Civic Engagement as a Second-Order Public Good:

The Cooperative Underpinnings of the Accountable State

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Abstract: Effective states provide public goods by taxing their citizens and imposing penalties for non-compliance. However, accountable government requires that enough citizens are civically engaged. We study the voluntary cooperative underpinnings of the accountable state by conducting a two-level public goods experiment in which civic engagement can build a sanction scheme to solve the first-order public goods dilemma. We find that civic engagement can be sustained at high levels when costs are low relative to the benefits of public good provision. This cost-to-benefit differential yields what we call a "leverage effect" because it transforms modest willingness to cooperate into the larger social dividend from the power of taxation. In addition, we find that local social interaction among subgroups of participants also boosts cooperation.

JEL codes: C92, D02, D72, H41

Keywords: civic engagement; public goods provision; punishment; experiment; cooperation

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Introduction

Voluntary cooperation alone cannot effectively provide vital public goods such as law and order, defense, clean air, and infrastructure. The funds that can be collected through voluntary donations would not nearly suffice to finance effective government because many would free ride on contributing. Instead, the financing of public goods must resort to coercion, the state's power to tax: government mandates its citizens to pay taxes and threatens to fine and punish them if they do not comply. However, state power to tax may be used not just to finance vital public goods but to finance private goods for the few and to redistribute to those in power. This poses a dilemma: state power is necessary to increase welfare but once the power is created, the danger of misuse, corruption, and oppression looms. Scholars have recognized this fundamental problem a long time ago, and two solutions to this dilemma have been implemented in modern states to keep government power in check. The first relies on the idea of distributing power, with multiple institutions monitoring each other (the "system of checks and balances" in the US constitution). The second — arguably required also to assure that governmental branches in the first scheme stay focused on their societal responsibility — relies on the idea of making government accountable to its citizens, typically through democratic elections that allow the citizens to vote leaders out of power.

This paper focuses on the need for *civic engagement* by the citizens as a social underpinning of the accountable (democratic) state. By civic engagement we mean that at least some citizens voluntarily engage in monitoring government performance and participating in political activities, for instance informing themselves of officials' actions and voting out of office those who are corrupt, incompetent, or biased towards narrow interests. Since an individual in a large polity has a negligible chance of influencing political outcomes through such costly action, the calculus of rational self-interest predicts that there would be no such voluntary civic engagement. In the absence of collective oversight by citizens, states can of course exist and can raise revenues. But while history's kings, emperors and autocrats often eschewed ruthless short-run rent extraction in order to maximize longer term gains, there is no reason to assume that states lacking citizen oversight would direct their power towards providing a socially optimal level and mix of public goods (e.g., Acemoglu et al. 2019, p. 51).

Our main point is that the fact that the (first-order) public goods problem can be solved by recourse to force does not mean that there is no role for voluntary cooperation and the free-riding problem that comes with it. We argue that the accountable use of force by the state requires civic engagement to solve a second-order public goods problem. In other words, rather than making the problem of collective action vanish, a democratic state's power of taxation necessitates the second-order public good of citizen engagement as a critical underpinning. Without civic engagement, power of the people to tax themselves is a concept devoid of reality.

In our experiment, subjects can engage in privately costly civic engagement which – if collectively provided at sufficient levels – enacts a sanction scheme that effectively deters free riding. Hence, provision of the second-order public good (PG) of civic engagement is needed to provide first-order PGs such as law and order, defense, clean air, and infrastructure. However, incentives are stacked against civic engagement since subjects are part of fairly large groups and cannot make a difference by themselves. Yet, our results show that a little civic engagement by the many can succeed to collectively provide institutions that solve the first-order public goods problem for all. Due to what we call the "leverage effect" of civic engagement, the provision of public goods is (indirectly) promoted and does not decrease over time. In the presence of small individual cost and a substantial (indirect) public benefit, civic engagement leverages the overall provision of (first-order) public goods in a sustainable (non-decreasing) way. We find that the leverage effect receives a boost when citizens can socially interact with each other in smaller communities and comment on one another's civic engagement.

Here's an illustration of the leverage effect with approximately relevant magnitudes. Very few citizens would turn over as much as a third of their annual earnings to their government on an entirely voluntarily basis, but a majority might occasionally follow the news and devote an hour or so to the civic duty of voting. A few hours of civic engagement by most citizens bolstered by deeper engagement of a few thus imposes accountability and bestows legitimacy on a government having the enforcement capacity to collect a third of GDP to finance public goods, without power having to be ceded to an unaccountable Leviathan.

The finding that the provision of public goods can be sustained over time via the leverage effect is a remarkable result. It runs counter to the conclusion emerging from literally hundreds of public good experiments where gradual decline of voluntary cooperation is a stylized fact (see Section 2). But this stylized fact emerged from the study of one-level PG games, and we are – to the best of our knowledge – the first to experimentally study civic engagement as a second-order PG.

With the territory thus uncharted, we need to make a number of novel design choices. We study a finitely repeated game in which the periods have a pre-stage, in which civic engagement can occur, and a main stage, in which the (first-order) public good is provided. In the "pre-stage" subjects solve colorful political or consumer puzzles. Solving a political puzzle models civic engagement, solving a consumer puzzle models private activity. Each successfully completed political puzzle helps to assure availability of the institution in the "main stage" and each consumer puzzle garnishes private earnings. In the period's main stage, subjects choose to allocate tokens between the public good and their (private) business activity, knowing whether a sanction mechanism for free riding has been created by civic engagement in its pre-stage.

Although token allocation in the main stage resembles conventional public goods experiments, we employ a novel payoff structure and framing. In our experiment, public sector funding directly increases the productivity of each subject's private business investment while also yielding shared benefits corresponding to clean air, public safety, and the like. Important to our conceptual argument and design is that cooperating is more costly in the first-order dilemma than in the second-order one, but civic engagement to provide the second-order PG is never payoff-maximizing to an individual, either. We vary the opportunity cost of civic engagement across treatments to test our conjecture that relative costliness of cooperation is key.

In short, our paper addresses the asymmetry in how past literature has treated centralized and decentralized sanctioning, overlooking the second-order social dilemma inherent in the citizen-state relationship, in a democracy. To be sure, when analyzing some applied public economics problems in most high-income countries, it is reasonable to take the presence of a government with effective powers of taxation as a given. In the broader scheme of things, however, it is inaccurate to imagine that whereas peer-to-peer sanctions require behavioral explanation, a democratically accountable state able to impose sanctions to support welfare-enhancing public goods provision can be assumed present in a world of materially selfish rational individuals. To do so is to exempt the state from the requirement of choice-theoretic micro-foundations that we impose on other domains of economic analysis. If strict material self-interest is incompatible with the civic engagement required by democracy, democracy may be impossible without the same kinds of motivations—norms, social identity, or reputational concerns, for example—that play large roles in the voluntary cooperation literature. With such motivations available, however, civic engagement provides the foundation of accountable government, much as social norms are said to serve as the "cement of society" (Elster, 1989).

Section 2 discusses the related literature. Section 3 describes our experimental design. Section 4 develops predictions. Section 5 presents and analyzes the results. Section 6 concludes and provides suggestions for future research.

2. Related literature

Starting in the 1980s, a large number of experimental studies using the linear voluntary contribution mechanism found that inexperienced subjects make substantial contributions to a public good, in spite of material incentives for free riding. These contributions *decline* with repetition, such that contributions to a public good in the experimental laboratory are indeed socially suboptimal (as predicted by standard theory, Samuelson 1954) unless auxiliary devices are brought to bear (Ledyard, 1995, Gächter and Herrmann, 2009). Much subsequent research has focused on mechanisms that lead to more sustained contributions, with experimental subjects found to engage in more stable cooperation when (a) they have the opportunity to

impose costly peer-to-peer sanctions (Fehr and Gächter, 2000), (b) they have a say over who their partners are (Page, Putterman and Unel, 2005) or can expel non-cooperators (Cinyabuguma, Page and Putterman, 2005), or (c) they are able to communicate and reach (non-binding) agreements (Isaac and Walker, 1988).

Although the traditional assumption that agents will never cooperate (absent infinite repetition or adequate reputational devices) is refuted by a large body of research (Sobel, 2005; Cooper and Kagel, 2016), it remains unclear how adequate voluntary cooperation can be for addressing large-scale public goods problems. Peer-to-peer sanctioning has itself been found capable of raising or helping to sustain contributions to a public good, but its costliness and partial mistargeting often mean that it delivers little or no net welfare gain unless agents interact over a sufficiently long period of time (Gächter, Renner and Sefton, 2008).

The power to tax is rightly viewed as an alternative to peer-based schemes. Experimental explorations of subjects' preference for peer-to-peer vs. centralized sanctioning institutions find that subjects opt for formal authority by cost-free voting (Markussen, Putterman and Tyran, 2014; Kamei, Putterman and Tyran, 2015), by costlessly selecting into a society with a central punisher (Nicklisch, Grechenig and Thöni, 2016; Fehr and Williams, 2017[1]), or by coordinating to meet a provision threshold (Andreoni and Gee, 2012). None of these approaches addresses the pure social dilemma aspect of civic engagement.

The centralized solution to the problem of public goods provision, namely the mandating of tax payments, presupposes use of an incentive mechanism in the form of penalties for nonpayment. These penalties are assumed to be deterrent, i.e., sufficiently certain and high in expectation so that the mandated contribution becomes privately payoff maximizing. There is a substantial experimental literature studying tax compliance (Andreoni *et al*. 1988; Torgler, 2002; Cummings *et al*. 2009).[2]

The bridging of the until-then largely unconnected literature on taxation under formal enforcement with studies on voluntary contributions began as part of a literature on choice of institutions and public goods provision. Institutional choice involving peer punishment was studied in several papers including Gürerk, Irlenbusch and Rockenbach (2006), Sutter, Haigner and Kocher (2010), and Ertan, Page and Putterman (2009). Opting into a formal punishment institution was studied by Kosfeld, Okada and Riedl (2009) and Andreoni and Gee (2012), who

[1] In Fehr and Williams (2017), subjects who select the institution with central punisher are able to impose some accountability on that agent because he or she is selected by majority vote each period. Their design abstracts from the second-order dilemma on which we focus, however, because like the choice of institution, selection of the central agent entails no monetary opportunity cost.

[2] For the roles of social norms, of beliefs regarding others' compliance, and of effects of government's democratic legitimacy on tax payment see Alm, 2019 and Kirchler *et al*. 2014.

let subjects select, via a provision-point type payment mechanism, to implement a scheme whereby the lowest contributor is punished.[3] Putterman, Tyran and Kamei (2011) investigated voting on the rules of punishment to be implemented by the formal authority. In Markussen, Putterman and Tyran (2014) and Kamei, Putterman and Tyran (2015) subjects choose between formal and peer-to-peer sanctions by voting in a ballot,[4] in Nicklisch *et al.* (2016) by "voting with their feet." These studies abstract from the present paper's concern that formal sanctioning schemes satisfying properties of democratic control and accountability presuppose some prior costly cooperation as an underpinning. By abstracting from the cooperative dilemma of creating accountable state power, they might inadvertently support the view that formal authority is a complete workaround of societies' large-scale social dilemma problem.

The literature on voluntary collective action has considered a number of channels and devices that might foster cooperation in the absence of deterrent penalties. We propose that the leverage effect of civic engagement via its magnification into accountable state enforcement power may be viewed as a distinctive addition to that set of devices. We vary the magnitude of the mechanism's leveraging power by studying different opportunity costs of civic engagement.

We also vary a second dimension: presence or not of possibilities to mobilize social feedbacks and image concerns in a "social circle" (see Masclet, Noussair, Tucker and Villeval 2003 who find that social feedback can increase cooperation). Our social circles are local subsets of the larger society that confronts the social dilemma of civic engagement, a feature that better represents polities wherein the society as a whole is far too large to support person-to-person feedback beyond the immediate group of friends, relatives and co-workers.

That social feedback and image concerns might play a role in explaining aspects of civic engagement such as voting or staying informed on public affairs, is a familiar idea, although one sometimes neglected by traditional economic analysis.[5] In social settings, including workplaces, churches, and family gatherings, political issues may be frequent topics of conversation, which means that complete inattention to candidates' names and positions could be embarrassing

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[3] Another strand of literature, outside of economics, considers creation of an automated punishment mechanism ("pooled punishment") through initially voluntary contributions; see Yamagishi (1986), Sigmund *et al*. (2010), Traulsen *et al*. (2012) and Zhang *et al*. (2014).

[4] Positive impacts of a democratic decision process on cooperation itself are suggested by experiments including Tyran and Feld (2006), Dal Bó, Foster and Putterman (2010), Sutter *et al*. (2010), Markussen *et al*. (2014), Sausgruber *et al*. (2019), and Kamei (2016, 2019).

[5] An advantage of the experimental approach is that we can vary the cost of civic engagement and the social image concerns separately while they may interact in the field. For example, Funk (2010) shows that the introduction of postal voting in Switzerland not only reduced the direct cost of participation but it also reduced social pressure to be seen at the ballot box. Funk shows that the latter effect tends to dominate in villages and small towns, leading to a decline in participation in Swiss villages.

and that attention to news coverage may be motivated in part by social image concern.[6] While there is a theoretical and experimental literature on why people incur a cost to go to the polls (e.g., Duffy and Tavits, 2008) or to acquire information about the choice put before them (e.g., Mechtenberg and Tyran 2019) despite a vanishingly small likelihood of casting a pivotal vote, we know of no laboratory experiments that study potential social image concerns as a factor in civic engagement. Studies attempting to demonstrate a correlation between voting and other prosocial actions are referenced by François and Gergaud (2019).

In a clever study, Della Vigna, List, Malmendier and Rao (2017) impute the value survey respondents in a south Chicago neighborhood attached to being able to tell others that they had voted in the 2010 U.S. congressional election. The authors hypothesized that respondents would suffer disutility from either lying about having voted or truthfully admitting that they had not. This implies that the more individuals in their social circle might ask them whether they voted, the more likely they are to have gone to the polls—a prediction their data confirm.[7]

Our experimental design differs from existing public goods experiments with respect to the relatively realistic manner in which we embed the large-scale public good in a two sector economy. The large majority of experiments in this field have used a design in which the return from contributing to the public good is constant, hence the social optimum entails devoting all resources to it. Although some experiments have used functional forms yielding interior equilibria (e.g., Laury and Holt, 2008; Cason and Gangadharan, 2015), we know of none that include our experiment's explicit mixed economy framing (see section 3).

3. Experimental Design

The first-order public goods (PG) problem in our experiment is to provide a PG that has two types of benefits: a direct benefit to all citizens (as usual in PG games) and an indirect benefit by

[6] Mechtenberg and Tyran (2019) note: An "idiot" in Athenian democracy was someone who was characterized by self-centeredness and concerned almost exclusively with private — as opposed to public — affairs. Declining to take part in public life, such as democratic government of the polis (city state), was considered dishonorable. "Idiots" were seen as having bad judgment in public and political matters. Mechtenberg and Tyran demonstrate experimentally that voter motivation to be informed about a common-interest problem is substantially higher than what standard theory of self-interested agents would predict. Dal Bó, Finan, Folke, Persson and Rickne (2017) show that Swedish voters weigh both competence and representativeness of background in choosing among municipal politicians and national legislators, implying *inter alia* a level of attentiveness inconsistent with the presumption of free riding.

[7] On the subjective cost of lying, see Abeler, Nosenzo and Raymond (2019). Gerber *et al*. (2012) report that 70% of U.S. respondents indicated sharing their voting choice with family and friends. Bond *et al*. (2012) use Facebook to post advertisements to "get out the vote!" along with a clickable "I voted" button. The treatment group, which in addition sees which of their friends had voted, has many more "I voted" clicks. Similarly, Gerber and Rogers (2009) show that messages emphasizing high expected turnout are more effective at motivating voters to turn out than messages emphasizing low turnout.

increasing the productivity of the private sector. The indirect effect captures the idea that the productivity of the private sector is aided by contract enforcement, an educated workforce, etc. The direct effect can be thought to result from enjoyment of public goods such as enforcement of safety standards and maintenance of public order.

A social dilemma exists in the provision of the first-order PG in that all relevant resources are initially controlled by individuals, and each obtains higher returns the more she allocates to her private activities and the less to the public sector, taking the total allocations to that sector by others as given. During most of the experiment,[8] subjects have the opportunity to resolve this dilemma by putting in place a sanction scheme that should deter free riding by penalizing non-contribution enough to make contributing privately optimal. The scheme can be put in place, however, only if there is enough participation in a costly civic engagement activity which is itself a – second-order – social dilemma. As with the first-order dilemma, it is always the case with the second-order dilemma that the less civic activity an individual performs, the more he or she earns, taking others' civic engagement as given.

Figure 1: *Treatments*

		Local social interaction	
		No	Yes
Opportunity cost of civic tasks	Low	*LowNo*	*LowYes*
	High	*HighNo*	*HighYes*

Notes: Opportunity cost, Low: completing a civic task forgoes earning 10 points from a private task, High: same forgoes earning 22 points from a private task. In treatments with local social interaction, subjects can share information about current civic task engagement and provide one another with evaluative feedback in social circles of 6 subjects. 5 sessions per treatment, 24 subjects per session. Total subjects: 480.

Figure 1 shows the treatment variations. We explore the role of relative cost of civic engagement by implementing two levels of opportunity cost of civic engagement. This cost is about four times vs. twice as high as needed to render civic engagement selfishly irrational (costs of 22 vs. 10). To explore the role of social interaction, we have treatments with and without local social interaction in so-called social circles. In these circles information about civic engagement is shared and members freely exchange evaluative feedback. The two dimensions of variation intersect in a two-by-two design.

[8] We can defer until later in this section description of the exception, some initial periods for familiarization with the first-order dilemma jointly called Part 1.

We implemented the maximum group sizes possible in the laboratory we use of 24 subjects, perhaps helping to make first-order public goods provision unsustainable by voluntary cooperation,[9] and to foster a sense of "society" rather than small group. We made social circle size 6, large enough to permit fairly rich internal dynamics (and similar to the number of acquaintances who might ask, for instance, whether one went to vote — cf. Della Vigna *et al*. 2017), but small enough to be sub-groups of the full subject population, and randomly and anonymously sprinkled around the lab to eliminate potential contamination by out-of-session payoff considerations.

The first-order public goods problem reflects realistic if stylized features of a large economy that are referenced as such in the subjects' instructions, e.g.: *"The main decision to be made and the main way in which you can earn points involves the allocation of resources between a private income-generating activity and a public sector. Allocating to your private activity is always beneficial to you, but private activity earns you more when the public sector is well funded. Not only does having a well-funded public sector raise the profitability of your private activity; a well-funded public sector also brings you direct benefits … similar to the benefits in everyday life from having safe roads, law and order, and clean air."*

Formally, in every period each individual *i* divides an endowment of 20 tokens between a private activity or business b_i, and a public activity p_i. The aggregate public sector allocation of all 24 participants is $P = \sum_{all\ j} p_j$ (*j* includes *i*). The individual's earnings points are then given by

$$Y_i(p_i|p_{-i}) = b_i \cdot V(P) + D(P) \qquad (1)$$

where $p_{-i} = \sum_{j \neq i} p_j$, $V(P)$ is the productivity of allocations to the private sector, and $D(P)$ is the per person direct benefit from public goods. Further specifying,

$V(P) = \alpha + \beta P$ for $P \leq P^*$, specifically $V(P) = 5 + (1/16)P$ for $P \leq 192$

$= \alpha + \beta P^*$ for $P > P^*$, specifically $V(P) = 5 + (1/16)192 = 17$ for $P > 192$, (2)

$P^* = 192$ being the socially optimal allocation to the public sector, and

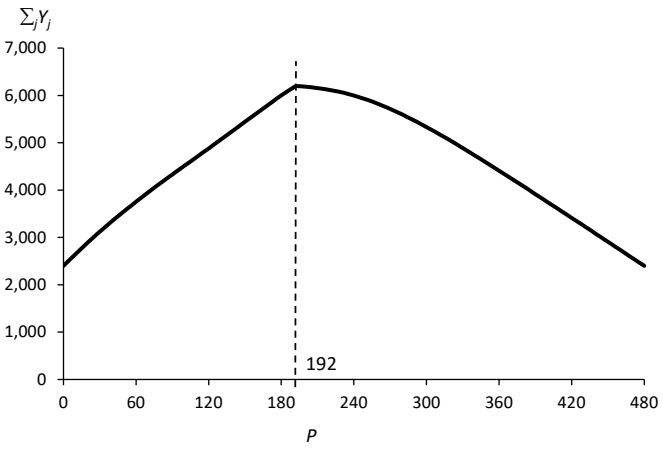
$$D(P) = \frac{101}{1-(1-101)\mathrm{Exp}[-0.025*P]} - 1. \qquad (3)$$

The piece-wise linear function *V* and the logistic function *D* were presented to subjects graphically and in a table rather than equation form, to ease comprehension (see Figures 1 and

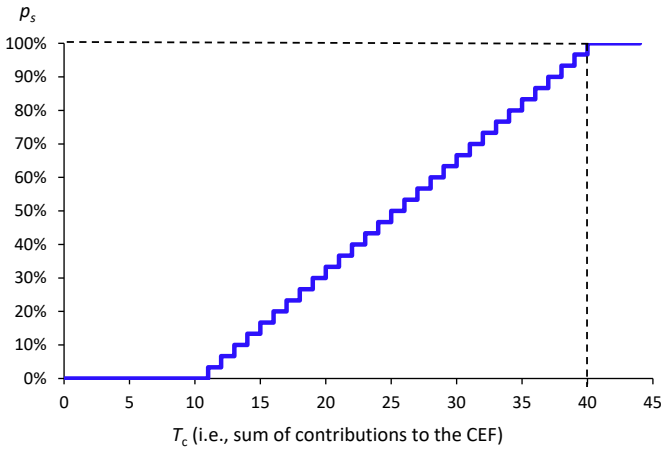
[9] The experimental literature fails to find a clear group-size effect on public goods contributions (see *inter alia* the discussion by Zhang and Zhu, 2011), so the large private payoff advantage of free riding, as described below, remains most central.

2 and Tables 1 and 2 of the Instructions in the online Appendix). *D* is modelled as a logistic function because it is plausible that some public goods like national defense, research, and major infrastructure projects yield little benefit until a substantial level of provision is achieved. As a simple way to capture the indirect effect of publicly funded infrastructure, contract enforcement etc. on the private sector, function V makes growing the public sector beneficial to the return of one's private activity up to *P** (i.e. 192 tokens). Contributions to the public sector beyond the optimum have no effect on private sector productivity.

Figure 2: Payoffs and penalties



(a) The sum of all 24 subjects' payoff ($\sum_i Y_i(P)$)



(b) The probability of having the penalty scheme ($p_s(T_c)$)

Avg. contribution of 23 others	Own allocation to public sector (p_i)					
	0	4	8	12	16	20
0	100	84	66	46	24	1
4	223	185	145	103	59	13
8	379	320	258	193	127	62
12	431	364	296	229	162	94
16	439	371	303	235	167	99
20	440	372	304	236	168	100

(c) Payoff schedule without penalty scheme ($Y_i(p_i, p_{-i})$)

Avg. contribution of 23 others	Own allocation to public sector (p_i)					
	0	4	8	12	16	20
0	~~100~~ ~~-44~~	~~84~~ 12	66	46	24	1
4	~~223~~ 79	~~185~~ 105	145	103	59	13
8	~~379~~ 235	~~320~~ 248	258	193	127	62
12	~~431~~ 287	~~364~~ 292	296	229	162	94
16	~~439~~ 295	~~371~~ 299	303	235	167	99
20	~~440~~ 296	~~372~~ 300	304	236	168	100

(d) Payoff schedule with penalty scheme

Figure 2(a) shows the aggregate earnings in a period taking the *V* and *D* functions together (i.e. summing (1) over all 24 participants). The social optimum is achieved when all subjects select $p_i = 8$, where $P = P^* = 192$. Figures 2(c) and 2(d) were included in subjects' instructions to convey how p_i and p_{-i} jointly determine *i*'s payoff, with 2(c) showing payoffs absent the penalty scheme, 2(d) those with the penalty scheme present. Figure 2(d) shows that subjects pay a fine of 18 points for each token by which their allocation to the public sector falls

short of the socially optimal 8, a penalty sufficient to render contributing the 8 tokens privately payoff-maximizing. Subjects are clearly better off with the penalty scheme (earnings are 258 at the social optimum) than without the scheme at the sub-game perfect Nash equilibrium level (zero contributions and earnings of 100 points). Absent the scheme, there is a strong private incentive to free ride, i.e., to allocate all one's endowment units to the private sector, regardless of others' choices. For example, when others put 8 tokens each into the public sector, subject *i* gains 121 points (≈ $1.08, see below) in a given period, raising her income for the period by 47% by switching from 8 to 0 tokens (see Online Appendix, Part III for details).

With respect to each period's main allocation decision, the instructions state "*you can establish a government apparatus to enforce adequate funding of the public sector, paralleling the existence of taxes [and penalties] in real life*." We detail that whether the first-order PG decisions are taken with or without the penalty scheme in place is determined in a "pre-stage" of each period. We represented contributions to the second-order public good in this pre-stage in a manner suggestive of real citizen engagement: subjects could either perform "private tasks" which net them additional private earnings, or "civic tasks" which add points to a civic engagement fund *(CEF)*. A larger *CEF* raises the likelihood that formal sanctioning is in place. As with the first-order allocation problem, our instructions framed civic engagement by explicit reference to its real-world analogues, for example "*it takes some civic engagement to establish a well-functioning government responsive to citizens' interests. Examples of civic engagement in the real world include reading or listening to information about public affairs, signing petitions, voting in elections, etc.*"

Figure 2(b) shows how the probability of having the sanction scheme in place in a period increases with civic engagement. If enough (40) civic tasks are performed by the 24 participants, the penalties are present with certainty; if too few (less than 11) are performed, there is no chance of penalties. Between these two levels, the likelihood of a scheme rises in thirty equal steps with the total number of civic tasks completed, T_c.

$$\begin{cases} p_s = 0, & T_c \leq 10; \\ p_s = (T_c - 10)/30, & 11 \leq T_c \leq 39; \\ p_s = 1, & T_c \geq 40. \end{cases} \qquad (4)$$

Were the switch from no scheme to scheme to occur at a single critical level of the *CEF* (= T_c), civic engagement would be a coordination rather than a social-dilemma problem (Marx and Matthews, 2000). But because scheme presence approaches certainty in small probabilistic steps, completing another private task is always the more profitable use of a subject's time.

Note that if $11 \leq T_c \leq 39$, the uncertainty about the scheme is resolved *before* the period's first-order allocation decisions are made.

Subjects earn either 10 (in *Low*) or 22 points (in *High* opportunity cost treatments) per private task completed, and there is sufficient time to complete four or more tasks in a period's pre-stage. Allocating the available time between task types is a social dilemma, since if all participants sacrificed completion of two private tasks to do two civic ones and if the scheme were accordingly assured, they would each be sacrificing 20 (or 44) points of earnings in the pre-stage while raising theoretically expected earnings in the main stage by 158 points. But each individual completing a civic task raises own expected earnings on the margin by less than six points[10], so foregoing completion of a private task to do one is never payoff maximizing.

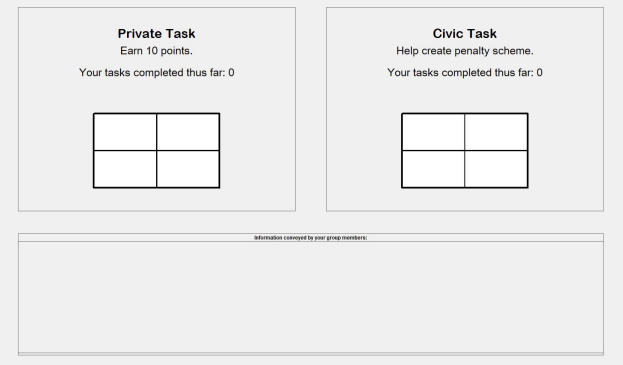
Figure 3(a) shows the task selection screen at which the pre-stage begins. It provides information on how many private and civic tasks a subject has completed thus far in the present period. In treatments with social interaction, subjects also see information about fellow social circle members' civic task completion thus far in the period if they completed those tasks by clicking the "submit and inform" rather than "submit" button. Doing so causes messages such as "subject B has completed his or her second civic task" to be displayed in the lower portion of the others' screens.

Figures 3(b) and 3(c) illustrate that each pre-stage task starts with a description of a consumer (private task) or politician (civic task). Task completion entails moving an icon into the correct quadrant of a two-dimensional grid. For example, a politician will be described as taking positions on two issues, say defense spending and environmental policy, each represented by one axis of the grid. A pop-up tells the subject whether a submitted answer was correct, offering the options of redoing it or of selecting a new task, if not.

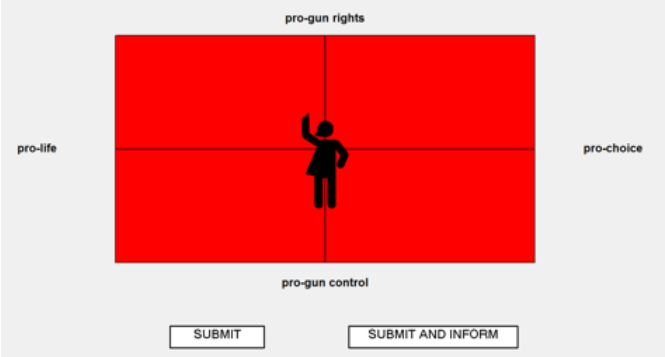
Figure 3(d) shows how we presented the probability of having the sanction scheme in place (the relative size of the blue segment) and how uncertainty was resolved (a spinning pointer to give salience to the probabilistic determination of penalty scheme presence).

[10] A subject who attaches high probability to T_c being at least 10 but less than 40 without her next civic task stands to gain at most $158/30 \approx 5.27$ points, by equation (4) and the 158 point main allocation stage earnings difference.

Figure 3: *Screen shots for provision of civic engagement ("pre-stage")*

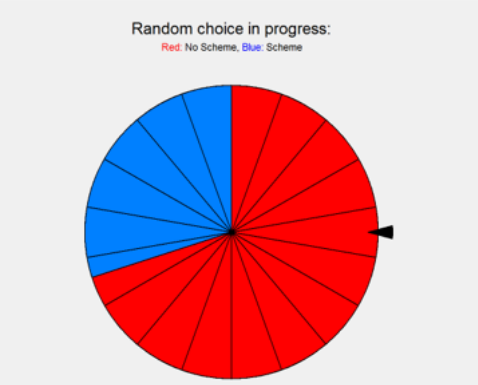


(a) Home base during the pre-stage



(b) Example of civic task grid screen

(c) Example of private task grid screen



(d) Example of spinning wheel (case of $p_s \approx 30\%$)

Notes: (a) pre-stage screen with choice of task type. Lower part of the screen conveys information by other group members about civic engagement (only *Yes* treatments). Panel (b) shows civic task example, with description on previous screen: "Senate candidate Wendy White favors unrestricted gun ownership and is committed to a woman's right to choose whether to continue or to terminate a pregnancy". Subjects are asked to move the icon to the correct quadrant (here: top right). Panel (c) shows private task example. The prior screen describes a consumer's preferences between home vs. restaurant eating and between gourmet vs. plain dishes. Panel (d) is seen after the 40 seconds for tasks ends (and, in *Low* treatments, after submitting evaluations of social circle members) if $11 \leq T_c \leq 39$. Outcome (scheme or no scheme) is displayed prior to period's main-stage allocation decision.

The experiment begins with 3 periods in which no sanction scheme is available, Part 1. This gives subjects an appreciation of the first-order dilemma and allows a break between sets of instructions, refreshing attention. We have 15 periods in Part 2, each having a pre-stage in which a scheme can be generated with enough civic engagement, because we want to study whether cooperation in providing the second-order public good, if any, can be sustained over time versus suffering gradual decay as is usually observed in finitely repeated voluntary contributions experiments.

In social interaction treatments, social circle membership and subject IDs remain fixed for all fifteen periods of Part 2. In those treatments, the number of civic tasks completed by

each social circle member in that period is shown at the end of each pre-stage, regardless of whether the "submit and inform" option has been used, and subjects are then asked to choose for each member an evaluation (from 1 = strongly disapprove to 5 = strongly approve). Each learns their average evaluation by others, and the average evaluation other members received, before going to the period's main stage token allocation decision.

Points earned are converted to U.S. dollars at the rate 195 points = $1 and paid out after all decisions are completed and the subjects answer a short survey on demographic items as well as on levels of political and social engagement.

4. Predictions

Contributions to first-order public good. Individuals whose goal is maximization of own money payoff would allocate no tokens to the public sector in one-shot play of our experiment's main stage without sanction scheme. In finitely repeated play, it is straightforward to predict zero allocations to the public sector again, if we assume common knowledge that all are own payoff maximizers. If common knowledge is dropped, there is the possibility that a selfish, rational individual would contribute to the sector if she believed that others might be of a different type, say reciprocators, and that they would contribute more in future periods in response to her own contribution, perhaps offsetting the cost of contributing now (e.g., Kreps *et al*. 1982). But heterogeneity of subject types and beliefs could combine with aversion to disadvantageous inequality (or to being "suckered" by less cooperative individuals) and with anticipation of late period defections to cause cooperation to decline over time. Past experiments on voluntary contributions to a public good, discussed in Section 2, consistently find an initial attempt to cooperate followed by a decline in contributions.

Hypothesis 1. *(a) Subjects' allocation decisions to the first-order public good absent a sanction scheme will follow the same pattern as in standard voluntary contribution experiments: substantial initial average contributions will be followed by decline to highly suboptimal levels. (b) When a deterrent sanction scheme is in place, subjects will respond rationally. That is, allocations and earnings will be higher with than without sanctions, in Part 2.*

Leverage effect. Predictions for each period's pre-stage (to provide the second-order PG) are qualitatively the same as those in its main stage (to provide the first-order PG) according to traditional economic theory assuming common knowledge of rational self-interest: zero cooperation. As with the main stage, some may nevertheless attempt cooperation at this stage too, hoping that others might follow. The forces leading towards cooperation's decline over time are in principle as much present in the pre-stages as in the main stages of our experiment's periods. We speculate, however, that these forces will not uniformly prevail. First,

the per-unit cost of cooperating is lower in the pre-stage: in treatments in which private tasks yield 10 points, less than 5 points of earnings per civic task completed (after factoring in the effect on p_s assuming $10 \leq T_c < 40$), versus over 15 points per token allocated to the public good by our earlier calculation. Second, rather than thinking marginally, some subjects may compare average cost and benefit: if all forego 20 points of pre-stage earnings to complete 2 civic tasks and gain 158 points in the main stage, each obtains a 690% profit ((158-20)/20 = 6.9), evidence of the leverage factor noted earlier. Although predictions in standard theory are the same for any positive cost of cooperating, laboratory public goods experiments have shown clearly that the amount of free riding is sensitive to its net cost, with free riding declining as the marginal per capita return approaches the typical unit return in one's private account (e.g. Ledyard, 1995). Third, checks on usurping government power for private benefit sometimes succeed in the real world, even though individuals should always free ride on voting, on signing petitions, on participating in demonstrations, etc., unless they are sources of direct utility. Such direct utility might in fact exist, for some (see literature on expressive voting, e.g. Tyran and Wagner 2018, Pickup, Kimbrough and de Rooj, 2019). Civic engagement actions may persist in practice because people value an accountable government, view their private cost of helping to achieve or sustain it as being relatively small, and enjoy self- and social-esteem benefits from doing their parts (e.g., Bénabou and Tirole, 2006). Our experiment explicitly frames second-order cooperation as civic engagement, which might potentially tap into such values.

It is not required that *everyone* incur the cost of cooperating, only that enough individuals do so to make scheme presence more likely. Consider, then, the cost-benefit calculus faced by a subject in our experiment. While our design carefully avoids having a switch point that changes the pre-stage social dilemma into a coordination problem, subjects can see a salient difference between the two regimes, when each pre-stage ends. In one regime, a subset of the 24 subjects have performed enough civic tasks to obtain the penalty scheme, and subjects can thus look forward to earnings of around 258 points each in the period's main stage. In the other regime, the subjects fail to perform enough of the tasks, and main-stage earnings can be predicted to be not far above 100 points. Although pre-stage cooperators may guess (and after some periods, also see) that not everyone shares their determination, and while such cooperators can reason that they would earn more points by being among the free riders, they may nonetheless resolve to continue cooperating, again comparing own cost and benefit *on average* rather than the margin. The concept of a *minimal profitable coalition* (Isaac, Walker and Williams, 1990) may be useful here: it identifies conditions under which a subset of subjects who cooperate can make themselves better off on average than if none had cooperated, although free riders outside that subset earn more still.[11] In addition, some

[11] If we assume all subjects complete 4 tasks (the observed mode) in each pre-stage, then an example of a profitable coalition would be 13 of 24 subjects each doing 3 civic tasks per period, raising the likelihood of the

cooperators may derive enough "warm glow" or utility from confirmation of identity (as "one of those who do the right thing") to resist the pull of free riding.

The behavioral, non-marginal, comparison between cooperation's cost and benefit to a 'cooperation coalition' member, illustrated by the 20 point versus 158 point comparison above, leads to:

Hypothesis 2. *(2a) The pattern of civic task completion will differ from that of main stage allocations if the private cost is perceived as modest enough relative to potential (shared) gains (Leverage effect); (2b) more civic tasks will be completed in the treatments with lower opportunity cost (10 vs. 22); and (2c) own civic task completion will vary positively with others' task completion.*

The possibility of heterogeneity of subject dispositions to cooperate in the pre-stage, mentioned in our discussion above, will also be tested empirically by using Part 1 allocation decisions and, later, exit survey indicators. We defer details until Section 5 for the sake of brevity.

Expected effect of social interaction. The idea of minimal profitable coalition sketched above does not require that there be verbal communication among the coalition members. Coalition membership can be implicit. However, increased social interaction among subjects might make cooperation more durable and may attract some less cooperative individuals toward mimicking cooperation to encourage others to do the same (Page *et al*. 2005). When the individual level of cooperation is made known to a small enough number of others to allow them to keep track, some low contributors may experience discomfort at benefiting without doing their part (peer pressure, in the phrase of Kandel and Lazear, 1992) and thus decide to engage in some civic tasks, although high cooperators noticing others free riding may conversely reduce their cooperation (e.g., Fischbacher and Gächter, 2010). Subjects could potentially respond differently to civic task completion by others in the social circle. Availability of explicit evaluative feedback might tip the net effect in favor of more cooperation, because cooperators can be expected to criticize non-cooperators, criticism that some will wish to avoid despite its non-material character (Dugar, 2013). Praise for cooperators may also slow their tendency to reduce cooperation in the face of others' free riding, but its effects are often less

formal scheme to almost 97%, thus assuring themselves earnings of 258 per period in the main stage, while earning only 10 in the pre-stage (in the treatment where private tasks yield 10 points each) whereas the 11 pre-stage free riders earn 40 in that part of the period and also earn 258 in the main stage. In this example, free riders earn 298 per period, cooperators 268, whereas both would earn only 140 per period were there no pre-stage cooperators.

strong. The relatively small social distance per se may also positively affect their cooperation behaviors. These factors lead to:

Hypothesis 3. *(a) Information sharing and evaluative feedback in social circles are associated with greater civic engagement, with (b) responsiveness to feedback content most likely when low contributors are subjected to others' criticism.*

We investigate Hypothesis 3 by looking for treatment differences due to presence of local social interaction, and, in treatments with such interaction, analyzing the effects of social circle members' civic task completions on own task performance and the impact on tasks done of feedback received relative to feedback given to others, conditioned by own performance.

5. Results

Five sessions per treatment were conducted in the Brown University Social Science Experimental Laboratory with a total of 480 subjects. In each session, 24 undergraduate students drawn from diverse majors participated, the vast majority having no previous experience of similar experiments.[12] The main results summarized (with details in the succeeding subsections) are:

First, when the scheme is unavailable, the provision of the first-order public good is highly inefficient. Contributions to the public sector rapidly decline to about 30% of the social optimum in Part 1, and fall to about 13% of the optimum in Part 2 periods that lack penalties.

Second, the first-order public goods problem is effectively solved when the sanction scheme is in place. Enough civic engagement is present in almost all (92%) periods to yield a positive probability of solving the first-order public goods problem, and the scheme is implemented in an average of 42% of cases across all treatments.

Third and perhaps most importantly, while we find the usual downward trend in allocation to the public sector in the main stage (first-order public goods problem) absent a penalty scheme, there is *no such trend* in number of civic tasks performed and thus in probability of obtaining the penalty scheme (second-order public good problem). Remarkably, subjects seem to be able to uphold civic engagement, consistent with the hypothesized "leverage effect".

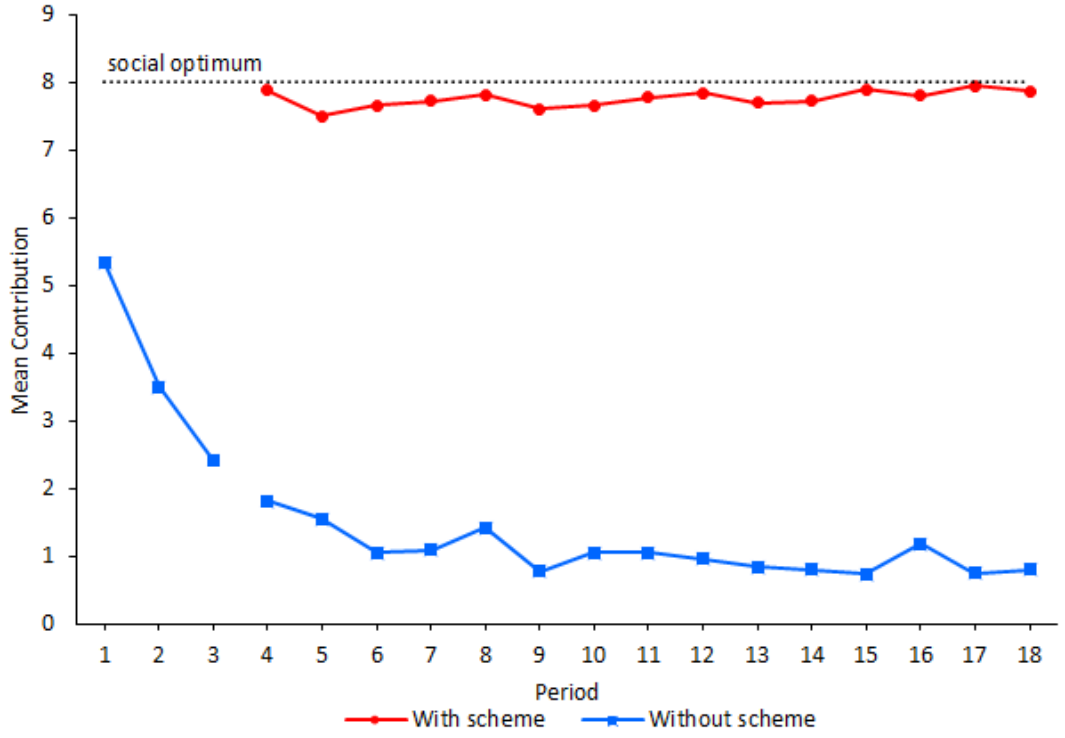
[12] The share of females among our subjects (55.6%) is not different from the share in the overall student population at Brown (54.5%, two-sided Fisher's exact test, $p = .63$). Each session took roughly 105 minutes from start of consent procedure to payment and departure from lab, with subjects earning an average of $27.11 including $16.39 from the 18 period main stages, $3.22 from private tasks completed in 15 period pre-stages, and the $7.50 show up payment.

Fourth, more civic tasks are performed when doing so is cheap and when subjects can engage in social interaction. Participants complete an average of roughly 29 civic tasks per period and thus achieve a 63% chance of obtaining the scheme in sessions of *LowYes* treatment, whereas they complete only about 17 civic tasks in *HighNo*, with a corresponding chance of 23%.

a. Main stage allocations, contributions to first-order public good

Figure 4 shows that the free-rider problem in providing the first-order public good was severe, and that the problem was almost perfectly solved when the sanction was present. More specifically, the figure shows the average allocation to the public sector, pooling our four treatments but distinguishing between periods played without penalty scheme and those in which the scheme was in place. The scheme was not available by design in Part 1 (periods 1 – 3) in all sessions.

Figure 4: *Mean contribution to first-order PG with and without penalty scheme*



Notes: Theoretical equilibrium entails contributions of 0 absent the scheme and 8 tokens with the scheme. Sanctions were not available in periods 1-3 (Part 1) and occurred in 42% of the cases when they were potentially available in periods 4 - 18 (Part 2). Contributions conditional on scheme presence or absence did not significantly differ by treatment (see Appendix Figure A.2).

The blue (lower) line for periods 4 – 18 shows average contribution for cases in which 10 or fewer civic tasks were completed (21 cases) and when a random draw (required because 11

≤ T_c ≤ 39) yielded no scheme (153/275 cases).[13] These observations are broadly in line with past public goods experiments. The average initial contribution of 5.3 is low compared to the endowment (20 tokens), but fairly high (67%) compared to the optimal allocation of 8. The rate of decline of contributions is comparatively rapid and, from period 5 onwards, average p_i stabilizes at around one token. There are no differences in Part 1 behaviors by treatment, which is as expected given that subjects were randomly assigned to treatments and that all instructions and parameters were identical in Part 1.[14]

The red (upper) line of Figure 4 shows average contributions to the first-order PG when the scheme is present (which happened in 122/275 cases as a result of a random draw plus 4 cases having 40 or more civic tasks completed). When the scheme is present, on average 85% of subjects behave optimally and contribute exactly 8 tokens.[15] High contributions during periods with the scheme do not spill over into periods without the scheme (see Table A.2 and Discussion). We summarize the discussion in

Result 1. *Absent penalties, contributions to the first-order public goods dilemma decline to inefficient levels close to the theoretical equilibrium. When a penalty scheme is present due to sufficient civic engagement, contributions are close to the social optimum. Both parts of H.1 are accordingly supported.*

b. Civic task completions, contributions to second-order public good

Figure 5 shows the number of civic tasks successfully completed (hereafter: completed) per subject in each Part 2 period, by treatment. Treatments *LowNo* and *LowYes* have high and sustained values of T_c, making the average likelihood of a penalty scheme 53.2% (T_c ≈ 26) and 62.6% (T_c ≈ 29), respectively. Consistent with our expectations associated with opportunity cost (H.2b), the numbers of tasks completed are smaller and likelihood of scheme accordingly lower in *HighYes* (T_c ≈ 20.5, p_s ≈ 34.8%,) and *HighNo* (T_c ≈ 17, p_s ≈ 23.2%). In *LowNo* and *LowYes*, about a quarter of subjects display average t_c levels ≥ 1⅔ per period, the level that would guarantee the scheme had others followed suit. Although the large majority of subjects complete some civic tasks in all treatments (as discussed further, below), the total number of civic tasks completed in a given period and session was between 11 and 39 in 275 of the 300 cases.

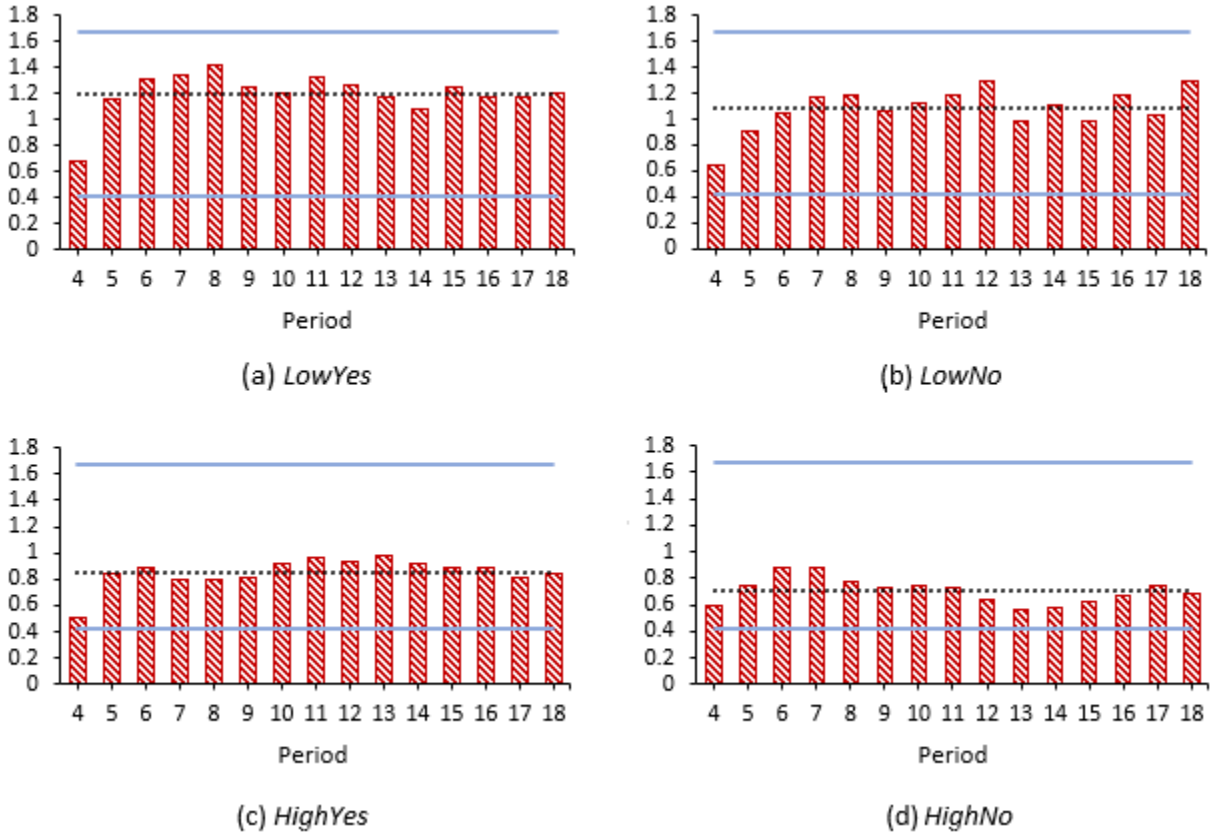
[13] We confirm that our experimental software implemented the probabilities effectively: a linear regression of scheme outcome on probability of scheme based on eq. (4) yields a coefficient of 0.976 (p < 0.001), with the constant term having a coefficient point estimate of -0.007 (p = 0.890).

[14] Regression analysis shows no differences in the rate of decline of contributions across treatments in Part 1. We also find no other significant differences across treatments in those periods (see Appendix Table A.1).

[15] There is some evidence of learning (see Figure A.1). A linear regression reveals that a typical session would converge to 100% contributing 8 under the scheme by its ninth period, but almost no session actually achieved scheme presence in nine of its 15 Part 2 periods.

Figure 5 shows that the number of civic tasks done increases during periods 4 to 6 or 7. This increase is most likely due to subjects requiring more than three periods to fully experience the decay of main stage contributions when there is no scheme. In other words, willingness to forego the private task alternative so as to achieve a sanction scheme probably rises during the initial Part 2 periods because subjects are observing a rising main stage earnings gap between with- and without-scheme states as scheme-free public sector allocations decline (Figure 4).

Figure 5: *Average civic by treatment and period*



Note: Panels show average per subject number of civic tasks (contribution to second-order public good) successfully completed during each period's pre-stage in Part 2, by period and treatment. The upper horizontal line indicates the level of civic tasks that guarantees the presence, the lower line the level that guarantees absence, of the scheme. The dashed line, between these, is the actual average civic tasks per subject in the treatment.

Figure 5 also shows no overall decline during Part 2 as a whole, nor for the last ten or five periods. This lack of a clear declining pattern in overall civic task completion is remarkable and highly unusual for the experimental public goods literature. Put differently, the familiar decline in cooperation evident for the main stage in Figure 4 and in dozens of replications of

finitely repeated linear public goods experiments is absent for our second-order public good of civic task completion, supporting H.2a.[16]

We confirm some slight overall upward trends and the absence of a downward trend in civic tasks by estimating regressions (shown in Appendix Table A.3 to conserve space). Table A.3(a) reports regressions of average per-subject civic task completion by session and period using a session-level random-effects model. Estimate (1), without trend control, shows that significantly more civic tasks are performed in *LowYes* and *LowNo* than in the omitted *HighNo*. Estimate (2) adds a *Period* variable which obtains a weakly significant small positive coefficient, leaving the coefficients on the treatment dummies remarkably unchanged. We attempt to add interaction terms between *Period* and each treatment dummy, but the variance inflation factor implies a multicollinearity problem. The point estimates, though accordingly unreliable, are suggestive of a roughly 1 to 2% average increase in completed civic tasks per period in the *LowNo*, *LowYes* and *HighYes* treatments, once the insignificant base trend estimate is subtracted from these coefficients. What is clear, at a minimum, is that downward sloping trends can be ruled out for the fifteen periods as a whole.

To be sure that the result is not due solely to the initial upticks visible in the leftmost bars of Figure 5's panels, we estimated regressions using the separate observations of each treatment and including later subsets of periods only (Table A.3, panel (b)). When we focus on the final ten periods (periods 9 – 18) or on any shorter final set of three or more periods (periods 10 – 18, 11 – 18, …, 16 – 18), no treatment shows a statistically significant time trend.[17] This absence of a trend during the last ten periods (9 – 18) and its various subsets leads us to affirm as our overall finding that the conventional decline of cooperation that has been replicated so many times in finitely repeated public goods experiments is absent with respect to the pre-stage civic tasks that constitute the crucial second-order public good in our experiment. Summarizing:

Result 2. *Most subjects complete some civic tasks in all treatments. There is no downward trend for civic engagement, i.e. in provision of the second-order public good (consistent with Hypothesis H.2a).*

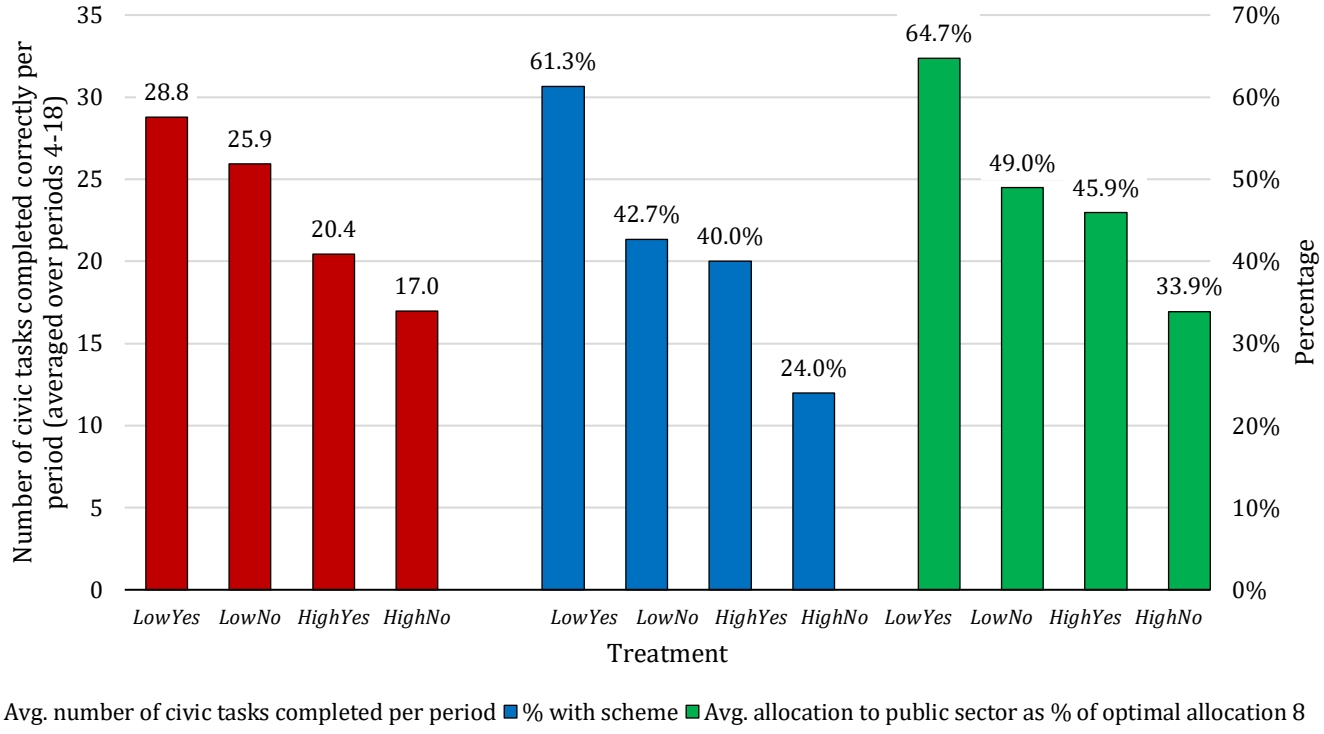
[16] Subjects succeed in completing more total tasks accurately in later than in earlier periods, and the number of private tasks completed goes up. See note on private task completions following the final panel of Table A.3.

[17] Table A.3(c) displays coefficients from regressions which use the observations of only one session at a time. We note that this diminishes the number of observations considerably and trends may be subject to some session-specific effects. These regressions find that the time trend variable for most period ranges remains insignificant in the data for most sessions.

c. Treatment effects

Figure 6 shows the main treatment effects. In particular, the figure shows the number of civic tasks completed per period (left panel), the share of sessions with a penalty scheme (middle panel), and the contribution to the public sector relative to the optimal allocation by treatment (see Table A.4 for more details). Overall, bars increase in all three dimensions from right (high cost of civic engagement and no local social interaction) to left (low cost and with social interaction). Having lower opportunity costs and added social interaction, rather than the alternative settings, is jointly associated with a rough doubling of contributions to the first-order PG (33.9% to 64.7%, right panel), with about a 70% increase in task completions for the second-order PG (left panel), and with an increase in the success rate in establishing a sanctions regime by 155% (or by a factor of 2.6, middle panel).

Figure 6: *Subjects' pre-stage and main stage behaviors in Part 2 by treatment*



Notes: Bars show averages for periods 4 – 18 (Part 2). Right panel: allocation to the public sector in the main stage as a percent of the socially optimal allocation, 8. Middle panel: success rate (% of Part 2 periods in which a scheme was achieved). Left panel: Number of civic tasks correctly completed by all 24 subjects. For treatment description, see Figure 1.

Mann-Whitney tests set an especially high bar to detect treatment differences because they use a single observation per session only -- although we had about 7,200 pre-stage

allocation choices made in some 50,000 individual visits to the task choice screen. Such tests show significant differences in civic task completion for *LowYes* vs. *HighYes* ($p < 0.01$), *LowYes* vs. *HighNo* ($p < 0.01$), and *LowNo* vs. *HighNo* ($p < 0.05$), and a marginally significant difference for *LowYes* vs. *LowNo* ($p < 0.10$) using one-tailed tests for differences predicted by hypotheses. Pooled comparisons for civic tasks are significant for the cost dimension (*Low* vs. *High*, $p < 0.01$), but insignificant for local social interaction (*Yes* vs. *No*, $p > 0.10$).[18]

Treatment differences in the percentage of periods played under a penalty scheme are significant for *LowYes* vs. *HighNo* ($p < 0.01$), *LowYes* vs. *HighYes* and *LowYes* vs. *LowNo* ($p < 0.05$), and marginally significant for *LowNo* vs. *HighNo* ($p < 0.10$, all one-tailed tests). For this outcome, we find significant differences for both cost (*Low* vs. *High* pooled) and local social interaction (*Yes* vs. *No* pooled) both with $p < 0.05$.[19]

Results for public sector allocations in Part 2 (i.e., contributions to the first-order PG) are almost identical to those for scheme presence because it almost fully accounts for differences in main stage allocations, although in the case of the tests with pooled treatments, both the test of difference between *High* and *Low* opportunity cost treatments and that of difference between *Yes* and *No* local social interaction treatments obtain *p*-values below 5%. Regressions with session random effects show that local social interaction raises contributions in the first-order PG (see Table A.3(a) columns (4) and (5)), where the coefficient on *LowYes* (2.466) differs significantly from that on *LowNo* (1.207, $p < 0.01$, chi-square test).

In sum, we have strong evidence that the opportunity cost of doing a civic task affected the number of tasks completed. Holding local social interaction constant at either setting, the treatment with *Low* cost sees significantly more civic tasks completed than the one with *High*

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[18] In our non-parametric tests, we report one-tailed tests whenever a comparison of two treatments or treatment pairs involves comparison of *Low* vs. *High* treatments or *Yes* vs. *No* treatments, covered by H2(b) and H3(a) respectively. We treat predictions for civic engagement as implying predictions for scheme presence given the law of large numbers, and we likewise treat differences in civic engagement as implying differences in first-order allocations to the public sector, by virtue of H1. Only differences between *LowNo* and *HighYes* treatment are not covered by our predictions, and for these comparisons two-tailed tests are used. Readers who so prefer can easily calculate corresponding two-tailed test *p*-values from *p*-values shown in our text and appendix tables.

[19] The cost of civic engagement clearly mattered for the failure of establishing the sanction scheme, as expected. In 21 cases, of which 18 were in *HighNo* and 3 in *HighYes*, T_c was 10 or less, while in 4 cases, 3 in *LowNo* and 1 in *LowYes*, T_c was ≥ 40. In line with our expectations, all failures to reach the threshold of scheme possibility have the higher opportunity cost of 22, with the large majority of these having low social interaction, and all cases of assuring the scheme with certainty have opportunity cost 10. Statistical tests reveal that the scheme is unavailable with certainty (i.e., $T_c < 11$) significantly more often under high vs. low cost (Mann-Whitney of *LowYes* and *LowNo* pooled vs. *HighYes* and *HighNo* pooled, $p = 0.013$), and is present with certainty more often with low vs. high costs ($p = 0.068$). Social interaction significantly reduces the risk of certain scheme absence under high opportunity cost (*HighYes* vs. *HighNo*, $p < 0.01$) according to session random effect probit regressions using session-level observations with bootstrapped standard errors.

opportunity cost of civic tasks, supporting H.2b. Evidence that having small-group social interaction makes a difference is weaker when judged solely by civic tasks completed, for which there is only a marginally significant difference between *LowYes* and *LowNo* by the MW test. However, the *LowYes* and *HighYes* bars exceed those of the *LowNo* and *HighNo* for all outcomes in Figure 6, and local social interaction is associated with many statistically significant differences for the penalty scheme and first-order allocation outcomes.

d. Dynamics of civic engagement: regressions

We now discuss treatment effects on individual-level civic engagement. Although these decisions are clearly not statistically independent of one another within a given session, we can partly mitigate the problem by adopting a random-effects specification, bootstrapped standard errors and some individual-specific controls. We use Tobit regressions due to the non-trivial number of observations in which an individual completed no civic tasks in a period's pre-stage. In these estimations, unlike Table A.3, we omit the *Period* terms which assume linear time trends and instead investigate in a more open-ended way how the previous choices on which subjects received feedback influence their civic engagement in the current period. We also use this format to test for persistence of subject-specific dispositions towards cooperation and to examine whether feedback from social circle members has an independent effect on subsequent effort choices.

Table 1 columns (1) – (3) show results when the observations from all four treatments are pooled, while columns (4) – (6) show results for the treatments with local social interaction only (*Yes*), since the effects of the relevant feedbacks can be investigated in those observations alone. Treatment dummy variables indicate significantly more civic engagement when its cost is low (*LowYes* vs. *HighNo*, *LowNo* vs. *HighNo*, *LowYes* vs. *HighYes* all differ at $p = 0.01$), but this effect is, unsurprisingly, less pronounced when social interaction counteracts the cost effect (*HighYes* vs. *LowNo*, $p = 0.051$). The results remain in line with the non-parametric tests, despite the presence of added controls. Differences between treatments differing only by presence or not of local social interaction are only sometimes significant, for instance for *HighYes* vs. *HighNo* ($p < 0.05$). Summarizing the non-parametric and regression results thus far:

Result 3. *More civic tasks are completed when the opportunity cost is lower, as predicted by H.2b. The effect of social interaction on civic task completion, predicted by H.3a, is significant only in the high cost treatments, although such interaction significantly increases main stage cooperation and achievement of a penalty scheme under both cost settings.*

Table 1: *Dynamics of subjects' civic task completions: subject-level analysis*

Dependent variable: The number of civic tasks completed by subject *i* in period *t*.

Independent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
(i) *LowYes* dummy	1.361***	1.140***	.979***	.734***	.597***	.460**
	(.218)	(.227)	(.211)	(.250)	(.153)	(.180)
(ii) *LowNo* dummy	1.039***	.882***	.679***	---	---	---
	(.248)	(.200)	(.247)			
(iii) *HighYes* dummy	.593**	.524***	.440**	---	---	---
	(.248)	(.159)	(.236)			
(iv) $p_{i,1}$ {own contribution in pd. 1}	.122***	.099***	.110***	.104***	.086***	.102***
	(.029)	(.021)	(.020)	(.031)	(.025)	(.024)
(v) $p_{i,3}/(p_{-i,3} + p_{i,3})$ {own pd. 3 contribution divided by session average}	.391***	.298***	.300***	.332***	.245***	.262***
	(.059)	(.046)	(.056)	(.084)	(.046)	(.082)
(vi) $t_{c,i,t-1}$ {# of own civic tasks completed in pd. *t*-1}	---	.403***	.384***	---	.389***	.367***
		(.035)	(.043)		(.052)	(.054)
(vii) $t_{c,-i,t-1}$ {avg. # of 23 other persons' civic tasks completed in pd. *t*-1}	---	-.048	.026	---	---	---
		(.099)	(.113)			
(viii) $t_{c,sc\text{-}i\ t-1}$ {lagged avg. # of civic tasks of others within own social circle}	---	---	---	---	.160***	.181**
					(.055)	(.071)
(ix) $t_{c,oth18,\ t-1}$ {lagged avg. # of civic tasks by others outside own social circle}	---	---	---	---	-.246**	-.0002
					(.103)	(.145)
Controls for Randomness	No	No	Yes	No	No	Yes
Constant	-1.792***	-1.711***	-1.784***	-.959***	-.932***	-1.325***
	(.269)	(.204)	(.169)	(.232)	(.200)	(.201)
# of Observations	7,200	6,720	5,712	3,600	3,360	3,024
# of left-censored observations	3,697	3,429	2,836	1,666	1,535	1,393
Wald χ^2	157.37	431.17	714.72	91.78	253.73	325.05
Prob > Wald χ^2	.0000***	.0000***	.0000***	.0000***	.0000***	.0000***
p-value for Wald χ^2 tests of coeff. diff.:						
H_0: (i) ≤ (ii)	.1467	.0653*	.1058	---	---	---
H_0: (i) ≤ (iii)	.0022***	.0011***	.0028***	---	---	---
H_0: (ii) = (iii)	.0505*	.0512*	.2758	---	---	---
H_0: (viii) ≤ (ix)	---	---	---	---	.0014***	.2778
VIF [#1]	1.51	2.13	2.41	1.21	1.90	1.71
	[1.40]	[1.54]	[1.62]	[1.14]	[1.46]	[1.30]

Notes: Individual random effect Tobit regressions. Columns (1) – (3) show results when the observations from all four treatments are pooled, while columns (4) – (6) show results for the *Yes* treatments alone. Numbers in parentheses are bootstrapped standard errors. All observations in Part 2 were included in column (1) and (4), those from period 5 onwards were included in columns (2) and (5) to allow for the one period lag terms. Estimates (3) and (6) include controls for random draw favorability and breaking of 'streaks' which require dropping also all period 5 and some other observations as detailed in the note on randomness controls beneath online Appendix Table A.5. The VIFs (Maximum [mean] value of variance inflation factors) indicate that there are no concerns of collinearity in all columns. *** Significant at the 1% level, ** Significant at the 5% level, * Significant at the 10% level (2-sided). *p*-values of Wald tests of significance of differences in coefficients are 1-sided where a predicted inequality is shown, otherwise 2-sided.

All specifications in Table 1 include two indicators of individual cooperativeness in order to investigate and control for the heterogeneity in cooperative inclination. We use $p_{i,1}$, subject *i*'s first allocation to the public sector prior to scheme availability and before any indication of others' inclinations, and $p_{i,3}/(p_{-i,3} + p_{i,3})$, the ratio of *i*'s to her session's average contribution in period 3, as proxies for the inclination to cooperate. The observation that both proxies obtain positive and highly significant coefficients leads us to state

Result 4. *Subjects who are initially relatively cooperative in the first-order social dilemma when a scheme is unavailable engage in more second-order civic task completion, ceteris paribus.*

Column (2) adds controls for both own and (twenty three) others' civic task completion during the previous period, $t_{c,i,t-1}$ and $t_{c,-i,t-1}$, respectively. The first variable obtains highly significant positive coefficients, indicating persistence of the individual's tendency to perform civic tasks, while the coefficients for the second variable are insignificant. In column (3), we add controls for session-specific idiosyncrasies in the patterns of random draws determining scheme presence (for example, apparent "streaks" of favorable or unfavorable draws, and timing of breaks in apparent "streaks"). We find the previous conclusions to be unchanged (see note following Table A.5 for details).

Columns (4) – (6) restrict attention to the treatments with small group social interaction to explore information feedback effects about the civic tasks done by others in *i*'s social circle. Column (4) parallels column (1) except for the sample restriction to *Yes* treatments. The highly significant coefficient on *LowYes* confirms that the opportunity cost is a significant determinant of the number of civic tasks performed. The coefficients on the cooperativeness proxies $p_{i,1}$ and $p_{i,3}/(p_{-i,3} + p_{i,3})$ show that Result 4 also holds in the restricted sample.

Column (5) adds the own lagged civic task variable $t_{c,i,t-1}$ and also offers the first opportunity to test whether reported lagged civic tasks of others in one's social circle—$t_{c,sc\text{-}i\,t-1}$—affects current civic task choice differently from lagged civic tasks by out-of-circle session participants—called $t_{c,oth18,\,t-1}$ (because each subject's session includes 18 others not in her social circle). Column (6) adds controls for randomness as in column (3). The estimated coefficients on other's task completions are positive and significant at 1% level for fellow social circle members in both columns (5) and (6), negative, and significant in column (5), for non-circle members' civic tasks, with a chi-square test indicating that the difference between the two coefficient estimates is significant at almost the 1% level in column (5). These estimates thus suggest that subjects' civic task performance responds positively to that of fellow social circle members, but not to that of those outside one's social circle.[20]

[20] We also investigated whether the number of civic tasks completed tends to converge over time within a social circle. The results, shown in Table A.6 and its note, suggest that although civic task completions did not

Result 5. *Others' civic task completions positively affect one's own, but only for members of the social circle, not for other session participants. This result supports a combination of conditional cooperation with impact of social proximity (low distance)*.

How did evaluative feedback, i.e. approval or disapproval by others in one's social circle, affect civic engagement? (see appendix Table A.5 for details). We first note that higher engagement tends to meet with more approval in the circle (correlation coefficient 0.667, $p < 0.001$; see also Figure A.3 for a scatterplot and regression). Using regressions similar to Table 1's, we then find that social disapproval tends to encourage civic engagement, although this only holds for individuals who completed moderately fewer civic tasks. Specifically, for those who completed up to half of a civic task less than the average in their session, getting negative feedback from their social circle members led to a significant increase in civic task completion. For those whose task completions were more than half a task below the session average, however, the negative feedback was followed by even less civic engagement.

The regression analysis is thus suggestive that the small group interactions in treatments *LowYes* and *HighYes* mattered. We identify two types of peer pressure that motivate civic engagement. One is leading by example (a positive influence of civic engagement on one's own, see also Table 1, estimates (5) and (6), var. (viii), Table A.5, estimate (1), var. (v)). The other is evaluative feedback, especially expressed disapproval (Table A.5, estimates (1) and especially (4)).

Result 6. *Subjects express approval to high completers of civic tasks, and vice versa for low completers, in their social circle. Disapproval tends to induce increased civic engagement (as H.3b predicts) except among the least civically engaged.*

e. A further look at heterogeneity of civic engagement

A closer look at variation among individuals can improve our understanding of whether the willingness to continue to engage in doing civic tasks is mainly attributable to a few hard-core cooperators in each session, or is a more widely shared tendency. Recall that it is hypothetically possible that a third or less of the 24 participants in a session are strongly cooperative or civic minded and are completing almost all of their session's civic tasks. We can quickly reject the most extreme forms of this conjecture, however, by sorting subjects from each treatment into bins based on their total civic task completion during periods 4 – 18 as a whole (see Figure A.4).

converge within groups, the average number of civic tasks diverged over time as between groups of given sessions, a pattern indicative of a more subtle form of within-group social influence, and one consistent with the significant coefficients $t_{c,sc\text{-}i\ t-1}$ and insignificant or negative coefficients for $t_{c,\text{-}i,t-1}$ and $t_{c,oth18,\ t-1}$.

We find that there do exist some "super-cooperators" who completed more than 2 or even more than 3 civic tasks per period, hence more than the number that would suffice to guarantee the penalty scheme if everyone were equally civically engaged.[21] There are also some complete free riders, but the share is low compared to the prediction of standard theory (19 percent). In the treatment most favorable to civic engagement *LowYes*, 72% of subjects completed an average of at least half of a civic task per period, and 51% did an average of at least one civic task per period. These proportions drop to 62% and 41% in the *LowNo* treatment, with *HighYes* being similar, consistent with our other findings that both a higher opportunity cost and absence of local social interaction negatively impact civic engagement. We conclude that our data are characterized by neither the extreme of a large majority free riding on a few high cooperators nor the opposite case of nearly equal civic engagement. This finding strikes us as quite concordant with civic engagement in modern societies, in which voting turnout ranges from around 55% of the voting age population in U.S. presidential and in Japanese parliamentary elections to around 75% of voters in elections of national parliaments in the U.K. and France. The numbers who attend campaign rallies, make calls on behalf of candidates, collect signatures on petitions, and join in protest marches are typically much smaller fractions of countries' citizenries.

f. Does civic engagement align with indicators of behavior outside the lab?

Our exit survey provides data on our subjects' interest and involvement in political activity and other proxies for civic engagement in their lives outside the experimental laboratory, along with data on gender, field of study, SAT scores etc. Two main findings emerge.

First, we find that several indicators of civic engagement in the field correlate well with experimentally elicited civic engagement. For example, those who say that they are politically interested and engaged, follow political events in the media, signed a petition or attended a rally, participated in a demonstration or strike, or voted in the last U.S. presidential election tended to complete more civic tasks in the experiment (all at 5% level or better, see Table A.8 for details). This finding is noteworthy because it suggests that our representation of civic engagement in the lab, however artificial, may indeed capture what was intended, and provides some external validity.

Second, the correlates of these self-reported measures differ between the experimental measures of civic engagement just mentioned (the second-order public good) and contributions to the first-order public good (e.g. in period 1, when the sanction scheme is absent). In particular, *none* of the indicators of civic engagement in the field named above (political

[21] About 4% of subjects perform more than 45 tasks in total, i.e. 3 on average (see Figure A.4 and Table A.7(B) for details).

interest and engagement etc.) is significantly correlated with initial contribution to the first-order public good in the laboratory. This finding of differential correlation is notable because it indicates that the two types of activities in the laboratory (completing civic tasks vs. allocating tokens to a public account) capture distinct characteristics of subjects. This is remarkable because both activities are contributions to (first-order and second-order) public goods, but the difference in framing and function apparently caused a difference in behavioral response in line with our design goal that the two forms of cooperation be qualitatively distinct, from a subject's standpoint.

6. Discussion and conclusion

In this paper, we've argued that viewing government's power to mandate tax payments as a way of avoiding the need for voluntary cooperation in the provision of public goods is in some respects misleading, since sustaining a government that is accountable to the people it serves requires that at least some of them be civically engaged, which presents its own social dilemma. Obtaining recourse to the power of taxation while maintaining control of government by the citizenry entails not skirting the problem of cooperation entirely, but replacing a more daunting (because very costly) cooperation problem with a more tractable one: an opportunity to leverage a small amount of voluntary civic engagement into an accountable power to tax. We designed a laboratory experiment representing in realistically framed terms the core economic problem of allocation between public and private sectors, and we added to it a framed second-order social dilemma problem requiring adequate voluntary engagement in order for an accountable state with sanctioning powers to emerge. While the second-order public good problem of civic engagement has the same social dilemma characteristic as does the first order problem of funding the public sector (i.e., it is always privately payoff-reducing to civically engage), the *average* cost of full second-order cooperation is only around 10.5% (two treatments) or around 23.2% (the remaining two) of the potential average gain from shifting the main game's payoffs from that of full free riding to that of the socially optimal equilibrium.[22] This qualitatively resembles the way in which the cost of civic engagement tends to be at least an order of magnitude (indeed, probably several orders of magnitude, for most) smaller than the typical tax burden.

We found that many subjects maintained fairly high second-order cooperation, especially at the lower average cost, and that in contrast with results on first-order public goods dilemmas that almost invariably show decay of cooperation unless punishment,

[22] Each subject must on average complete 1⅓ civic tasks per period to achieve the penalty scheme with certainty, which costs 16⅔ points (36⅔ points) in foregone private task earnings in the treatments in which a private task generates 10 (22) earnings points. These average opportunity costs of the scheme constitute 10.5% and 23.2%, respectively, of the 158 point difference of subject earnings at the private versus social optimum of the main stage.

communication, or exclusion or other group sorting devices are brought to bear, there is no indication of a declining trend in performing civic tasks. Allowing subjects to signal their cooperation in the second-order interaction to others within a small sub-group and exposing them to one another's normative evaluations also appeared to increase civic engagement: individual civic task completion responds more positively to task completion by other social circle members than to that by session participants outside the circle, and moderately below-average civic task completers increase task completion in response to criticism. Subjects successfully provided themselves with a contribution-mandating government in 61.3% of periods in the treatment with low opportunity cost of civic engagement and with local social interaction (*LowYes*), versus 24.0% of periods with high opportunity cost and no local interaction (*HighNo*) and 42.7% and 40.0% of periods in treatments with lower opportunity cost without sub-group interaction (*LowNo*) and with higher opportunity cost with sub-group interaction (*HighYes*).

Our results suggest that being one dilemma removed from the first-order problem and being able to solve the larger first-order problem at a more modest cost might constitute another in the set of conditions under which cooperation resists decaying over time. Perhaps the main mechanism at work is a behavioral one involving the comparison of average rather than marginal costs in an environment in which the cost of sharing in the solution of the macro social dilemma through second-order cooperation appears comparatively small and others seem inclined to share the burden. This is the leveraging effect mentioned in our introduction: while only a handful of subjects seem prepared to sustain allocations of 40% of their endowments to the public sector in the main stage interaction, well over half of subjects seem ready to forego a smaller private pre-stage payoff to help put formal sanctions in place. The small pre-stage cost may strike these individuals as a good deal, although it is as selfishly rational to free ride in the pre-stage as in the main one. What (if any) role was played by existing norms of civic engagement remains a subject for future research.

The analogy between pre-stage cooperation in our experiment and civic engagement in democracies is a fairly close one, although the cost of civic engagement in the real world is if anything usually far smaller relative to the cost of meeting one's tax obligation. No one supposes that modern governments could raise revenues in the neighborhood of 30 – 40% of GDP by asking citizens or companies to voluntarily contribute. But we do frequently see half or more of a country's adult citizens devoting an hour or two of their time to casting a vote in an election, and a few more hours to learning what candidates' positions are by reading newspapers, magazines, and materials on the internet, listening to radio and TV broadcasts, and talking with friends and family members. Our treatments under-represent the difference in magnitudes, and to the extent that one can extrapolate to environments in which civic engagement costs only 1%, 0.1%, or 0.01% as much as do tax payment, its results may be

thought of as implying rates of success in building governmental enforcement power nearer to 100%.[23] For the more costly task of taking to the street to defend democracy against threat or to bring down a dictatorship, however, our subjects' considerable share of cooperation failures are near to the mark and resonate with concerns for democracy's long-run survival.

One important dimension left out of our design is the role of partisan factors in motivating participation in the political process. One could argue that many participate in the political process more because they wish to support one faction or viewpoint over another than because they feel compelled to perform a disinterested civic duty—though even partisans face collective action dilemmas.[24] Still, beyond partisan reasons to pay attention to and participate in the political process, many do appear to obtain direct utility benefits from participation, as suggested by Della Vigna *et al*. (2017) and François and Gergaud (2019). Social influences in family, school, and community settings may bring fulfillment of responsibilities as a citizen to take its place alongside obedience to such norms as not stealing others' possessions and not lying, as a positive attribute of identity that most may wish to signal not only to others but also to themselves. Considering that large numbers of people go to the trouble of sorting and recycling much of their waste, though the tangible benefits will go mainly to unrelated others in future cohorts, it is not so surprising that many people incur some modest costs out of civic responsibility. Willingness to make these and still stronger sacrifices (see Ticchi, Verdier and Vindigni, 2013) to uphold institutions which simultaneously foster public welfare and prevent the abrogation of power by a few individuals, may be crucial to sustaining the kind of political institutions that some viewed as the natural goal of history, in the late 20th century, but that many now judge to be increasingly vulnerable.

[23] That is, with 40% success when second-order cooperation costs 23.3% of state power's social dividend and 61% success when second-order cooperation costs 10.5% as much as that dividend, a variety of extrapolations (not all being strictly linear) to these much lower costs would bring the success rate up to or near to 100%. We could study such low second-order costs while maintaining the full social dilemma aspect in our current design only by having dramatically larger session sizes, perhaps through linking multiple labs.

[24] Although there is a free-rider dilemma within each partisan camp, the psychology that spurs participation might be bolstered by the competition (e.g. Markussen, Reuben and Tyran 2014), which is absent in the citizenship dilemma studied by us.

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Supplementary Online Appendix

for Kamei, Putterman and Tyran:

"Civic Engagement as a Second-Order Public Good"*

[for online publication only]

Tables of Contents

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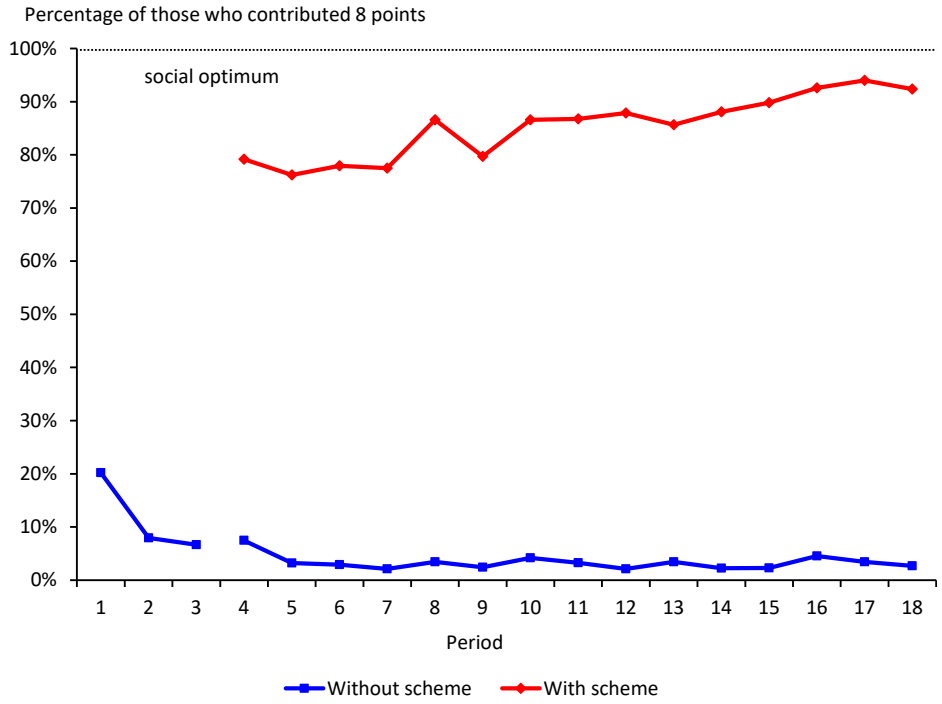
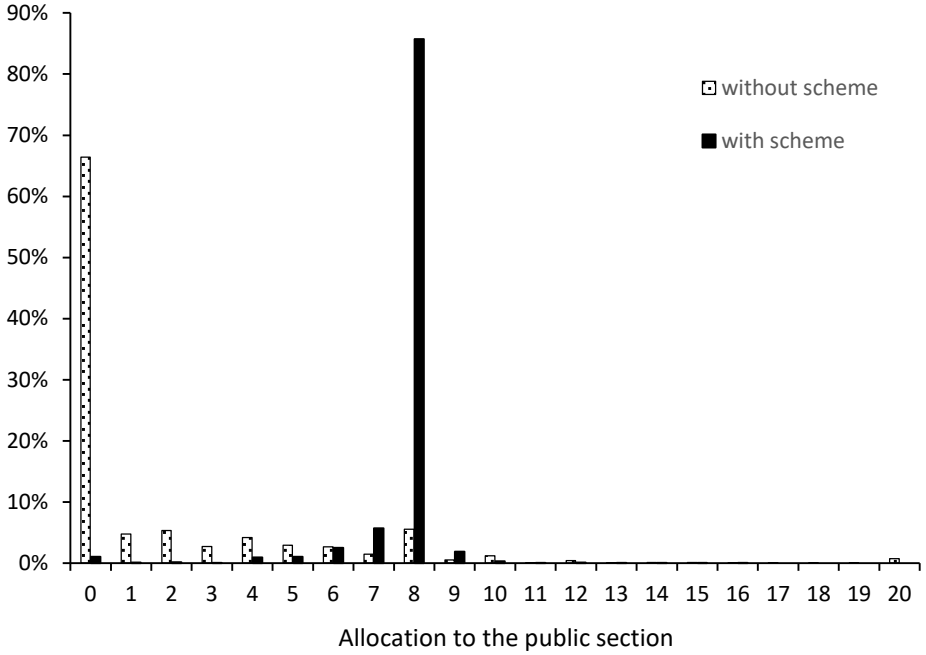
Additional tables and figures

Figure A.1: *Share of subjects contributing social optimum, and frequencies of contributions to public sector, with and without penalty scheme*



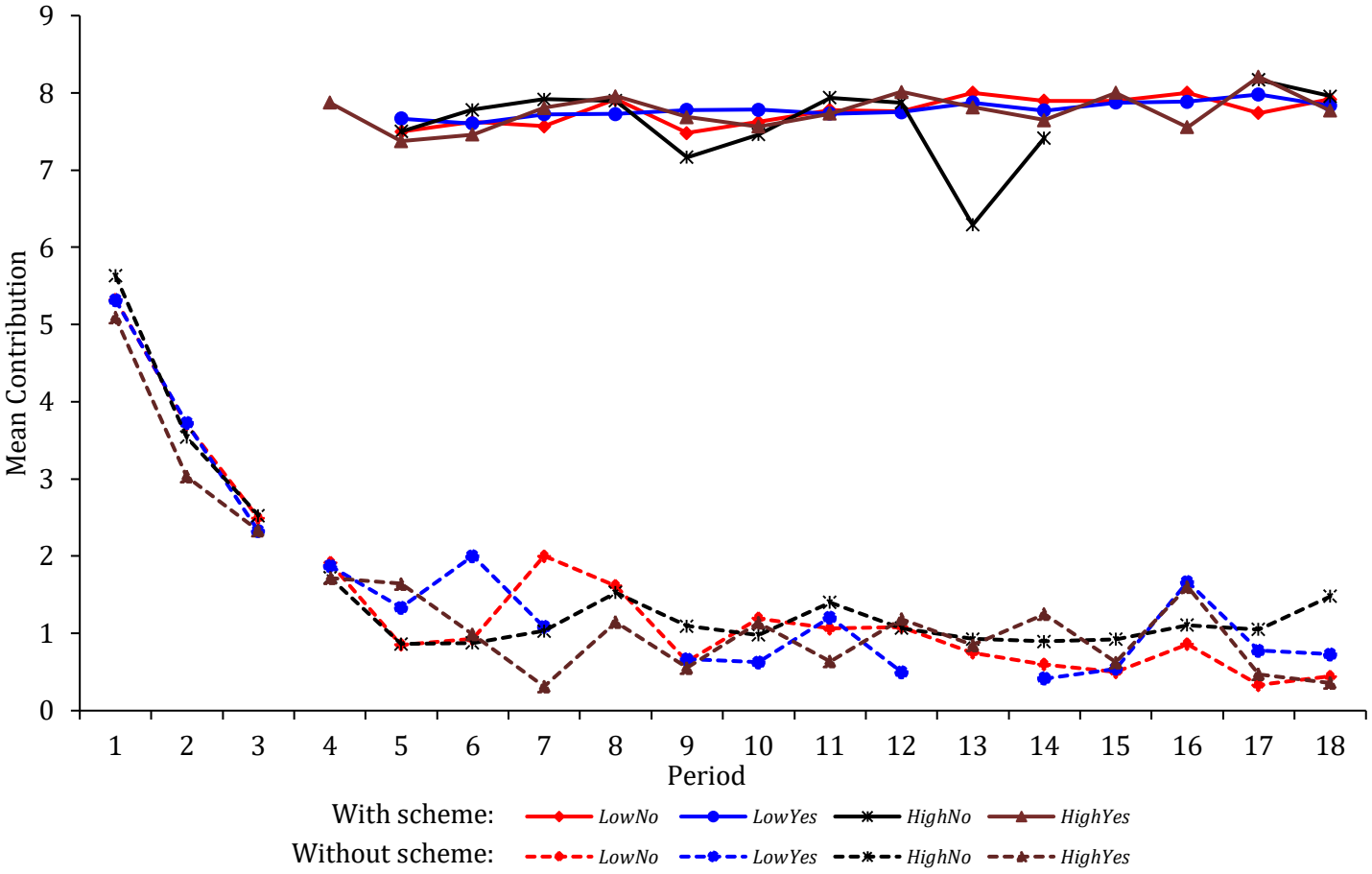
(a) The trend of percentage of subjects who contributed eight points to the public sector in presence and in absence of penalty scheme



(b) Distribution of subjects' allocations to the public sector (all periods)

Note: Both panels include data from all four treatments.

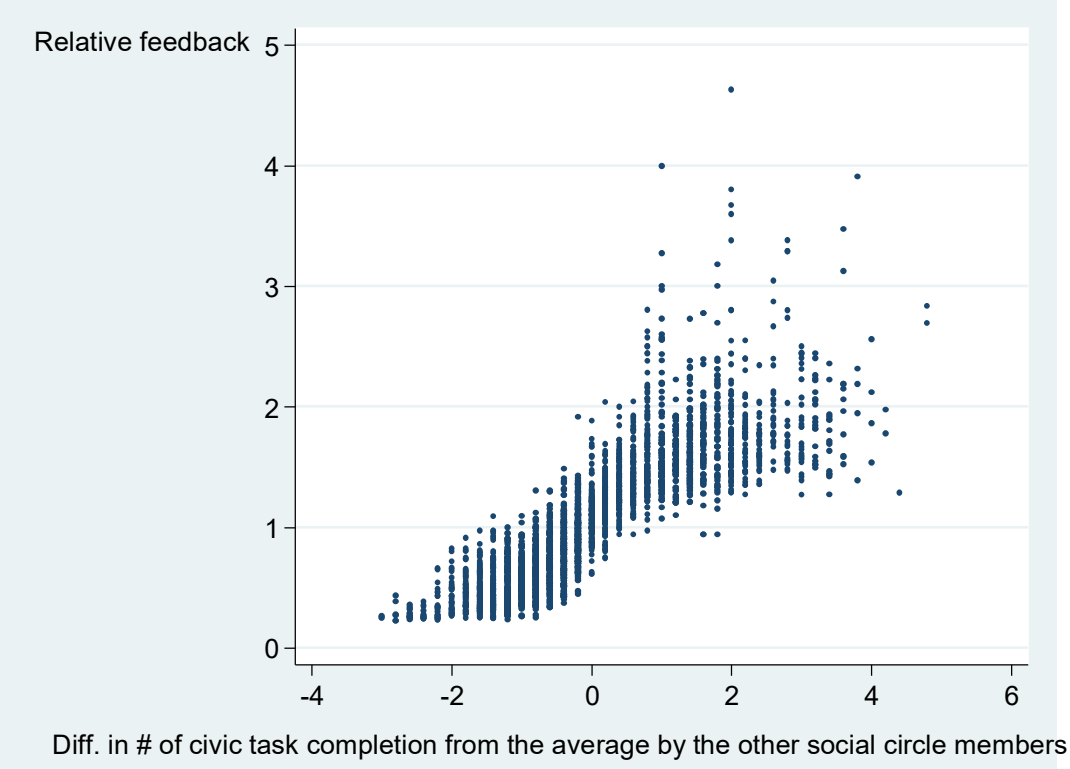
Figure A.2: *Average per subject allocation to the public sector by period and treatment (supplementing Figure 4 of the paper)*



Notes: No groups had the penalty scheme in period 4 in the *LowNo*, *LowYes* and *HighNo* treatments. No groups had the scheme in periods 15 and 16 in the *HighNo* treatment. All groups had the scheme in periods 8 and period 13 in the *LowYes* treatment. Points without scheme are left unconnected between periods 3 and 4 here and in Figure 4 to emphasize that absence of the scheme is a design feature in periods 1 – 3 whereas it results endogenously from civic task completion (and in most cases also from a random draw outcome) in periods 4 – 18.

Figure A.3: *Relative feedback received as a function of the difference of completed civic tasks from mean civic tasks in the individual's social circle*

(A) Scatterplot



Note: Observations by individual and period of Part 2. Relative feedback (y-axis) is average feedback received by a given individual *i* in period *t* (on the scale from 1 = strongly disapprove to 5 = strongly approve), divided by average feedback received by the other 5 social circle members.

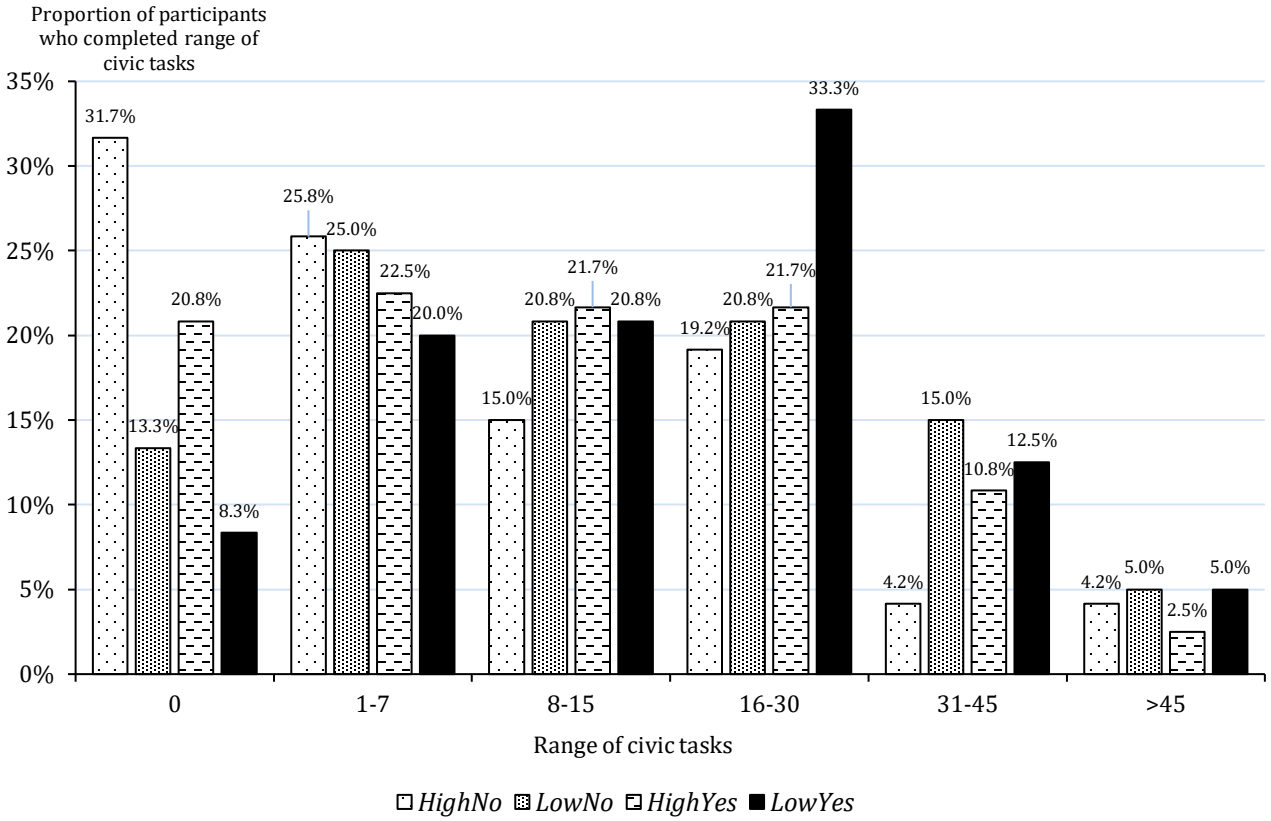
(B) Estimating partial correlation between relative feedback and relative civic task completion

Dependent Variable: Relative feedback received by subject *i* in period $t \in \{4, 5, \ldots, 17, 18\}$

Independent Variable:	(1)
Difference in the number of civic tasks completed by *i* from the average by the other social circle numbers	.365***
	(.020)
Constant	1.048***
	(.000)
# of observations	3,600
F	316.55
Prob > F	.0000***

Notes: Individual fixed effect linear regression with robust standard errors clustered by session. The numbers in parentheses are standard errors. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Figure A.4: *Distribution of subjects by completed civic tasks in total*



Notes: 15 tasks per subject represents an average of one task per period. Hence, 1–7 means a positive number of tasks, but in less than half the periods; 8–15 means a subject may have done tasks in more than half of periods but not more than one per period on average; 16–30 means the subject may have done at least one task each period, but not more than 2 per period on average; 31–45 means more than two but less than three tasks per period on average; and > 45 means more than three tasks per period on average.

Table A.1: *Public sector allocations in Part 1 (periods 1 to 3): test for treatment differences*

Dependent Variable: Allocation by subject *i* in a given period {1, 2, or 3} to the public sector

Independent Variable:	(1)	(2)	(3)	(4)	(5)
LowYes dummy {= 1 for the *LowYes* treatment; = 0 otherwise}	-.023 (.499)	-.033 (.554)	.158 (.750)	-.032 (.568)	-.017 (.522)
HighNo dummy {= 1 for the *HighNo* treatment; = 0 otherwise}	-.165 (.580)	-.170 (.562)	.412 (.768)	-.170 (.642)	.222 (.593)
HighYes dummy {= 1 for the *HighYes* treatment; = 0 otherwise}	-.586 (.650)	-.576 (.654)	-.504 (.849)	-.577 (.497)	-.330 (.598)
Period number {= 1, 2, 3}	---	-2.102*** (.117)	-1.993*** (.197)	---	---
LowYes dummy × Period number	---	---	-.099 (.298)	---	---
HighNo dummy × Period number	---	---	-.306 (.293)	---	---
HighYes dummy × Period number	---	---	-.037 (.326)	---	---
Period 2 dummy {= 1 for Period 2; = 0 otherwise}	---	---	---	-2.358*** (.172)	-2.048*** (.307)
Period 3 dummy {= 1 for Period 2; = 0 otherwise}	---	---	---	-4.182*** (.219)	-3.978*** (.524)
LowYes dummy × Period 2 dummy	---	---	---	---	.136 (.439)
HighNo dummy × Period 2 dummy	---	---	---	---	-.688 (.503)
HighYes dummy × Period 2 dummy	---	---	---	---	-.723 (.536)
LowYes dummy × Period 3 dummy	---	---	---	---	-.217 (.603)
HighNo dummy × Period 3 dummy	---	---	---	---	-.575 (.694)
HighYes dummy × Period 3 dummy	---	---	---	---	-.021 (.847)
Constant	2.749*** (.375)	6.964*** (.444)	6.755*** (.582)	4.943*** (.367)	4.781*** (.373)
# of observations	1,440	1,440	1,440	1,440	1,440
# of left(right)-censored observations	490(4)	490(4)	490(4)	490(4)	490(4)
Log likelihood	-3215.09	-3056.34	-3055.76	-3055.32	-3052.79
p-value (two-sided) for Chi-squared test H_0: Period 2 dummy = Period 3 dummy	---	---	---	< .0000***	.0002***
Maximum [mean] value of variance inflation factors[#1]	1.50 [1.50]	1.50 [1.38]	11.00[#1] [9.79]	1.50 [1.33]	5.33 [4.20]

Notes: Individual random effect Tobit regressions with bootstrapped standard errors. Numbers in parentheses are standard errors. [#1] As in Table A.3(a), we have serious collinearity when we include interaction terms between the Period number variables and the treatment dummies. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table A.2: *Possible "carry-over" of cooperative inclination from periods with to periods without sanction scheme*

Dependent Variable: Total amount allocated by the 24 subjects in a given session in period *t* (where *t* > 6) to the public sector

	LowNo		*LowYes*		*HighNo*		*HighYes*	
Independent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Average allocation to the public sector in Part 1	-.333 (1.166)	-.135 (.407)	.435 (.412)	.482*** (.147)	.0344 (.081)	.0362 (.082)	.904*** (.265)	.891*** (.255)
# of times the scheme was in place during periods *t – 1* and *t – 2* (= 0, 1 or 2)	-1.462 (2.763)	3.360 (3.400)	1.640 (1.777)	-.251 (4.801)	-4.853** (1.960)	-4.800** (1.984)	3.426 (3.859)	3.986 (3.719)
proportion of Part 2 periods having scheme in period *t – 3* or earlier (back to period 4)	-44.334*** (14.296)	-41.696*** (12.596)	-13.506** (5.582)	-.744 (11.666)	-35.206*** (6.555)	-34.672*** (6.753)	-3.507 (12.924)	-.051 (12.547)
civic tasks completed in period *t* relative to average civic tasks in period *t*-3, *t*-2, and *t*-1[#1]	---	21.619** (10.674)	---	-15.065 (16.735)	---	1.814 (4.530)	---	16.645* (8.773)
Constant	67.025 (108.053)	21.619 (41.146)	-13.567 (38.730)	-10.862 (28.129)	33.715*** (8.068)	31.594*** (9.718)	-58.920** (25.768)	-76.083*** (26.353)
# of observations	30	30	20	20	47	47	35	35
R-squared	.3101	.4156	.3871	.4729	.5188	.5207	.3052	.3796
Maximum [mean] value of variance inflation factors[#2]	1.07 [1.05]	1.30 [1.17]	1.30 [1.20]	1.40 [1.32]	1.09 [1.06]	1.14 [1.07]	1.33 [1.23]	1.34 [1.19]

Notes: Session random effects linear regressions. The numbers in parentheses are standard errors. Only observations in periods of a session in which the scheme was not implemented are included. [#1] A ratio used to control for whether the number of civic tasks completed by the subjects in the session was high or low in *t* (a period the session failed to achieve a scheme) relative to the session's average civic tasks in the previous three periods. [#2] The VIFs indicate that there is no reason for concern about collinearity in any column. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Discussion of Table A.2: we check whether, in sessions that achieved the high contributions associated with the scheme during many periods, participants also attempted to achieve main stage cooperation by contributing to the public sector even without the scheme. Conceivably, successful imposition of the scheme in many periods positively affects cooperation behaviors in the main stage in future periods due to an institutional spill-over effect (e.g., Kamei, 2016) or so-called behavioral spill-over phenomenon (e.g., Bednar *et al.* 2012). In the event, the data if anything support the opposite conjecture, that participants become convinced that their successful instances of main stage cooperation

occurred only because of the presence of the scheme. Scheme presence and thus high main stage contributions in more past periods if anything reduces main stage contributions in periods lacking the scheme. Specifically, we use the data from only those periods of a session in which the scheme was not achieved, estimating separate regressions for each treatment. The dependent variable is total main stage allocation to the public sector in a given period in a session. Coefficients on share of the most recent two periods in which a penalty scheme was present are generally negative and are significant for one treatment (*HighNo*), and coefficients for percentage of still earlier periods having the scheme are all negative and reach statistical significance in all treatments except *HighYes*. There are significant indications that groups that had higher Part 1 contributions (added as a control in some specifications) have higher contributions in their Part 2 periods without scheme in the *LowYes* and *HighYes* treatments—thus, willingness to engage in main stage cooperation without a scheme is persistent, but is not positively affected by experiencing scheme presence *per se*. Session-level effort to achieve the scheme via performing pre-stage civic tasks in the period in question is included as a control in one of each pair of estimates. It obtains a positive coefficient in three of four treatments that is significant in one treatment (*LowNo*) and marginally significant in another (*HighYes*). The finding that high contributions with scheme presence if anything reduce contributions when the scheme is absent are reminiscent of the finding of Cinyabuguma *et al*. (2005) that when groups achieve nearly full cooperation in a VCM due to the threat of low contributors being expelled and suffering drastic reductions in earnings, cooperation "crashes" towards zero as soon as the threat is removed—in that case, in the known final period of play.

Table A.3: *Trends in the number of civic tasks completed per person*

(a) Session random effects regressions pooling all treatments, with observations at level of session and period

Dep. V	Civic tasks completed			Contribution to first-order PG		
	(1)	(2)	(3)	(4)	(5)	(6)
(i) *LowYes* dummy	0.492***	0.492***	0.330**	2.466***	2.466***	1.437
	(0.122)	(0.122)	(0.132)	(0.507)	(0.508)	(0.981)
(ii) *LowNo* dummy	0.374***	0.374***	0.077	1.207**	1.207**	-1.815
	(0.130)	(0.130)	(0.126)	(0.550)	(0.551)	(1.125)
(iii) *HighYes* dummy	0.145	0.145	-0.083	0.963	0.963	-0.258
	(0.129)	(0.129)	(0.143)	(0.645)	(0.646)	(1.141)
(iv) Period	---	0.007*	-0.009	---	0.020	-0.100
		(0.004)	(0.004)		(0.038)	(0.042)
(v) Period × *LowYes*	---	---	0.015**	---	---	0.094
			(0.006)			(0.060)
(vi) Period × *LowNo*	---	---	0.027***	---	---	0.275**
			(0.007)			(0.095)
(vii) Period × *HighYes*	---	---	0.021**	---	---	0.111
			(0.008)			(0.072)
Constant	0.707***	0.633***	0.805***	2.712***	2.496***	3.814***
	(0.110)	(0.112)	(0.098)	(0.428)	(0.636)	(0.828)
# of Observations	300	300	300	300	300	300
Wald χ^2	20.59	33.57	49.53	26.87	26.79	76.94
Prob > Wald χ^2	0.000	0.000	0.000	0.000	0.000	0.000
P-value for Wald χ^2 tests of signif. coeff. diff.						
H_0: (i) ≤ (ii)	0.090*	0.090*	0.017**	0.002***	0.002***	0.001***
H_0: (i) ≤ (iii)	0.001***	0.001***	0.001***	0.003***	0.003***	0.036**
H_0: (ii) = (iii)	0.018***	0.019***	0.223	0.681	0.681	0.155
H_0: (iv) + (v) ≥ 0	--	--	0.097*	--	--	0.439
H_0: (iv) + (vi) ≥ 0	--	--	0.000***	--	--	0.020**
H_0: (iv) + (vii) ≥ 0	--	--	0.053*	--	--	0.428
Maximum [mean] value of variance inflation factors (VIF)	1.50 [1.50]	1.50 [1.38]	11.72# [10.41#]	1.50 [1.50]	1.50 [1.38]	11.72# [10.41#]

Notes: Session random effects linear regressions. Numbers in parentheses are robust standard errors. Session average data are used. The reference group is observations in *HighNo*. *LowYes*, *LowNo* and *HighYes* are dummy variables set to 1 for observations of the corresponding treatment. "Civic tasks completed" is the per-person number of civic tasks, "contribution to first-order PG" is the average contribution in the main stage. Interactions v to vii are with treatment dummies. *** Significant at 1%, ** at the 5%, * at 10% (2-sided). # VIF > 10 is sign of serious multicollinearity. Wald χ^2 tests are 1-sided when predicted inequality is shown, otherwise 2-sided.

(b) Coefficient estimates from session random effects regressions by treatment, with observations at level of session and period

Treatment: Data:	*LowYes*	*LowNo*	*HighYes*	*HighNo*
Periods 6 – 18	-0.016*	0.002	0.002	-0.017***
	(0.009)	(0.006)	(0.010)	(0.0063)
Periods 7 – 18	-0.017**	-0.001	0.003	-.014
	(0.007)	(0.008)	(0.012)	(.0086)
Periods 8 – 18	-0.017***	0.001	-0.001	-0.008
	(.006)	(.010)	(0.013)	(0.009)
Periods 9 – 18	-0.010	0.004	-0.007	-0.004
	(0.009)	(.017)	(0.012)	(.011)
Periods 10 – 18	-0.010	0.000	-0.016	-0.001
	(0.009)	(0.016)	(0.016)	(0.009)
Periods 11 – 18	-0.014	-0.001	-0.021	0.007
	(0.010)	(0.013)	(0.019)	(.0112)
Periods 12 – 18	-0.003	0.007	-0.023	0.022
	(0.012)	(0.021)	(0.023)	(0.022)
Periods 13 – 18	0.010	0.043	-0.029	0.034
	(0.014)	(0.028)	(0.018)	(0.027)
Periods 14 – 18	0.016	0.043	-0.024	0.037
	(0.023)	(0.052)	(0.019)	(0.034)
Periods 15 – 18	-0.014	0.080	-0.021	0.031
	(0.014)	(0.088)	(0.029)	(0.042)
Periods 16 – 18	0.017	0.058	-0.025	0.013
	(0.027)	(0.113)	(0.028)	(0.061)

Notes: Each entry is the estimated coefficient (and in parentheses, standard deviation) of the period variable in linear regression with session random effects with robust standard errors, for the periods indicated by the row heading and the session observations of the treatment indicated by the column heading. Session average data are used as in panel (a). *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Discussion: As explained in the paper, we estimated regressions to study the trends in the number of civic tasks completed per person, changing the included periods (periods 6 – 18, 7 – 18, 8 – 18, 9 – 18, 10 – 18, 11 – 18, …, 16 – 18). As in panel (a), the dependent variable is the average per person number of civic tasks completed in a given period and the Period variable is included to identify a linear trend if present. Session average observations were used because correlations within sessions are expected. The following table indicates the coefficient estimates and *p*-values for the Period variable in each treatment. As explained in the paper, no treatment shows a statistically significant trend if we focus on the final ten periods (periods 9 – 18) or on any shorter final set of three or more periods.

(c) Coefficient estimates from regressions by session, with observations at level of session and period

	Data used for regressions:										
	Pds. 6-18	Pds. 7-18	Pds. 8-18	Pds. 9-18	Pds. 10-18	Pds. 11-18	Pds. 12-18	Pds. 13-18	Pds. 14-18	Pds. 15-18	Pds. 16-18
LowYes-s1	-.046***	-0.041**	-0.033*	-0.027	-0.015	0.010	0.012	0.013	0.025	0.013	0.063
	(0.013)	(0.015)	(0.016)	(0.020)	(0.022)	(0.028)	(0.029)	(0.037)	(0.049)	(0.072)	(0.179)
LowYes-s2	0.004	-0.011	-0.027**	-.031**	-0.040**	-0.039*	-0.031	-0.015	-0.054	-0.058	0.021
	(0.016)	(0.016)	(0.011)	(0.013)	(0.014)	(0.019)	(0.027)	(0.035)	(0.041)	(0.069)	(1.594)
LowYes-s3	-.020**	-.015**	-0.010	-0.010	-0.011	0.0005	0.007	0.031	0.05	-.033**	-0.042
	(0.007)	(0.007)	(0.006)	(0.007)	(0.01)	(0.01)	(0.019)	(0.027)	(0.052)	(0.007)	(0.017)
LowYes-s4	0.002	0.003	0.001	0.011	0.016	-.035*	-0.031	-0.025	-0.013	-0.004	-0.042
	(0.01)	(0.014)	(0.021)	(0.027)	(0.042)	(0.015)	(0.018)	(0.023)	(0.028)	(0.041)	(0.102)
LowYes-s5	-0.019	-0.022	-0.015	0.007	-0.001	0.013	0.030	0.048	0.071	0.013	0.083
	(0.013)	(0.017)	(0.021)	(0.013)	(0.014)	(0.014)	(0.016)	(0.024)	(0.039)	(0.037)	(0.017)
LowNo-s1	-0.003	-0.012	-0.003	0.008	0.012	0.013	-0.016	0.017	-0.008	-0.092	-0.042
	(0.015)	(0.015)	(0.017)	(0.018)	(0.024)	(0.035)	(0.038)	(0.036)	(0.065)	(0.08)	(0.153)
LowNo-s2	0.023	0.022	0.032	0.058	0.049	0.043	0.083	0.148	0.246	.404**	0.417
	(0.028)	(0.034)	(0.041)	(0.038)	(0.048)	(0.063)	(0.076)	(0.091)	(0.113)	(0.041)	(0.102)
LowNo-s3	0.008	0.010	0.004	-0.003	-0.020	-0.033	-0.033	-0.008	0.000	0.05	0.125
	(0.009)	(0.01)	(0.013)	(0.018)	(0.019)	(0.022)	(0.031)	(0.031)	(0.047)	(0.053)	(0.051)
LowNo-s4	-0.011	-0.022	-0.027	-.047*	-0.043	-0.017	-0.012	0.019	0.000	0.058	-0.25
	(0.017)	(0.019)	(0.025)	(0.021)	(0.027)	(0.023)	(0.031)	(0.036)	(0.067)	(0.16)	(0.068)
LowNo-s5	-0.005	-0.004	-0.004	0.005	0.003	-0.010	0.010	0.042	-0.021	-0.021	0.042
	(0.006)	(0.008)	(0.011)	(0.012)	(0.019)	(0.024)	(0.035)	(0.043)	(0.027)	(0.047)	(0.051)
HighYes-s1	.037***	.043***	.047***	.039***	.031**	.040**	.052**	0.030	-0.004	0.004	.042***
	(0.008)	(0.007)	(0.009)	(0.01)	(0.01)	(0.015)	(0.018)	(0.023)	(0.01)	(0.02)	(0.000)
HighYes-s2	-0.003	-0.007	-0.009	-0.014	-0.012	-0.015	-0.043	-0.036	0.033	0.071	-0.021
	(0.009)	(0.01)	(0.012)	(0.014)	(0.019)	(0.03)	(0.031)	(0.053)	(0.039)	(0.055)	(0.111)
HighYes-s3	-.027***	-.026***	-.026*	-0.015	-0.021	-0.013	0.006	-0.008	-0.033	-0.021	0.021
	(0.006)	(0.008)	(0.012)	(0.01)	(0.013)	(0.018)	(0.014)	(0.021)	(0.023)	(0.034)	(0.043)
HighYes-s4	0.005	0.003	-0.013	-0.036	-.067***	-.077***	-.070***	-.063*	-0.038	-0.075	-0.104
	(0.015)	(0.02)	(0.024)	(0.026)	(0.011)	(0.01)	(0.014)	(0.025)	(0.033)	(0.028)	(0.043)
HighYes-s5	0.000	0.003	-0.002	-0.008	-0.010	-.041**	-.060***	-.067***	-.079***	-.083**	-.063*
	(0.009)	(0.01)	(0.012)	(0.016)	(0.025)	(0.014)	(0.006)	(0.008)	(0.007)	(0.013)	(0.009)
HighNo-s1	0.003	0.006	0.0042	0.004	0.002	0.017	0.016	0.045	0.038	0.013	-0.083
	(0.007)	(0.007)	(0.008)	(0.011)	(0.015)	(0.016)	(0.026)	(0.025)	(0.035)	(0.059)	(0.034)
HighNo-s2	-0.01	0.002	0.012	0.017	0.016	0.034	.073**	0.077	.079**	0.025	-0.063
	(0.014)	(0.013)	(0.013)	(0.016)	(0.023)	(0.033)	(0.026)	(0.037)	(0.057)	(0.067)	(0.077)
HighNo-s3	-.026**	-.023*	-0.014	-0.014	-0.017	-0.002	0.018	.045**	.058**	.033**	0.042
	(0.009)	(0.01)	(0.008)	(0.011)	(0.015)	(0.017)	(0.02)	(0.013)	(0.018)	(0.0071)	(0.017)
HighNo-s4	-.033***	-.041***	-.039***	-.040**	-.024*	-0.031	-.052***	-.068**	-.092***	-.088*	-0.063
	(0.009)	(0.007)	(0.011)	(0.016)	(0.012)	(0.017)	(0.013)	(0.017)	(0.013)	(0.02)	(0.026)
HighNo-s5	-0.017	-0.013	-0.002	0.012	0.019	0.017	.054*	.071*	0.100	.171**	0.229
	(0.016)	(0.019)	(0.02)	(0.02)	(0.024)	(0.0344)	(0.024)	(0.029)	(0.043)	(0.03)	(0.043)

Notes: Each session is identified by its treatment name and a session number s1, s2, … s5, based on the chronological order in which it took place. Numbers in parenthesis are robust standard errors. The dependent variable is the average per person number of civic tasks completed in a given period, and the independent variable includes only the period number variable. Each treatment has five rows because there were five sessions. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Note on private task completions: Partly due to learning to accomplish private and civic tasks more quickly and accurately, the total number of tasks successfully completed rises from an average of about 1.7 in period 4 to about 3.9 in period 18. Most of the increase takes the form of private tasks, thus translating into somewhat higher total earnings in later than in earlier periods. Our focus is on the trend in completion of civic tasks since their aggregate number (plus random draw when required) determines whether there is a penalty scheme for the first-order dilemma and the opportunity cost of completing a civic task does not change over time. The average number of private tasks completed is 2.1, 2.8, 2.3 and 2.9 in the LowYes, LowNo, HighYes and HighNo treatments, respectively.

Table A.4.A: *Summary Statistics and Session-level Mann-Whitney Test Results*

	LowYes	*LowNo*	*HighYes*	*HighNo*	All
Avg. pub. sector alloc. in Part 1	3.79 (0.83)	3.84 (0.29)	3.49 (0.53)	3.9 (0.80)	3.75 (0.62)
Avg. total civic tasks per pre-stage	28.8 (3.1)	25.9 (4.0)	20.4 (4.0)	17.0 (6.4)	23.0 (6.3)
Avg. no. of periods with scheme	9.2 (61.3%) (1.3)	6.4 (42.7%) (1.5)	6.0 (40.0%) (2.4)	3.6 (24.0%) (2.9)	6.3 (42.0%) (2.8)
Cases $T_C < 11$, $11 \leq T_c \leq 39$, $T_c > 39$	0, 74, 1	0, 72, 3	3, 72, 0	18, 57, 0	21, 275, 4
Avg. pub. sector alloc. in Part 2	5.18 (0.66)	3.92 (0.84)	3.67 (1.17)	2.71 (1.04)	3.87 (1.25)
Avg. Part 2 pub. alloc. w/o scheme	1.04 (0.57)	1.09 (0.39)	0.94 (0.44)	1.08 (0.32)	1.04 (0.41)
Avg. Part 2 pub. alloc. w/scheme	7.76 (0.17)	7.77 (0.06)	7.73 (0.19)	7.73 (0.36)	7.75 (0.19)

Note: Standard errors in parentheses below values. Allocations to public sector are listed in per subject terms. Total civ. tasks is avg. no. of civic tasks correctly completed by 24 subjects in a given pre-stage.

B. Summary statistics and session-level Mann-Whitney tests for pooled treatment pairs

	10 (*LowYes* + *LowNo*)	22 (*HighYes* + *HighNo*)	Mann-Whitney test: 10 vs. 22	H (*LowYes* + *HighYes*)	L (*LowNo* + *HighNo*)	Mann-Whitney test: H vs. L
Avg. pub. alloc. in Part 1	3.82 (0.59)	3.69 (0.68)	0.545	3.64 (0.68)	3.87 (0.57)	0.364
Avg. civic tasks	27.4 (3.7)	18.7 (5.3)	0.001***	24.6 (5.5)	21.5 (6.9)	0.128
Avg. periods with scheme	7.8 (43.3%) (2.0)	4.8 (26.7%) (2.8)	0.010**	7.6 (42.2%) (2.5)	5 (27.8%) (2.6)	0.028**
Cases $T_C < 11$, $11 \leq T_c \leq 39$, $T_c > 39$	0, 146, 4	21, 129, 0	0.006***, 0.214, 0.034**	3, 146, 1	18, 129, 3	0.228, 0.214, 0.252
Avg. pub. sector alloc. in Part 2	4.55 (0.97)	3.19 (1.16)	0.012**	4.43 (1.20)	3.32 (1.09)	0.029**
Avg. Part 2 pub. alloc. w/o scheme	1.06 (0.46)	1.01 (0.37)	0.821	0.99 (0.48)	1.08 (0.34)	0.450
Avg. Part 2 pub. alloc. w/scheme	7.77 (0.12)	7.73 (0.26)	1.000	7.74 (0.17)	7.75 (0.23)	0.683

Notes: Standard errors in parentheses below values; Mann-Whitney test results are one-sided *p*-values except those for Part 1 decisions, for cases of $11 \leq T_c \leq 39$ and for the two final rows, which are two-sided. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

C. Session-level Mann-Whitney tests for individual treatments

Avg. pub. sector alloc. in Part 1	*LowYes*	*LowNo*	*HighYes*
LowNo	0.917	---	---
HighYes	0.465	0.251	---
HighNo	0.602	0.754	0.465

Avg. civic tasks	*LowYes*	*LowNo*	*HighYes*
LowNo	0.087*	---	---
HighYes	0.005***	0.117	---
HighNo	0.005***	0.014**	0.232

Avg. periods with scheme	*LowYes*	*LowNo*	*HighYes*
LowNo	0.012**	---	---
HighYes	0.029**	0.594	---
HighNo	0.004***	0.081*	0.165

Cases $T_c < 11$	*LowYes*	*LowNo*	*HighYes*
LowNo	.	---	---
HighYes	0.068*	0.136	---
HighNo	0.026*	0.026**	0.131

Cases $11 \leq T_c \leq 39$	*LowYes*	*LowNo*	*HighYes*
LowNo	0.219	---	---
HighYes	0.219	1.000	---
HighNo	0.062*	0.131	0.131

Cases $T_c > 39$	*LowYes*	*LowNo*	*HighYes*
LowNo	0.219	---	---
HighYes	0.159	0.136	---
HighNo	0.159	0.068*	.

Avg. pub. alloc. in Part 2	*LowYes*	*LowNo*	*HighYes*
LowNo	0.014**	---	---
HighYes	0.038**	0.602	---
HighNo	0.005***	0.087*	0.174

Avg. Part 2 pub. alloc. w/o scheme	*LowYes*	*LowNo*	*HighYes*
LowNo	0.754	---	---
HighYes	0.917	0.465	---
HighNo	0.917	0.917	0.347

Avg. Part 2 pub. alloc. w/scheme	*LowYes*	*LowNo*	*HighYes*
LowNo	0.754	---	---
HighYes	0.754	0.465	---
HighNo	0.624	0.462	0.807

Note: *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level. All tests are one-tailed (see paper, fn. 18) except those for Part 1 decisions, those for the two final panels, and tests of *LowNo* vs. *HighYes* (2-tailed).

Table A.5: *Dynamics of subject-level civic engagement incorporating the impact of evaluative feedback*

Independent Variable:	(1)	(2)	(3)	(4)
(i) *LowYes* dummy	.469**	.481***	.480**	.495***
	(.204)	(.183)	(.219)	(.183)
(ii) $p_{i,1}$ {contribution in pd. 1}	.099***	.100***	.100***	.099***
	(.028)	(.024)	(.021)	(.026)
(iii) $p_{i,3}/(p_{-i,3} + p_{i,3})$ {own pd. 3 contribution divided by session average}	.261***	.260***	.260***	.258***
	(.095)	(.088)	(.073)	(.066)
(iv) $t_{c,i,t-1}$ {# of own civic task completion in pd. t-1}	.344***	.353***	.336***	.294**
	(.080)	(.095)	(.115)	(.128)
(v) $t_{c,sc\text{-}i\,t-1}$ {lagged avg. # of civic tasks of others within own social circle }	.191***	.128	.169	.146
	(.067)	(.135)	(.128)	(.109)
(vi) $t_{c,oth18,\,t-1}$ {lagged avg. # of civic tasks by others outside own social circle}	-.024	-.030	-.037	.123
	(.169)	(.138)	(.135)	(.152)
(xi) positive deviation of feedback in pd. t-1 {= max{average feedback received by subject i – 3, 0}}	-.096	-.107	-.097	-.057
	(.086)	(.100)	(.104)	(.098)
(xii) negative deviation of feedback in pd. t-1 {= max{3 – average feedback received by subject i, 0}}	-.164**	-.157*	-.098	.234**
	(.075)	(.081)	(.118)	(.095)
(xiii) average others' feedback {$\in$ [1,5]: average feedback the others in subject i's social circle received	---	.084	.089	.095
		(.147)	(.120)	(.120)
(xiv) positive deviation of feedback in pd. t-1 × $relciv_{sc}$+ in pd. t-1	---	---	.008	---
			(.078)	
(xv) positive deviation of feedback in pd. t-1 × $relciv_{sc}$- in pd. t-1	---	---	-.154	---
			(.506)	
(xvi) negative deviation of feedback in pd. t-1 × $relciv_{sc}$+ in pd. t-1	---	---	.046	---
			(.515)	
(xvii) negative deviation of feedback in pd. t-1 × $relciv_{sc}$- in pd. t-1	---	---	-.064	---
			(.093)	
(xviii) positive deviation of feedback in pd. t-1 × $relciv_{S}$+ in pd. t-1	---	---	---	.020
				(.085)
(xix) positive deviation of feedback in pd. t-1 × $relciv_{S}$- in pd. t-1	---	---	---	-.299
				(.505)
(xx) negative deviation of feedback in pd. t-1 × $relciv_{S}$+ in pd. t-1	---	---	---	-.366
				(.601)
(xxi) negative deviation of feedback in pd. t-1 × $relciv_{S}$- in pd. t-1	---	---	---	-.419***
				(.100)
Constant	-1.111***	-1.253***	-1.281***	-1.414***
	(.260)	(.378)	(.334)	(.309)
# of Observations	3,024	3,024	3,024	3,024
# of left-censored observations	1,393	1,393	1,393	1,393
Wald χ^2	245.26	195.01	333.49	403.78
Prob > Wald χ^2	.0000***	.0000***	.0000***	.0000***
Maximum [mean] value of variance inflation factors[#1]	3.91	4.64	9.45[#]	11.21[#]
	[1.91]	[2.15]	[3.12]	[3.68]

Notes: Individual random effect Tobit regressions. Observations from *LowYes* and *HighYes* treatments, only. Number in parentheses are bootstrapped standard errors. The dependent variable is the number of civic tasks completed by subject i in period t. $relciv_{sc}$+ = max{own civic tasks – avg. civic tasks by other social circle members, 0}, $relciv_{sc}$- = max{avg. civic tasks by other social circle members – own civic tasks,0}, $relciv_{S}$+ = max{own civic tasks – avg. civic tasks by the 23 others, 0} and $relciv_{S}$- = max{avg. civic tasks by the 23 others – own civic tasks,0}. All observations in the *LowYes* and *HighYes* treatments when variable (vii) and (viii) are defined are used. All specifications control for randomness of scheme, see the following *Note on randomness controls* for details. [#] Concern for collinearity. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Note on randomness controls. In specifications (3) and (6) of the text's **Table 1** and in all specifications of **Table A.5**, we include controls for patterns of random draw outcomes to which subjects may have responded as noted by other studies of what is sometimes labeled a "hot hand phenomenon." In

periods 4 – 18 of our experiment, whether a session's subjects confronted the main stage PG problem with or without the aid of a penalty scheme was determined by the aggregate number of civic tasks they performed (T_c), but since that number lay in the 11 to 39 range in most periods, a random draw with probability (T_c – 10)/30 of a scheme resulting also helped to determine presence or absence of the scheme. Given that most subjects performed some civic tasks and that earnings of virtually all subjects were higher with than without the scheme, random draw outcomes yielding the scheme despite a probability < 1 may have been viewed as "lucky." At the same time, subjects would rationally prefer to achieve the scheme by doing as few civic tasks as possible because of the opportunity cost in the form of foregone private task earnings. The "hot hand" idea suggests in our context that a series of favorable random draw outcomes might encourage subjects to take a greater risk of scheme achievement failure by performing fewer civic tasks, and that a break in such a "streak" might conversely jolt them into increasing the number of civic tasks completed. (Psychologically, luck and civic tasks may have been perceived as substitutes in the production of the penalty scheme, thus when luck is perceived to be high, fewer civic tasks would be believed to be needed, and conversely when luck is perceived to have declined.) We checked for such effects by adding two sets of controls in the relevant columns of Table 1 and to all columns of Table A.5. First, we controlled for the level of "luckiness" in each period, defined as $1 – ((T_c – 10)/30)$ if there was a positive scheme outcome in the period and $– ((T_c – 10)/30)$ if there was a negative outcome. We included two controls thus calculated: one with the "luckiness" value of the most recent past period ($t – 1$) only, the other with the average of the "luckiness" values of all periods prior to ($t – 1$) in which a random draw had been required in the session. Including these two variables required that there be at least two prior periods, hence observations of the dependent variable begin with period 6 (leaving periods 4 and 5 as required lags). Second, we controlled for whether a series of either "lucky" or "unlucky" random draw outcomes had been "snapped" or "broken" in period $t – 1$, treating that as having occurred so long as the random draw outcomes of period $t – 2$ and $t – 1$ differed. We defined two control variables: *break+* if a no scheme outcome in $t – 2$ had been succeeded by a scheme outcome in $t – 1$, and *break-* if the converse occurred. Both controls were assigned the value 0 if the scheme state of $t – 2$ was the same as that of $t – 1$. If a break had occurred, the relevant break variable (either *break+* or *break-*) was assigned the integer value equaling the number of successive prior periods for which the scheme state had been the same as in period $t – 2$. For example, if a session experienced $0 < ((T_c – 10)/30) < 1$ (hence, need for a random draw) in multiple periods before period 11, if a scheme had been achieved in periods 7, 8, 9 and 10 but not in periods 6 and 11 of that session, then *break-* had value 4 and *break+* had value 0 in the regression for period 12 civic task completion. Defining the two 'break' variables also requires omitting observations from periods 4 and 5. In those few cases in which a random draw was not required in a particular session and period, we omit the session's observations for the following period because we cannot define the "luckiness" measure of $t – 1$ for it, and in later periods, we calculate the "luckiness" measure for periods leading up to and including $t – 2$ by simply leaving out the periods without draws when calculating the average. The number of periods prior to a "break" are calculated by counting from the last consecutive period having a random draw, only. We found significant or marginally significant coefficients on the "luckiness" controls and likewise on the "break" controls, especially break-. However, as Table 1 shows, adding these controls does not qualitatively change our other results. The full regression results are available on request.

Table A.6: *Average coefficient of variation (C.V.) of civic tasks completed, within and across social circles, plus ratios and trends*

	LowYes			*HighYes*			Pooled *Yes*		
	Avg. Coefficient of Variation			Avg. Coefficient of Variation			Avg. Coefficient of Variation		
period	within groups	across groups	ratio	within groups	across groups	ratio	within groups	across groups	ratio
4	1.03	0.37	2.78	1.34	0.25	5.28	1.19	0.31	3.79
5	0.89	0.25	3.59	1.03	0.42	2.45	0.96	0.33	2.87
6	0.87	0.27	3.18	1.09	0.47	2.35	0.98	0.37	2.66
7	0.76	0.31	2.50	1.21	0.40	3.04	0.99	0.35	2.80
8	0.77	0.32	2.40	1.16	0.44	2.65	0.96	0.38	2.54
9	0.86	0.40	2.15	1.18	0.49	2.41	1.02	0.44	2.29
10	0.81	0.30	2.74	1.03	0.41	2.49	0.92	0.35	2.60
11	0.81	0.42	1.93	1.08	0.45	2.42	0.95	0.43	2.18
12	0.84	0.38	2.23	1.05	0.54	1.93	0.95	0.46	2.05
13	0.87	0.44	2.00	1.13	0.40	2.80	1.00	0.42	2.38
14	0.92	0.46	2.00	1.07	0.50	2.16	1.00	0.48	2.08
15	0.85	0.43	1.96	1.14	0.45	2.52	1.00	0.44	2.25
16	0.90	0.48	1.87	1.17	0.50	2.32	1.04	0.49	2.10
17	0.87	0.48	1.82	1.22	0.50	2.45	1.05	0.49	2.14
18	0.92	0.52	1.78	1.24	0.54	2.29	1.08	0.53	2.04
reg. coef.†	0.000	0.016***	-0.101***	0.001	0.011**	-0.872	0.001	0.014***	-0.835***
	(.005)	(.003)	(.019)	(.007)	(.004)	(.060)	(.006)	(.001)	(.022)

† Regression coefficient for period on column variable in an OLS linear regression with robust standard errors. Standard error of coefficient in parenthesis. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Note: For each social circle and period, we calculated the C.V. of civic tasks completed by each subject, then calculated the average of these C.V.s, displayed in the left column for each treatment. For each session and period, we calculated the C.V. of the average of civic tasks in each of its four social circles, then calculated the average of these between-group C.V.'s for each treatment, displayed in the middle column. We also calculated the ratio of the latter two averaged C.V.s, displayed in the third column. Finally, we estimate an OLS regression of the averaged C.V.s and C.V. ratio of each period on the semi-continuous *Period* number and a constant, and we display the estimated coefficient on *Period* and its significance level at the bottom of the relevant column. Our conclusion, described in footnote 20 of the paper, is that there was no trend towards convergence of numbers of civic tasks completed by the members of given social circles, but that the numbers of civic tasks completed within different social circles of a session tended to become more different over time. This implies that although social circle members did not perfectly align on number of civic tasks performed, they exerted some influence on one another's behaviors because the choices within groups diverged over time relative to the choices within other groups. This kind of result would obtain if, for example, members of some groups exhibited rising, those of other groups constant, and those of still other groups declining numbers of completed civic tasks, with the passage of time, with each individual adjusting in the corresponding direction but retaining the same within group dispersion. Other patterns such as ones entailing "exchanging of place" between pairs within groups, and still more complicated variations, could also be consistent with the pattern of C.V.'s and with our conclusion.

Table A.7: *Tests of differences in percentages of subjects falling in different civic task completion bins of Figure A.4*

A. Testing the difference in the percentage of complete free riders (left-most bars in Figure 7)

	LowYes	*LowNo*	*HighYes*
LowNo	.082*	---	---
HighYes	.008***	.034**	---
HighNo	.008***	.008***	.056*

Notes: Two-sided *p*-values. We first counted the number of complete free riders for each session. We then performed session-level Mann-Whitney tests to study the treatment differences.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

B. Testing the difference in the percentage of 'super-cooperators' (those who completed more than two [including more than three] civic tasks per period)

	LowYes	*LowNo*	*HighYes*
LowNo	.671	---	---
HighYes	.393	.239	---
HighNo	.071*	.056*	.133

Notes: Two-sided *p*-values. We first counted the number of super-cooperators for each session. We then performed session-level Mann-Whitney tests to study the treatment differences.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

C. Testing the difference in the percentage of those who completed an average of more than one civic tasks per period (i.e., the number of completed civic tasks is more than 15)

	LowYes	*LowNo*	*HighYes*
LowNo	.130	---	---
HighYes	.018**	.595	---
HighNo	.008***	.140	.338

Notes: Two-sided *p*-values. We first counted the number of subjects who completed an average of more than one civic task per period for each session. We then performed session-level Mann-Whitney tests to study the treatment differences.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table A.8: *Correlations between Survey Responses and Behaviors*

Survey question: \ Behaviors:	Number of observations	Average number of civic tasks completed in Part 2 by a given subject[#1]	Contribution to the public sector in period 1 by a given subject
Female dummy {= 1 for female subjects; = 0 for male subjects}	469	.029 [.484]	.188*** [.000]
Econ major dummy {= 1 for economics students; 0 otherwise}	448	-.074* [.076]	-.198*** [.000]
Number of economics courses taken	472	-.073 [.132]	-.165*** [.000]
SAT math scores	369	-.006 [.919]	-.272*** [.000]
Level of interest in politics[#2]	480	.203*** [.000]	.033 [.466]
Political view (higher = more liberal)[#3]	480	.208*** [.000]	.110** [.016]
Keeps up with events[#4]	480	.172*** [.000]	.050 [.279]
Voted[#5]	415	.135*** [.010]	.007 [.893]
Civic norm strength[#6]	480	.081* [.073]	.054 [.236]
Sign or rally[#7]	480	.124** [.012]	.071 [.119]
Political engagement[#8]	480	.143*** [.002]	.088* [.054]
Trust in others' fairness[#9]	477	.066 [.247]	.090** [.048]

Notes: The partial correlation coefficients of specified behavior variables with survey responses. The numbers in square brackets are *p*-values. The number of observations is not equal to 480 in some questions because some subjects did not answer those questions. [#1] *p*-values in this column were calculated based on linear regressions (including constants) with robust standard errors clustered by session because subjects' civic engagement activities could have been correlated with each other. [#2] This variable scored from 1 = not at all interested to 4 = very interested. [#3] Political self-description from 1 = very conservative to 7 = very liberal. [#4] How much person follows political events via media, from 1 = almost never to 6 = multiple times per day. [#5] Conditional on being a citizen, voted (=1) or didn't vote (= 0) in the 2016 U.S. election. A substantial number of missing values is associated with the considerable numbers of international students at the university. [#6] How justified is cheating in one's favor in dealings with public sector? This has 3 components: claiming government benefits, avoiding paying fare on public transit, cheating on taxes. Each part is coded from 1 = always justifiable to 10 = never justifiable. The Civic norm strength variable averages the three scores. If any one of the three is missing, we average the other two scores. If two or three are missing, we treated the variable as missing. [#7] Average of two activities a person can "have done" (= 3), "might do" (= 2) or "would never do" (= 1). The activities are: signing a petition, and attending a rally. [#8] Average of the four activities: signing a petition, attending a rally, joining a peaceful demonstration, and joining a strike. 3 = "have done" and 1 = "would never do". [#9] Trust in others on a ten-point scale. *** Significant at 1%, ** 5%, * 10%.

II. Instructions used in the Experiment

[Instructions for Part 1:]

INSTRUCTIONS FOR PART 1

This experiment involves a set of decisions by 24 participants, yourself included, in which others' decisions can affect your earnings, and your decisions can affect their earnings. Whenever you are shown feedback on the decisions of others, their real identities will be kept anonymous, but please be assured that reported decisions are indeed those of the actual participants and never fictitious participants simulated by a computer program or members of the experimenter team.

No communication between participants will be permitted during the remainder of the experiment. You are also not permitted to use your phone, tablet computer, or programs other than the designated experiment software, to communicate with others or to look up information. Members of the experiment team will check that this rule is adhered to. You will have an opportunity to ask questions before actual decision-making begins. We ask that you devote your full attention to the experiment while it is in progress.

In the instructions and the experiment itself, we'll be using two different currencies or units of account. The first kind of currency, called **tokens**, is something you are given each period to allocate as you wish in order to earn the second kind, called **points**. Throughout the experiment, you can try to accumulate points. The more points you accumulate, the more money you will be paid (privately) at the end. The rate at which points convert to money (dollars) at the end of the experiment is 195 points = $1 (each point translates into about 0.5 cents). Your overall earnings will be calculated at the end of the session and paid to you in cash, to the nearest 5 cents. As you'll see below, while the value of a point is small, your total earnings can still be substantial. Please listen carefully to the instructions and ask questions if something is unclear.

<u>Decisions and earnings</u>

The main decision to be made and the main way in which you can earn points involves the allocation of resources between a private income-generating activity and a public sector. Allocating to your private activity is always beneficial to you, but private activity earns you more when the public sector is well funded. Not only does having a well-funded public sector raise the profitability of your private activity; a well-funded public sector also brings you direct benefits (in the experiment, points), similar to the benefits in everyday life from having safe roads, law and order, and clean air.

You and your fellow participants face the problem of how to fund your public sector. In the first part of the experiment, the only way to fund the public sector is through voluntary allocations. In the second part, in contrast, you can establish a government apparatus to enforce adequate funding of the public sector, paralleling the existence of taxes in real life. The specifics of how you and fellow participants can create this apparatus—which we'll be calling a "penalty scheme," during Part 2—will be explained after the first part of the experiment is over. We now provide further details about the allocation decision between the private activity and the public sector.

<u>More about the main allocation problem</u>

In each period, you and each other participant will be endowed with 20 tokens that you must decide how to allocate between two options. As mentioned above, one option is a private activity, the other the public sector that serves all 24 participants. You can assign any integer number of tokens (including zero) to the public sector, assigning the rest of that period's 20 tokens to your private activity. Examples include: 0 to the public sector, 20 to the private activity; 7 to the public sector, 13 to the private activity; 14 to the public sector, 6 to the private activity. These are among the twenty-one possible ways you can allocate your twenty tokens. Each of you makes an allocation decision with your own 20 tokens separately and simultaneously, learning of the others' decisions only afterwards.

The number of points you earn from tokens that you allocate to your private activity depends on the number of tokens put in the public sector in that period by you and the other participants. Suppose the number of tokens you use for private activity is b (for "business"), and the number you allocate to the public sector is p (for "public"). Since you always start with 20 tokens, it's required that $b + p = 20$. We'll call the sum of the p's chosen by all 24 participants P. The points of earnings you get out of your private business investment b are b*V, where V is the value that a token generates in your private activity. A key dimension of the decision problem is that V depends on P. The minimum value V can take is 5, its value when $P = 0$. As Figure 1 below shows, V rises as P goes up, reaching a maximum value of 17 when $P = 192$ (i.e. when the average p among the 24 participants is 8). Finally, V remains at the same value—17—for any $P > 192$. Table 1, on the page that follows, shows the values that V has at various levels of P (intermediate levels of P are omitted in order to conserve space).

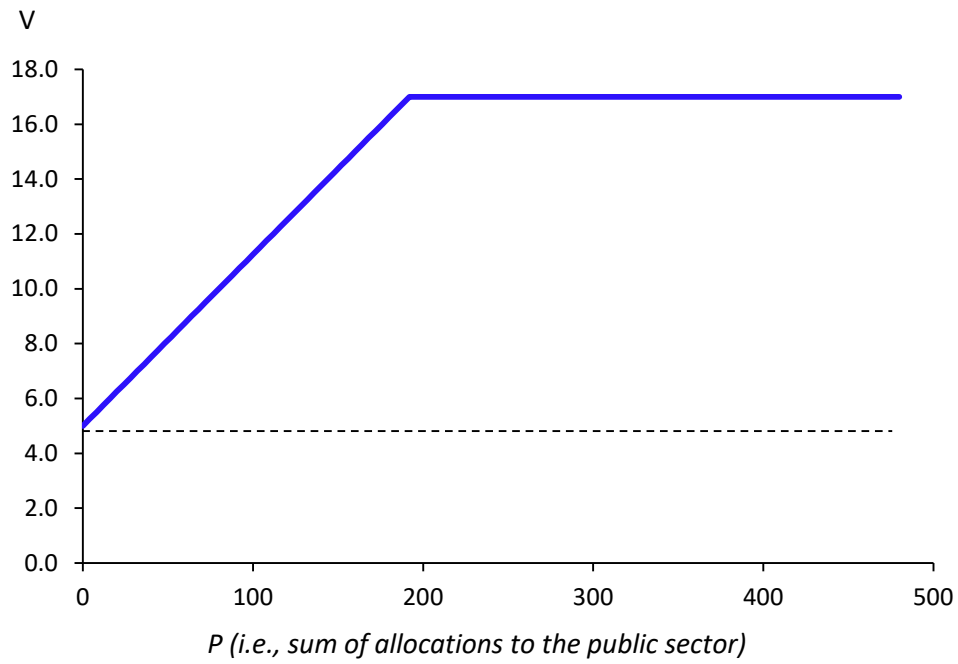


Figure 1. V as a function of *P*

P	0	20	40	60	80	100	120	140	160	180	200	220
V	5	6.3	7.5	8.8	10	11.3	12.5	13.8	15	16.3	17	17

P	240	260	280	300	320	340	360	380	400	420	440	460	480
V	17	17	17	17	17	17	17	17	17	17	17	17	17

Table 1: Value of V as a function of *P*

In addition to *P*'s effect on your earnings by influencing the value, V, of the tokens you assign to your private activity, *P* also affects your earnings in a second, more direct way. Each participant in the experiment receives a number of earnings points that rises as *P* does and that goes equally to participants without regard to differences in their individual choices of *p*. We'll call *D* (for "direct") the number of points that each participant gets directly given *P*. Using this terminology, we can say that an individual's total earnings in a period are (*b**V) + *D*, that is each participant gets his or her earnings from private activity (*b**V), influenced by *P* through its effect on V, plus *D*, his or her direct earnings from *P*.

The way in which *D* rises as *P* goes up is indicated by the curve in Figure 2, below. It shows that *D* rises slowly at first as *P* goes up from zero, then *D* rises more rapidly, then finally *D* rises slowly again as *P* approaches its maximum value of 100 points. Table 2, on the next page, indicates *D*'s numerical value at various levels of *P*.

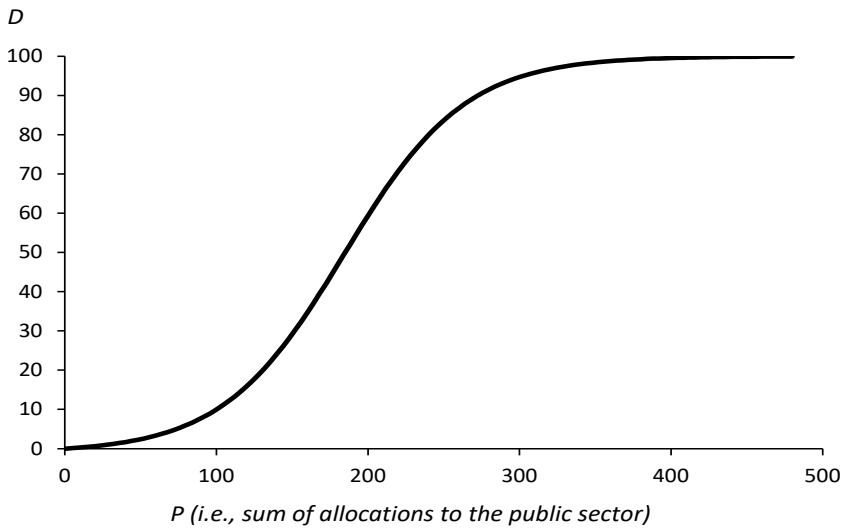


Figure 2. Direct earnings *D* as a function of *P*

P	0	20	40	60	80	100	120	140	160	180	200	220
D	0.0	0.6	1.7	3.3	5.9	10.0	15.9	24.1	34.7	46.8	59.3	70.7

P	240	260	280	300	320	340	360	380	400	420	440	460	480
D	79.9	86.8	91.6	94.7	96.7	98.0	98.8	99.2	99.5	99.7	99.8	99.9	99.9

Table 2. Direct earnings *D* as a function of *P*

The ways in which V (the return from each token used for private activity) and *D* (the direct benefit from the public sector) depend on *P*, plus the way you and others allocate the 20 tokens you are given each period, determine your overall earnings in a period. These relationships as a whole can be represented by a table in which the columns correspond to different allocations of tokens to the public sector by you and the rows correspond to different average allocations of tokens by the other 23 participants. To make the presentation more compact, the table shows only own and others' average allocations that are divisible by four.

	Own allocation to public sector					
Average allocation of 23 others	0	4	8	12	16	20
0	100	84	66	46	24	1
4	223	185	145	103	59	13
8	379	320	258	193	127	62
12	431	364	296	229	162	94
16	439	371	303	235	167	99
20	440	372	304	236	168	100

Table 3: Earnings as a function of your allocation (*p*) versus the average allocation *p* of the other 23 participants

We've shaded the diagonal entries of the table, which represent situations in which you and the others in your group happen to allocate the same amounts (or for the others, the same amount on average) to the public sector. For example, the entry *185* (second row from top, second column from left) is the total amount that you would earn if you allocated 4 of your 20 tokens to the public sector and 16 of your tokens to your private activity and if the other 23 participants also allocated an average of 4 tokens each to the public sector. Notice that among these shaded diagonal cells, the one in which your earnings would be highest is that in which you and the others on average allocate 8 tokens to the sector, giving you 258 points of earnings. That's more than two-and-a-half times your earnings if all participants put 0 in the sector, and the fact that it occurs when all allocate 8 tokens to the sector is consistent with the fact that V reaches its maximum value when *P* = 192 (= 24x8) (see Figure 1). It is also consistent with the fact that the rate of increase of *D* has begun to slow at *P* = 192 in Figure 2.

Two further things to note are the following. First, your earnings are not sensitive to *how* others allocations add up to a given average; any combination of choices by others that generates a given average has the same impact on your earnings. Second, what you earn <u>does</u> change if your own allocation varies, taking the average allocation of the others as given. For example, suppose that the others allocate an average of 8. You earn more by allocating less than 8 yourself, as shown by the cells to the left of the one with shaded value 258. The largest number in the table, 440, is what you would earn if others assigned all of their tokens to the public sector, while you used all of yours for your private activity.

The task of allocating a 20 token endowment between the two activities, and the figures and tables above describing the consequences of your own and others' decisions, apply not only to Part 1, which the current instructions focus on, but also to Part 2. As mentioned earlier, Part 2 will differ from Part 1 in that there will be a possibility of creating a penalty scheme to encourage allocations to the public sector. We leave details to be explained after Part 1 ends. Part 1 includes 3 periods, Part 2 has 15 periods, and in both parts each period has this kind of interaction at its core.

Operationally, each period of Part 1 will unfold as follows. You'll initially see a screen telling you the period number and indicating that you have 20 tokens to allocate. When you click continue, you'll be asked to decide how many (if any) of the 20 tokens you wish to allocate to the public sector (the rest automatically goes to your private activity). When you're satisfied with your decision, you click submit. When everyone has submitted their decisions, you'll see a screen showing your overall results for the period, and when you click continue you'll see a final screen showing the amount that each of the other 23 participants assigned to the public sector

this period, plus the amount that each of them earned. You can take a moment to absorb this information, then click continue to begin the next period.

Please take some time now to study the instructions so far, including the earnings table. Then try to answer the following four comprehension questions, which will also appear on your computer screen. Raise your hand if you have any questions, and an experimenter will come to you.

Comprehension questions:

1. What is it that is measured by the vertical dimension (axis) of Figure 1, above?
   a. The number of points you earn for each token you assign to your private activity (which varies with the total amount you and others put in the public sector, i.e. *P*).
   b. The number of points you earn for each token you assign to the public sector.
   c. Your total earnings during a period, which depends only on *P*.
2. Please test your understanding of Table 3. Suppose that you were to put 12 of your 20 tokens into the public sector and that the other participants put an average of 8 of their 20 tokens into the public sector. What would you earn in the period?
   a. 127 points. b. 193 points. c. 258 points. d. 296 points.
3. Suppose that you allocate 8 tokens to the public sector and that the other 23 participants allocate 312 tokens in total to the public sector. Answer the following questions:
   a. What is *b* (the number of tokens you allocate to your private activity)? _____
   b. What is V (the per-unit productivity of your private activity)? _____
   c. What are your earnings from your private activity (i.e., *b**V)? _____
   d. Each member receives the same direct benefit (i.e., *D*) from the public sector. What is *D* in this example? _____
   e. What are your total earnings (i.e., *b**V + *D*)? _____
4. Consider another situation. Suppose that you allocate 4 tokens and that the other 23 participants allocate 256 tokens in total to the public sector. Answer the following questions:
   a. What is *b* (the number of tokens used for your private activity)? _____
   b. What is V (per-unit productivity of your private activity)? _____
   c. What are your earnings from your private activity (i.e., *b**V)? _____
   d. Each member receives the same direct benefit (i.e., *D*) from your group's public sector. What is *D* in this example? _____
   e. What are your total earnings (i.e., *b**V + *D*) in this period? _____

As a reminder, you will have three interactions in this part. After these three periods, we will pause for instructions about the second part, which has fifteen periods. The experiment will begin when everyone is ready. Does anyone have any questions?

Please begin.

INSTRUCTIONS FOR PART 2

The remaining fifteen periods of the experiment have a core structure identical to those of the first three periods. That is, in what we'll henceforth call the "main stage" of each period, you and other 23 participants each decide how to allocate 20 tokens between your private activity and the public sector. However, whereas the allocation decision was strictly voluntary in Part 1, there is the possibility that, through some initial civic activity, you can **establish a government** that can alter the main stage problem by making contributing to the public sector a requirement that is subject to penalty if not fulfilled. The allocation to the public sector that would be required to avoid a penalty will be 8 of your 20 tokens, which, as you will recall, is the allocation at which total earnings of participants are maximized (see again Table 1 of the Part 1 instructions). Under the penalty scheme, if you assign no tokens to the public sector, your earnings will be determined as in Part 1 except that a penalty of 144 points will be assessed against your earnings. If you assign 4 tokens to the public sector, your penalty will be 72 points. More generally, you pay a penalty of 18 points for each token by which you fall short of allocating 8 tokens to the public sector. If you assign 8 or more tokens to the public sector, you will incur no penalty, so your earnings will be determined exactly as in Part 1. Points lost to penalties are simply lost from your earnings; they are not transferred to other participants.

Earnings when a penalty scheme is in place are illustrated by Table 3' (like Table 3, examples of allocations divisible by 4 only are shown, for brevity). The numbers with strikethrough are earnings in the absence of the penalty scheme and the numbers immediately below them are the earnings after subtraction of the penalty.

Average *p* of 23 others	Own allocation to the public sector					
	0	4	8	12	16	20
0	~~100~~ -44	~~84~~ 12	66	46	24	1
4	~~223~~ 79	~~185~~ 105	145	103	59	13
8	~~379~~ 235	~~320~~ 248	258	193	127	62
12	~~431~~ 287	~~364~~ 292	296	229	162	94
16	~~439~~ 295	~~371~~ 299	303	235	167	99
20	~~440~~ 296	~~372~~ 300	304	236	168	100

Table 3'. Earnings as a function of your allocation (p) and the average allocation of the other 23 participants. Numbers below "struck through" numbers are your earnings net of penalties.

When is the scheme present?

As stated above, having a penalty scheme to enforce tax obligations implies establishing a government. In the experiment, as perhaps in the real world, it takes some **civic engagement** to establish a well-functioning government responsive to citizens' interests. Examples of civic engagement in the real world include reading or listening to information about public affairs, signing petitions, voting in elections, etc. Each of the fifteen periods of Part 2 will include an extra stage before the main stage—we'll call it the "pre-stage"—during which you'll have the opportunity to engage in an activity—completing "**civic tasks**"—that is a way of representing civic engagement for purposes of the experiment. You and your fellow participants can assure that you'll have a penalty scheme in place in a period's main stage by carrying out enough of these tasks. During the pre-stage, "**private tasks**" that add to your personal earnings without helping to build governing capacity will also be available as another way of using your time. Each task, whether civic or private, takes about 10 - 15 seconds to complete, and a total of 90 seconds will be available each period for the task portion of the pre-stage. Each correctly completed private task yields ten [twenty-two] points of earnings. Such earnings points are added to your overall accumulation and convert to real money at the same rate as other points at the end of the experiment. They do **not** affect how many tokens you have available to allocate in the period's main stage; that number remains 20, regardless of how many tasks you complete. Completing a **civic** task adds ten points to a "**civic engagement fund,**" and the number of points in that fund (or, to put it differently, the total number of civic tasks completed by all 24 participants) determines whether there will be a penalty scheme operating in the period's main stage. If you and other participants complete 40 or more civic tasks in total (a little less than 2 per person, on average, putting 40x6 = 240 points in the civic engagement fund), the penalty scheme will be in place during the main stage. If none of you completes any civic tasks, or if only a small number (10 or less) of civic tasks are completed, there will definitely not be a penalty scheme in that period. In that case, the main stage of that period will proceed exactly as in Part 1. In between the no penalty situation (10 or less completed civic tasks) and the penalty-with-certainty situation (40 or more completed civic tasks), presence of a penalty scheme is determined by the exact number of tasks completed and by a random draw procedure. Specifically, if the total number of civic tasks completed, call it n, is between 11 and 39, then the computer will implement a random draw with probability $(n - 10)/30$ that there will be a penalty scheme in place in the period's main stage. For instance, if a total of 20 civic tasks are completed, there will be a random draw with a $(20 - 10)/30 = 1/3$ chance that a penalty scheme will be in place, leaving a 2/3 chance that there will be no penalty scheme. If 25 civic tasks are completed, the chance of having the penalty scheme will be $(25 - 10)/30 = ½$ (= 50%), with equal chance of having no penalty scheme. With 35 civic tasks completed, the probability of having the scheme in that period is $(35 - 10)/30 \approx 83\%$. And so on. When a random draw is required, it will be carried out at the end of the pre-stage part of the period,

with a spinning roulette-wheel-like image appearing on your screen and the outcome (i.e., whether there will or will not be a penalty scheme in that period) then being displayed before the main stage begins. The period's main stage always takes place with full knowledge, shared identically by all 24 participants, as to whether a penalty scheme is in place or not.

Information sharing and feedback. [this section appears only in high social interaction treatments]

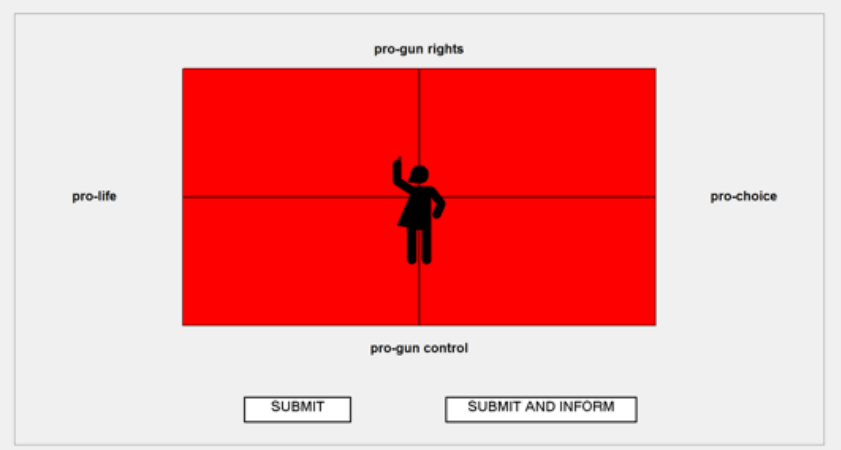
In the real world, you might wish to share with others the fact that you registered to vote, went to the polls, read up on candidates' positions, or took part in some other civic activity. Sharing with others information about your completion of civic tasks is also possible in the experiment. In particular, each of you will be a member of a social circle or group which consists of yourself and 5 other randomly chosen participants. Its membership will remain fixed for the remaining periods. Each time you complete a civic task and are ready to submit your work (generating points for the civic engagement fund), you can click a button that says "Submit," or you can instead click (at no extra cost) a button labeled "Submit and Inform." If you chose the latter button, an announcement that you've completed a civic task will be shown to your fellow group members the next time they visit the task choice screen. Each group member is identified by a letter (A, B, C, D, E or F) that is assigned randomly and remains fixed for the rest of the experiment. (Which participants belonged to each group, and who was associated with which letter, will not be revealed either during or after the experiment.) Just before the end of the pre-stage, the total number of civic tasks completed by each member of your group will be displayed next to their identification letter—this time, regardless of whether the individual used the "inform my group" option. The pre-stage will end with an opportunity to provide feedback to the others in your group (social circle), and for them to do the same to you. Specifically, you will be asked to select one of five possible comments: strongly disapprove, disapprove, neutral (neither approve nor disapprove), approve, or strongly approve. On the final pre-stage screen, you'll then be shown what feedback other group members submitted about you.

More about pre-stage tasks.

When a Part 2 period begins, always with its pre-stage, you'll see a screen on which you select whether the first task you want to do will be a private or a civic task. Once you click on your choice, you'll begin that task. The private and civic tasks are very similar. Each task begins with a description of a person differing in two dimensions or characteristics. When you click continue, you'll see a two-dimensional grid. There, you'll left click on a person-shaped icon, drag the icon to whichever of the four quadrants corresponds to the description, drop it in place (you'll see the chosen quadrant turn blue, while the remaining quadrants remain red), and submit that decision. [[Local social interaction treatments only:] For private tasks, you do that by simply clicking on the "Submit" button; for civic tasks, you can click on either the "Submit" button or on the "Submit and Inform" button, keeping in mind that the latter

generates a message to those in your group (social circle).] In the civic tasks, you'll be identifying the position of a public official or politician with regard to two issue dimensions, for example importance of environmental protection, importance of cutting taxes, importance of defense spending, etc. Note that you cannot go back from the grid screen to view the verbal description, although you are free to take notes to help you remember it. If you answer incorrectly, you will be offered the chance to try again beginning with rereading that description. You will also be offered the alternative option of moving on to a fresh task of either civic or private type.

Example. You'll see a description reading: "Senate candidate Wendy White favors unrestricted gun ownership and is committed to a woman's right to choose whether to continue or to terminate a pregnancy." You click continue and see a grid with axes labeled "pro-gun rights"/"pro-gun control", and "pro-life"/"pro-choice" (see below). To earn the available ten [twenty-two] points, you left click on the icon and drag it to the pro-gun and pro-choice quadrant, drop it in place, and submit your answer. A pop-up on the screen tells you if your answer is correct and prompts you to return to the screen at which you choose another civic or private task. When you return to that screen, it will be updated to show how many private and how many civic tasks you've completed in the period, thus far [[High social interaction treatments:], and will display messages about others' civic task completions if they've used the "Submit and Inform" option].



In the private tasks, you'll be identifying the "market position" of a consumer with regard to two features of his or her shopping or purchasing preference; for example, feature one can be preference for restaurant versus home meals, and feature two can be preference for gourmet dishes versus simple foods.

Example. "John Smith eats out frequently at a local diner." You click continue and see a grid with axes labeled "home"/"restaurant" and "gourmet"/"plain". You left click on the icon and drag it to the restaurant and plain quadrant, drop in place, and submit your answer. As with a

civic task, you'll see a pop-up indicating whether your answer is correct. If incorrect, you can choose to try again or go on to another task, exactly as with civic tasks.

Note that at both the screen showing the description of the public official or consumer and the screen showing the four quadrant grid, the experiment software requires you to spend a minimum of three seconds before you can continue or submit your answer. This time requirement is to encourage you to pay attention to the tasks, rather than engage in random clicking.

As mentioned, the tasks part of the pre-stage will last for a total of 40 seconds. When that time runs out, you'll see a screen saying "Time's up!" and you'll be informed of the number of civic tasks completed by each of the five others in your group or social circle. You'll be asked then to give feedback to each of them from among the five options mentioned above (strongly approve, approve, neutral, disapprove, and strongly disapprove). On the next screen, you'll be shown the average feedback you yourself received from them. The pre-stage then ends with a screen on which you learn how many civic engagement tasks were completed in total (from all 24 participants, combined). The spinning wheel indicating random choice will be shown if applicable, and you'll learn whether a penalty scheme will be in place in the main stage of the period. Then, once you click continue, you'll go to the main stage, which will work exactly as in the earlier periods except when there is a penalty scheme.

Before Part 2 begins, please answer the comprehension questions which will also appear on your computer screen. Raise your hand if you have any questions and we'll come to you to help provide answers. [Answers shown in brackets.]

1. There will be fifteen periods in this second and final part of the experiment. How many of these periods will begin with a pre-stage involving private and civic tasks?
   a. Each period will begin with a pre-stage. [*]
   b. Only the first period begins with a pre-stage.
   c. A few randomly chosen periods have pre-stages.

2. Suppose that the penalty scheme is in place during a period's main stage. How many points will you be losing as a penalty if you assign 5 tokens to the public sector? _____ [(8 – 5)x18 = 3x18 = 54]

   How many points will you lose as a penalty if you assign 9 tokens to the public sector? _____ [no points]

3. Suppose that in a certain period each participant completes exactly two civic tasks. Which best describes the effect on presence or not of a penalty scheme in the period's main stage?

a. There will definitely be a penalty scheme. [*]
b. There will be a random draw to determine whether there is a penalty scheme.
c. There will definitely not be a penalty scheme.

4. Suppose that in a certain period each participant completes exactly one civic task. Which of the following applies?
   a. There will definitely be a penalty scheme.
   b. There will be a random draw with probability (24/30) that there will be a penalty scheme.
   c. There will be a random draw with probability (24 – 10)/30, i.e. 14/30, that there will be a penalty scheme. [*]
   d. There will definitely not be a penalty scheme.

5. For each private task you complete during the pre-stage, how many points are added to your earnings? _____ [10 or 22, depending on treatment]

6. Suppose that, based on the number of civic tasks completed in a certain period, there is a probability of 21/30, or 70%, that there will be a penalty scheme in the main stage. When will you be informed of the outcome of the random draw regarding the penalty scheme?
   a. Whether there will or will not be a penalty scheme this period is indicated at the end of the pre-stage. [*]
   b. The result of the random draw is announced only at the end of the main stage. Main stage decisions themselves are therefor made with knowledge of the 70% chance of penalty, but not knowing whether the penalty in fact applied until the end of the period.

III. Private and Social Optimum in main-stage allocation problem without sanction

A. Optimal allocation decision of a material payoff maximizer

The payoff of subject *i* in the main stage is given by:

$$Y_i(p_i; p_{-i}) = b_i * V(P) + D(P)$$

$$= (20-p_i)*(5 + (1/16)P) + \frac{101}{1+100\text{Exp}[-0.025*P]} - 1 \text{ for } P < 192;$$

$$= (20-p_i)*17 + \frac{101}{1+100\text{Exp}[-0.025*P]} - 1 \text{ for } P > 192.$$

Here, p_i (b_i) indicates *i*'s allocation to public activities (business), $p_i + b_i = 20$, p_{-i} is the sum of the others' allocations to public activities, and $P = p_i + p_{-i}$. As $V(.)$ has a kink at $P = 192$, we need to consider two cases as below. In each of the two cases, $\frac{\partial Y}{\partial p_i}$ is negative always, regardless of the size of p_{-i}. This suggests that the situation *i* faces is a social dilemma.

Case 1: When $P < 192$:

$$\frac{\partial Y}{\partial p_i} = -5 - \frac{P}{16} + (20 - p_i)\frac{1}{16} + 101\frac{2.5\text{Exp}[-0.025*P]}{(1+100\text{Exp}[-0.025*P])^2}$$

$$= -3.75 - \frac{p_{-i}}{16} - \frac{p_i}{8} + 101\frac{2.5\text{Exp}[-0.025*P]}{(1+100\text{Exp}[-0.025*P])^2} < 0 \text{ for any } p_i.$$

This is because $101\frac{2.5\text{Exp}[-0.025*P]}{(1+100\text{Exp}[-0.025*P])^2} < 2.5.$

Notice that $101\frac{2.5\text{Exp}[-0.025*P]}{(1+100\text{Exp}[-0.025*P])^2} = \frac{101}{1+100\text{Exp}[-0.025*P]}\frac{1}{100}\frac{1+100\text{Exp}[-0.025*P]-1}{(1+100\text{Exp}[-0.025*P])} \cdot 2.5.$

Here, $\frac{101}{1+100\text{Exp}[-0.025*P]}\frac{1}{100} < 1$, and $\frac{1+100\text{Exp}[-0.025*P]-1}{(1+100\text{Exp}[-0.025*P])} = 1 - \frac{1}{(1+100\text{Exp}[-0.025*P])} < 1.$

Case 2: When $P \geq 192$:

$$\frac{\partial Y}{\partial p_i} = -17 + 101\frac{2.5\text{Exp}[-0.025*P]}{(1+100\text{Exp}[-0.025*P])^2} < 0.$$

Notice that as discussed in Case 1 above, the second term is less than 2.5.

These calculations show that it is materially beneficial for *i* to contribute zero to the public sector, regardless of the size of p_{-i}.

B. Social optimum

As is the case for Part A, we need to consider two cases.

Case 1: When $P < 192$:

$$\sum_{all\ j} Y_i\,(p_i; p_{-i}) = \sum_{all\ j}\left[(20 - p_i)\cdot(5 + P/16) + \frac{101}{1+100\mathrm{Exp}[-0.025*P]} - 1\right]$$

$$= (20N - P)\cdot(5 + P/16) + \frac{101N}{1+100\mathrm{Exp}[-0.025*P]} - N\,,$$

where $N = 24$ (the number of players in a given session).

$$\frac{d}{dP}\sum_{all\ j} Y_i\,(p_i; p_{-i}) = -1\cdot(5 + P/16) + (20N - P)\cdot 1/16 + 101N\frac{2.5\mathrm{Exp}[-0.025*P]}{(1+100\mathrm{Exp}[-0.025*P])^2}$$

$$= -\frac{P}{8} + 25 + 101N\frac{2.5\mathrm{Exp}[-0.025*P]}{(1+100\mathrm{Exp}[-0.025*P])^2}.$$

Clearly, $\frac{d}{dP}\sum_{all\ j} Y_i\,(p_i; p_{-i}) > 0$ always, because $\frac{2.5\mathrm{Exp}[-0.025*P]}{(1+100\mathrm{Exp}[-0.025*P])^2} > 0$ and $-\frac{P}{8} + 25 > 0$ when $P < 192$.

Case 2: When $P \geq 192$:

$$\sum_{all\ j} Y_i\,(p_i; p_{-i}) = \sum_{all\ j}\left[(20 - p_i)\cdot 17 + \frac{101}{1+100\mathrm{Exp}[-0.025*P]} - 1\right]$$

$$= (20N - P)\cdot 17 + \frac{101N}{1+100\mathrm{Exp}[-0.025*P]} - N.$$

$$\frac{d}{dP}\sum_{all\ j} Y_i\,(p_i; p_{-i}) = -17 + 101N\frac{2.5\mathrm{Exp}[-0.025*P]}{(1+100\mathrm{Exp}[-0.025*P])^2}.$$

Here, the second term (the slope of the logistic function times *N*) has a maximum when P = 192 since the inflection point is less than 192. As $101N\frac{2.5\mathrm{Exp}[-0.025*P]}{(1+100\mathrm{Exp}[-0.025*P])^2} = 1.065$ when $P = 192$, $\frac{d}{dP}\sum_{all\ j} Y_i\,(p_i; p_{-i}) < 0$.

These calculations show that the social optimum occurs when $P = 192$. Panel (a) of Figure 2 in the main text shows the graph of $\sum_{all\ j} Y_i\,(p_i; p_{-i})$ as a function of *P*.