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 coercive 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. Coercion without expert explanation does not increase voluntary contributions, nor does explanation without coercion, 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 raises 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 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; plus 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 recent wave of interest by 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, we suspect 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. We note that 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 other 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 described above 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 tradeoff 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 taxpayer contributions depend on the existence, and source of, expert advice?
2. Do taxpayer contributions depend on the threat of penalty for insufficient contribution?
3. What is the interaction between these two sources of authority on behavior?

In addressing these questions, we also examine whether there is a differential impact of advice offered implicitly by the experimenter and advice offered explicitly by an outside expert. 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) states, "it is unavoidable that the experimenter is in a position of authority relative to subjects," having both legitimacy and expertise. [5] Zizzo 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 and 4, we present and analyze the results, first in tabular form and then with regression analysis. In section 5 we step back and discuss the results from a broad perspective, and in section 6 relate them to the existing literature. Section 7 concludes.

2. Research Design

2.1. Experimental procedures

The experiments were conducted at the Institute for Social Research at the University of Michigan from June 2010 through October 2010.[6] Most of our subjects were undergraduates at

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

[6] All sessions were run by the same person, an assistant of the researchers.

the University of Michigan. The average age of subjects was 21 years, with 51% being female; 9% 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 9 treatments and 27 sessions. Each session had 12-16 subjects. In total, we had 412 subjects. Each subject participated in only one treatment. There were three parts to each treatment. Part 1 consisted of 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. At the beginning of each period of Part 1, subjects were endowed with an income of 100 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 were 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 central public-goods experiment was in Part 1. We adopted a stranger design: in each of the twenty periods of Part 1, subjects were randomly re-matched into groups of four. No one ever learned the identity of the other members of his or her group.

At the beginning of each period, subjects were asked to contribute to a group project thee return from which depended on the total "contributions" of the group members. Each subject earned the same amount from the group project:

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

where $T = \sum_{i=1}^{4} t_i$, and t_i denotes the contribution of subject *i* to the group project, and where $i = \{1,2,3,4\}$. 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 always less than 8, so the Nash equilibrium is always to contribute nothing.

At the end of each period subjects were provided with an "income screen" that reminded them of their contribution to the group project. They also learned the combined contributions of the other members of their group in that period, their income from the group project and their income from the private project. In addition, if penalties were possible in that treatment, they also learned whether they paid a fine, and what their total income in that period was.

One novelty of our design is that the production function for the group project was not explicitly provided 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 will contribute.[7] The calculator added these two values to get the total group contribution and calculate the subject's income from the group project. Subjects were able to use the calculator as many times as they wanted to 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 matters for subjects' contributions.

2.2.1. The baseline control treatment

In the baseline control treatment, we asked subjects to decide how much to contribute to the group project, making it a voluntary (non-linear) public good game.

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

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

The Nash equilibrium has each subject 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

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[7] The calculator screen also contained a reminder of the treatment details.

[8] Even though we do not expect that subjects try out all the possibilities, it is straightforward for them to see that, given the contributions of the others in their group, contributing more to the group project always decreases one's own payoff.

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.

2.2.2. Treatments examining the effect of authority "in"

We designed all of the treatments such that the Nash equilibrium and the socially optimal level of contributions do not change.

In the second treatment (denoted NOEXP), the experimenter asked subjects to contribute the socially optimal amount, but did not provide any explanation. 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)."

In the third treatment (EXP), the experimenter asked subjects to contribute the socially optimal level, and *explained* that this is the efficient outcome. The previous sentence 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 next set of treatments we investigated the effect of providing different forms of expert advice, so as to provide evidence about the effect of the authority "in" on contributions. For the fourth treatment (GRAD_EXP), we explained that a group of experts, graduate students in economics, had verified that equal contributions of 34 tokens would in fact maximize the sum of all members' payoffs.[9] The precise language was:

"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). A group of graduate students at the UM Department of Economics has determined that, **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. You have been provided with an information sheet that lists the names of these graduate students and a summary of their achievements."

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[9] This was not a deceit. In fact four University of Michigan economics graduate students made this calculation.

The fifth treatment (FAC_EXP) was exactly the same as the previous treatment, except that the expert panel consisted of four economics faculty members instead of graduate students.[10] The precise language was:

"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). A group of faculty members at the UM Department of Economics has determined that, **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. You have been provided with an information sheet that lists the names of these faculty members and a summary of their achievements."

2.2.3. Treatments examining the effect of authority "to"

The final set of four 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 second through fifth treatments.) Each subject was told that in each period, with probability 0.25, the computer will perform an "audit" on the subject. If an audit takes place, a penalty will be assessed if the subject contributed less than 34 tokens. The penalty will equal twice the amount that the subject's contribution falls 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 risk-neutral subject is given by

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

for $t_i \leq 34$. Because the expected punishment is small, the Nash equilibrium does not change; it remains a zero contribution.[11] 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.

[10] Again, a group of four University of Michigan economics professors really did make this calculation.
[11] A relatively low punishment is chosen both to parallel actual tax evasion penalty regimes and to maintain the same level of predicted contributions throughout the treatments.

A summary of the experimental design is provided in Table 1. Figure 1 shows a typical screenshot from the experiment.

Table 1.

Treatment	Number of sessions	Number of subjects	(Audit probability; penalty rate)	Level of expertise
BASELINE	3	44	-	No suggested level
NOEXP	3	48	-	Suggestion w/o explanation
EXP	3	40	-	Suggestion with explanation
GRADEXP	3	44	-	Suggestion with grad
FACEXP	3	48	-	Suggestion with faculty
PEN_NOEXP	3	44	(0.25; 2)	Suggestion w/o explanation
PEN_EXP	3	48	(0.25; 2)	Suggestion with explanation
PEN_GRADEXP	3	48	(0.25; 2)	Suggestion with grad
PEN_FACEXP	3	48	(0.25; 2)	Suggestion with faculty

2.2.4. Attitudes and demographic information

Parts 2 and 3 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 (it paid 600 tokens with probability 0.5 and nothing otherwise).[12]

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 tradeoff between one's own payoff and the sum of payoffs to oneself and another subject:[13]

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.

We classify the subjects who picked Option 1 as the "fair," Option 2 as the "efficiency maximizer," and Option 3 as the "selfish/rational."

In Part 3 we asked the subjects their gender, age, undergraduate major, as well as two other questions which measure tax morale and trust for 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

Table 2 shows the mean contribution levels (and standard deviations[14]) for each of the nine treatments, averaged first over all periods and then separately for the first five and last five periods.

3.1. The effect of suggestion

In all of the treatments, the median contribution is significantly greater than the Nash equilibrium of zero (p-values < 0.06).[15] This is true even in the BASELINE treatment, when no

[12] 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.
[13] The computer randomly chooses one of the participants with equal odds, and implements her decision.
[14] Standard deviations are calculated using all observations (not averages over sessions) to show the variations across individuals and periods. Later, when we do statistical testing, we average contributions over sessions to create independent observations.

suggested contribution is mentioned.[16] However, by far the lowest mean contribution occurs when the subjects are given no suggestion about how much to contribute, 10.10 tokens contributed compared to a minimum of 12.15 in the other eight treatments. Comparing the baseline treatment with NOEXP, we see that the contributions increase by approximately 35%. This is illustrated in Figure 2. Except for FACEXP, contributions are significantly higher than in the BASELINE treatment; *p*-values are less than 0.06 for all comparisons, except for FACEXP, when it equals 0.26.

The data in Table 3 indicate that the suggestion is, in fact, focal for some subjects. When contributing exactly 34 tokens is not mentioned (BASELINE), essentially no one contributes exactly that amount. Over all the other treatments, however, when a 34-token contribution is mentioned, approximately 19% of period contributions are exactly 34. Notably, suggesting a 34-token contribution also reduces the fraction of periods in which *more* than 34 tokens are contributed, compared to all treatments except the one where no explanation at all is offered (NOEXP).

Table 4 shows the fraction of individual contributions that equal zero. Note that the fraction of zero contributions is not tightly correlated with mean contributions. For example, the treatment with the highest mean contribution (PEN_EXP) also has one of the highest fractions of periods with zero contributions. This is consistent with the fact that this treatment has the largest variation of contributions.

3.2. The effect of providing a social-benefit explanation

We have learned that suggesting a contribution level increases contributions. What about providing an explanation for that suggestion, in particular an explanation about the potential benefits of widespread contributions to the public good? Here the results are striking. A social-benefit explanation increases the mean contribution significantly by 2.70 (*p*-value = 0.06) compared to the case where there is a suggestion but no explanation (treatment NOEXP), but only when it is offered in the context of a penalty regime (Table 2). See Figure 3 for a graphical demonstration. Contributions to the public good increase approximately 50% with an explanation

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[15] Unless otherwise mentioned, all tests are one-tailed Wilcoxon tests. Note that nonparametric analysis requires independent observations. Therefore, we average contributions over sessions to create independent observations per treatment.

[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.

and penalty compared to the BASELINE treatment. In contrast, a social-benefit explanation without a penalty is accompanied by a decrease in mean contributions of 0.43. The difference is largely due to the fact that, without a penalty, the explanation prompts a substantial increase in the fraction of zero contributions, from .28 to .37, especially in the last five periods, where the fraction of zeros goes from .35 to .50, the highest of any treatment. This is somewhat offset by an increase in the fraction of those contributing exactly 34, from .14 to .19. With the penalty, though, there is a slight decrease in the fraction of zero contributions, from .36 to .35, and a notable decrease in zero contributions in the last five periods, from .46 to .40. This is the first indication that, in these settings, authority "in" and authority "to" have important interactive effects.

Figure 3 reveals another interesting finding. In the PEN_EXP treatment, contributions do not decay over periods nearly as much as in the other treatments.[17] While in this and many other experiments on public goods it is common to observe a decline in contributions over periods, it is noteworthy that the interaction of authority in and authority to is strong enough to prevent contributions from substantially declining over periods.

3.3. The effect of authority "in": Outside expert corroboration

In our research design, the authority "in" aspect of government authority is captured by two versions of expertise about the social benefit of group-wide contributions to the public project. We envisioned these two versions, corroboration of the social benefit by graduate students and by faculty—as steps toward more expertise.

The results do not support the hypothesis that authority "in" or expertise increases contributions. Nor do they support that student subjects are more influenced to contribute by faculty, rather than graduate student, corroboration of the social benefit of widespread contribution. In the no-penalty regimes, comparing the results of EXP to the GRADEXP treatments shows that graduate student expertise increases mean contributions by just 0.02. Comparing EXP to FACEXP shows that faculty corroboration of the social benefit actually *decreases* contributions by 0.98. However, none of the pairwise comparisons between EXP, GRADEXP, and FACEXP is statistically different from zero (the *p*-values are all larger than

[17] This finding is supported by a regression analysis. When contributions are regressed on period, we find a p-value of 0.26 for this particular treatment.

0.26). These patterns may in part be explained by student subject suspicions about the expertise, or motivation, of faculty "experts." It also may be that the "unattributed" explanation is in fact attributed to the experimenter who, as already discussed, is a kind of authority figure herself. Finally, a suggestion in and of itself may convey expertise.[18]

In addition, in the penalty regimes neither kind of expert testimony increases contributions. Graduate student expertise decreases contributions by 1.71, and faculty expertise reduces contributions by 1.34. Similarly, none of the pairwise comparisons is statistically distinguishable from zero (the smallest p-value is 0.14).

3.4. The effect of authority "to": Penalties

Adding a probabilistic penalty for contributing less than the socially efficient amount increases the expected private return to contributing, although it does not budge Nash equilibrium behavior from a zero contribution. We can get a sense of the effect on contributions of a penalty by comparing the contribution pattern for each setting of explanation/expertise, that is comparing the results in NOEXP to PEN_NOEXP, EXP to PEN_EXP, GRADEXP to PEN_GRADEXP, and FACEXP to PEN_FACEXP.

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 decreases mean contributions by 0.48. However, the difference is not significantly distinguishable from zero (p-value = 0.41). In contrast, when the suggestion is accompanied by an explanation, adding a penalty (insignificantly) increases mean contributions, by 2.65, 0.92, and 2.19 for the three treatments with explanations (p-values > 0.14). An interesting finding is that, even though a penalty doesn't affect the contributions significantly when all periods are considered, adding a penalty increases contributions significantly between treatments EXP and PEN_EXP, and treatments GRADEXP and PEN_GRADEXP (p-values = 0.02) when attention is restricted to just the last 5 periods.

3.5. Varying Gradient

[18] It has been suggested to us that referring to economics faculty or graduate students might change the subjects' framing of the experiment—they now realize it is an "economics" experiment. This seems unlikely because the economics background of the experimenters was indicated in the consent form and in the background remarks communicated at the outset of the experiment.

As the second and third columns of Table 2 show, for nearly all treatments there is a significant drop-off in mean contributions between the first 5 periods and the last 5 periods (all p-values < 0.06, except for PEN_EXP, which is 0.26).[19] But the drop-off varies substantially across treatments, both in absolute and in percentage terms. Thus the treatments do not simply affect the initial contribution levels, with a uniform dilution over time, but affect both the initial levels and the gradients.

A suggested contribution level seems to be more effective at increasing early-period contributions; its drop-off is the lowest (absolutely and as a percentage of initial contributions) than any other treatment except for treatment PEN_EXP, which has by far the smallest drop-off. We can see this in the second and third columns of Table 4, which show that the fraction of the contributed amounts that are exactly zero increase between the first and last five periods for all of the treatments. There is a clear positive correlation across treatments between the decline in mean contributions and the increase in the fraction of zero contributions. Both are smallest for the PEN_EXP treatment.

In general, the biggest beginning-to-end drop-off in contributions comes in the expert advice treatments. One possible explanation is that, in these treatments, subjects 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.

4. *Regression Analysis*

With multivariate regression analysis we can explore more precisely the interaction effects of the treatment elements and also investigate the relationship to contribution decisions of subject attributes and attitudes. In all of the results, we report robust standard errors clustered at the session level.

[19] This decline in average giving is the subject of a large literature in its own right. See Fischbacher and Gachter (2010) for a recent contribution and review.

We begin in Table 5 with an OLS regression that includes all period observations. In this and subsequent tables the variable *suggestion* takes a value of 0 for no suggested level of tax, and 1 otherwise, so that *suggestion* equals 0 only for treatment BASELINE. The variable *explanation* takes the value 1 if the treatment is either EXP or PEN_EXP, so that it refers to an explanation without expert corroboration. The variables *grad expert* and *faculty expert* are indicator variables equal to one when there is graduate student corroboration and faculty corroboration, respectively. The variable *penalty* takes the value 0 when there is no fine and a value of 1 when there is a fine. The variable *period* takes values from 1 to 20.[20] 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 simply the age of the subject. 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 5, only *suggestion* significantly affects contributions, increasing contributions by 2.64. This is consistent with the tabulated results discussed earlier. Specification (2) shows that each of the three attitudinal variables *gamble*, *fair*, and *efficiency* is 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,

[20] As a robustness check, we also investigated a specification that allowed for a non-linear learning process, with no qualitative difference in the results reported here.

which documents that people with fairness or efficiency concerns would give up their own earnings in order to help others.[21]

Also, 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. Neither *taxmorale* nor *trustinpublicofficials* has a significant partial association with the level of contributions.[22] Of the three demographic variables, only *female* affects the magnitude of contributions in a statistically significant way, and does so positively. Two aspects of the main results change when the whole set of explanatory variables is included. First the *explanation* variable now attracts a significant positive coefficient. Second, the negative estimated coefficient on *gamble* just fails the test for statistical significance, presumably because there is correlation between it and the newly included explanatory variables. In all specifications, the estimated coefficients on the *period* variable suggest a substantial downward drift over time in the level of contributions.

The regression specifications shown in Table 6 add interaction terms of the penalty treatment with the three expertise treatments. They show that the combination of an explanation and a penalty increases contributions, but neither by itself. Notably, this interaction effect is least strong when the explanation is backed up by graduate student or faculty corroboration, but is quantitatively significant (a coefficient in excess of three), and is statistically different than zero whenever the non-treatment explanatory variables are included in the regression specification. Now we see that having a penalty increases contributions only in conjunction with an explanation, and especially so when there is an explanation without supporting expertise.[23]

Tables 7 and 8 repeat the specifications with interactions effects for contributions in the first five and last five periods, respectively. Comparing the results of these two sets of regressions reveals that the positive effect on contributions of making a suggestion fades away over time, being associated with about five more tokens in the early period but only about half of that by the end. In contrast, the impact of the combination of an explanation and a penalty is stronger near the end of the experiment's duration than at the beginning. In specification (3) of Table 8, explanation has a positive effect of 1.4, but combined with a penalty, the explanation is

[21] See, for example, Andreoni (2006), Camerer (2003), Charness and Rabin (2002), and, Fehr and Schmidt (2006).
[22] 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.
[23] We have also estimated a Tobit regression specification. The qualitative results do not change, with the exception that the variable *gamble* now significantly affects contributions.

associated with 6.6 more contributions. Of particular interest is the time pattern of the estimated effect of authority "in." In the last five periods, but not in the initial five periods, both the graduate-student and faculty corroboration reduce contributions, absent a penalty (the former being statistically different from zero). In the faculty case, the effect essentially goes to zero when combined with a penalty, but the graduate-student explanation with a penalty increases contributions at the end.

Table 9 pursues the extensive effect on contributions with the results of a linear probability model of contributing any positive amount. It confirms that female subjects are more likely to give a positive amount.[24] The penalty reduces the probability of contributing any positive amount.

5. Discussion

In these experiments, simply asking subjects to contribute to a public project increased their contributions by over a third, even though their private self-interest dictated that they contribute nothing. This is consistent with the view that is relatively easy for authorities to increase the frequency or level at which people deviate from their material self-interest. But why? A subject might be trying to please the experimenter, either because she is herself a figure of authority or because she has kindly offered compensation and otherwise been courteous, so that the pro-social behavior is a kind of reciprocity. Alternatively, subjects may respond to a suggested level of contribution because they believe it is good advice that, if followed, will improve their outcome. The experimenter "knows" something that the subject does not. Finally, a subject may contribute when there is a penalty for not doing so, either because of how this changes the material incentives or because it conjures up something about the social value of contributing; this is the authority "to" punish non-compliers. In addition, we might imagine that the suggestion of a particular contribution level facilitates, at least temporarily, coordination on a group equilibrium with higher contributions and better outcomes for all subjects.

Our experimental treatments do not provide much evidence that subjects increase their contributions in order to improve their outcomes. Merely providing a social-benefit explanation for a contribution does not increase contributions. Nor does buttressing the explanation with

[24] However, this does not mean that females give more conditional on giving anything. The average contribution of female subjects is 14.7, while the average contribution of male subjects is 11.8. However, when we only look at contributions that are positive, male subjects contribute 20.9 on average, while female subjects contribute 19.1.

testimony from apparent experts other than the experimenter herself—economics graduate students and faculty. Indeed, expertise on its own seems to crowd contributions out a bit, the more so the greater the apparent expertise.[25]

Nor do we find evidence that penalties themselves induce more pro-social contributions. Indeed, penalties for contributions below the socially efficient level generally seem to decrease contributions slightly.

What we do find is that the combination of authority "in" and authority "to" increases contributions. To our knowledge, this is the first laboratory experiment demonstration of this interactive effect. An explanation that widespread contributions to the public project can improve everyone's outcome, when coupled 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.

The reinforcing effect of authority "in" and authority 'to" may be interpreted as a legitimacy effect. Subjects for whom penalties may otherwise reduce contributions, whether because of motivational crowd out (i.e., moving from intrinsic to extrinsic motivation) or a "hidden cost of control,"[26] 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). The same results can also be interpreted as evidence of a different kind of legitimacy effect, in which the imposition of penalties provides 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." This works to counteract the otherwise negative consequences of offered expertise that we have found in this experiment. This finding is consistent with the view argued compellingly by Tyler (2006) that people are more likely to obey rules, including but not limited to penalties, if those rules seem fair (right) and legitimate.

It may also be that offering expert advice triggers a kind of motivational crowding out similar to what is triggered in other contexts by monetizing pro-social behavior. The argument goes as follows: offering a selfish reason for contributions ("if everyone does it, you'll be better

[25] As Christian Traxler noted to us, in the aftermath of the financial crisis and recession, in 2010 many people may not be inclined to think of economists as "experts" on much of anything. We note, though, that the issue in question in the experiment is far removed from these macroeconomic events.
[26] See Falk and Kosfeld (2006).

off") causes some people to switch their mental framing of the contribution decision from a pro-social to a selfish one.

We find that merely suggesting a level of contribution level, with no explanation or penalty, significantly increases contributions, suggesting that minimal intervention can affect voluntary socially efficient behavior. But, beyond that, when we consider providing outside expert justification for the requested level of contribution or assessing penalties for contributions below that level, neither on its own is successful, but only the combination of an expert explanation plus sanctioning works.

6. 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.[27] By requesting that subjects give a particular contribution, and sometimes penalizing their failure to comply with this request, the paper also relates to substantial literature on tax compliance games.[28]

As noted above, there is a substantial psychology literature on authority and obedience.[29] However, both the economics of voluntary contributions to public goods and the experimental literature on tax compliance largely ignore the role of authority. An exception is Cadsby et al. (2006), which studies the consequences of an explicit demand of compliance in a tax evasion experiment. Our baseline results are similar to theirs, as we also find important effects of a request to give at a certain level.[30] 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

[27] Ledyard (1995) reviews the early literature, and Andreoni et al. (2007) summarizes some of the more recent studies.

[28] 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." Feld and Tyran (2006), Galbiati and Vertova (2008), and Kube and Traxler (forthcoming) 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.

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

[30] In this way, the paper also relates to Andreoni and Rao (2011) who show that communication in lab experiments can dramatically influence altruistic behavior. Pointing in the opposite direction, Dale and Morgan (2010) show that suggesting contribution levels depresses average giving in the lab.

the role of authority “in” and authority “to,” and to understand their complementarities, are, to our knowledge, the first of their kind.

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 inter-generational 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 non-expert advice can have a significant impact on decisions. Chaudhuri, Graziano, and Maitra (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.

Finally, 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 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.

7. 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 often backfire.

The results of the experiments described in this paper suggest that authority “to” and authority “in” interact in ways not heretofore understood. Coercion without explanation does not increase voluntary contributions, nor does explanation without coercion. Together, they induce more contributions than any other combination of policies. 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 2. Mean contribution levels

Treatment	Mean Contribution	Mean Contribution First 5 periods	Mean Contribution Last 5 Periods
BASELINE	10.10 (14.24)	12.65 (14.82)	7.52 (12.92)
NOEXP	13.56 (15.15)	17.50 (18.05)	10.14 (12.92)
EXP	13.13 (15.40)	16.39 (15.40)	10.82 (16.25)
GRADEXP	13.15 (14.53)	17.90 (16.03)	8.10 (12.52)
FACEXP	12.15 (13.53)	17.23 (14.66)	8.62 (11.98)
PEN_NOEXP	13.08 (15.58)	16.80 (15.17)	9.85 (15.43)
PEN_EXP	15.78 (15.95)	17.30 (15.69)	15.01 (16.98)
PEN_GRADEXP	14.07 (15.34)	17.25 (16.24)	11.19 (14.13)
PEN_FACEXP	14.34 (14.22)	18.97 (15.50)	10.60 (12.75)
Standard deviations in parentheses			

Table 3. Proportion of contributions less than, equal to, and above 34

Treatment	Less Than 34	Exactly 34	More Than 34
BASELINE	0.94	0.00	0.06
NOEXP	0.80	0.14	0.06
EXP	0.79	0.19	0.02
GRADEXP	0.81	0.17	0.02
FACEXP	0.83	0.15	0.03
PEN_NOEXP	0.78	0.17	0.04
PEN_EXP	0.69	0.27	0.04
PEN_GRADEXP	0.76	0.22	0.02
PEN_FACEXP	0.80	0.19	0.01

Table 4. Proportions of observations with contribution exactly equal to 0

Treatment	**All Periods**	**First 5 periods**	**Last 5 periods**
BASELINE	0.34	0.30	0.42
NOEXP	0.28	0.22	0.35
EXP	0.37	0.26	0.50
GRADEXP	0.28	0.20	0.41
FACEXP	0.33	0.26	0.41
PEN_NOEXP	0.36	0.25	0.46
PEN_EXP	0.35	0.31	0.40
PEN_GRADEXP	0.36	0.25	0.44
PEN_FACEXP	0.32	0.23	0.43

Table 5. Baseline OLS regressions

DEP. VARIABLE= contribution/tax			
suggestion	2.640**	3.080*	3.099**
	(1.257)	(1.499)	(1.356)
explanation	1.159	1.542	2.067*
	(1.115)	(1.079)	(1.131)
grad expert	0.248	-0.0329	-0.0929
	(0.796)	(0.932)	(0.993)
faculty expert	-0.111	-0.649	-0.715
	(1.429)	(1.135)	(1.084)
penalty	1.313	1.422	1.163
	(0.873)	(0.862)	(0.869)
period	-0.453***	-0.453***	-0.453***
	(0.0449)	(0.0449)	(0.0449)
gamble		-0.853*	-0.654
		(0.425)	(0.443)
fair		4.996***	4.650***
		(0.990)	(0.947)
efficiency		8.878***	9.301***
		(1.859)	(1.833)
age			0.0119
			(0.144)
female			2.637**
			(0.975)
econ			1.431
			(1.471)
taxmorale			0.314
			(0.312)
trustinpublicofficials			0.479
			(0.341)
Constant	10.06***	9.213***	8.300
	(0.898)	(1.750)	(5.000)
Observations	8240	8240	8240
R-squared	0.008	0.057	0.098
*Significant at 10%, **significant at 5%, ***significant at 1% Robust standard errors are in parentheses. (Two tailed-results reported.)			

Table 6. Including interaction terms

DEP. VARIABLE= contribution/tax			
suggestion	3.496**	3.678**	3.966***
	(1.389)	(1.577)	(1.377)
explanation	-0.425	-0.185	0.0945
	(1.636)	(1.335)	(1.214)
grad expert	-0.404	-0.169	-0.715
	(1.239)	(1.226)	(1.385)
faculty expert	-1.405	-1.349	-1.761
	(2.098)	(1.558)	(1.338)
penalty	-0.478	0.160	-0.662
	(1.115)	(0.904)	(0.645)
explanation * penalty	3.125	3.326*	3.861**
	(1.986)	(1.908)	(1.863)
grad expert * penalty	1.400	0.370	1.338
	(1.361)	(1.730)	(1.781)
faculty expert * penalty	2.665	1.449	2.159
	(2.713)	(2.247)	(2.043)
period	-0.453***	-0.453***	-0.453***
	(0.0449)	(0.0449)	(0.0449)
gamble		-0.882**	-0.678
		(0.418)	(0.436)
fair		4.960***	4.598***
		(0.991)	(0.951)
efficiency		8.944***	9.324***
		(1.858)	(1.829)
age			0.0157
			(0.145)
female			2.699***
			(0.960)
econ			1.486
			(1.473)
taxmorale			0.319
			(0.311)
trustinpublicofficials			0.478
			(0.338)
Constant	10.06***	9.325***	3.503
	(0.898)	(1.734)	(4.730)
Observations	8,240	8,240	8,240
R-squared	0.010	0.058	0.069

*Significant at 10%, **significant at 5%, ***significant at 1%
Robust standard errors are in parentheses. (Two tailed-results reported.)

Table 7. OLS regressions for the first 5 periods

DEP. VARIABLE= contribution/tax			
suggestion	4.850**	5.225**	4.952**
	(2.076)	(2.016)	(1.973)
explanation	-1.119	-0.952	-0.462
	(1.769)	(1.280)	(1.157)
grad expert	0.400	0.355	0.00192
	(1.576)	(0.990)	(0.774)
faculty expert	-0.279	-0.429	-0.613
	(1.440)	(1.317)	(1.249)
penalty	-0.704	-0.191	-1.155
	(1.681)	(1.780)	(1.389)
explanation * penalty	1.615	1.881	2.567
	(2.197)	(2.022)	(1.783)
grad expert * penalty	0.0538	-0.827	0.204
	(2.049)	(2.005)	(1.579)
faculty expert * penalty	2.450	1.243	1.938
	(2.879)	(2.686)	(2.534)
period	-1.595***	-1.595***	-1.595***
	(0.176)	(0.176)	(0.176)
gamble		-1.165***	-0.894**
		(0.413)	(0.395)
fair		5.711***	5.011***
		(1.107)	(1.070)
efficiency		8.285***	8.694***
		(1.728)	(1.692)
age			0.170
			(0.195)
female			3.495***
			(1.025)
econ			-0.0382
			(1.868)
taxmorale			0.325
			(0.365)
trustinpublicofficials			0.577
			(0.406)
Constant	12.65***	12.54***	3.109
	(1.777)	(2.356)	(5.686)
Observations	2,060	2,060	2,060
R-squared	0.011	0.061	0.077

*Significant at 10%, **significant at 5%, ***significant at 1%
Robust standard errors are in parentheses. (Two tailed-results reported.)

Table 8. OLS regressions for the last 5 periods

DEP. VARIABLE= contribution/tax			
suggestion	2.615	2.671	2.621
	(1.893)	(2.190)	(1.854)
explanation	0.683	0.960	1.352**
	(1.013)	(0.820)	(0.641)
grad expert	-2.042*	-1.580*	-1.922**
	(1.150)	(0.913)	(0.909)
faculty expert	-1.517	-1.345	-1.594
	(2.557)	(1.912)	(1.681)
penalty	-0.292	0.482	-0.200
	(0.965)	(0.993)	(0.896)
explanation * penalty	4.485**	4.741**	5.239***
	(1.659)	(1.854)	(1.821)
grad expert * penalty	3.388**	2.104	2.895**
	(1.251)	(1.390)	(1.370)
faculty expert * penalty	2.275	0.920	1.394
	(3.308)	(2.896)	(2.718)
period	-0.664***	-0.664***	-0.664***
	(0.166)	(0.166)	(0.166)
gamble		-0.960*	-0.709
		(0.516)	(0.536)
fair		4.819***	4.470***
		(1.107)	(1.085)
efficiency		10.48***	11.03***
		(2.103)	(2.074)
age			-0.0377
			(0.147)
female			3.053**
			(1.229)
econ			0.494
			(1.308)
taxmorale			0.152
			(0.414)
trustinpublicofficials			0.314
			(0.386)
Constant	7.523***	6.956**	3.699
	(1.693)	(2.646)	(5.634)
Observations	2,060	2,060	2,060
R-squared	0.022	0.086	0.098
*Significant at 10%, **significant at 5%, ***significant at 1% Robust standard errors are in parentheses. (Two tailed-results reported.)			

Table 9. Linear probability model

DEP. VARIABLE= contributed			
suggestion	0.064	0.081	0.066
	(0.043)	(0.053)	(0.043)
explanation	-0.098	-0.098	-0.076
	(0.073)	(0.082)	(0.073)
grad expert	-0.004	-0.017	-0.029
	(0.055)	(0.033)	(0.017)
faculty expert	-0.049	-0.063	-0.073**
	(0.035)	(0.037)	(0.030)
penalty	-0.085***	-0.078**	-0.114***
	(0.026)	(0.033)	(0.018)
explanation * penalty	0.107	0.120	0.150
	(0.084)	(0.102)	(0.094)
grad expert * penalty	0.007	-0.012	0.027
	(0.072)	(0.069)	(0.052)
faculty expert * penalty	0.088	0.057	0.082
	(0.073)	(0.064)	(0.061)
period	-0.012***	-0.012***	-0.012***
	(0.001)	(0.001)	(0.001)
gamble		-0.051***	-0.036**
		(0.013)	(0.013)
fair		0.175***	0.143***
		(0.034)	(0.032)
efficiency		0.193***	0.222***
		(0.065)	(0.060)
age			0.002
			(0.004)
female			0.181***
			(0.028)
econ			-0.012
			(0.067)
taxmorale			0.012
			(0.011)
trustinpublicofficials			0.007
			(0.014)
Constant	0.782***	0.837***	0.584***
	(0.037)	(0.067)	(0.141)
Observations	8,240	8,240	8,240
R-squared	0.025	0.083	0.119
*Significant at 10%, **significant at 5%, ***significant at 1% Robust standard errors are in parentheses. (Two tailed-results reported.)			

FIGURES

Figure 1: A screenshot from the experiment

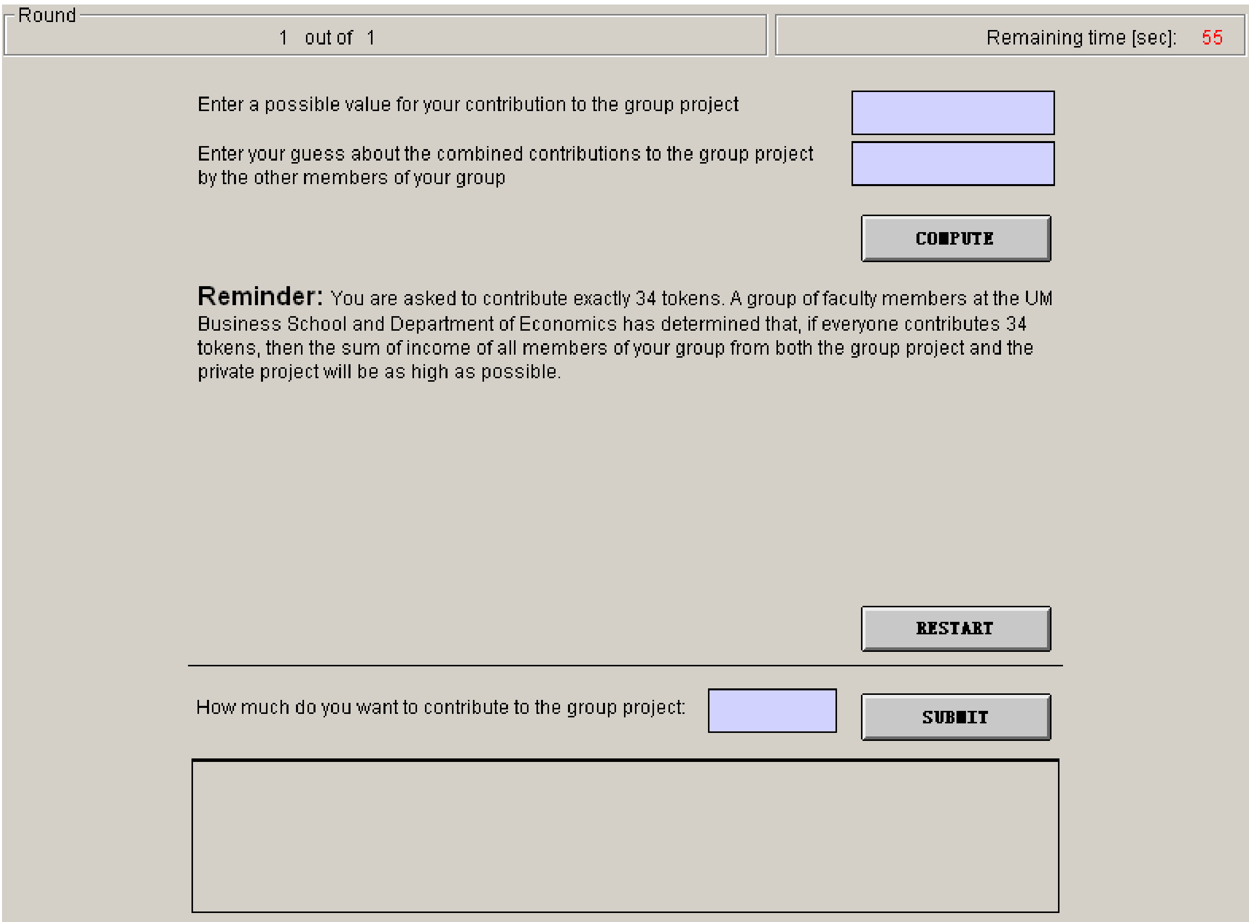


Figure 2: Mean Contribution to the Public Good: BASELINE versus NOEXP Treatment

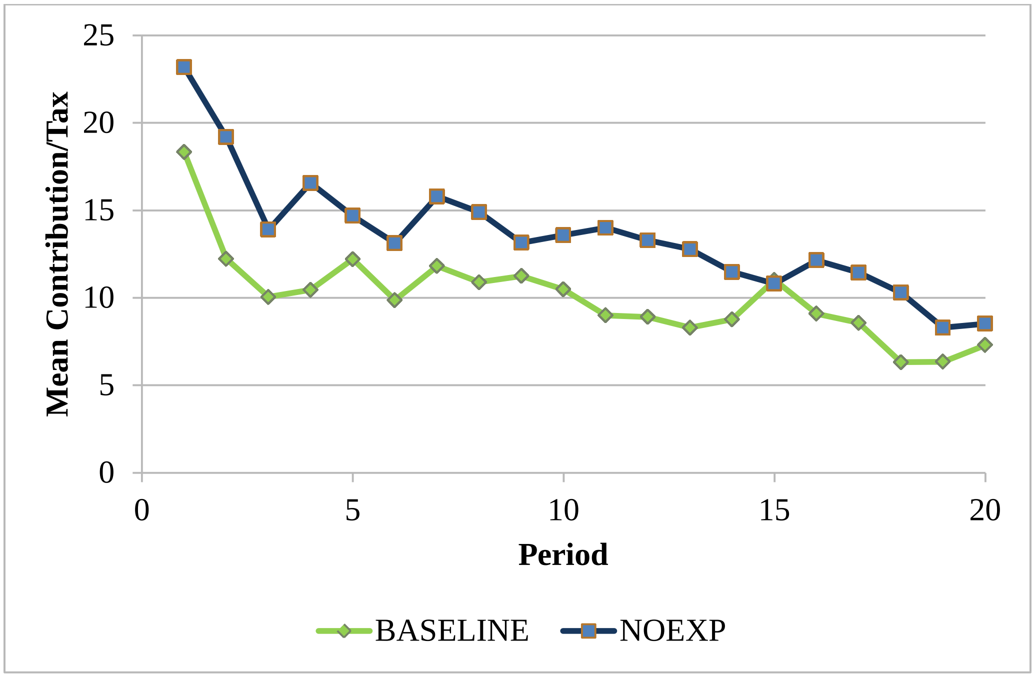
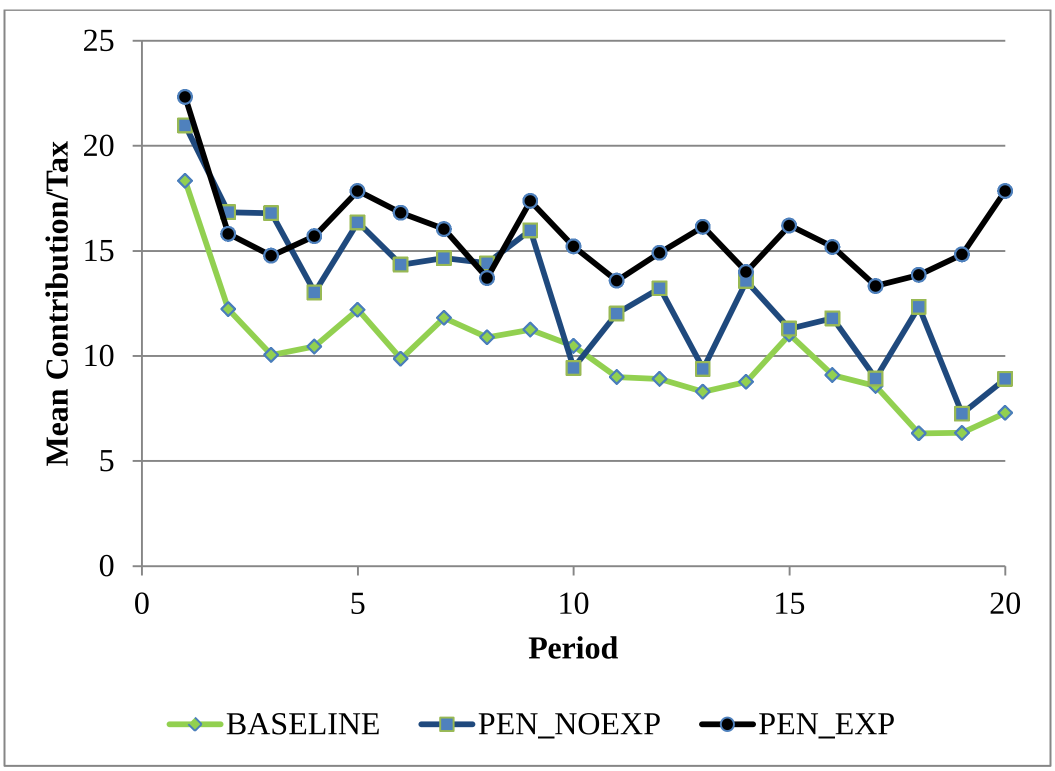


Figure 3: Mean Contribution to the Public Good: BASELINE, PEN_NOEXP and PEN_EXP Treatments



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Instructions

[We provide instructions for the PEN_FACEXP treatment. Instructions for the other treatments can be requested from the authors.]

Thank you for agreeing to participate in this experiment. Please make sure your cell phones are turned off to avoid interruptions during the meeting.

This experiment deals with the economics of decision making. Your participation in this experiment is voluntary. You will be compensated for your participation; if you read the instructions carefully, you can, depending on your and other people's decisions, earn a considerable amount of money in addition to the $5 for showing up.

The experiment proceeds in two parts:

Part 1 consists of 20 decision-making periods. The following pages describe Part 1 of the experiment in detail. After Part 1 of the experiment is over, you will be provided with instructions for Part 2. Part 2 will have two activities. While you wait to be paid, you will be asked to fill out a questionnaire.

During the experiment your earnings will be calculated in Tokens. To compute your earnings from Part 1, the computer will randomly pick a period. Each period is equally likely to be selected. Your earnings in Part 1 will be equal to your earnings in that period. Your earnings from Part 2 are given by the sum of your earnings in both activities. To determine your final earnings in Tokens we will add up your earnings from Part 1 and Part 2. At the end of the experiment the total amount of Tokens you have earned will be converted to US Dollars at the following rate:

100 Tokens = 1.00 US Dollars

Your computer has been assigned an anonymous ID number that you will be informed of. Your decisions and payoffs from the experiment will be recorded to that ID number. At no time will your name be linked to that ID number. Instead, at the end of the experiment, each of you will be called in order, by ID number, to receive your payment in private.

Please do not communicate with the other participants during the experiments. Should you have any questions, please raise your hand.

Instructions for Part 1

Part 1 consists of 20 periods. At the end of the experiment one period will be randomly selected to determine your earnings from Part 1. In each period you will be linked, via computer, to three other randomly chosen individuals in this room to form a group. The composition of your group will change every period. No one will ever learn who the other members of your group were.

At the beginning of each period, you will be endowed with an income of 100 Tokens. Each period, you will be asked to contribute to a group project that earns income in the form of tokens. **Your contribution to the group project** is simply the number of tokens you have transferred from your endowment of 100 to the group project. The **total contribution to the group project** is the total number of tokens transferred to the group project by all members of your group, including you.

Your income from the group project is determined only by the **total contribution:** the sum of your contribution and the contributions of all others in your group. The income of all group members from the group project is calculated in the same way. Hence, every group member will receive the same income from the group project.

You can learn precisely how **your income from the group project** depends on the group's **total contribution** by using a Calculator that will be available to you once the experiment starts. To use the Calculator, you enter two numbers: (1) your contribution to the group project and (2) a guess about what the other members of your group will contribute. The Calculator will add these two values to get the total group contribution and calculate your income from the group project. You will be able to use the calculator as many times as you need before you decide how much to contribute to the group project.

The tokens that you have not contributed to the group project are automatically invested in a private project:

Your investment in the private project = 100 – your contribution to the group project

The private project pays **10** times the amount invested. Therefore, your income from the private project is given by

Your income from private project = 10 * your investment in the private project

The calculator will also compute the level of your investment in the private project and your income from the private project when you enter the two values explained previously. Figure 1 presents the calculator.

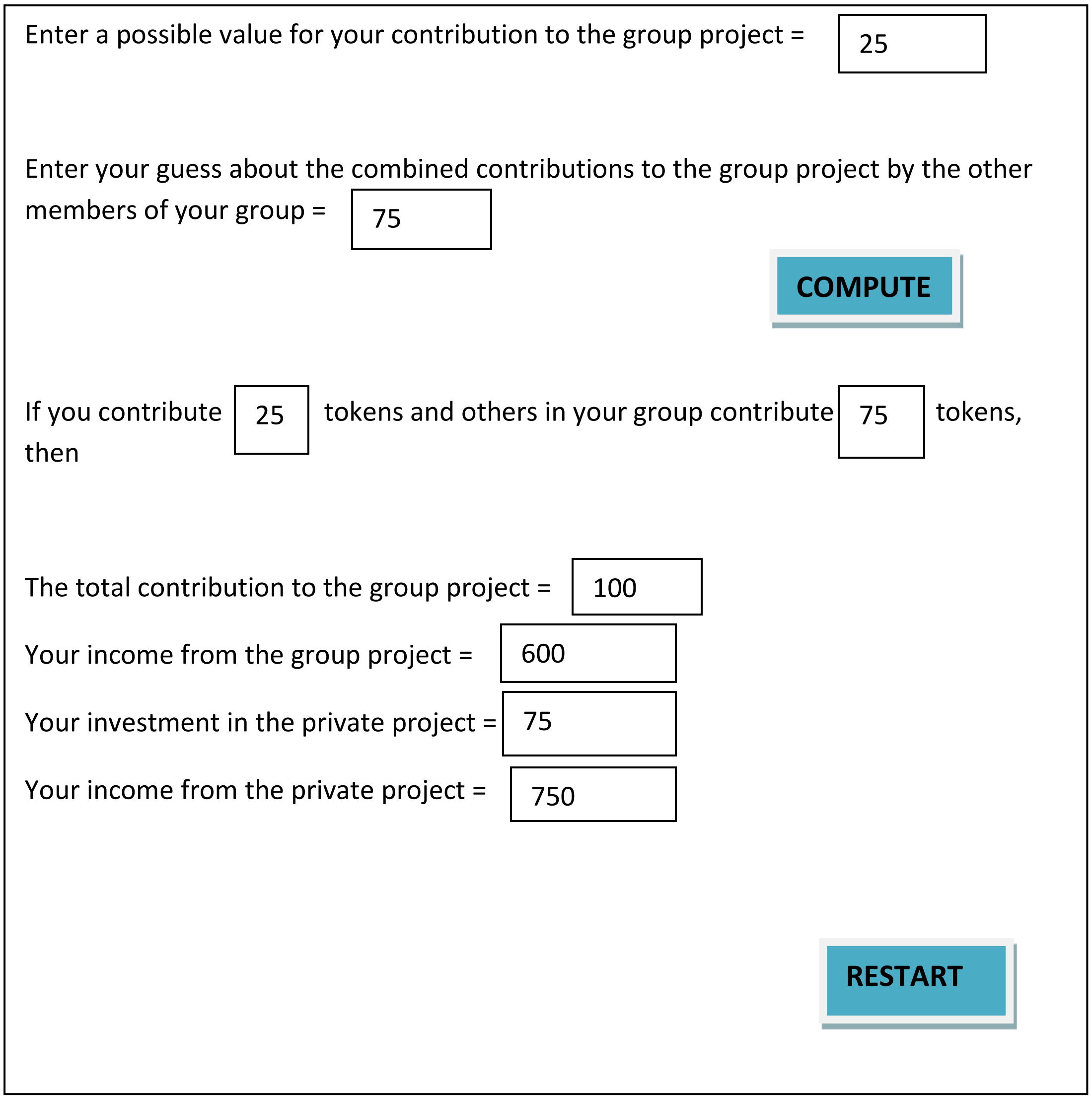

Figure 1: Calculator

The values entered in Figure 1 are only for illustration and are not in any way meant to suggest how you should allocate your tokens. Let us now describe Figure 1 in more detail. Suppose you enter a value of 25 as your possible contribution to the group project and 75 as your guess about the combined contributions to the group project by the other members of your group. When you press the button "compute," the calculator will calculate the total contribution to the group project and show your income from the group project (600 in this example), your investment in the private project (75), and your income from the private project (750).

If you press "restart", you will be able to enter different values in the calculator. Because it is offered just as to help you make your decision, the values you enter in the calculator will not affect your earnings in any way. You are allowed to use it as many times as you like until the 5 minutes of decision time has run out. Note that you are not required to use the calculator.

You can make your decision any time within the 5-minute period. However, once you make your decision you cannot go back and change it. You will enter your decision in the "decision box" as shown at Figure 2, and then pressing the "Submit" button.

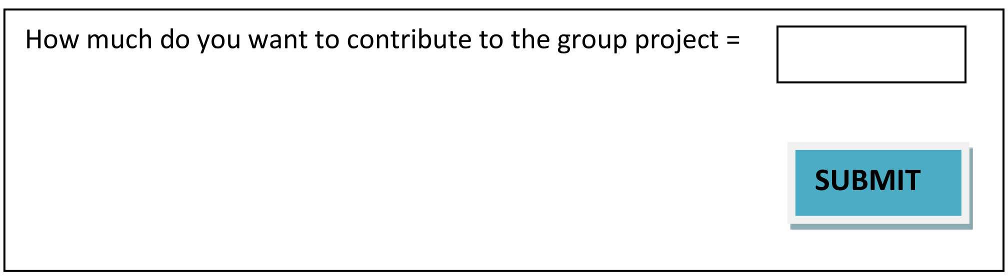

Figure 2: Decision Box

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). A group of faculty members at the UM Department of Economics has determined that, **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. You have been provided with an information sheet that lists the names of these faculty members and a summary of their achievements.

Next, for each participant, the computer will randomly select a letter, a, b, c or d. The computer is equally likely to select each letter. If the letter **a** is selected, and if you contributed less than 34 tokens, then you will be fined. The fine is equal to **two** times the amount your contribution falls short of 34. If the computer selects number **b**, **c**, or **d**, you will not be fined no matter what you have contributed. The computer selects a letter randomly and separately for each of you. So if someone contributes less than 34, that person has a one in four chance of being fined.

The following summarizes your total income in a given period. If the computer randomly selects b, c or d, then there is no fine (even if you have contributed less than 34 tokens), and your income that period will simply be the income from the group project plus income from your private project:

Your total income = Income from the group project + income from your private project

If computer randomly selects letter a, then you will pay a fine if you contributed less than 34 tokens. The fine will be equal to two times the difference between your contribution and the expected contribution:

Fine = 2 * (34 tokens – your contribution)

If you contributed 34 tokens or more, no fine will be levied.

Therefore your total income that period will equal:

Your total income = Income from the group project + Income from your private project – Fine

To help with your decision, the Calculator also provides “your total income if you are fined” and “your total income if you are not fined” when you enter a possible value of your contribution and your guess about the other group members’ contributions to the group project. See Figure 3 for a picture of your calculator that shows information about fines. As in Figure 1, the numbers shown are purely for illustration, and are not meant to indicate what decisions you should make.

In addition, at the beginning of each round, you will see a history of your previous contributions and others’ contributions in your group to the group project, your fine and your total income.

Enter a possible value for your contribution to the group project = 25

Enter your guess about the combined contributions to the group project by the other members of your group = 75

COMPUTE

If you contribute 25 tokens and others in your group contribute 75 tokens, then

The total contribution to the group project = 100

Your income from the group project = 600

Your investment in the private project = 75

Your income from the private project = 750

Your total income if you are fined = 1332

Your total income if you are not fined = 1350

RESTART

Figure 3: Calculator

At the end of each period you will be provided with an “income screen” like the one shown in Figure 4. The income screen will remind you of your contribution to the group project. You will also learn the actual combined contributions of the other members of your group this period, your income from the group project and your income from the private project. In addition, you will learn whether you paid a fine, and what your total income this period is.

Your contribution to the group project =

Combined contributions of the other members of your group to the group project =

Your income from the group project =

Your income from the private project =

Your randomly selected letter is “a”. Fine =

Total income this period=

Figure 4: Income Screen

In Part 1 this exercise will be repeated 20 times (where groups are randomly formed each period). After Part 1 is over, we will give you the instructions for Part 2. You will learn which one of the 20 periods has been selected for your payment at the end of the experiment.

The List of the Faculty Members

Robert Barsky is a professor in the Department of Economics at the University of Michigan. He received his B.A. from the University of Michigan in 1979, and his Ph.D. from MIT in 1985. He arrived at Michigan in 1986. Professor Barsky's primary research interests are consumer confidence, the role of oil in the global economy, and the stock market.

Christopher House is an associate professor in the Department of Economics at the University of Michigan. He received his B.S. from the University of Massachusetts at Amherst in 1992, his M.A. from Northeastern University in 1994, and his Ph.D. from Boston University in 2001. He arrived at the University of Michigan in 2001. His current research is concerns aggregate investment dynamics and the macroeconomic impacts of current U.S. tax policy.

Dmitriy Stolyarov is an associate professor in the Department of Economics at the University of Michigan. He received his M.S. and B.S. from the Moscow Institute of Physics and Technology in 1993, and his Ph.D. in Economics from the University of Pennsylvania in 1999. He arrived at the University of Michigan in 1999. His research has addressed income and consumption inequality as well as the role of technological change in explaining stock market fluctuations.

Jing Zhang is an assistant professor in the Department of Economics at the University of Michigan. She received her B.E. from Northern Jiao Tong University in 1996, her M.S. from the University of Science and Technology of China in 1996 and her Ph.D. from the University of Minnesota in 2005. She arrived at Michigan in 2005. Her research focuses on structural change in open economies and the impacts of the financial crisis on firm behavior and exporting decisions.

Instructions for Part 2

Part 2 consists of two activities. Your income in Part 2 is the sum of your earnings in both activities. Your earnings in each activity depend on your decisions and also on chance. Once you finish an activity you will not be able to go back. You will learn your earnings from both activities (as well as your earnings from Part 1) at the end of Part 2.

The actual decision you make in all activities is up to you. **There is no right or wrong answer**. Just choose the one you like best.

Activity 1:

In activity 1 you are asked to make a decision between the following 5 gambles:

Gamble 1 pays 200 tokens with certainty.

Gamble 2 has a 50 percent chance of paying 150 tokens and has a 50 percent chance of paying 300 tokens.

Gamble 3 has a 50 percent chance of paying 100 tokens and has a 50 percent chance of paying 400 tokens.

Gamble 4 has a 50 percent chance of paying 50 tokens and has a 50 percent chance of paying 500 tokens.

Gamble 5 has a 50 percent chance of paying 0 tokens and has a 50 percent chance of paying 600 tokens.

Which gamble do you choose?

____

Activity 2:

In activity 2 you will be randomly matched with one other participant. Nobody will ever learn whom they were matched with. You are asked to decide between the following three options:

Option 1: You will receive 200 tokens and your paired participant will receive 200 tokens.

Option 2: You will receive 175 tokens and your paired participant will receive 300 tokens.

Option 3: You will receive 225 tokens and your paired participant will receive 100 tokens.

Similarly your paired participant will decide between these options. The computer will randomly choose one of you with equal chances and implement his/her decision. For example, suppose you picked option 2 and your paired participant picked option 3. If your chosen option is randomly selected to be implemented, then you will receive 175 tokens and your paired participant will receive 300 tokens. However, if your paired participant's decision is randomly selected to be implemented, your paired participant will receive 225 tokens and you will receive 100 tokens (assuming your paired participant picked 225 tokens for himself/herself and 100 tokens for you). The example described here is only for demonstration purposes, and is not meant to suggest how you should make your decision in any way.

Which option do you choose?

____

Part 3

1. Age:
2. Gender:
3. Major:
4. How would you summarize your view on the following statement: Cheating on taxes, if you have the chance, can never be justified.

a. I completely agree
b. I agree most of the time
c. I agree
d. I am indifferent
e. I disagree
f. I disagree most of the time
g. I completely disagree

5. How would you summarize your view on the following statement: Public officials can usually be trusted to do what's right.

a. I completely agree
b. I agree most of the time
c. I agree
d. I am indifferent
e. I disagree
f. I disagree most of the time
g. I completely disagree