An Experimental Test of the Public-Goods Crowding-Out Hypothesis

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This paper presents an experimental test of the proposition that government contributions to public goods, funded by lump-sum taxation, will completely crowd out voluntary contributions. It is found that crowding-out is incomplete and that subjects who are taxed are significantly more cooperative. This is true even though the tax does not affect the Nash equilibrium prediction. This result is taken as evidence for alternative models that assume people experience some private benefit from contributing to public goods. (*JEL* C92, H41)

Several recent theoretical papers suggest that government policies toward privately provided public goods may be completely neutral. Peter G. Warr (1982, 1983), Russell D. Roberts (1984, 1987), and Theodore C. Bergstrom et al. (1986) show that government contributions to, for example, charitable organizations should crowd out private contributions dollar for dollar. B. Douglas Bernheim (1986) shows that this result can also extend to distortionary taxes. When one includes public goods within families, Bernheim and Kyle Bagwell (1988) show that all government taxes, subsidies, and transfers can have neutral effects on the consumption of all goods, both public and private.

The common feature of these results is that public goods act as conduits for "undoing" the effects of taxes and transfers. A dollar from my pocket to the public good can be retrieved by reducing the contributions I voluntarily make to the public good by a dollar. Similarly, a dollar from my pocket to yours can be retrieved if I reduce my contribution to the public good by a dollar and you increase yours by a dollar. Even if you and I contribute to two different public goods, as long as there is a third person who contributes to both, then we can find a chain of effects that will undo the transfer. In each case these actions restore the original (unique) Nash equilibrium consumption levels.

Several theoretical papers have questioned the generality of these neutrality results. For instance, Bernheim (1986) and Bernheim and Bagwell (1988) have argued that the theory predicts much more neutrality than is credible. Andreoni (1988a) argues that the pure public-goods model predicts that the fraction of contributors to a public good will shrink to zero as the economy gets large. This implies that the public-goods model is not likely to describe privately provided public goods in large economies, such as National Public Radio or the American Red Cross. I have shown (Andreoni, 1989, 1990) that if one assumes some private value to the act of giving, such as receiving a "warm-glow," then neutrality breaks down, and government contributions to charity will incompletely crowd out private contributions.[1] In addition, several

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[1]Similar models have been formulated by Howard Margolis (1982), Robert Sugden (1982, 1984), Richard Cornes and Todd Sandler (1984), Oded Stark (1985), Richard Steinberg (1987), Thomas R. Palfrey and Howard Rosenthal (1988), and John O. Ledyard (1992).

other studies provide indirect evidence that lead one to question the model of public goods and free-riding behavior. As noted in the recent survey by Robyn Dawes and Richard Thaler (1988), experimental subjects are typically more cooperative than predicted by the Nash equilibrium. Also, Daniel Kahneman and Jack L. Knetsch (1992) show that people's willingness to pay for public goods is positively related to their "moral satisfaction" from giving, not just the level of the public good per se; and Andreoni and John Karl Scholz (1990) find evidence of reference-group effects in charitable donations.

Despite these findings, the question of public goods crowding out remains fundamentally an empirical one. Several econometric studies have examined the crowding-out hypothesis with respect to charitable contributions. Using aggregate data, Burton A. Abrams and Mark A. Schmitz (1978, 1984) suggest that government grants crowd out private contributions at the rate of about 28 percent. Using similar data, Charles T. Clotfelter (1985) puts crowding-out on the order of only 5 percent. More recently, Bruce Kingma (1989) uses data on individual contributions to public radio to estimate crowding-out of roughly 13 percent.

While these econometric studies have failed to find a significant policy relevance for the neutrality results, they unfortunately cannot provide any direct evidence on neutrality theories per se. For instance, it is impossible to know whether the incomplete crowding-out found by Kingma (1989) and others is the result of certain institutional features not captured by the model, or whether it is due to individual preferences that are different than those assumed in public-goods models. One way to identify these possible differences is to control the institution but change the tax rates and then let preferences for giving express themselves naturally. The purpose of the study reported here is to provide such controls with a laboratory experiment. I will compare two groups that use the same institution to provide a pure public good, but who face different levels of "taxation" and "government" provision. The public good will be provided in a value-free environment; that is, subjects only know that if they contribute more to a public fund then other subjects will benefit financially at their expense, and vice versa. This controlled environment is intended to provide the most favorable conditions for complete crowding-out. If these conditions do produce complete crowding-out, then this may suggest that theories of institutions may be preferable to explain the econometric results. On the other hand, if it is found that crowding-out is incomplete, then this suggests that the taxation itself has affected the subjects' desire to give, and that preferences for giving may also be playing an important role.

The following section of this paper will discuss the experimental design for testing crowding-out. Section II will discuss the results of the experiment. Section III is a conclusion.

I. Experimental Design

A necessary condition for complete crowding-out is that the Nash equilibrium (before and after taxation) is interior. That is, it must be optimal, in equilibrium, for all subjects to contribute to the public good. This will require that a public-goods mechanism be designed that will yield an interior equilibrium, yet be easy for subjects to understand. This feature of the experiment presents a break with the majority of public-goods experiments conducted in the past. The standard public-goods experiment is designed so that giving zero to the public good is the Nash equilibrium for all subjects, while giving all of one's endowment to the public good is the symmetric Pareto efficient outcome (see e.g., Gerald Marwell and Ruth E. Ames, 1981; R. Mark Isaac et al., 1984, 1990; Oliver Kim and Mark Walker, 1984; Andreoni, 1986b; Isaac and Walker, 1988).[2] While this design is an ele-

[2] For an example of an experiment with an interior equilibrium, see Isaac et al. (1985).

TOTAL INVESTMENT BY THE OTHER TWO GROUP MEMBERS \ YOUR INVESTMENT	*0*	*1*	*2*	*3*	*4*	*5*	*6*	*7*
0	0	1	3	6	9	10	11	10
1	1	4	8	11	14	15	15	14
2	5	9	14	18	20	21	20	17
3	12	17	22	26	28	28	25	22
4	21	28	33	36	37	35	32	27
5	34	40	45	48	47	44	39	32
6	49	56	60	61	59	54	47	38
7	68	74	77	76	72	64	55	44
8	90	95	96	93	86	76	64	51
9	115	118	117	111	102	89	74	58
10	143	144	140	131	119	103	85	66
11	175	173	166	153	137	118	97	75
12	210	205	193	177	157	134	109	84
13	248	239	223	203	178	151	122	93
14	290	276	256	230	201	169	136	103

FIGURE 1. THE NO-TAX PAYOFF MATRIX

gant way to test free-riding, it obviously cannot be used to test crowding-out.

With this in mind, an experiment was designed in which a public good was provided by three subjects. Each subject was given a budget of seven tokens. Each subject could "play" from zero to seven of these tokens, with the payoff from any play depending on the number of tokens played by all of the group members. All subjects moved simultaneously. Subjects in the "no-tax" group were told that payoffs would be determined from the payoff matrix given in Figure 1, where payoffs are denominated in cents.

The matrix in Figure 1 was generated from a Cobb-Douglas utility function of the form $U_i = A[(w - g_i)^{1-\alpha}G^{\alpha}]^{\gamma}$, where g_i is the number of tokens played by the individual, $G = g_1 + g_2 + g_3$ is the total number of tokens played by the group, and w is an underlying level of "income." The parameters A, α, γ, and w are chosen to meet specifications of the Nash equilibrium outcome, Pareto efficient outcome, equilibrium payoff levels, and concavity of utility. As can easily be verified, the parameters of the utility function were set such that the unique Nash equilibrium is for each subject to play three tokens, and the symmetric Pareto efficient point is where each plays six tokens.[3] Therefore, the section of the payoff matrix where each subject chooses between three and six, inclusive, corresponds to the payoff matrix in the standard public-goods experiment. In this sense, this experiment adds a *border* around the *standard* payoff matrix. One should be careful to note that this change is not trivial. In particular, the border transforms a game with a dominant strategy equilibrium to one with a simple Nash equilibrium. The lack of a dominant strategy makes the game more difficult for subjects and makes the predictions about individual behavior less strict. These effects will be addressed below.

To test crowding-out, a second group of subjects was presented with identical instructions, but was given the payoff matrix in Figure 2. The matrix in Figure 2 was generated from the matrix in Figure 1 by

[3]In the first no-tax trial and in the tax-a trial the payoff to (3,5) was 47, rather than 48, as listed above. I thank Charles Holt for pointing out that in the original matrix the outcome {2,3,4} was also an equilibrium, while {3,3,3} is the only equilibrium in the matrix given in Figure 1. This did not appear to influence the results.

		YOUR INVESTMENT					
		0	1	2	3	4	5
	0	33	36	37	35	32	27
	1	45	48	47	44	39	32
	2	60	61	59	54	47	38
	3	77	76	72	64	55	44
TOTAL	4	96	93	86	76	64	51
INVESTMENT	5	117	111	102	89	74	58
BY THE	6	140	131	119	103	85	66
OTHER TWO	7	166	153	137	118	97	75
GROUP	8	193	177	157	134	109	84
MEMBERS	9	223	203	178	151	122	93
	10	256	230	201	169	136	103

FIGURE 2. THE TAX PAYOFF MATRIX

simply making the minimum possible contribution of each subject two tokens, rather than zero. This is meant to simulate a tax of two tokens on all subjects which is then donated to the public good. To present the game in terms of voluntary contributions to the public good, the choices were relabeled to start at zero. While the equilibrium in the *no-tax* condition was three tokens for all, the equilibrium in this *tax* condition is one token for all. Likewise, while the symmetric Pareto efficient allocation in the no-tax condition was six tokens for all, in the tax condition it is four tokens for all. Hence, the effect of the taxation is simply to trim the border around the standard payoff matrix described above, but not to affect the standard matrix at all; taxation does not alter or eliminate the equilibrium outcome or any of the (symmetric or nonsymmetric) Pareto efficient allocations.

Notice that, while the games presented in Figures 1 and 2 have the same Nash equilibrium prediction, they differ in their geometry, as well as in the amount of information presented in them. It is possible, therefore, that differences between play in these games may be due to the differences in presentation of the games rather than to real differences in incentives (Andrew Schotter et al., 1993). For that reason two more conditions are included in the experiment. The first is intended to test for the possibility that subjects may have a tendency to choose an average strategy, that is, one near the center of the distribution of choices. An average choice in Figure 1 is three or four tokens, while an average choice in Figure 2 is two or three tokens. To the extent that subjects tend to choose the average, it will bias the results in favor of finding incomplete crowding-out. For this reason a condition was run just as the no-tax condition, except that subjects were told that each subject must make a minimum investment of two tokens in the group investment. To help with this decision, subjects were also shown a payoff matrix similar to Figure 1, except that the cells that were unattainable were left blank. This will be called the *min* condition. The min condition maintains the same average choice as the no-tax condition but has the same payoff opportunities present in the tax condition. Hence, if subjects have a tendency to choose the average, then the subjects in the min condition will invest less, on average, than those in the tax condition.

Another possible presentation bias is that the payoff matrix for the tax condition has only 66 cells, versus the 120 cells in the no-tax condition. Since the no-tax condition may require greater cognitive effort, differences in behavior may reflect a greater degree of confusion. For this reason I also ran a condition in which subjects faced the payoff matrix identical to that seen in the tax condition (Figure 2), except that two additional columns, 6 and 7, and four additional rows, 11–14, were added to restore the matrix to eight columns and 15 rows. These additional columns do not alter the Nash equilibrium or the Pareto efficient allocations. In particular, columns 6 and 7, as well as column 5, are dominated by column 4. Hence, from a strictly strategic standpoint, it should never be in the interests of players to choose either of these additional strategies. Call this condition the augmented-tax condition, or *tax-a*. This condition introduces the tax, but maintains the cognitive complexity of the no-tax condition.

Each trial of the experiment required 18 subjects. Six trials were conducted: two of the no-tax condition, two of the tax condition, and one each of the min and tax-a conditions. In total, 108 subjects were involved in this experiment. Subjects were always recruited in groups of 36 and were then randomly divided into two groups of 18. The two groups were put in separate rooms and always participated in different conditions of the experiment. This was done in order to maintain the greatest control of random assignment of subjects to the various conditions. All subjects were drawn from intermediate and advanced undergraduate courses in economics, and none had been in economics experiments in the past. In each trial the instructions were read aloud to all subjects.[4] Hence, the payoffs and procedures were all common knowledge. It has been shown in previous public-goods experiments that it is important to give subjects experience in the game, and to eliminate the potential for strategic play. Hence, the procedures of the experiment were designed to provide experience for subjects and to diminish the possibility for strategic play. Subjects were told that they would be randomly assigned to a group of three subjects and that they would play the game with those same two other subjects for four decision rounds. After that they would be randomly reassigned to a new group of three subjects for an additional four rounds. This would continue for a total of 20 rounds. Hence, each subject would be in five different groups over the course of the experiment. After each round, subjects were told their choice in the last round, the sum of their partners' choices, and their payoff in that round. Subjects were not allowed to communicate, and at no time in the experiment did the subjects know which two of the remaining 17 subjects were in their group, nor did they know the exact choices or payoffs of any other subjects.[5] Each of these experiments lasted about 1 hour and 40 minutes. Subjects were paid anonymously, in cash, at the end of the experiment and earned an average of \$13.36 (standard deviation = \$2.48), ranging from \$7.98 to \$19.98.

Since this paradigm is new, I also sought to verify that subjects understood the instructions and the payoff matrix. For this reason a questionnaire was administered at the end of the experiment. Subjects were asked two questions that tested their understanding of the best response. Overall, 87.5 percent of the subjects answered both questions correctly, while 97.2 percent answered at least one of the two correctly, leaving two subjects who answered both questions incorrectly. Both of these subjects were included in the data reported here, but virtually identical results are obtained if the data on both subjects are dropped.[6] On the basis

[4]The instructions were administered by the same person in both rooms, despite their physical separation. A copy of the instructions used in the experiment may be obtained from the author upon request.

[5]The experiment was conducted with a single computer in the back of each room. The computer was programmed to receive input from the "decision forms" of all subjects, match groups, and print out "reports" for every subject. The decision forms and reports were collected and distributed by hand by graduate-student assistants.

[6]The questionnaire was not administered to the first no-tax trial or to the tax-a trial, but it was administered to the other four trials. A total of 72 of the 108 subjects were given the questionnaire. Of the two subjects who missed both questions, one was in the first tax condition, and one was in the min condition. There were no systematic differences across conditions or trials. A copy of the questionnaire is available from the author upon request.

of this questionnaire, it seems that the incentives of the game were well understood by the subjects.

II. Results

Average contributions by round are given in Table 1 for both the tax and no-tax groups. The average contributions in the tax condition have been written to include the tax. For instance, a choice of 0 by subjects facing the matrix in Figure 2 has been recorded as a choice of 2. This is done to make the real choices of the no-tax and tax groups directly comparable. One can see immediately some pronounced differences between these two groups. Looking first at the contributions by round, averaged over both trials, one sees that the no-tax group, on average, reaches or exceeds its equilibrium of three tokens per subject in only three of 20 rounds. On the other hand, the tax group reaches or exceeds its equilibrium of three tokens per subject in 18 of 20 rounds. This difference is highly significant.[7]

The second thing to notice from Table 1 is that the tax is not crowded out on average. Complete crowding-out would predict that average contributions, including the tax, should be the same. However, giving differs by 0.57, indicating an average crowding-out of 71.5 percent. A t test comparing the means of the no-tax and tax conditions finds the difference to be significantly different beyond the $\alpha \leq 0.01$ significance level, with $t = 3.38$.[8] This provides some evidence that crowding-out is incomplete. However, looking only at the end games (rounds 4, 8, 12, 16, and 20) one sees that the tax is completely crowded out in two of five end games, with average crowding-out increasing to 84 percent. The difference between the two conditions is still significant, however, but only at the $\alpha \leq 0.05$ level.[9] This suggests that the end games may be systematically different from the first three rounds of any four-round game. I will examine this possibility below.

Since the taxed group cannot contribute in negative numbers, simply looking at averages may understate the extent of crowding-out. In statistical terms, the above results may be biased by floor effects. To account for this possibility, I also look at the frequencies of contributions at all possible levels. Table 2 lists the frequency of contributions at every level for the two groups. The contributions of the tax group have again been restated to include the tax.

The first thing to examine in Table 2 is whether the existence of the tax made it more or less likely for the subject to choose moves outside the standard payoff matrix. To check this, collapse the data in Table 2 into three categories: contributions below the standard payoff matrix (i.e., 0–2), contributions within the standard payoff matrix (i.e., 3–6), and contributions above the standard payoff matrix (i.e., 7). The three figures for the no-tax condition are 0.400, 0.576, and 0.024, and for the tax condition they are 0.371, 0.587, and 0.042. A test for the difference between these two frequencies yields $X^2_{[2]} = 4.466$, which is nonsignificant. This suggests that shifting the border around the standard payoff matrix did not affect the probability that choices strayed out of the standard payoff matrix.

If taxation did not change people's propensity to choose moves in the standard payoff matrix, then one can ask whether it altered their choices among the elements within the standard matrix. To test this, collapse the cells outside of the standard

[7] A chi-square test yields a coefficient of $X^2_{[1]} = 31.278$, with $n_1 = n_2 = 20$. The difference is also significant if the two trials are considered separately ($X^2_{[1]} = 49.031$ with $n_1 = n_2 = 40$), or if one considers every group ever matched in the experiment ($X^2_{[1]} = 19.69$ with $n_1 = n_2 = 240$). This chi-square test is a test for the differences in two frequency distributions. The test is discussed in Wayne W. Daniel (1978 pp. 352–9).

[8] This test is $t = (\bar{x}_1 - \bar{x}_2)/\sqrt{\hat{\sigma}_1^2/n_1 + \hat{\sigma}_2^2/n_2}$ (see Daniel, 1978 pp. 174–82). Standard deviations are calculated across subjects. For the no-tax condition $\sigma = 0.65$, and for the tax condition $\sigma = 0.77$, while $n_1 = n_2 = 36$.

[9] For the no-tax condition the mean is 2.63 (standard deviation [SD] = 0.830), while for the tax condition the mean is 2.95 (SD = 0.799). This yields $t = 1.648$, which is significant for a one-sided test.

TABLE 1—AVERAGE REAL CONTRIBUTIONS TO THE PUBLIC GOOD PER ROUND FOR THE NO-TAX AND TAX CONDITIONS

Round	No-tax condition Trial 1	No-tax condition Trial 2	No-tax condition Average	Tax condition Trial 1	Tax condition Trial 2	Tax condition Average	Overall difference
1	2.94	2.83	2.89	4.05	3.66	3.86	0.97
2	2.66	2.55	2.61	3.72	3.72	3.72	1.12
3	3.33	2.61	2.97	3.38	3.72	3.55	0.58
4	3.11	2.38	2.75	2.77	2.77	2.77	0.02
5	2.72	3.33	3.03	4.27	3.61	3.94	0.91
6	2.72	2.72	2.72	3.44	3.38	3.41	0.69
7	2.88	3.05	2.97	4.11	2.94	3.53	0.56
8	2.72	2.66	2.69	3.22	2.88	3.05	0.36
9	2.88	2.55	2.72	4.05	3.55	3.80	1.09
10	3.22	3.11	3.17	3.77	3.11	3.44	0.27
11	2.88	2.83	2.86	3.61	3.22	3.42	0.56
12	2.61	2.55	2.58	3.00	3.00	3.00	0.42
13	2.77	2.83	2.80	3.77	3.33	3.55	0.75
14	2.61	2.66	2.64	3.44	2.83	3.14	0.50
15	2.77	2.61	2.69	3.27	2.94	3.11	0.42
16	3.05	2.72	2.89	2.94	2.88	2.91	0.02
17	3.27	2.77	3.02	3.50	3.61	3.56	0.54
18	2.44	3.05	2.75	3.72	2.72	3.22	0.48
19	3.05	2.38	2.72	3.38	2.83	3.11	0.39
20	2.11	2.38	2.25	3.16	2.83	3.00	0.75
Average:	2.84	2.73	2.78	3.53	3.18	3.35	0.57

Notes: The number of observations each round is 18 per trial, 36 per condition. The contributions reported in the tax condition include the tax.

TABLE 2—FREQUENCY DISTRIBUTION OF REAL CONTRIBUTIONS TO THE PUBLIC GOOD FOR ALL ROUNDS

Condition	Contribution 0	1	2	3	4	5	6	7
No-tax trial 1	0.097	0.086	0.202	0.350	0.138	0.041	0.047	0.036
No-tax trial 2	0.086	0.058	0.269	0.338	0.152	0.069	0.013	0.011
Average:	0.091	0.072	0.236	0.344	0.145	0.055	0.030	0.024
Tax trial 1			0.327	0.230	0.186	0.141	0.063	0.050
Tax trial 2			0.413	0.250	0.169	0.108	0.025	0.033
Average:			0.371	0.240	0.177	0.125	0.044	0.042

Notes: The total number of observations is 360 per trial, 720 per condition. The contributions reported in the tax condition include the tax.

payoff matrix, and consider the distribution of moves over the cells 0–2, 3, 4, 5, 6, and 7. By simply looking at Table 2 one sees that the tax group plays 3 much less often, but plays 4, 5, and 6 much more often. A test of the difference in these two distributions yields $X^2_{[5]} = 34.27$, which is highly significant. This effect can also be seen by simply comparing the frequency of plays of 4 or more between the tax and no-tax groups. The tax group chooses 4 or more 38.9 percent of the time, compared to 25.6 percent for the no-tax subjects, a difference which is highly significant ($X^2_{[1]} = 28.68$). This again suggests that the tax encourages cooperative behavior.

These effects are maintained if the data are organized by subjects. For each subject, tally the number of times the person chooses 4 or more. This gives a total of 36 observa-

TABLE 3—FREQUENCY DISTRIBUTION OF REAL CONTRIBUTIONS TO THE PUBLIC GOOD FOR END GAMES

	Contribution							
Condition	0	1	2	3	4	5	6	7
No-tax trial 1	0.133	0.044	0.200	0.422	0.100	0.022	0.033	0.044
No-tax trial 2	0.100	0.044	0.311	0.388	0.088	0.055	0.000	0.011
Average:	0.116	0.044	0.255	0.405	0.094	0.038	0.016	0.027
Tax trial 1			0.466	0.244	0.155	0.077	0.044	0.011
Tax trial 2			0.477	0.277	0.166	0.055	0.011	0.011
Average:			0.472	0.261	0.161	0.066	0.027	0.011

Notes: The total number of observations is 90 per trial, 180 per condition. The contributions reported in the tax condition include the tax.

tions each for the no-tax and tax conditions. Comparing the means of these two groups one finds $t = 2.41$, which is significant beyond the $\alpha \leq 0.01$ level.[10] This again shows that tax subjects are significantly more likely to make cooperative moves.

As indicated earlier, there may be less difference in the behavior of the two groups in the end games than in the first three periods of each four-round game. Hence, consider Table 3, which lists the same information as above for only the end games. Again, looking at the difference in the distribution over 0–2, 3–6, and 7, I calculate $X^2_{[2]} = 2.164$, which is nonsignificant. So even in the end games the tax does not affect the probability that the play will be in the standard payoff matrix. Looking at plays 0–2, 3, 4, 5, 6, and 7, one can calculate $X^2_{[5]} = 12.48$, which is significant at the $\alpha \leq 0.03$ level. Simply comparing the fraction of plays of 4 or above, I find $X^2_{[1]} = 3.93$, which is significant at the $\alpha \leq 0.05$ level. Finally, reorganizing the data by subject, one can compare whether the tax subjects are more likely to choose moves of 4 or more. This difference is again significant.[11] Hence, the results for the full sample are upheld when I consider only the end games.

These results indicate that when the group is taxed, it is more likely to choose cooperative moves as opposed to equilibrium moves. This provides further evidence of incomplete crowding-out. It remains to be seen, however, if part or all of these results can be attributed to pure presentation effects rather than to changes in incentives. This can be addressed by looking at the min and tax-a conditions, which were designed to test for presentation effects. The mean choice in the min condition is 3.35, with a standard deviation across subjects of 0.77. The mean choice in the tax-a condition was 3.50, with a standard deviation of 0.94.[12] These figures are remarkable in their similarity to the tax condition presented in Table 1. The average in the min condition is identical to the overall average in the tax condition, and the average in the tax-a condition is almost identical to the average in the first

[10] See footnote 8 for a discussion of this test. The mean proportion of choices of 4 or more is 0.256 (SD = 0.129) for the no-tax group and 0.389 (SD = 0.271) for the tax condition.

[11] The no-tax group chooses 4 or more with an average frequency of 0.178 (SD = 0.239) in the end games, while the proportion for the tax group was 0.267 (SD = 0.302). This difference yields a t statistic of 1.38, which is only marginally significant at the $\alpha \leq 0.09$ level for a one-sided t test. The reduced significance for end games is not surprising since the number of observations on each subject is reduced, which makes the data much more coarse. As I will show, the outcomes in the min and tax-a conditions were very similar to the outcomes in the tax condition. If one considers the min and tax-a subjects along with the tax subjects, it is possible to increase the number of subjects considered under the tax and increase the power of the test. Doing this generates $t = 1.82$, which is significant at the $\alpha \leq 0.04$ level.

[12] As above, the levels in the tax-a condition are adjusted to include the tax.

trial of the tax condition. Comparing the means of the min and tax-a conditions to the tax condition, I find that there are no statistically significant differences.[13] However, comparing the means of min and tax-a to the no-tax condition, I find significant differences for both.[14] Results similar to the above hold if I use either the min or tax-a data rather than the tax data.

III. Discussion and Conclusion

This paper presents an economic laboratory experiment designed to test the public-goods crowding-out hypothesis. The experiment controlled for the institution by inducing preferences for a pure public good. Hence, the design was intended to present the most favorable circumstances for complete crowding-out to be observed. The principal result of the experiment is that crowding-out is incomplete. The existence of a tax that is donated to the public good does not affect the propensity of subjects to make contributions in the standard payoff matrix, that is, between the equilibrium move and the symmetric Pareto efficient move. However, the tax does affect the allocation of moves within the standard payoff matrix. In particular, the tax makes subjects much more likely to choose cooperative moves rather than Nash equilibrium moves. The subjects who are taxed are significantly more cooperative than those who are not taxed.

This experiment suggests several ways in which research on public goods can be extended. First, this experiment has considered relatively small groups. This was done for tractability. Other public-goods experiments have typically considered groups of 4–10 subjects, with one even venturing groups of 100 (Isaac et al., 1990). It will be important to see if incomplete crowding-out extends to these more familiar environments. Second, this experiment has employed a between-groups design, that is, different subjects were in different conditions, rather than a within-groups design in which the same subjects are in different conditions at different times. While the between-groups design was chosen to avoid possible order effects that could bias results, other inferences could be made by extending the paradigm to a within-groups comparison. Third, the repeated nature of these games opens up the possibility that the tax could simply be affecting the rate at which subjects converge to equilibrium behavior. More experience with play in end games could address this possibility. Fourth, in this experiment the government's provision was exogenous. A recent experiment by David S. Brookshire et al. (1989) increased contributions by shifting a disproportionate share of the marginal benefits to a single contributor. They found that this endogenous increase in giving can "seed" giving by others. This could mean that increasing gifts by other players, rather than the government, could possibly "crowd in" rather than crowd out contributions. Finally, these results suggest that experiments could be conducted to help refine theories of behavior that could generate incomplete crowding-out.

While this experiment does not provide direct evidence for the motives that people may have for contributing more in the presence of taxation, the behavior in the experiment is broadly consistent with the hypothesis that people get pleasure from the act of contributing to the public good. For instance, if people like to give but also get utility from the public good, they may give less to the public good if they feel their generosity is being "taken advantage of." When the public good is supplied partly by taxation, then the tax provides a lower bound on the extent to which their generosity can be exploited by others. In this experiment, for instance, everyone in the tax condition is guaranteed that everyone else will donate at least two tokens to the public good. Hence, taxation reduces the extent to which generosity can be exploited. So even though the Nash equilibrium contribution is three tokens for all (including the tax, if any), the tax may make it more ap-

[13] For min vs. tax, $t=0$, and for tax-a vs. tax, $t=0.580$.

[14] For min vs. no-tax, $t=2.66$, and for tax-a vs. no-tax, $t=2.91$.

pealing for warm-glow givers to contribute 4, 5, or 6.[15]

Although this experiment finds significant evidence of incomplete crowding-out, the 71-percent crowding-out that is identified is quite large relative to econometric studies which put crowding-out of charitable contributions at only 5–28 percent. In contrast to the outside world, the controlled setting of the laboratory deliberately eliminates other factors, such as sympathy, political or social commitment, peer pressure, institutional considerations, or moral satisfaction associated with particular causes that may influence contributions to public goods in general. The fact that the crowding-out measured here is much higher than in econometric studies indicates that perhaps these other factors may also be having important effects on individual contributions.

The results of this paper suggest that there may be a need for more study of the determinants of people's preferences for giving and the factors that can encourage cooperative behavior. It may also be beneficial to examine theories of fund-raising, social pressure, and peer-group effects that may manipulate or take advantage of such preferences for giving. All of this may lead to a better understanding of the successful provision of so many public goods by the private sector.

[15] See John H. Miller and Andreoni (1991) for evidence of a similar decision process in standard public-goods experiments. A behavioral heuristic which requires no warm-glow from giving but which would generate identical behavior is that subjects desire cooperative payoffs but adopt protective "sucker aversion" strategies. I am grateful to Vernon Smith for this suggestion.

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