Distinguishing the Role of Authority "In" and Authority "To"

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Abstract: Authority, and the behavioral response to authority, is central to many important questions in public economics, but has received insufficient attention from economists. In particular, research has not differentiated between legitimate power and the presumption of expert knowledge, what we call authority "to" and authority "in." In this paper we report on the results of a series of lab experiments designed to distinguish the effects of the two sources of authority on contributions to a public project. The results suggest that authority "to" and authority "in" interact in ways not heretofore understood. Penalizing non-social behavior without expert explanation does not increase voluntary contributions, nor does expert explanation without the threat of penalty, but together they induce more contributions than any other combination of policies. We interpret these findings to indicate that the reaction to an authority depends on whether that authority is perceived to be legitimate.

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1. Introduction and Motivation

A wide range of behaviors raise doubts about whether, in all situations, individuals successfully pursue their self-interest. Many people are attracted to default options, say on pension offerings from their employer.[1] People often forgo financial education, even when it is inexpensive to obtain and when having it would substantially change their behavior.[2] Some dutifully comply with the tax system, even though the odds of being detected and penalized suggest that some evasion would be privately optimal.[3]

The idea that people respond to authority provides a unifying explanation of these, and many other, behaviors. Employees choose the default pension plan because they infer that their employer, a benign authority figure with financial expertise, has chosen in their interest. Workers save more after receiving employer-sponsored financial education because an authoritative instructor emphasized the importance of planning for retirement. Households comply with their tax liability in the face of a favorable tax evasion "lottery" because the tax authority has instructed them to do so and may sanction them if they do not; in addition, households know that by collecting taxes to provide services, the government may benefit them.

We believe that authority, and the behavioral response to authority, is central to many important questions in public economics, but has received insufficient attention from economists. Its relative absence is surprising given the burgeoning interest of economists in psychology, and the historical prominence—indeed notoriety—of the experiments on authority and obedience by Stanley Milgram (1974). Milgram famously showed that, when prompted by a white-coated authority figure, subjects were often quite willing to administer what were apparently painful shocks to people (who were actually confederates of the experimenter) behind a glass door. Subsequent commentators, notably Morelli (1983), differentiated between legitimate coercive power and the presumption of expert knowledge; we refer to these aspects of authority as authority "to" and authority "in," respectively.[4] Reaction to the former may be characterized as "obedience," while response to the latter might be better denoted as "deference." Neither kind of

[1] See, for example, Madrian and Shea (2001), Choi, Laibson, Madrian, and Metrick (2004), and Beshears, Choi, Laibson, and Madrian (2009).
[2] Lusardi and Mitchell (2007), Bernheim and Garret (2003), and Martin (2007) investigate the consequences of financial knowledge and education.
[3] Feld and Frey (2002) discuss this issue.
[4] These different sources of authority are also delineated in French and Raven's (1959) classic descriptions of coercive and expert power.

response need be a mistake; responding to authority does not require that people care about anything but consumption of standard goods and services.

While responding to the incentives or information offered by an authority may be rational, it is likely that people will respond differently to the same actions, depending on whether those actions were taken by a (government) authority or by an individual. Many experimental results, such as those reported in Blount (1995), suggest that beliefs about what motivated another person and about the appropriateness of those motives, their "intentionality," are critical to explaining behavior toward that person. Furthermore, ultimatum games with multiple players suggest that responders care about whether proposers are unfair to *them*, but do not care much about how the proposer treats others. Government policies often do not single out particular individuals other than through enforcement actions. Thus, a key unanswered question is the extent to which individuals ascribe human qualities like kindness, meanness, or distrust to a (government) authority and react in similar ways as they do to individuals who exhibit these characteristics.

In this paper we report on the results of a series of lab experiments designed to distinguish the effects of the two sources of authority on contributions to a public project. We adopt a lab-based, experimental approach because we want, eventually, to develop and refine hypotheses about the role of authority in economic decision making. Distinguishing these aspects of authority is inherently difficult to do with observational study; it is hard to find circumstances where these features of government or authority have been randomized across jurisdictions. In the lab, however, one must of course be careful to distinguish the effects of the experimenter's scrutiny and authority from the effects of other sources of authority.

We focus on a public-goods experiment in particular because we hypothesize that the details of the economic environment are likely to affect the influence of authority on individual behavior. If so, gathering observations from specific environments will be useful and a public goods problem is an attractive place to start. One attraction of this setting is that lab-based public goods experiments have been studied extensively, so our findings can be compared with a variety of benchmarks. In addition, the setting of public goods experiments resembles a broad set of important public economics applications, including tax compliance and charitable contributions.

Our experiments are designed to distinguish the effects of two sources of a government's authority: the expertise to know the appropriate trade-off between private and public activities, and the power to punish those who diverge from the "appropriate contribution."

More specifically, we examine three questions:

1. Do experimental subjects' contributions depend on the existence, and source of, expert advice and explanation for the advice?
2. Do experimental subjects' contributions depend on the threat of penalty for insufficient contribution?
3. What is the interaction between these two sources of authority on behavior?

This examination is crucial to the interpretation of lab experiments focused on individuals' behavior toward an authority figure such as a government, because both internal validity and external validity questions are germane. Internal validity issues, often called "experimenter demand effects" (EDE), arise when behavior by experimental subjects depends on experiment-specific cues about what constitutes appropriate behavior. As Zizzo (2010, p.77) states, "it is unavoidable that the experimenter is in a position of authority relative to subjects," having both legitimacy and expertise.[5] Zizzo (2008, p. 6) notes that the Milgram experiment is "an extreme case of EDE at work in an experiment where the effect of such social EDE was itself the objective of the experiment." External validity questions arise when, for example, the authority of the real-world tax enforcer (often referred to as the tax authority) is crucial to behavioral response, and is not meaningfully replicated in a lab.

We proceed as follows. In section 2, we describe the experimental research design. In sections 3, 4, and 5 we present and analyze the results, first in tabular form and then with regression analysis. In section 6 we focus on calculator use during the treatments and how it relates to contribution behavior. In section 7 we step back and discuss the results from a broad perspective, and in section 8 relate them to the existing literature. Section 9 concludes.

2. Research Design

2.1. Experimental procedures

The experiments were conducted at the Institute for Social Research at the University of Michigan. Most were conducted from June 2010 through October 2010, and about two-fifths

[5] Levitt and List (2007) also describe various mechanisms behind experimenter demand and "scrutiny effects."

were conducted under the same conditions from November 2012 through January 2013.[6] Most of our subjects were undergraduates at the University of Michigan. The average age of subjects was 21 years, with 54% being female; 11% of the students were economics majors. Instructions were read aloud to the subjects to create common knowledge. The experiment was programmed and conducted with the software z-Tree (Fischbacher, 2007). Experimenter-subject interaction was minimal; immediately after the instructions were read and questions were answered, the experimenter left the room.

The experiment consisted of five main treatments and 45 sessions. Each session had 12-16 subjects. In total, we had 668 subjects. Each subject participated in only one treatment. There were three parts to each treatment. Part 1 consisted of the public-goods experiment, which had 20 decision-making periods. After Part 1 of the experiment, subjects were provided with instructions for Part 2, which consisted of two activities. Part 3 was a short questionnaire. On average, sessions lasted for 75 minutes.

During the experiment, subjects' earnings were calculated in tokens. After Part 1 was completed, a computer randomly selected one period from Part 1 on which to base earnings from that part. Subjects' final earnings were given by the earnings of subjects in that period plus their earnings in both activities of Part 2. At the end of the experiment, the total amount of tokens earned was converted to cash at a rate of 100 tokens per one U.S. dollar. Average earnings were approximately $22.

2.2. The public-goods experiment

The public-goods experiment occurred in Part 1 of each session. We adopted a stranger design: in each of the 20 periods of Part 1, subjects were randomly re-matched into groups of 4. No one ever learned the identity of the other members of his or her group in any period.

At the beginning of each period, subjects were endowed with an income of 100 tokens and asked to contribute to a group project the return from which depended on the total contributions of the group members. Each subject earned the same amount from the group project:

$$v(T) = 8T - 0.02T^2,$$

[6] All sessions were run by one of two assistants (with similar demographics) who received the same training by the researchers and also worked together to agree on the exact procedures to follow during the entire experiment.

where $T = \sum_{i=1}^{4} t_i$, and t_i denotes the contribution of subject *i* to the group project, and where $i \in \{1,2,3,4\}$. Thus, the production function for the public good was non-linear in contributions, and the marginal return was decreasing with total contributions. The tokens that a subject did not contribute to the group project (100 - t_i) were invested in a private project. The private project paid 10 times the amount invested, yielding 10(100 - t_i). Note that the private return to contributing to the group project was less than 8, so the Nash equilibrium is always to contribute nothing.

At the end of each period, each subject was provided with an income screen that reminded the subject of his or her contribution to the group project. This screen also informed the subject of the combined contributions of the other members of his or her group in that period, the subject's income from the group project and the subject's income from the private project. In addition, if penalties were part of the treatment, each subject also learned whether he or she had been fined and the amount of his or her total income in that period. Figure 1 shows a typical screenshot from the experiment.

One novelty of our design is that the production function for the group project was not explicitly described to the subjects. Instead, they were provided with a calculator screen that revealed the income from the group project upon entering two numbers: (1) one's own contribution to the group project, and (2) a guess about what the other members of the group contributed.[7] The calculator added these two values to get the total group contribution and the subject's income from the group project. Subjects were able to use the calculator as many times as they wanted before they decided how much to contribute to the group project. Thus they could, in principle, solve for the Nash equilibrium.[8] The main reason that we used non-linear returns and chose not to provide direct information about the payoff structure is to make the socially efficient level of contributions less obvious to the subjects, and therefore to make subjects potentially more sensitive to the information provided. This is important because we examine whether expertise and the source of expertise matter for subjects' contributions.

[7] The calculator screen also contained a reminder of the treatment details (see Figure 1).
[8] Even though we do not expect that subjects tried out all the possibilities, it was straightforward for them to see that, given the contributions of the others in their group, contributing more to the group project always decreased one's own payoff.

2.2.1. The control treatment

In the control treatment, we asked subjects to decide how much to contribute to the group project, with no suggested amount provided, making it a voluntary (non-linear) public good game. We refer to this as the NO SUGGESTION treatment.

To summarize, in the control set-up each subject earned

$$\pi_i = 10(100 - t_i) + 8T - 0.02T^2 .$$

In the Nash equilibrium, each subject would contribute zero to the group project. On the other hand, the socially optimal level of the group project is the level of contribution that maximizes the joint payoffs. In particular, at the social optimum, total contributions are 137.5. To achieve that level, each subject would have to contribute approximately 34 tokens.

In the second treatment (denoted BASELINE), the instructions ask subjects to contribute the socially optimal amount, but did not provide an explanation for asking that amount. The only difference in the instructions is the addition of the following sentence:

"In each period, even though you can contribute any amount of your endowment to the group project, you are asked to contribute exactly 34% of your endowment (34 tokens)."

2.2.2. Treatments examining the effect of authority "in"

In the next three treatments, we explore the effect of what we call authority "in" (AUTH-IN), where the experimenter provides an explanation for the suggested contribution amount, sometimes supported by a statement of expert agreement. Three varieties of this main treatment were conducted. For each of these varieties the key sentence of the instructions was replaced by the following paragraph:

"In each period, even though you can contribute any amount of your endowment to the group project, you are asked to contribute exactly 34% of your endowment (34 tokens). **If everyone contributes 34 tokens**, then the sum of income of all members of your group from the group project and the private project will be as high as possible."

In the first variety only this sentence was added. In the second and third varieties we investigated the effect of providing different forms of external expert support for the suggested contribution level. In the second variety, we explained that a group of experts, four graduate students in economics, had verified that equal contributions of 34 tokens would in fact maximize

the sum of all members' payoffs.[9] In the third variety the expert panel consisted of four economics faculty members instead of graduate students.[10] Our main focus in this paper will be the total effect of authority "in," and, therefore, in what follows we aggregate the three varieties of expert advice. In Section 5 we briefly discuss the differences in behavior associated with different sources of expertise.[11]

2.2.3. Treatments examining the effect of authority "to"

The remaining treatments examined the effect of authority "to" in the form of a probabilistic penalty for failure to contribute a minimum amount. (The other aspects of these treatments were the same as in the BASELINE and AUTH-IN treatments.) Subjects were told that in each period, with probability 0.25, the computer would perform an "audit" of the subject and would assess a penalty if the subject contributed less than 34 tokens. The penalty was set equal to twice the amount that the subject's contribution fell short of 34. If there is no audit (with probability 0.75), then there is no fine even if the subject contributed less than 34 tokens.

With the probabilistic penalty, the expected income of a subject is given by

$$\pi_i = 10(100 - t_i) + 8T - 0.02T^2 - 0.25(2)(34 - t_i)$$

for $t_i \leq 34$. Note that the expected punishment is small enough that the Nash equilibrium remains a zero contribution.[12] This is true even under extreme degrees of risk aversion. The reason is that, even if the audit rate were 100%, the marginal cost of withholding a token would only be two tokens; the private return to contributing to the public project is still less than the return to the private project. Contributing zero tokens to the group project weakly dominates any other level of contribution.

We implement the authority "to" treatments both in the absence of any experimenter/expert explanation (denoted AUTH-TO) and in conjunction with the authority "in" aspect, denoted AUTH-BOTH. As we discuss later, it is the AUTH-BOTH treatments that have the largest effect on contributions.

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[9] This was not a deceit. In fact, four University of Michigan economics graduate students made this calculation. Subjects were provided with short bios of the students.

[10] Again, a group of four University of Michigan economics professors did, in fact, make this calculation.

[11] The results do not support the hypothesis that named, presumably expert, authority increases contributions. Nor do they support that student subjects are induced to contribute more by faculty, rather than graduate students, corroboration of the social benefit of contribution.

[12] A relatively low punishment is chosen both to parallel actual tax evasion penalty regimes and to maintain a similar level of predicted contributions throughout the treatments.

A summary of the experimental design is provided in Table 1.

Table 1. Summary of Experimental Design

Treatment		Number of sessions	Number of subjects	(Audit probability; penalty rate)	Level of expertise
NO SUGGESTION		9	124	-	No suggested level
BASELINE		9	136	-	Suggestion w/o explanation
AUTH-IN	EXP	3	40	-	Suggestion with explanation
	GRADEXP	3	44	-	Suggestion with grad support
	FACEXP	3	48	-	Suggestion with faculty support
AUTH-TO		9	132	(0.25; 2)	Suggestion w/o explanation
AUTH-BOTH	PEN_EXP	3	48	(0.25; 2)	Suggestion with explanation
	PEN_GRADEXP	3	48	(0.25; 2)	Suggestion with grad support
	PEN_FACEXP	3	48	(0.25; 2)	Suggestion with faculty support

2.2.4. Attitudes and demographic information

Parts 2 and 3 of the study were designed to collect information about the subjects' risk aversion, social preferences, attitudes toward taxes and government, and demographic attributes. We will use these measures as covariates in the analysis of the experimental results. In Part 2, subjects were asked to perform two separate activities. The first activity, designed to obtain information about risk preferences, asked subjects to choose from among five gambles. The first gamble was degenerate – it paid 200 tokens with certainty. The remaining four gambles involved a 50-50 chance of receiving either a small or a large amount. The expected value of these gambles increased – with the smaller amount decreasing and the larger increasing – up to Gamble 5. That gamble involved the greatest risk and the highest expected value, paying 600 tokens with probability 0.5 and nothing otherwise.[13]

[13] We thus use an ordered lottery selection design in ranking subjects with respect to their risk preferences. See Harrison and Rutström (2008) for a detailed discussion of this and other risk-elicitation procedures.

In the second activity of Part 2, subjects were randomly matched with another participant and had to decide between the three following options that vary a dimension of social preferences—the trade-off between one's own payoff and the sum of payoffs to oneself and another subject:[14]

1. You will receive 200 tokens and your paired participant will receive 200 tokens.
2. You will receive 175 tokens and your paired participant will receive 300 tokens.
3. You will receive 225 tokens and your paired participant will receive 100 tokens.

Below we refer to the subjects who picked Option 1 as "fair," Option 2 as "efficiency maximizer," and Option 3 as "selfish/rational."

In Part 3 we asked the subjects their gender, age, undergraduate major, as well as two other questions that are designed to measure tax morale and trust in public officials. In particular, subjects were asked whether they agree with the following statements: (1) "cheating on taxes, if you have the chance, can never be justified" (tax morale), and (2) "public officials can usually be trusted to do what's right" (trust in public officials). These two questions were adapted from similar questions asked in the World Values Survey.

3. Tabular Results

Figure 2 shows the mean contribution per period for our five main treatments. It is evident from the figure that, with the exception of the NO SUGGESTION treatment, contribution levels in all our treatments are similar in the first few periods. However, contributions in the AUTH-BOTH treatment are consistently higher than in the rest of the treatments after the initial few rounds. The difference between this treatment and the others is more pronounced as the experiment progresses. Therefore, in the analysis that follows we will examine not only the contribution averages over all periods, but we will also study separately behavior in the later periods of the experiment.

In the following analysis we use session averages as independent observations. In total there are nine independent data points per treatment. Mean contribution levels (and standard errors) for each treatment, including sub-treatments, averaged over all periods and then

[14] The computer randomly chooses one of the participants with equal odds, and implements her decision.

separately for the first five and last five periods, are provided in Table A1 in the online appendix.[15]

3.1. The effect of suggestion

Consistent with previous experiments on public goods, in all of the treatments the median contribution is significantly greater than the Nash equilibrium of zero (p-values < 0.01, Wilcoxon signed-rank tests). This is true even in the NO SUGGESTION case.[16] By far the lowest mean contribution occurs when the subjects are given no suggestion about how much to contribute, 8.61 tokens (with a standard error of 0.61) contributed compared to a minimum of 12.85 (with a standard error of 0.81) in the other four treatments. Comparing the no-suggestion treatment with BASELINE, where a suggestion for the level of contribution is made without explanation, we see that in the latter treatment contributions increase by approximately 50% to 12.99. This is illustrated in Figure 2. We also find that, on average, contributions fall off sharply as the 20 periods of the experiment progress. In two out of five treatments the average contribution level in the last five periods is less than half its level in the first five periods.

Table 2a is a triangular table that displays the differences in the mean contribution across treatments. Two-tailed t-tests show that contributions in the NO SUGGESTION treatment are lower than in each of the other treatments including the BASELINE treatment (all comparisons are significant at 1% significance level).[17]

The data in Table 3 indicate that the suggestion is, in fact, focal for some subjects. When contributing exactly 34 tokens is not mentioned (NO SUGGESTION), essentially no subject contributes exactly that amount. In the BASELINE treatment, however, 15% of period contributions are exactly 34.

3.2. The effect of authority "in"

We have learned that having an authority, the experimenter, merely suggest ("ask" for, in the instructions provided) a contribution level increases contributions.[18] What if the

[15] http://www-personal.umich.edu/~neslihan/AuthorityAppendix.pdf
[16] Our findings in the baseline treatment are in line with most other non-linear public goods experiments. Laury and Holt (2008) provide a discussion of public goods experiments with a non-linear design.
[17] We have also performed Wilcoxon tests and find the results to be very similar.
[18] Of course the effect depends on the magnitude of the recommendation relative to the Nash equilibrium contribution.

experimenter provides an explanation for that suggestion, in particular an explanation about the potential benefits of widespread contributions to the public good? We find that, in the absence of a penalty regime, a social-benefit explanation does not increase the mean contribution. Indeed, a social-benefit explanation without a penalty is accompanied by a slight *decrease* in mean contributions of 1.4%, from 12.99 to 12.85, although this still amounts to a substantial increase of 47% over the NO SUGGESTION mean. As we emphasize below, the impact is more evident when an explanation is provided *and* a punishment for low contributions applies: the effect of authority "in" is much larger when there is a punishment provided, a mean increase of 2.29, or 18.4 percent, from 12.43 to 14.73 (*p*-value = 0.02). This difference typically increases as the experiment progresses. For example, if we only look at the last 5 periods of the experiment, the difference in contributions between AUTH-TO and AUTH-BOTH increases to 3.19, or 35.1 percent (*p*-value = 0.02, see Table 2b).

3.3. The effect of authority "to"

Levying a probabilistic penalty for contributing less than the socially efficient amount increases the expected net private return to contributing, although it preserves the characteristic that Nash equilibrium behavior is a zero contribution. We can get a sense of the influence on contributions of a penalty by comparing the contribution pattern for each setting of explanation/expertise (i.e., by comparing the results in BASELINE to AUTH-TO and by comparing the results in AUTH-IN to AUTH-BOTH).

The results suggest that the impact of authority "to" depends on the extent of authority "in." When no explanation is offered for the suggested contribution, adding a penalty actually decreases mean contributions by 3.8 percent, from 12.99 to 12.43, although the difference is not significantly distinguishable from zero (*p*-value = 0.55). In contrast, when the suggestion is accompanied by an explanation, adding a penalty increases mean contributions by 15.2 percent, from 12.85 to 14.73 (*p*-value = 0.11). As discussed above, the difference is higher and more significant if we focus on the later periods in the experiment. For example, the difference in contributions between AUTH-IN and AUTH-BOTH becomes 3.09 corresponding to a 33.7 percent increase in the last five periods (*p*-value = 0.05).

3.4 Authority "in" and authority "to"

As the two previous sections suggest, what makes a difference to contributions is the combination of authority "in" (an expert explanation) and authority "to" (a possible penalty for less-than-socially optimal contributions). Contributions to the public good increased 71.1 percent with an explanation and penalty compared to the NO SUGGESTION treatment (*p*-value < 0.01), and more tellingly by 13.4 percent compared to the BASELINE treatment (*p*-value = 0.14). The percentage difference in contributions between the BASELINE and AUTH-BOTH rises to 40 percent when we consider only the last five periods, with the difference being significant at the 5 percent level (*p*-value = 0.03).

This and the previous results showing a stronger effect of the AUTH-BOTH treatments are due to a smaller decline in contributions as the experiment proceeds in the AUTH-BOTH treatments (see Figure 2).[19] In public goods experiments it is common to observe a decline in contributions over periods. Here we see that the combination of authority "in" and authority "to" is strong enough to reduce the amount by which contributions decline as the experiment proceeds. Thus, relative to the no suggestion condition, the authority treatments do not simply affect the initial contribution levels, with a uniform dilution over time, but affect both the initial levels and the gradients. Table 3 also shows that the higher average contribution in AUTH-BOTH is related to a noticeably higher fraction of period contributions exactly at 34, but not to a higher likelihood of a contribution in excess of 34.

4. Regression Analysis

With multivariate regression analysis, we can explore more precisely the partial effects of the treatment elements and also investigate the relationship to contribution decisions of subject attributes and attitudes.[20] In all of the results, shown in Table 4, we report robust standard errors clustered at the session level. The first three columns show the results of OLS regressions where the dependent variable is the individual's contribution to the public project. The second set of three specifications contains linear probability models of whether the subject contributed exactly 34 tokens in a period. Within each set of three specifications the first column

[19] The drop-off in contributions across treatments is also evident in Table A1.
[20] In the paper we use OLS regressions and linear probability models for multivariate regression analysis. We have also performed Tobit regressions, whenever appropriate, and found that none of our qualitative results change. These results appear in Tables A2 and A4.1 of the appendix.

includes all observations, the second includes only the first five periods, and the third only the last five periods.

In Table 4 the variable NO SUGGESTION takes a value of 1 for no suggested level of tax, and 0 otherwise, the other treatment variables are also coded as dummy variables, and BASELINE is the omitted treatment. The variable *period* takes values from 1 to 20.[21] The variable *gamble* takes values from 1 to 5, where 1 corresponds to the riskless lottery and 5 corresponds to the riskiest lottery. The variable *fair* takes a value of 1 if a subject chooses the fair option in activity 2 of Part 2, and a value of 0 otherwise. The variable *efficiency* takes a value of 1 if a subject chooses the "efficiency maximizer" option in activity 2 of Part 2, and value of 0 otherwise. The variable *age* is the age of the subject in years. The variable *female* takes a value of 1 if the subject is female and 0 otherwise. The variable *econ* takes a value of 1 if the subject is an economics major and 0 otherwise. The variable *taxmorale* takes values from 1 to 7, where 1 means that a subject "completely disagrees" that cheating on taxes can never be justified, and 7 implies that a subject "completely agrees" with that statement. The variable *trustinpublicofficials* also takes values 1 to 7 where 1 implies that a subject "completely disagrees" that public officials can usually be trusted to do what's right, and 7 implies that a subject "completely agrees" with that statement.

In specification (1) of Table 4, NO SUGGESTION significantly affects contributions, decreasing contributions by 4.30 compared to the baseline treatment with a suggestion but no expert explanation. This is consistent with the tabulated results discussed earlier. Estimates of the other treatment effects also corroborate the conclusions we drew from the tabular results: exposing the subjects to both authority "in" and authority "to" increases their contributions, while neither treatment by itself does. The point estimate of 1.86 on the "authboth" treatment is about 43 percent of the size of the estimated effect from suggesting a contribution level. With our sample size the effect of both authority "in" and authority "to" is only significant at the 20 percent level (p-value = 0.16).

Columns 2 and 3 repeat this specification including only contributions from the first five and last five periods, respectively. Comparing the results of these two sets of regressions reveals that the negative effect on contributions of offering no suggestion fades over time, being

[21] As a robustness check, we also investigated a specification that allowed for a non-linear learning process, which yielded no qualitative difference from the results reported here.

associated with just below six less tokens in the early period but only about half of that by the end. In contrast, the combination of authority "in" and authority "to" is notably *stronger* near the end of the experiment. By the last five periods, it is associated with 3.78 more tokens contributed, significantly different from zero at a 5 percent confidence level.

The results in columns 1-3 also indicate that the attitudinal variables *fair* and *efficiency* are each significantly associated with contributions in the expected direction. The estimated coefficient on the variable *gamble* has a negative sign and is marginally significant even though the degree of risk aversion should not affect contributions in our experiment. In contrast, the estimated coefficients of *fair* and *efficiency* are large, positive, and highly significant; these effects are relative to "selfish" subjects. These results are consistent with the existing experimental literature, which documents that people with fairness or efficiency concerns are willing to give up their own earnings in order to help others.[22]

While the estimated coefficients of both *taxmorale* and *trustinpublicofficials* are positive, only *trustinpublicofficials* has a (marginally) significant partial association with the level of contributions.[23] Of the three demographic variables, only *female* is (positively) associated with the magnitude of contributions in a statistically significant way.[24] In all specifications, the estimated coefficients on the *period* variable suggest a substantial downward drift over time in the level of contributions.

Columns 4 through 6 of Table 4 explore, using a linear probability model, the determinants of subjects contributing *exactly* 34 tokens, which is the contribution level that both the authority "in" and authority "to" treatments highlight. This is of particular interest if subjects treat the authority's justifications literally. The same patterns seen in specifications (1) through (3) emerge, and the effect of the combined authority treatments becomes even clearer. Considering all 20 periods the combination of authority treatments increases the probability of contributing exactly 34 tokens by 8 percent (compared to an overall mean of 13.8 percent), which is significantly different from zero at a 5 percent confidence level; by the last five periods

[22] See, for example, Andreoni (2006), Camerer (2003), Charness and Rabin (2002), and Fehr and Schmidt (2006).
[23] This result is not particularly surprising considering that the transfers in the experiment are not between the subject and a government authority or public officials.
[24] As expected, due to the random assignment of subjects to treatment, adding the attitudinal variables as explanatory variables does not greatly change the other estimated coefficients. Regression results without control variables can be found in Table A3 of the online appendix.

the combined treatment is estimated to increase the probability by 10 percent, significant at a 1 percent standard.

We note that, although other things equal, females contribute more, they are not more likely to contribute exactly 34 tokens. Nor does professed trust in public officials have a significant partial association with giving exactly 34 tokens, while it did affect average giving, at least at a 10 percent confidence level.

5. Dissecting the Interaction of Authority "In" and Authority "To"

When combined, authority "in" and authority "to" appear to have a non-additive positive effect on giving. To better understand the sources of that interaction effect, we examine these treatments with a higher degree of resolution, looking in greater detail at differences in behavior associated with different sources of expertise.

Our experiments treated subjects with three different kinds of expertise. Table A1 in the online appendix shows that, relative to the BASELINE treatment, all three of these expertise treatments led subjects to give a bit less, on average, in the first five rounds, although based on Wilcoxon rank-sum tests, we cannot reject a null hypothesis of no differences from BASELINE in average contributions at standard levels of statistical significance. Note that we see the same lower average levels of giving in the first five rounds in the pure authority "to" treatment (p-value=0.06). Overall, however, when the experimenter was the expert offering a justification for giving 34 tokens, we observe a statistically insignificant but higher average level of giving (13.40) relative to baseline (12.99). Interestingly, this overall difference derives from the substantially slower decay in giving as the rounds progress in the experimenter expertise treatment. Compared both with the BASELINE treatment and the two other expertise treatments, we see relatively little drop-off in contributions in later rounds.

Table A1 also shows that this same qualitative pattern holds true when authority "in" is combined with authority "to." We observe a stronger effect of AUTH-BOTH treatments if the explanation is given by the experimenter (the PEN_EXP treatment of Table 1). An experimenter explanation along with a penalty increases mean contributions by 2.79 relative to the baseline (p-value = 0.12). When we consider the last 5 periods, this difference becomes 6.24 (p-value = 0.01). The other expertise treatments have generally positive but insignificant effects. Again, compared with the other expert advice treatments, in the PEN_EXP treatment we see relatively

little drop-off in contributions (see Table A1). In the PEN_EXP treatment, the average level of giving is 17.30 in the first 5 rounds and 15.01 in the last 5 rounds. While there are no significant differences in contributions between the PEN_EXP and other expert advice treatments (PEN_GRADEXP and PEN_FACEXP, pooled) when all periods are considered, contributions in the PEN_EXP treatment are significantly higher than those in the other expert treatments in the last 5 rounds (p-value = 0.07).

The regression results provided in Table A4 of the appendix tell a similar story, though with greater statistical precision. The overall effect of the PEN_EXP treatment is positive (4.03 more tokens) and statistically significant at the 5 percent level. In the last five periods, the difference between the BASELINE and the PEN_EXP treatment increases to 7.77 tokens and it is significant at the 1 percent level. Indeed we find that contributions in the PEN_EXP treatment are significantly higher than in all other treatments including PEN_GRADEXP and PEN_FACEXP (all p-values are less than 0.12 when we consider all periods and all p-values are less than 0.02 when we consider the last 5 periods). Meanwhile, there are no significant differences between the BASELINE and the other expertise treatments. Figure A1 of the online appendix shows the mean contribution per period for treatments BASELINE, PEN_EXP, PEN_GRADEXP and PEN_FACEXP. The figure also makes it clear that while in the earlier periods there are no differences between these treatments, in the later periods PEN_EXP generates higher contributions to the group project.

One possible explanation for why we observe steeper drop-off of contributions in the expert advice treatments is that, in these treatments, subjects may feel a sense of betrayal that the advice offered has failed to be helpful (or even relevant, given that most of their group members are not contributing large amounts). They react in an anti-social way more so than they do when the advice is offered in an impersonal way (by the experimenter, without attribution to particular "expert" individuals), because the impersonal process does not trigger the feelings of intentionality and reciprocity that the advice linked to real people does. People react differently to the same behavior of a person and an impersonal actor, as Blount (1995) suggested.

6. Controlling for the calculator usage

Recall that in our experiments subjects were able to use a calculator to enter their guesses about the combined contributions by the other members of their group and their own contribution

in order to calculate the returns from different combinations. For most of our sessions we captured the data on calculator usage.[25]

Data from the subjects' use of the calculator function provides further evidence that these treatments had an influence on respondents' decision-making process. Table A5 of the appendix shows that tendency to use the calculator varied somewhat by treatment. Subjects turned to the calculator most often (49% of subject-periods) in the AUTH-TO treatment and least (40% of subject-periods) in the NO SUGGESTION treatment. In a linear probability model of calculator usage (see Table A6 of the appendix), the AUTH-TO treatment is significantly associated with a higher probability of calculator usage compared to the BASELINE, which is consistent with the relatively low contributions we observe in this treatment. We do not observe significant differences in the other treatments. In addition, we observe that less calculator usage as the experiment progresses, that less risk-averse individuals use the calculator function less, and that individuals who are classified as fair have a higher probability of using the calculator.

Next we construct an indicator for whether the subject used the calculator before entering a contribution level. When we include an additional dummy variable for calculator usage in our regression analysis of contributions, calculator usage affects contributions negatively at the 10 percent significance level (Table 5). It also affects negatively the probability of contributing exactly 34 at the 1 percent significance level. In addition, controlling for the calculator usage, the impact of AUTH-BOTH is now bigger and the results are more significant.

7. Discussion

To interpret our experiments, it is useful to adapt the framework of Benabou and Tirole (2011) and represent respondent choices with the utility function

$$U(g|G_{-},a) = v(g|G_{-},a) + \omega(a)g + \mu E(\omega|g,a)$$

where g is an individual's contribution to the public good, G_{-} is the sum of everyone else's contributions to the public good, and a is the form of authority imposed on the environment. In our context, the form of authority includes suggested contributions, expert advice, and penalties. Preferences may vary across respondents, but we will suppress any individual subscripts for simplicity.

[25] We failed to capture the data for the first 7 sessions of our experiment.

In this interpretation, subjects have three broad motives for giving. First, they have the standard, strictly pecuniary reasons to give, captured by $v(g|G_{-}, a)$. Authority could, in principle, influence these motives by providing information about the technology for public good provision or by imposing penalties as a function of giving. Second, subjects may be motivated to give by the "warm glow" it produces. The term $\omega(a)$ captures that glow, and allows its relative importance to depend on authority. If warm glow derives in part from a sense of "doing good," authority could affect glow by influencing subjects' views about the social value of giving. Finally, subjects may be motivated to give by a concern for how they are perceived, either by themselves or by others. In particular, the term μ captures the reputational, or image-based, benefits of giving. It is the weight that subjects give to beliefs about (expectations of) their intrinsic, warm-glow, motives for giving, $\omega(a)$. Those beliefs naturally depend on how much the subject gives to the public good, g. Following Benabou and Tirole (2011), authority may also influence those beliefs or image-based benefits by changing either the strictly pecuniary or the warm glow benefits of giving. By changing these direct incentives to give, authority affects beliefs about a respondent's motives that are rationally drawn from his or her behavior.

All of our experiments were designed so that, if only the pecuniary motive $v(\cdot)$ were operative, then no subject would ever give anything to the public good. This leads us to focus on how authority may influence the warm glow and perception (image) motivations for giving.

The substantial, positive response to a simple request to give (BASELINE) is consistent with both warm glow and image-based motivations for giving. Respondents may see the experimenter as an authority requesting a contribution that will benefit others. They, therefore, give at a higher rate both for the gratification of giving to a social good and to be perceived (by the authority) as more giving.

Subsequent treatments indicate, however, that sincere warm glow motives are modest and that image concerns are more important. We see this in the fact that providing an explicit social justification for the request does not increase contributions. Buttressing the explanation with testimony from apparent experts other than the experimenter herself – economics graduate students and faculty – also fails to increase giving. In this way, it seems less important to respondents that they are actually doing something socially beneficial. Instead, they appear to value being perceived, either by the authority or by themselves, as doing something selfless.

This view of the importance of image concerns is underscored by the evidence that when authority takes the form of penalties this fails, by itself, to induce more pro-social contributions, and indeed may decrease contributions slightly. Prior work has shown that the introduction of explicit pecuniary incentives can "crowd out" intrinsic motivation (e.g., Gneezy and Rustichini, 2000; Fehr and Gächter, 2010). The Benabou and Tirole (2011) framework adapted above offers a reason why. The benefits to image from apparently selfless giving are diminished when extrinsic incentives make it more difficult to draw inference about tastes for warm glow. You may be giving in part because you are selfless and in part to avoid a fine.

The results on the interaction effects of authority "in" and authority "to" tell us still more about how authority influences image-based motivations for giving. We find that the combination of expertise and penalties increases contributions. Explaining that widespread contributions to the public project can improve everyone's outcome, and re-enforcing that message with a penalty for less-than-socially optimal contributions, is successful in raising contributions, and results in an average level of contribution that exceeds the average in all the other treatments we administered. To our knowledge, this is the first laboratory experiment demonstration of this interactive effect.[26]

The reinforcing effect of authority "in" and authority 'to" indicates that certain forms of authority influence image-based concerns for giving more strongly when those forms are viewed as legitimate.[27] Subjects for whom penalties may otherwise reduce contributions, whether because of motivation crowding or a "hidden cost of control,"[28] are less likely to react this way when there is a good reason for the penalty (it supports a socially efficient level of the public project). Alternatively, the imposition of penalties may be interpreted as lending support for the offered explanation for contributing to the public project: "we think the advice is good enough that we are willing to penalize those that don't follow it." In either case, we see evidence that obedience to legitimate authority is image-enhancing in ways that dominate the motivational crowding that might emerge from authority's influence on strictly pecuniary motives for giving.

[26] Other studies find intrinsic motivations and extrinsic incentives can be complementary. See Bowles and Polania-Reyes (2012) for a review of those studies. We are not aware, however, of studies showing the complementarity effects of these two forms of authority.

[27] A referee has insightfully noted that there is a misalignment between the party that has the authority "in" (graduate students or faculty) and the party that has the authority "to" (the experimenter, presumably distinct from the graduate students and faculty), making it harder for the one to legitimize the other.

[28] See Falk and Kosfeld (2006).

Overall, this finding is consistent with the view promoted by Tyler (2006) that people are more likely to obey rules if those rules seem fair and legitimate.

Our finding that different forms of authority have interacting effects calls for further study. For example, real world choice environments vary substantially in their complexity and familiarity to market participants. When the setting is complex or unfamiliar, authority "in" would likely have greater importance and this seems likely to influence the effects of authority "to." Another open question is the importance of the authority's physical or psychological proximity. The subjects and the experimenter in our study are physically and, perhaps, psychologically close. Our experiments thus leave it unclear whether the subtle effects of authority on image-based motivations for giving would obtain when authorities are remote. Would a remote and anonymous tax authority or human resources administrator have the same influence on taxpayers or retirement savers? If subjects are concerned largely with their own self-image, they might respond similarly to such distant authorities. If, instead, the experimenter is the principal audience, a remote authority may have much less influence. Future experiments can shed light on these issues and, eventually, guide field and observational study as well.

8. Related Literatures

As a laboratory study of voluntary contributions to a public good, this paper relates to a large experimental literature concerned with this canonical economic problem.[29] By asking that subjects give a particular contribution, and sometimes penalizing their failure to comply with this request, the paper also relates to a substantial literature on tax compliance games.[30]

As noted above, there is a substantial psychology literature on authority and obedience.[31] However, both the economics of voluntary contributions to public goods and the experimental

[29] Ledyard (1995) reviews the early literature, and Andreoni et al. (2008) summarizes some of the more recent studies.

[30] Torgler (2002) and Alm and Jacobson (2007) review much of this literature. Our penalty treatments are especially close to the public goods experiments on "mild laws." Tyran and Feld (2006), Galbiati and Vertova (2008), and Kube and Traxler (2011) all study the effects of random auditing with non-deterrent sanctions. In these experiments, like ours, the expected penalties are so small that they should have no deterrent effect on rational players. These papers also find effects of such penalties.

[31] Cialdini and Goldstein (2004) provide an excellent review. There is some, mostly qualitative, evidence in this literature to indicate that authority "in" and authority "to" may have complementary effects on behavior.

literature on tax compliance largely ignore the role of authority.[32] An exception is Cadsby et al. (2006), who study the consequences of an explicit demand of compliance in a tax evasion experiment.[33] In particular, Cadsby et al. (2006) investigate the effects of describing compliance with a tax scheme as a requirement, not merely as the way to avoid a probabilistic penalty. Our baseline results are similar to theirs, in that we find important effects of a *request* to give at a certain level.[34] Our work is distinct, however, in that we build on this finding in an attempt to understand better why the simple request to comply is effective by exploring the role of, and interaction among, providing an explanation (sometimes provided by an expert) and a penalty for contributions less than the suggested amount. In particular, our efforts to distinguish the role of authority "in" and authority "to," and to understand their complementarities, are, to our knowledge, the first of their kind.[35]

As we study the role of authority "in," our paper also relates to an experimental literature concerned with the effects of advice on equilibrium play. Schotter and Sopher (2003, 2007) study intergenerational games in which advice can be passed from outgoing to incoming players via free-form messages. Importantly, the only source of authority for these outgoing players is their brief previous experience. These experiments suggest that even nonexpert advice can have a significant impact on decisions. Chaudhuri et al. (2006) show that advice in the form of common knowledge (i.e., publicly announced to all members of the group) is most successful at increasing contributions, which they argue is because it facilitates successful socially efficient high levels of contributions. To our knowledge, the role of outside expert advice, and its quality, has not yet been studied in public-good games.

As we study the interactions between simple requests and requests backed by material penalties, our paper relates to a literature on the crowd-out of intrinsic motivation by extrinsic

[32] Fehr et al. (2013), in an experimental economics study of authority in a different context, demonstrate a willingness to pay for, in our terms, authority "to." Subjects reveal a demand for control, per se, above and beyond any material benefits that authority provides.

[33] In a collusion experiment, Sonntag and Zizzo (2013) study the role of authority. As we do, Sonntag and Zizzo find a positive effect of asking to cooperate, but they do not find an effect of giving an efficiency explanation (in an environment where there is no punishment).

[34] We did not study the potentially interesting distinctions between requests and demands for compliance.

[35] These aspects of our study also distinguish it from others that find important effects of requests or recommendations. Chaudhuri and Paichayontvijit (2010), for example, study the effects of the experimenter's recommendation, or offer of additional incentive, to coordinate at the socially optimal level in a minimum effort game; Andreoni and Rao (2011) show that the communication of simple requests can dramatically influence altruistic behavior in lab experiments. Pointing in the opposite direction, Dale and Morgan (2010) show that suggesting contribution levels depresses average giving in the lab.

incentives. Frey (1997), for example, differentiates between *intrinsic* motivation under which taxpayers comply with tax liabilities because of "civic virtue" and *extrinsic* motivation in which they pay because of threat of punishment, and suggests that increasing extrinsic motivation may "crowd out" intrinsic motivation by making people feel that they pay taxes because they have to, rather than because they want to. Gneezy and Rustichini (2000) argue that this explains why parent tardiness *increased* after an Israeli day care center instituted monetary fines for late pick-up of children. Similarly, Scholz and Lubell (2001) find that the level of cooperation in certain settings declines significantly when penalties are introduced; and Falk and Kosfeld (2006) find "hidden costs of control" – implementing a minimum performance requirement causes most agents to reduce their overall performance in response. Our paper contributes to this literature by identifying, to our knowledge for the first time, complementarities between intrinsic and extrinsic motivations.

While not in the context of authority, we note that Dal Bó and Dal Bó (2012) study the effect of a moral message on cooperation in a linear two-person public-good game. As in our experiments, they find that moral suasion is ineffective unless it is accompanied by punishment. In their study, punishment does not come from the authority but from peers (other subjects in the experiment). Moreover, in our nonlinear public-goods environment the socially efficient action is not obvious to the subjects; we manipulate the subjects' information without making any moral statements. In addition, in our setting the punishment is directed from the authority, and without changing the best-responses of the individuals.

9. Conclusion

Governments can provide value by mobilizing resources to provide public goods, and can use their coercive power to enforce tax remittances from citizens who would prefer to be free riders. Governments often also have an information advantage regarding which activities would benefit citizens. They have the authority "to" enforce the law, including their tax law, and authority "in" the provision of public goods.

History shows clearly, however, that not all governments act in the interest of all citizens, and therefore many citizens are suspicious of the information that government provides to justify its actions and are resentful of the powers it uses to enforce tax obligations. Pleas to comply for the social good often go unheeded, and heavy-handed enforcement can backfire.

The results of the experiments described in this paper suggest that authority "to" and authority "in" interact in ways not heretofore understood. While the interaction effects we observe may be limited to a particular range of parameters, where neither form of authority is overwhelmingly strong, the results of our experiments indicate that coercion without explanation does not increase voluntary contributions, nor does explanation without coercion. Together, they induce more contributions than any other combination of policies we evaluated. In our interpretation, this reveals that how people react to authority depends on whether this authority is perceived to be legitimate.

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TABLES

Table 2a: Differences in Mean Contributions, By Experimental Treatment

	BASELINE	AUTH-IN	AUTH-TO	AUTH-BOTH
NO SUGGESTION	4.39*** (1.02)	4.24*** (1.01)	3.83*** (0.72)	6.12*** (0.97)
BASELINE		-0.15 (1.15)	-0.56 (0.91)	1.74 (1.12)
AUTH-IN			-0.41 (0.90)	1.88 (1.11)
AUTH-TO				2.30** (0.85)

Each entry shows the difference between the mean contribution of the column treatment and the mean contribution of the row treatment. The standard error of the difference is in parentheses. The mean contribution in the NO SUGGESTION treatment is 8.61.
*Significant at 10%, **significant at 5%, ***significant at 1% (Two-tailed t-tests.)

Table 2b: Differences in Mean Contributions in Last Five Periods, By Experimental Treatment

	BASELINE	AUTH-IN	AUTH-TO	AUTH-BOTH
NO SUGGESTION	3.11** (1.41)	3.52** (1.37)	3.42*** (1.12)	6.61*** (1.43)
BASELINE		0.41 (1.45)	0.32 (1.21)	3.50** (1.51)
AUTH-IN			-0.09 (1.18)	3.09** (1.47)
AUTH-TO				3.19** (1.27)

Each entry shows the difference between the mean contribution in the last five periods of the column treatment and the mean contribution in the last five periods of the row treatment. The standard error of the difference is in parentheses. The mean contribution in last five rounds of the NO SUGGESTION treatment is 5.66.
*Significant at 10%, **significant at 5%, ***significant at 1% (Two-tailed t-tests.)

Table 3: Fraction of all Contributions at Focal Amounts, By Experimental Treatment

Treatment	Exactly 0	Between 1 and 34	Exactly 34	More Than 34
NO SUGGESTION	0.405 (0.010)	0.543 (0.010)	0.006 (0.002)	0.044 (0.004)
BASELINE	0.316 (0.009)	0.489 (0.010)	0.146 (0.007)	0.049 (0.004)
AUTH-IN	0.325 (0.009)	0.485 (0.010)	0.168 (0.007)	0.022 (0.003)
AUTH-TO	0.379 (0.009)	0.441 (0.010)	0.132 (0.007)	0.048 (0.004)
AUTH-BOTH	0.344 (0.009)	0.407 (0.009)	0.224 (0.008)	0.025 (0.003)
Each entry shows the fraction of all contributions in each experimental treatment that fell into the bin identified by the column heading. Standard errors are in parentheses.				

Table 4: OLS Estimates of Experimental Treatment Effects on Individual Contribution Levels and on Individual Probabilities of Contributing Exactly 34 Tokens

DEP. VARIABLE=	Contribution			Contributed 34		
	All 20 periods	First 5 periods	Last 5 periods	All 20 periods	First 5 periods	Last 5 periods
	1	2	3	4	5	6
nosuggestion	-4.30***	-5.83***	-2.77*	-0.139***	-0.228***	-0.087***
	(1.09)	(1.13)	(1.40)	(0.020)	(0.026)	(0.022)
authin	-0.06	-1.07	0.65	0.023	0.050	0.008
	(1.08)	(0.88)	(1.29)	(0.024)	(0.033)	(0.027)
authto	-0.34	-1.62	0.84	-0.006	-0.036	0.015
	(0.96)	(1.09)	(1.24)	(0.027)	(0.034)	(0.030)
authboth	1.86	-0.59	3.78**	0.080**	0.036	0.101***
	(1.29)	(1.07)	(1.68)	(0.031)	(0.037)	(0.037)
period	-0.50***	-1.62***	-0.55***	-0.006***	-0.032***	-0.003
	(0.04)	(0.16)	(0.11)	(0.001)	(0.006)	(0.003)
gamble	-0.57*	-0.67*	-0.73*	-0.010	-0.012	-0.012
	(0.34)	(0.35)	(0.41)	(0.008)	(0.008)	(0.009)
fair	5.56***	6.39***	5.11***	0.063***	0.086***	0.051***
	(0.71)	(0.81)	(0.75)	(0.018)	(0.021)	(0.017)
efficiency	9.70***	8.54***	11.29***	0.137***	0.136***	0.142***
	(1.46)	(1.38)	(1.75)	(0.038)	(0.036)	(0.040)
age	-0.1	-0.07	-0.09	0.000	0.000	0.000
	(0.08)	(0.13)	(0.08)	(0.002)	(0.002)	(0.002)
female	1.24*	2.71***	1.11	-0.018	0.012	-0.031
	(0.72)	(0.89)	(0.85)	(0.015)	(0.019)	(0.020)
econ	0.73	-0.26	0.03	-0.004	-0.017	-0.020
	(1.04)	(1.22)	(1.08)	(0.020)	(0.025)	(0.020)
taxmorale	0.27	0.27	0.11	0.005	0.009	0.008
	(0.23)	(0.26)	(0.30)	(0.005)	(0.005)	(0.005)
trustinpublicofficials	0.38*	0.49*	0.29	0.003	0.008	0.002
	(0.22)	(0.29)	(0.26)	(0.006)	(0.007)	(0.007)
Constant	14.85***	18.30***	17.07***	0.163***	0.219***	0.098
	(2.86)	(3.62)	(4.11)	(0.058)	(0.069)	(0.081)
Observations	13,360	3,340	3,340	13,360	3,340	3,340
R-squared	0.12	0.10	0.11	0.071	0.095	0.065

In columns 1-3, the dependent variable is the subject's level of giving in a period. In column 1, all rounds are included. Columns 2 and 3 restrict attention to giving in the first 5 and in last 5 rounds, respectively. In Columns 4-6 the dependent variable is an indicator variable equal to 1 if the subject gave exactly 34 tokens in a round, and otherwise equal to 0. In column 4, all rounds are included. Columns 5 and 6 restrict attention to behavior in the first 5 and the last 5 rounds, respectively. Standard errors robust to arbitrary within-session correlation of the error terms are in parentheses. (Two-tailed results reported.) *Significant at 10%, **significant at 5%, ***significant at 1%

Table 5: Accounting for Calculator Use: OLS Estimates of Experimental Treatment Effects on Individual Contribution Levels and on Individual Probabilities of Contributing Exactly 34 Tokens

DEP. VARIABLE=	Contribution			Contributed 34		
	All 20 periods	First 5 periods	Last 5 periods	All 20 periods	First 5 periods	Last 5 periods
	1	2	3	4	5	6
nosuggestion	-4.65***	-6.72***	-2.71*	-0.140***	-0.247***	-0.080***
	(1.13)	(1.28)	(1.53)	(0.023)	(0.032)	(0.023)
authin	0.79	-1.04	1.93	0.031	0.033	0.037
	(1.10)	(0.99)	(1.26)	(0.027)	(0.038)	(0.025)
authto	-0.28	-2.07*	1.14	-0.018	-0.059*	0.007
	(1.04)	(1.16)	(1.34)	(0.024)	(0.031)	(0.026)
authboth	2.52*	-0.39	5.33***	0.095***	0.032	0.136***
	(1.39)	(1.17)	(1.69)	(0.029)	(0.039)	(0.034)
period	-0.52***	-1.74***	-0.48***	-0.008***	-0.039***	-0.004
	(0.05)	(0.20)	(0.13)	(0.001)	(0.007)	(0.003)
gamble	-0.68**	-0.80**	-0.72*	-0.008	-0.015*	-0.008
	(0.30)	(0.37)	(0.40)	(0.007)	(0.008)	(0.010)
fair	6.02***	7.37***	5.20***	0.081***	0.103***	0.062***
	(0.79)	(0.85)	(0.85)	(0.019)	(0.022)	(0.019)
efficiency	10.77***	9.06***	12.75***	0.159***	0.154***	0.157***
	(1.55)	(1.50)	(1.80)	(0.043)	(0.039)	(0.043)
age	-0.03	0.01	-0.01	0.002	0.002	0.002
	(0.09)	(0.15)	(0.07)	(0.002)	(0.002)	(0.002)
female	1.51**	2.72***	1.49*	-0.011	0.013	-0.022
	(0.73)	(0.94)	(0.80)	(0.013)	(0.017)	(0.017)
econ	0.16	-1.13	-0.21	-0.004	-0.022	-0.018
	(1.17)	(1.35)	(1.18)	(0.022)	(0.028)	(0.020)
taxmorale	0.37	0.20	0.38	0.007	0.007	0.012**
	(0.25)	(0.29)	(0.31)	(0.005)	(0.006)	(0.006)
trustinpublicofficials	0.38	0.39	0.34	-0.000	0.005	0.002
	(0.22)	(0.31)	(0.26)	(0.006)	(0.008)	(0.007)
calculatorused	-1.12*	-2.82**	-1.12	-0.062***	-0.111***	-0.049***
	(0.61)	(1.12)	(0.72)	(0.014)	(0.025)	(0.015)
Constant	13.34***	20.21***	11.76***	0.159**	0.331***	0.036
	(2.92)	(4.09)	(4.13)	(0.061)	(0.088)	(0.085)
Observations	11,120	2,780	2,780	11,120	2,780	2,780
R-squared	0.13	0.12	0.14	0.089	0.114	0.087

In columns 1-3, the dependent variable is the subject's level of giving in a period. In column 1, all periods are included. Columns 2 and 3 restrict attention to giving in the first 5 and in last 5 periods, respectively. In Columns 4-6 the dependent variable is an indicator variable equal to 1 if the subject gave exactly 34 tokens in a period, and otherwise equal to 0. In column 4, all periods are included in the sample. Columns 5 and 6 restrict attention to behavior in the first 5 and the last 5 periods, respectively. *Different from Table 4, these estimates include among the regressors a variable indicating whether the subject used the payoff calculator before making a contribution.* Standard errors robust to arbitrary within-session correlation of the error terms are in parentheses. (Two-tailed results reported.) *Significant at 10%, **significant at 5%, ***significant at 1%

FIGURES

Figure 1: A Screenshot From the Experiment

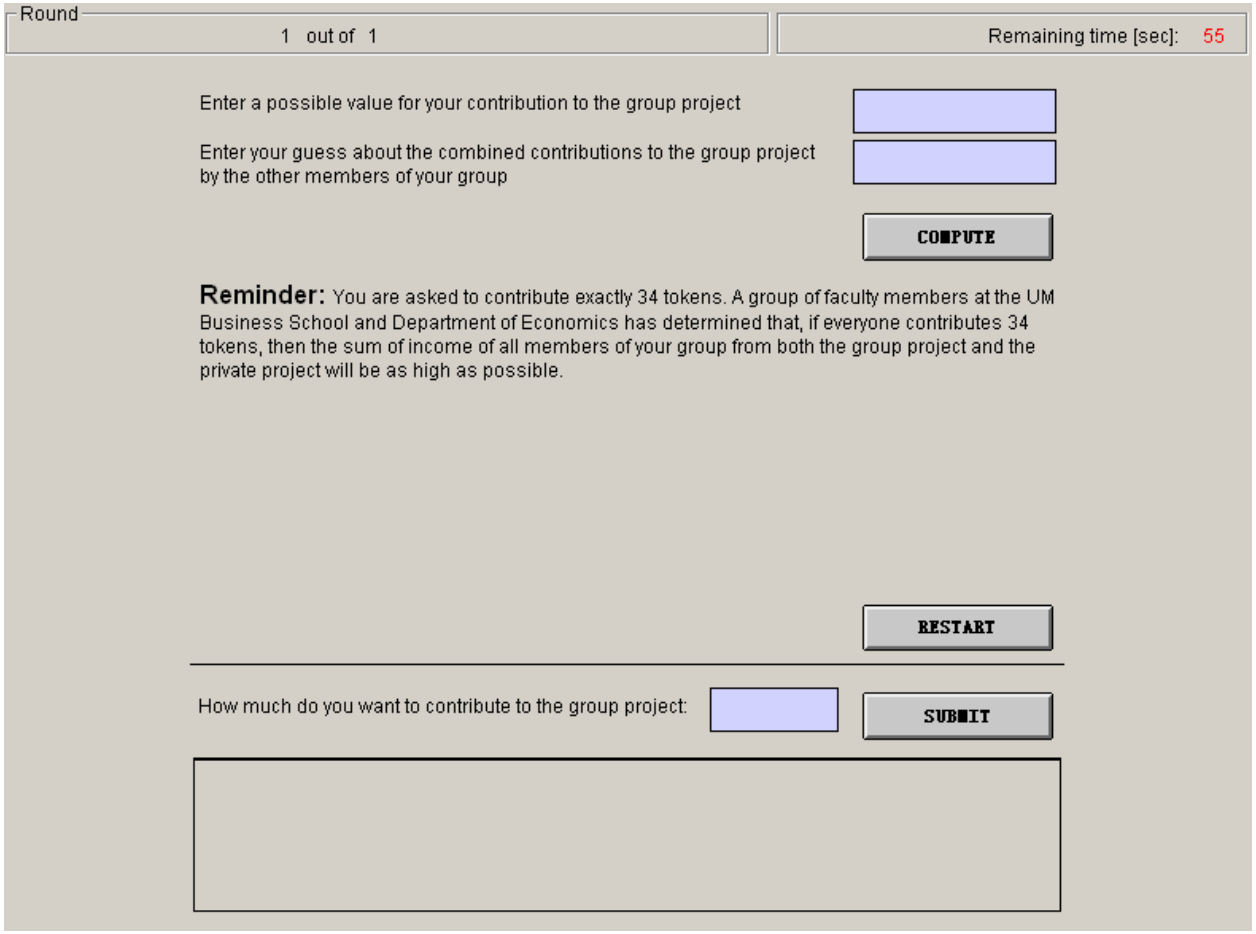

Figure 2: Mean Per Period Contribution to the Public Good, By Experimental Treatment