

Passionate Providers and the Possibility of Public Commitment

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1. Introduction

The fact that individual preferences are both heterogeneous and inherently unobservable presents a fundamental challenge to the practical problem of public goods provision. Samuelson (1954) was skeptical as to whether any decentralized mechanism would exist that could provide incentives for individuals to reveal information about preferences. Theoretical work in mechanism design by Hurwicz (1975), Ledyard and Roberts (1974), Green and Laffont (1977) and Walker (1980) formalized Samuelson's conjecture, lending credence to his skepticism. In particular, the familiar "voluntary contributions mechanism" (VCM) has poor incentives for efficient provision of the public good.

In light of these problems with incentives for public goods provision, many economists were surprised by early experimental work on public goods demonstrating that, while the VCM seldom achieved perfect outcomes, the results were not as dismal as the "full free riding" predicted by standard (self-interested) theories. Thus, for many years the literature of theorists, archival data economists, and experimenters has contained attempts to identify elements of both the economic "environment" and the economic "institution" (Smith (1982)) that behaviorally promote increased contributions. Many of each have been studied and so identified. In terms of the economic environment (most notably the condition of individual preferences) one of the most powerful is the marginal-per-capita return, first identified in Isaac and Walker (1984) and explored more deeply in Isaac and Walker (1988), Isaac, Walker and Williams (1994) and, in simulation modelling, in Arifovic and Ledyard (2012).

There have been several experiments looking at either endowment or preference heterogeneity in the VCM, but Ledyard (1995) characterized the preponderance of the

evidence at the time of his survey as indicative of heterogeneity being a “weak” effect. Nonetheless, interest in heterogeneity continues, motivated in part by the casual empiricism that in numerous public goods situations individuals, firms, and even countries or groups of countries can vary greatly in their net preference for providing a public good.

More specifically, there has been some research on how a small proportion of high preference (highly motivated, or, as we call them here, “passionate”) contributors can influence the larger group (see Brookshire et al., 1986; Reuben and Riedl, 2009, and Isaac and Norton, 2013).

Our approach in the present experimental investigation is closely related to the setup used in Isaac and Norton (2013). In their experiment, each session consisted of four blocks of seven periods. In the first block, the subjects participated in the standard VCM. In each of the subsequent blocks, the game was augmented with one of the three treatments – an exogenous tax, and endogenous tax determined using a median voter mechanism, and the endogenous tax treatment combined with the introduction of passionate providers (who they call “agents of grace”).

This experiment attempts to make clear the impact of passionate providers on their larger group. Our central manipulations are the availability of the tax mechanism (commitment) and the intra-group treatment process consisting of the timing of when the exogenous preference change that creates passionate providers occurs. We also report control sessions in which no passionate providers are introduced. We discuss the genesis of these treatments in turn.

The availability of a tax mechanism alongside the VCM was motivated for Isaac and Norton (2013) by the historical observation that what is called the “Third Great Awakening” in the United States produced two different strands of how individuals with (newly) passionate preferences transmitted their preferences to their groups. On the one hand, this era produced numerous private institutions of charity such as the Salvation Army and homes for orphans and unwed mothers. Yet, others in the era turned away from private collective action to governmental action. The most famous example was the 18th Amendment to the U.S. Constitution that prohibited the sale of alcoholic beverages. In terms of economic policy, a prominent face of religious revival in this period was the

so-called “Social Gospel” in which Christian revivalism merged with calls for government regulation, taxation, and expenditures championed by the Populist and Progressive movements. In the Isaac and Norton (2013) experiment, the authors wished to examine first how individuals who receive a change in preferences alter both voting and giving behavior compared to their peers. However, a second focus of the Isaac and Norton paper was to ask how these newly passionate providers influenced the voting and giving behavior of the other members of the group.

The current study was designed to sharpen the focus on the effects on the passionate providers. The size of the group was reduced to allow more sessions to be conducted. Phases I and II from the Isaac and Norton (2013) experiment were eliminated. Instead, all groups began in the endogenous taxation mode, with one passionate provider introduced at the end of either the 10th or the 20th period with 30 total periods per session. As in Isaac and Norton (2013), the individuals were transformed into passionate providers by decreasing the return they received from investments in the private good.

The timing of when passionate providers are introduced is not motivated by a specific historical era. Instead, the variation in timing corresponds to the recognition that “path dependence” is an important phenomenon. In our 30-period experiment, we implement the exogenous change in preferences after period 10 or after period 20 to generate variation in the experienced “path” before passionate providers can attempt to improve the situation. The logic behind this manipulation is that the longer a degenerate situation continues the more difficult the extrication.

The rest of the paper is organized as follows. Section 2 details the experimental design while Section 3 provides hypotheses for what we should expect to see in the experimental data. We provide an analysis of the experiment in Section 4 and place the results in context when we close with our discussion in Section 5.

2. Experimental Design

Experiments were conducted in the XS/FS laboratory at Florida State University (FSU). Participants were recruited using ORSEE (Greiner, 2004) from a pool of undergraduate students at FSU and were pre-screened to exclude those who had previously participated in any public goods experiments. The experimental software was

programmed in z-Tree (Fishbacher, 2007). Participants were paid a \$10 show up fee in accordance with XS/FS lab protocol and earned additional money for decisions made in the experiment.

There were two stages in each experimental session. In the first stage, subjects played a one-shot strategy method public goods game in randomly created five-player groups. Our intention was to use the decisions made by subjects in the first stage to classify them by their contribution type and compare with their decisions in the second stage, as in Fischbacher et al. (2001). However, since the elicited contribution schedules did not correspond with observed behavior in the second stage, we do not elaborate any further on the data collected from Stage 1. After the first stage was completed, subjects were randomly rematched into new groups of five for the second stage. Stage 2 consisted of 30 periods of the VCM experiment in fixed groups. Subjects received an endowment of 100 tokens in each period and faced a marginal per capita return (MPCR) equal to 0.5.

We introduced two treatments in the VCM experiment. First, we varied the time when the Passionate Providers were introduced, after either period 10 or period 20. Second, we varied whether or not the endogenous tax mechanism was available to the group. In addition, we conducted control sessions in which we followed the same procedure used to introduce Passionate Providers, but did not actually implement any changes to the incentives of anyone in the group. These control treatments provide a baseline against which we evaluate the effect of the passionate providers.

We now explain the procedure used to introduce the Passionate Providers. Before the beginning of the experiment envelopes were placed at each of the computer terminals. Participants were instructed to leave them unopened until period 10 or 20, depending on the treatment, at which point in time, all participants were told, “You may now open your envelope we gave you at the beginning of the experiment which contains a piece of paper telling you what your return to the individual account will be for the next [10 or 20] periods. Please do not share this information with anyone.”¹ The subjects for whom the return to the individual account was reduced to 0 are referred to as the Passionate

¹ The language that appears on the sheet of paper for participants is one of the following two messages: (1) “...your earnings for each token invested in the individual account will remain at one experimental ECU per token in each period,” and (2) “...tokens invested in your individual account will yield you zero experimental currency units (that is, no earnings at all).”

Providers.² The *Control* sessions, in which no player's incentives were changed, also allow us to check that there are no effects from the thrill of opening the envelopes.

Next we describe the endogenous tax institution. At the beginning of every period, each group member submits a vote for his preferred tax level. Votes can be whole numbers between 0 and 100 inclusive. The tax is determined using a median voter mechanism where the median of the submitted votes is selected to be the tax. That tax is applied to each participant's 100 token endowment and the collected tax revenue is multiplied by 0.8 and then placed into the group account. That is, 20 percent of the tax revenue is lost through inefficiency.³ This inefficiency provides the necessary tension to generate a meaningful choice between voluntary provision and the tax institution. After the taxes are collected, the subjects participate in the VCM with their remaining endowments. For example, if the selected tax rate is 30%, a total of 150 tokens will be collected and 120 tokens will be deposited into the group account. Then, each participant chooses how to allocate the remaining 70 tokens of their endowment between their individual and group accounts.

We conducted 18 sessions of experiments with a total of 59 groups. Table 2 summarizes the number of groups per treatment, including the *Control* sessions. With each of the 295 subjects making decisions across 30 periods this generated a panel of 8,850 observations.

3. Hypotheses

At the aggregate level, we examine two main hypotheses. The first concerns the effect of the endogenous tax mechanism. Isaac and Norton (2013) found evidence that the endogenous choice of a tax regime improved the level of public good provision. Thus, we first examine whether the effect is replicated in the current design.

² Initially, we lowered the return to the individual account to 0.25, thereby increasing the MPCR for the Passionate Providers to 2. We did this for four groups in both the Tax-10 and Tax-20 condition. However, as in Reuben and Reidl and Isaac and Norton, we observed some Passionate Providers failing to follow their dominant strategy. In all subsequent sessions we made the value of individual tokens zero, effectively raising the MPCR for passionate providers to infinity.

³ The 20% loss may also be interpreted as representing a preference for voluntary provision, or as being used for some other form of government spending.

Table 2: Groups Per Treatment

	Tax Mechanism	No Tax Mechanism
Pass. Prov. Introduced After Period 10	8 Groups	8 Groups
Pass. Prov. Introduced After Period 20	12 Groups	8 Groups
Control: Same Mechanism Used After Period 10	5 Groups	5 Groups
Control: Same Mechanism Used After Period 20	8 Groups	5 Groups

Hypothesis 1. The total provision of the public good will be higher in the sessions with the endogenous tax mechanism than in sessions without the tax.

Secondly, the data in Isaac and Norton (2013) suggested that introducing passionate providers (agents of grace) did not lead to complete crowding out of contributions by the other group members. In the current experimental investigation, we examine the effect of introducing the passionate providers both with and without the option of an endogenous tax mechanism.

Hypothesis 2. The total provision of the public good will be higher in the *Treatment* sessions than in the *Control* sessions after the period in which the Passionate Providers are introduced. That is, increased contributions will only lead to partial crowding out.

In the endogenous tax treatments, there are two channels through which the level of provision may be affected. Specifically, the non-Passionate Providers may respond by changing their contribution decisions or by changing their votes. On the other hand, in the no-tax treatments, we can examine Hypothesis 2 when the players can only respond by altering their contribution decisions. Given that decay across periods is typical in many public goods environments, the *Control* sessions will allow us to calibrate a baseline for the comparison required by this hypothesis.

At the individual level, there are two main outcome variables of interest: residual contributions and votes. Residual contributions are calculated by dividing contributions by the after-tax endowment. This variable is intended to make contributions decisions comparable across groups with different selected tax levels. When the selected tax level

is 100%, residual contributions are undefined. As noted later, this causes some problems for the data analysis.

First we examine how the introduction of the Passionate Providers affects the residual contributions of others in the group. Note that Passionate Providers cannot act on their newfound dominant strategy until the period after their incentives are manipulated. Thus, we anticipate that other group members will only start responding to the increase in contributions by Passionate Providers in the second period after their introduction. The evidence presented by Isaac and Norton (2013) suggests that there will be weak positive reciprocal behavior.

Hypothesis 3. The non-Passionate Provider group members will respond with higher residual contributions in the periods following the Passionate Providers' introduction.

Some scholars might think that non-Passionate Providers will have no reason to respond with higher residual contributions because the Passionate Providers lack intention. While it is true that the Passionate Providers' contributions are high merely from following their dominant strategy there are some problems with believing other group members shouldn't respond. First, the other group members do not know that the Passionate Providers have received an exogenous change in preferences that lead them towards higher contributions. Second, other scholars have noted that individuals seem to reciprocate towards the aggregate (Croson, 2007).

The notion that non-Passionate Providers will reciprocate with higher residual contributions following the introduction of the Passionate Providers seems probable given the above arguments. However, the effect is not necessarily linear. In the literature on gift exchange a high wage seems to generate some initial high effort, but the effect is dampened over time (see Gneezy and List (2006), and the replication in the lab by Kube et al. (2012)). If similar mechanisms are at work in the public goods environment those results suggest that we will see a non-linear and concave reaction (over time) to the introduction of the Passionate Providers.

Hypothesis 4. The reciprocal response of non-Passionate Provider group members to the introduction of a Passionate Provider will be positive but will weaken over time.

Turning to the path dependence treatments there are two reasons we think that non-Passionate Providers will respond with higher contributions when the introduction of the Passionate Providers occurs following Period 10 rather than Period 20. First, path dependence suggests that when group members experience low contributions for a prolonged period of time their willingness to cooperate will sour and the introduction of the Passionate Providers will have a smaller effect. Second, the future benefits from cooperation are different if the Passionate Providers are introduced after Period 10 (which has 20 remaining periods) or Period 20 (which has 10 remaining periods). Although this does not change the free-riding equilibrium one might imagine a boundedly rational agent that notices the greater benefits to cooperation over 20 periods, but fails to apply backward induction.

Hypothesis 5. The non-Passionate Provider group members will, on average, reciprocate with higher contributions when the Passionate Providers are introduced after Period 10 rather than after Period 20.

The remaining hypotheses regard the tax institution and voting. In Isaac and Norton (2013) groups who were not able to develop trust in the VCM opted for higher taxes as a commitment device. Given the unraveling trust and reciprocity in the VCM environment it seems likely that falling contributions will lead to increased taxes.

Hypothesis 6. When individuals observe low residual contributions their response will be to vote for higher levels of taxation.

Moving to our specific treatments and our motivation, there is an interesting question about how the Passionate Providers will alter their voting behavior once their preferences are changed. Because Passionate Providers recognize that (1) their dominant strategy is for full contribution to the group account while (2) others have a dominant strategy to free ride, it seems probable that the Passionate Providers will vote for higher taxes to secure greater earnings.

Hypothesis 7. Passionate Providers will cast higher votes in the period following the exogenous preference change compared to non-Passionate Providers.

4. Results

4.1 Aggregate Results

We first examine the data as they pertain to Hypothesis 1, which addresses the influence of the option of an endogenously-determined tax. Figures 1a – 1d display the average total *provision* of the public good with and without the tax institution (in the endogenous tax treatments, this means the total provision of the public good from both the tax and the residual voluntary contributions, after accounting for the 20 percent inefficiency in the tax mechanism). The figures only display the periods before the (potential) passionate providers are introduced. For comparison, we break down both the Period 10 intervention and the Period 20 intervention by the treatment and control groups. This provides a sense of the “baseline” similarity between the treatment and control sessions, since they are equivalent up until the intervention.

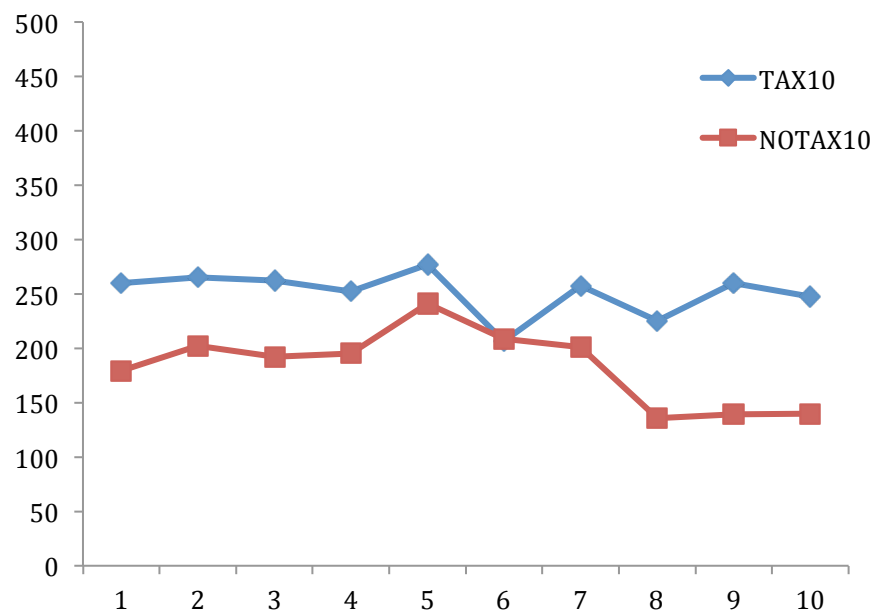


Figure 1a: Average Provision Before Introduction of Passionate Providers in Period 10

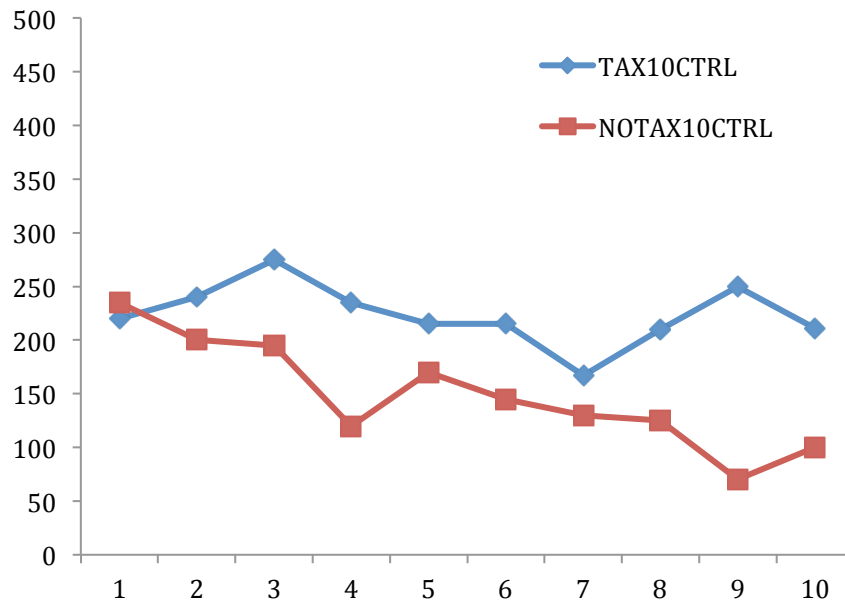


Figure 1b: Average Provision Before Period 10; Control Experiments

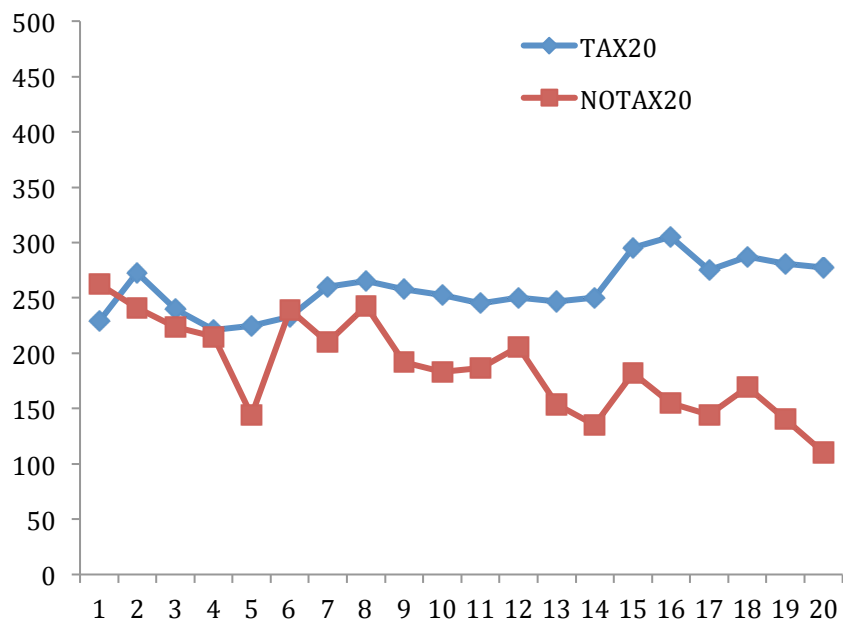


Figure 1c: Average Provision Before Introduction of Passionate Providers in Period 20

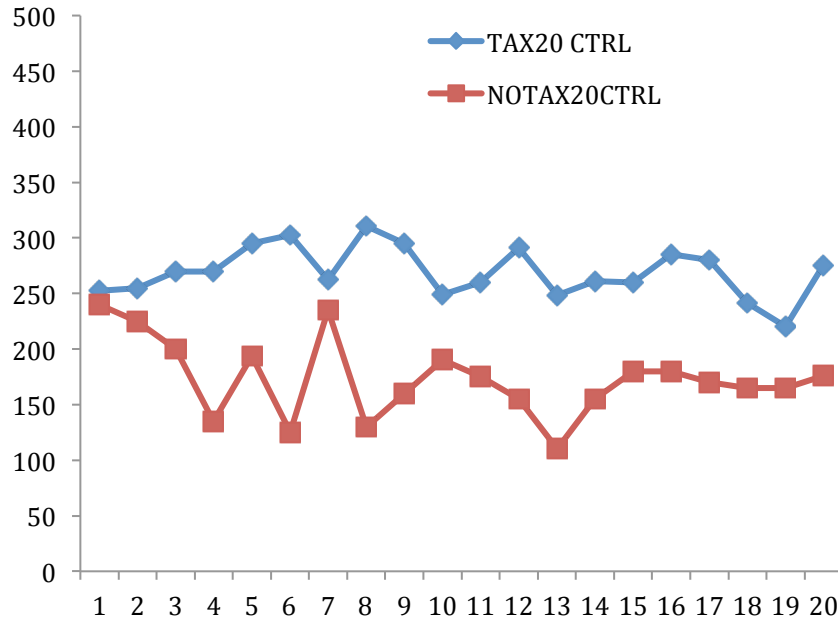


Figure 1d: Average Provision Before Period 20: Control Experiments

A visual inspection of Figures 1a – 1d appears to support Hypothesis 1. The average provision from the no-tax treatment is only greater than the provision with the endogenous tax in four periods (out of a possible 60). We can test for the effect of the endogenous tax option using a simple regression on the medians of the form

$$MEDIAN = f(CONSTANT, PERIOD, DUMTAX, PERIOD * DUMTAX)$$

where *DUMTAX* is a dummy variable representing a period with the tax institution. As in Figures 1a – 1d, we only include data from before the (potential) introduction of the passionate providers. To confirm the support provided by visual inspection of Figures 1a – 1d, we should find a positive and statistically significant coefficient for at least one of the explanatory variables involving the dummy for the tax, in each of the four cases (the two treatments and the two controls). Table 3 presents the results.

The regression results are consistent with the view from Figures 1a – 1d: in sign, the existence of the endogenous taxation institution raises the level of contributions and flattens the per-period decay in seven of the eight cells, and one of the effects is statistically significant in each of the four comparisons. (In the Treatment, 10-Period and

Control, 20-Period sessions, the significant effect is on the level; in the other two cases the significant effect is on a flattening of the decay.)

Table 3: Regression results on the effects of the availability of the endogenous tax institution.

	Tax vs. No Tax 10 Periods	Tax vs. No Tax 10 Periods Control	Tax vs. No Tax 20 Periods	Tax vs. No Tax 20 Periods Control
<i>CONSTANT</i>	220.13 (17.73)***	231.333 (17.96)***	247.13 (10.32)***	194.21 (13.35)***
<i>PERIOD</i>	-6.66 (2.86)**	-14.97 (2.90)***	-5.75 (0.86)***	-2.00 (1.11) *
<i>DUMTAX</i>	44.23 (25.08)*	10.47 (25.41)	-18.14 (14.59)	85.02 (18.87)***
<i>PERIOD*DUMTAX</i>	4.29 (4.04)	11.70 (4.09)**	8.56 (1.22)***	1.05 (1.57)

Standard errors are in parentheses. *** = $p < .01$; ** = $p < .05$; * = $p < .10$

We turn now to the second aggregate effect hypothesis, which addresses the issue of crowding out. If there is extensive crowding out, then increased contributions by passionate providers may be met with a countervailing reduction in others' contributions, whether directly or, in the tax treatments, through a change in their voting decisions. With extensive crowding out, any pulse upward by the passionate providers ought to be offset by the other participants. As a result, one would expect the provision path to exhibit either (a) no effect from introducing passionate providers, relative to the Control sessions, or (b) a temporary pulse upwards, followed by a more intensive decay in contributions than in the Control sessions once crowding out manifests.

We can investigate these effects statistically, again with a simple regression in which the median provision of the public good is explained by a *Constant*, the *Period* in the session, a dummy variable for the intervention period (to capture transitory restart effects), and then both a level change dummy for *Passionate Providers* and an interaction

term between *Period* and *Passionate Providers* to see how the latter affected decay. The results from the regressions on the variables of interest (*Passionate Providers* and its interaction with *Period*) are reported in Table 4.

Table 4: Coefficients and associated p values related to crowding out of the effects of Passionate Providers.

	Coefficient on Passionate Providers	Coefficient on Period x Passionate Providers
Tax With Passionate Providers at Period 10	20.13 (p = .34)	0.86 (p = .40)
No Tax With Passionate Providers at Period 10	83.96 (p = .03)**	-1.16 (p = .52)
Tax With Passionate Providers at Period 20	57.75 (p = .003)***	0.52 (p = .42)
No Tax With Passionate Providers at Period 20	61.45 (p = .005)***	-0.34 (p = .65)

Not surprisingly (see Andreoni (1993) and Isaac and Norton (2013)) these results seem most consistent with a lack of complete crowding out, in support of Hypothesis 2. In all four cells the introduction of Passionate Providers induces a positive level effect (statistically significant in three of four cells). The post introduction effect on period decay is mixed, small in magnitude, and never statistically significant.

We now turn to a presentation of the data at the individual level.

4.2 Individual Results --- Residual Contributions

First, we address the key question as to whether Passionate Providers have a positive influence on the residual contributions of the other group members. Recall that Reuben and Riedl found no such influence, while Isaac and Norton identified the following effect: the introduction of Passionate Providers (who they call “agents-of-grace”) did not raise the level of residual contributions by the other players, but it did

flatten the per-period decay. One of the problems in comparing Reuben and Riedl, on the one hand, and Isaac & Norton on the other is that the former used a standard VCM while the latter used the same endogenous tax institution implemented here. In this paper, we address these differences by investigating both the non-voting (no-tax) VCM (as in Reuben and Riedl) and the combined VCM-tax mechanism (as in Isaac and Norton). We begin with the no-tax VCM.

In the standard VCM (i.e without voting on an endogenous tax), the “residual contributions” of the non-Passionate Providers are simply their contributions to the public good. Figures 2a and 2b present the average (residual) provision level (the total contributions made by non-Passionate Providers) over time for the NV10 and NV20 sessions, respectively. In each figure, we compare the treatment sessions with the control sessions. For the treatment sessions, we sum the contributions of the four non-Passionate Providers. Thus, in order to facilitate the comparison, for the control sessions, we sum the contributions of the 5 players (since nobody is a Passionate Provider) and multiply the total by 0.8 to account for the additional player.

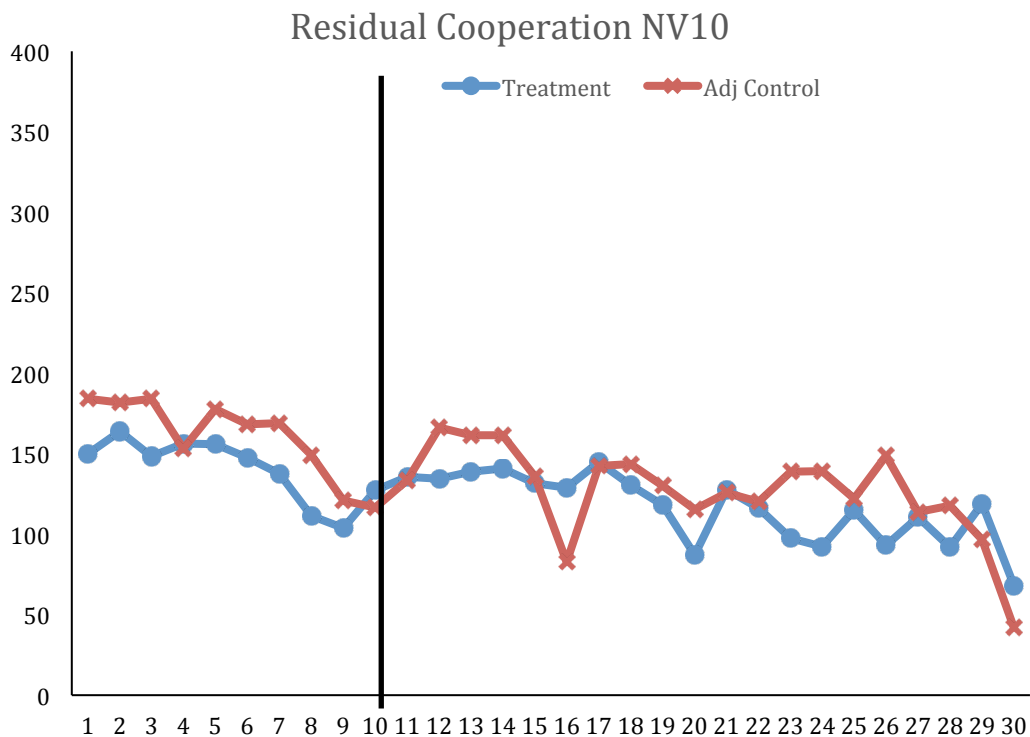


Figure 2a: Residual contributions in the No-Tax, Period 10 sessions

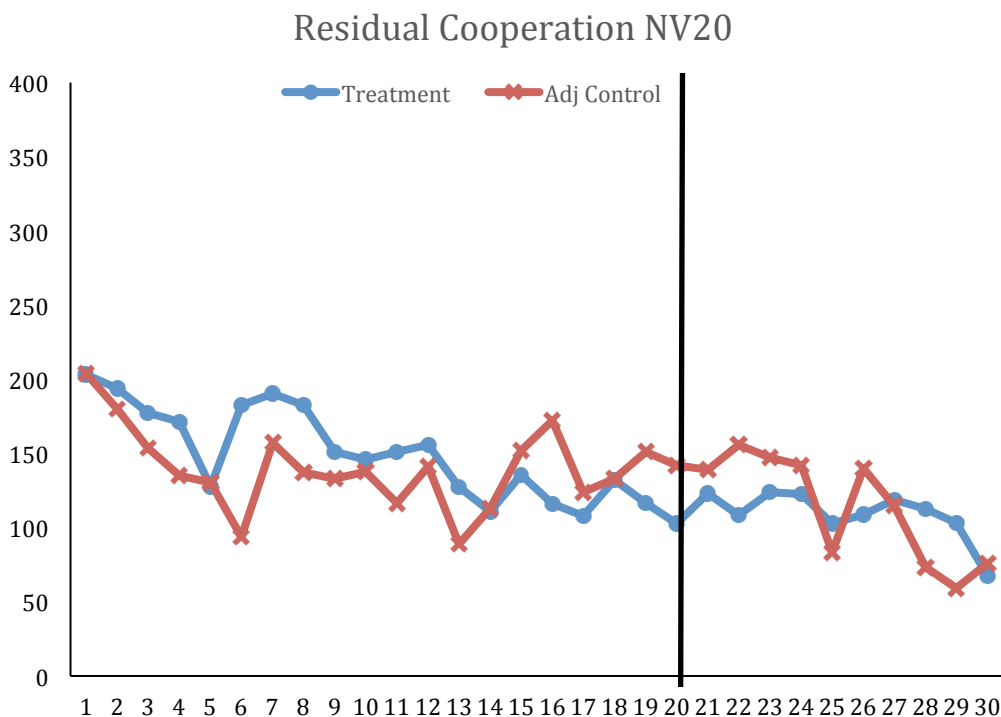


Figure 2b: Residual contributions in the No-Tax, Period 20 sessions

A cursory examination of Figure 2a does not provide much evidence of a difference between contributions in the treatment and adjusted control sessions. On the other hand, in Figure 2b, there is some indication that the per-period decay after the intervention period is flatter in the treatment sessions than in the control sessions.

For a deeper analysis, we conduct a regression that is an analog to the one conducted in Isaac and Norton (2013). Specifically, we calculate an index of “residual cooperation”, which we call *RESID*, by dividing the number of tokens contributed by an individual by the after-tax endowment (the number of tokens remaining after their endowment is taxed). In the No-Tax treatment, *RESID* is simply the individual’s contribution to the public good divided by 100. In the Tax treatment, *RESID* is undefined in a period when the group selects a 100% tax. As we will explain in some detail further below, the incidence of groups that select a 100% tax present some difficulties for our estimation strategy. The regression model is given by

$$RESID = f(CONSTANT, PERIOD, TAX, PP, PP*PERIOD, GROUP_PP, \\ GROUP_PP*PERIOD, STAGE2, STAGE2*PERIOD, INTV, EOS)$$

where

PERIOD indicates the current period;

TAX indicates the tax level voted by that person's group in that period (only included in the Tax treatments);

PP is a dummy variable used to indicate an individual whose preferences have been altered to make her a Passionate Provider;

*PP*PERIOD* is the interaction term of *PP* and *PERIOD*;

GROUP_PP is a dummy variable for an individual who is, in the current period, interacting with a Passionate Provider (i.e. all non-Passionate Providers in Treatment sessions after the Passionate Providers are introduced);

*GROUP_PP*PERIOD* is the interaction term of *GROUP_PP* and *PERIOD*;

STAGE2 is a dummy variable for all periods after the intervention period, whether or not any Passionate Provider is introduced (i.e. in both treatment and control sessions);

INT is a dummy variable for the intervention period itself; and

EOS is a dummy variable for the end-of-session period.

This equation is as similar as possible to that of Isaac and Norton (2013), and obviously the coefficients of interest here are the variables related to the residual cooperation of the non-Passionate-Providers: *GROUP_PP* and *GROUP_PP*PERIOD*. It is important to recall, as in Isaac and Norton, that *GROUP_PP* is *not* the change in level at the intervention period; that is found by the calculation of *GROUP_PP* plus the change indicated by *GROUP_PP*PERIOD* evaluated after the intervention (period 11 or period 21). In Isaac and Norton (2013), the unusual result was a kind of rotation: the sign on the level variable was negative and insignificant, but there was a statistically significant flattening of the decay.

We begin with a presentation of the results from the No-Tax sessions. We report both a random-effects OLS with standard errors clustered at the group level and a random-effect Tobit estimation.

Table 5: Coefficients and associated standard errors relating to residual cooperation.

	No-Tax-10 OLS	No-Tax-10 Tobit	No-Tax-20 OLS	No-Tax-20 Tobit
<i>CONSTANT</i>	0.458 (0.046)***	0.466 (0.049)***	0.460 (0.034)***	0.443 (0.038)***
<i>PERIOD</i>	-0.014 (0.005)***	-0.019 (0.004)***	-0.009 (0.002)***	-0.011 (0.002)***
<i>PP</i>	0.214 (.242)	0.221 (0.134)*	0.023 (0.611)	0.044 (0.492)
<i>PP*PERIOD</i>	0.016 (0.008)*	0.032 (0.006)**	0.013 (0.023)	0.021 (0.019)
<i>GROUP_PP</i>	0.015 (0.112)	0.010 (0.077)	-1.253 (0.523)**	-1.905 (0.301)***
<i>GROUP_PP*PERIOD</i>	0.000 (0.005)	0.001 (0.077)	0.051 (0.021)**	0.078 (0.012)***
<i>STAGE2</i>	-0.034 (0.107)	-0.025 (0.065)	0.464 (0.432)	0.839 (0.268)***
<i>STAGE2*PERIOD</i>	0.009 (0.008)	0.010 (0.005)*	-0.016 (0.017)	-0.031 (0.011)***
<i>INTV</i>	-0.014 (0.029)	-0.027 (0.046)	0.024 (0.047)	0.030 (0.062)
<i>EOS</i>	-0.097 (0.042)**	-0.179 (0.049)***	0.026 (0.036)	0.021 (0.064)
Wald Chi ²	475.9	354.62	132.18	166.17
No. of Obs.	1950	1950	1950	1950

Standard errors are in parentheses. *** = $p < .01$; ** = $p < .05$; * = $p < .10$

The results were interesting to us, to put it mildly. For the “No-Tax 10” sessions there is no statistically significant effect of the transformation of one of the individuals into a Passionate Provider. This effectively reproduces the result in Reuben and Riedl (2009). On the other hand, when, as in Isaac and Norton (2013), the Passionate Providers are introduced near the end of the session, we get a pattern very similar to Isaac and Norton; namely the same rotation pattern with a flattening of the per-period decay. Thus, for the No-Tax sessions, we report a mixed result for Hypothesis 3, which is the hypothesis that we most hoped to clarify with this research. Namely, there are (what are now) reproducible situations in which the introduction of Passionate Providers changes the residual contributions of the other group members, but there are clearly (now reproducible) situations in which it does not.

We turn now to the sessions with the endogenous voting. The equivalent regressions for these Tax sessions are presented below in Table 6. Again, both estimation types are random-effects and the OLS sessions have errors clustered at the group level. Notice that in Table 6, except for the *CONSTANT* term and the additional *TAX* variable, there are few of the explanatory variables that are statistically significant. This includes all of the variables relating to whether the introduction of the Passionate Providers alters the residual contributions of the other individuals. Thus Hypothesis 3 is not supported in these endogenous-tax experiments. However, examining the data suggests that the poor predictive power of the model may be driven, partly, by an unintended consequence of one of our design choices. In our new design, we adopted a longer number of periods with the endogenous taxation institution and stable preferences. In the Tax-10 sessions this would be the periods from 11- 30. In the Tax-20 sessions this would be the periods from 1-20. Isaac and Norton (2013) discussed the possibility of the endogenous taxation institution with median voting having a “ratchet effect” upwards towards 100% contributions. The ratchet effect upwards does not, *per se*, cause us problems. However, the frequency with which the groups voted for a 100% tax presents a serious problem, since “residual contribution” is no longer defined. This causes a potential selection effect, with some groups switching in and out of the sample based on their selection into and out of the 100% tax regime. To illustrate, Table 7 tracks the incidence of 100% taxes across the Tax-20-Control sessions; similar issues arise in the Tax-10 and the Tax-20 sessions.

Table 6: Coefficients and associated standard errors relating to residual cooperation.

	Tax-10	Tax-10	Tax-20	Tax-20
	OLS	Tobit	OLS	Tobit
<i>CONSTANT</i>	0.365 (0.052)***	0.334 (0.052)***	0.368 (0.026)***	0.345 (0.045)***
<i>PERIOD</i>	-0.005 (0.003)*	-0.007 (0.004)	-0.008 (0.001)***	-0.012 (0.002)***
<i>PP</i>	0.484 (0.137)***	0.831 (0.135)***	0.303 (0.347)	0.303 (0.523)
<i>PP*PERIOD</i>	-0.002 (0.007)	-0.006 (0.006)	0.012 (0.014)	0.024 (0.020)
<i>GROUP_PP</i>	0.039 (0.056)	0.040 (0.077)	0.048 (0.225)	0.074 (0.320)
<i>GROUP_PP*PERIOD</i>	-0.002 (0.003)	-0.002 (0.004)	-0.005 (0.008)	-0.009 (0.013)
<i>STAGE2</i>	-0.028 (0.028)	-0.017 (0.064)	0.018 (0.190)	0.255 (-.260)
<i>STAGE2*PERIOD</i>	0.001 (0.002)	0.001 (0.005)	-0.001 (0.007)	-0.005 (0.010)
<i>INTV</i>	0.021 (0.002)	0.020 (0.045)	-0.008 (0.040)	-0.024 (0.065)
<i>EOS</i>	0.002 (0.032)	-0.002 (0.051)	-0.047 (0.046)	-0.072 (0.072)
<i>TAX</i>	0.001 (0.000)***	0.002 (0.001)***	0.001 (0.000)***	0.001 (0.000)***
Wald Chi ²		211.74	183.21	299.52
No. of Obs.	1,690	1,690	2,355	2,355

Standard errors are in parentheses. *** = p < .01; ** = p < .05; * = p < .10

Table 7: Selection out of the *RESID* sample due to groups voting 100% tax (in the Tax-20 Control sessions).

Period	Sess. 1	Sess. 2	Sess. 3	Sess. 4	Sess. 5	Sess. 6	Sess.7	Sess. 8	# “in”
1									8
2									8
3									8
4				X					7
5				X					7
6									8
7									8
8				X					7
9	X			X					6
10				X					7
11				X					7
12						X			7
13	X					X			6
14	X			X					6
15						X			7
16	X			X					6
17	X			X					6
18	X			X					6
19				X		X			6
20	X			X		X			5
21	X			X					6
22	X			X		X			5
23	X			X		X			5
24	X			X					6
25	X			X					6
26	X			X			X		5
27	X			X					6
28	X			X					6
29	X			X			X		5
30				X		X	X		5

There is a strong possibility that the noise introduced by different sessions “bouncing” in and out of the sample damaged the predictive power of the model. Unfortunately, there is no obvious good way to correct for this problem. Defining the residual contribution of a full-tax group as any number in the open interval (0, 100) is both arbitrary and likely just incorrect. We could drop every session that at any time voted a 100% tax, but as can be seen above, this not only would eliminate a large number of the sessions, it could introduce a selection bias as to the individuals kept in the sample. Ultimately, the No-Tax experiments provide a relatively straightforward test for Hypothesis 3, and it is supported in 1 of the 2 environments.

For the Tax-10 and the Tax-20 treatments, there are 20 periods after the intervention, and thus there is scope for non-linearity in the effect (over time) of the Passionate Providers on the residual contributions of others. We addressed this by adding a squared version of *GROUP_PP*PERIOD* to the regressions. The coefficient on this non-linear term was never statistically significant and changed none of the interpretations reported above, so we therefore do not accept Hypothesis 4 that the Non-Passionate Providers will respond in a concave effect to the Passionate Providers.

Likewise, the data do not support our Hypothesis 5. In the endogenous taxation sessions, there is no difference in the behavior of residual cooperation between Tax-10 and Tax- 20. And, in the no-taxation sessions, the result is the opposite of that predicted by Hypothesis 5; the stronger effect comes in No-Tax-20.

4.3 Individual Results --- Voting

One way to test Hypothesis 6 is to analyze the effect of lagged group-level residual contributions has on individual voting behavior. We examine the data from all 30 periods of the tax sessions and use additional dummy variables and interaction terms to capture restart effects after the stoppage. Moreover, we include variables that allow us to detect any differential effects on Passionate Providers versus non-Passionate Providers. This allows us to test Hypothesis 7, which asks whether the voting behavior of the Passionate Providers in the period after their exogenous change in preferences is different from the voting behavior of others in the group.

The regression model is given by

$$VOTE = f(CONSTANT, PERIOD, GRPX (lagged), RESIDGRPX (lagged), PP, PP*PERIOD, GROUP_PP, GROUP_PP*PERIOD, RESTART, RESTART*PP),$$

where the familiar explanatory variables are as previously defined and

VOTE is an individual's vote in period *t*;

GRPX (lagged) is the total of all group contributions made in the previous period (including the tax revenue collected);

RESIDGRPX (lagged) is the total of residual group contributions made in the previous period (the voluntary contributions made by the group members out of their after-tax endowment);

RESTART is a dummy variable for the period immediately after the exogenous preference change is implemented; and

*RESTART*PP* is the interaction between *RESTART* and *PP*.

The results are reported in Table 8, separately for the Tax-10 sessions and the Tax-20 sessions. Most notably, we find a significant negative effect of the lagged residual contributions by the group on individual votes. That is, higher voluntary (residual) contributions lead to lower votes. On the other hand, the lagged total group contributions (i.e. including the collected tax revenue and the residual gifts) have a significant positive effect on votes. To some extent, this positive effect is indicative of a “ratchet effect”, whereby higher taxes breed higher votes and in turn higher taxes. However, it also partly counteracts the negative effect of residual contributions on votes. Thus, the evidence is somewhat mixed, even if partially confirming, in relation to Hypothesis 6.⁴

On the other hand, we find no evidence that Passionate Providers respond more than non-Passionate Providers, even in the periods immediately following the exogenous

⁴ In Isaac and Norton (2013), the authors test how early cooperation in a standard VCM affects tax outcomes later in the experiment when an endogenous tax institution was available. They found that higher levels of cooperation reduced the propensity for groups to adopt inefficient taxes. There is difficulty in conducting a similar test in this experiment for two reasons. First, there is no clear demarcation of “early cooperation” and “later taxation” that the stages in Isaac and Norton (2013) provided. Second, there is no standard VCM to base “early cooperation” on. Given these limitations we believe the influence of lagged residual contributions on voting behavior represents a close approximation to the insight from Isaac and Norton (2013).

preference change. Thus, we do not accept the conjecture of Hypothesis 7 that Passionate Providers increase their votes more significantly than non-Passionate Providers.

Table 8: Coefficients and associated standard errors relating to votes.

	Tax-10 OLS	Tax-10 Tobit	Tax-20 OLS	Tax-20 Tobit
<i>CONSTANT</i>	29.57 (7.476)***	27.91 (10.879)***	27.18 (7.139)***	19.31 (11.045)*
<i>PERIOD</i>	-0.2528 (0.5433)	-0.606 (0.7194)	0.4496 (0.255)*	0.4293 (0.310)
<i>PP</i>	0.0974 (9.773)	4.285 (16.757)	-2.184 (12.458)	-9.581 (19.500)
<i>PP*PERIOD</i>	0.2439 (0.5466)	0.4969 (0.3247)	0.0468 (0.3796)	0.3053 (0.3827)
<i>GROUP_PP</i>	-0.2102 (8.136)	-4.859 (7.329)	4.378 (12.005)	14.091 (24.498)
<i>GROUP_PP*PERIOD</i>	0.2827 (0.6883)	0.7425 (0.7706)	-0.261 (0.4988)	-0.5465 (0.9705)
<i>GRPX (lagged)</i>	0.0701 (0.016)***	0.0909 (0.023)***	0.0974 (0.0198)***	0.1908 (0.0205)***
<i>RESIDGRPX (lagged)</i>	-0.068 (0.026)***	-0.1032 (0.021)***	-0.118 (0.0204)***	-0.2462 (0.029)***
<i>RESTART</i>	4.0059 (2.995)	7.493 (7.1201)	-2.365 (3.588)	-0.8375 (8.497)
<i>RESTART*PP</i>	3.769 (13.26)	3.529 (14.704)	10.576 (6.347)*	21.412 (19.818)
Wald Chi ²		58.32	1035.11	297.38
No. of Obs.	1,160	1,160	1,740	1,740

Standard errors are in parentheses. *** = $p < .01$; ** = $p < .05$; * = $p < .10$

5. Discussion

In this study, we investigated the extent to which the decay of cooperation in a public goods environment may be reversed by the introduction of Passionate Providers and/or the availability of a tax mechanism. Following the approach in Isaac and Norton (2013), Passionate Providers were created by implementing an exogenous preference change to one group member already embedded in a group with four others. This preference change resulted in a dominant strategy to contribute to the group account for the Passionate Provider. Moreover, we varied whether the exogenous change in preference occurred after 10 periods or 20 periods, in order to examine the role of “path dependence” in how these newly transformed agents influenced their other group members. Finally, in the sessions where it was made available, the median voter tax mechanism provided an additional channel through which the agents could increase public good provision.

At the aggregate level, we find some supporting evidence for the hypothesis that increased contributions by Passionate Providers only led to partial crowding out of contributions by the other group members. In addition, in the periods prior to the introduction of the Passionate Providers, the availability of a tax mechanism either increased the level of provision or flattened the per-period decay in each of the different passionate provider treatments.

At the individual level, we find mixed evidence for the principal hypothesis that Passionate Providers have a significant effect on the residual contributions of the other group members. In sessions where the passionate providers are introduced after just 10 periods (with or without the tax mechanism), we replicate the result obtained by Reuben and Riedl (2009), that Passionate Providers do not influence the other group members. However, in sessions where they are introduced near the end of the session, after 20 periods (without the tax mechanism), we replicate the pattern observed by Isaac and Norton (2013), in which the level effect on residual contributions is negative, but the decay is flattened significantly, causing a kind of rotation around the path observed in the control sessions without passionate providers. Although our results do not provide the clarification we had hoped for regarding Hypothesis 3, they clearly indicate that the influence of Passionate Providers is sensitive to other key aspects of the environment.

Furthermore, while we did not find evidence of any influence when the tax mechanism is available, the analysis is considerably hindered by the frequency with which groups voted for a 100% tax.

In terms of the voting behavior of the group members, we found some evidence that lower lagged residual (voluntary) contributions made by other group members increased the vote submitted by an individual. On the other hand, higher total group contributions (combined tax and private contributions) appear to increase the vote submitted by an individual. This latter result is consistent with the “ratchet effect” discussed in Isaac and Norton (2013) while the former is consistent with the notion that the tax serves as a commitment device to gain higher contributions at an efficiency cost. Finally, in the individual voting data we found no evidence to suggest that Passionate Providers are more sensitive than non-Passionate Providers when it comes to votes after the exogenous preference change. Instead, it seems either that the voting behavior of Passionate Providers is not responding to their newfound incentives, or, that Passionate Providers are hoping to forgo the inefficient tax and instead elicit a reciprocal response from others. Thus, while our experimental study provides some answers, many of our hypotheses remain unsettled, with more research required to clarify the importance of preference heterogeneity or the availability of a tax mechanism for sustaining cooperation.

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