the person you are matched with is making a choice, a Waiting screen will appear. During the session you may use the What If... and Record screen to make calculations or review the history of play by switching to these other screens while you are waiting. You will be automatically switched to your main screen when it is time for you to make a choice. We will now view the Waiting screen. It will not be active. You will automatically return to these Instructions.

CLICK ON @048Waiting@007 now.

@015THE OUTCOME SCREEN@007

After all of the participants have completed their choices, the Outcome screen will appear. The outcome screen summarizes what happened at the end of each period for ten seconds. It reports the three choices made during the period and both participants' earnings. You will automatically return to these Instructions.

CLICK ON @048Outcome@007 now.

@015RECORD SCREEN@007

The computer will display the Outcome screen for ten seconds and then switch to the Record screen. The Record screen will be displayed for twenty seconds, but you may proceed to the next period by CLICKING ON @047RETURN@007. You may access this screen from either of the choice screens or the waiting screen.

The record screen displays the period outcomes and updates your earnings balance. Both the investor's and the collector's record screens display information in the following order: the period (Per), the collector's share (Coll Share), investor's share (Inv Share), the amount saved to balance (Amount Saved), the gross return from investment (Gross Return), the collector's period earnings (Coll Earnings), the investor's period earnings (Inv Earnings), your period earnings (Period Earnings), and your balance (Balance).

Yellow highlights will be used to indicate your type that period. Green highlights will be used to indicate your period earnings.

We will now view the record screen. It will be empty since you haven't made any choices, but once the screen is full you can @031PAGE UP@007, @031LINE UP@007, @031PAGE

DOWN@007, or @031LINE DOWN@007 to review previous outcomes. You will automatically return to these Instructions.

CLICK ON @048Record@007 now.

During the experiment a period ends once everyone has left the record screen either by CLICKING ON @047RETURN@007 or by being timed out. Remember that you can always return to the record screen from your choice screens or while waiting.

We have now completed the instructions. Again, it is important that you remain silent and do not look on other peoples' work. At the end of the experiment you will be paid your total balance in cash.

If you have a question, please raise your hand.

Appendix B: Treatment Means

Session	W	Design	Periods 1 to 20	Periods 21 to 40	Periods 41 to 60
Sess02	0.2	CDC	(36,46)	(18,32)	(39,51)
Sess11	0.2	CDC	(31,46)	(18,25)	(30,60)
Sess15	0.2	DCD	(18,30)	(38,43)	(20,29)
Sess03	0.4	CDC	(47,40)	(33,26)	(50,47)
Sess04	0.4	CDC	(45,36)	(35,13)	(48,47)
Sess20	0.4	CDC*	(47,42)	(38,13)	(49,46)
Sess13	0.4	DCD	(32,22)	(50,42)	(35,14)
Sess14	0.4	DCD	(28,44)	(48,37)	(35,13)
Sess18	0.4	DCD*	(36,16)	(48,43)	(35,17)
Sess09	0.5	CDC	(53,31)	(49,03)	(53,40)
Sess01	0.6	CDC	(63,21)	(58,03)	(65,24)
Sess06	0.6	CDC	(59,27)	(46,26)	(62,29)
Sess19	0.6	CDC*	(62,32)	(59,01)	(62,34)
Sess07	0.6	DCD*	(52,14)	(63,26)	(57,06)
Sess12	0.6	DCD	(49,22)	(65,23)	(54,11)
Sess08	0.6	DCD	(54,11)	(65,25)	(58,04)
Sess17	0.6	DCD	(50,21)	(64,28)	(58,04)
Sess05	0.8	CDC	(79,14)	(75,07)	(81,15)
Sess10	0.8	CDC	(81,13)	(76,06)	(81,16)
Sess16	0.8	DCD	(66,18)	(81,14)	(79,02)

Note. Mean earnings by session: All treatments (U, R) are denoted in cents. * denotes experienced subjects.

Acknowledgments

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