

The Demand for Bad Policy When Voters Underappreciate Equilibrium Effects

Ernesto Dal Bó

Pedro Dal Bó

Erik Eyster

UC Berkeley

Brown University

LSE*

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Abstract

We study whether people can fail to choose efficient policies (or institutions) and the reasons why such failure may arise. More precisely, we experimentally show that a large proportion of people vote against policies that would help them overcome social dilemmas. In addition, we show that this is linked to subjects failing to fully anticipate the equilibrium effects of policies. By eliciting subjects' beliefs about how others will behave under different policies, we show that inaccurate expectations of equilibrium behavior of others affect voting. In addition, relying on a structural approach, we find a significant share of subjects who do not anticipate how their own behavior will change with policy. This combined failure to anticipate the equilibrium consequences of policy drives a full majority, on average, to support bad policies, placing an important hurdle for the ability of groups to resolve social dilemmas through democratic means.

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1 Introduction

The political economy field has developed several explanations for why bad policies are implemented when good policies are available. Standard explanations blame aspects of the policy production process including agency problems, incompetent policy-makers, and prevailing institutions failing to efficiently resolve competitive tensions.¹ In this paper, we shift the focus away from policy-makers and institutions, and provide experimental evidence showing that part of the blame for bad policy may lie with the voters. While it is standard in the literature to assume that voters tend to be correct about the relative value of the options they face, we show with a simple experiment that people may demand bad policies as they may fail to correctly predict the equilibrium impact that policies have on behavior and welfare.

We present data from an experiment in which subjects had to choose whether to participate in a prisoners' dilemma game or an alternative game under which both cooperation and defection are taxed but the latter is taxed more. In this alternative game cooperation is a dominant action and it leads to higher payoffs than the prisoners' dilemma game, both in theory and in practice.

While subjects do on average earn higher payoffs under the alternative game than under the prisoners' dilemma, a significant share of them (more than 50%) choose to play the prisoners' dilemma game. This result is robust to different voting institutions and orders of play. The fact that a large share of subjects choose to play the prisoners' dilemma game

¹Agency problems include the phenomenon of discretion under limited electoral accountability (e.g., Barro 1973, Ferejohn 1986) and capture (e.g., Stigler 1971, Peltzman 1976, Coate and Morris 1995). For accounts of why inept types may self-select into policymaking see among others Dal Bó and Di Tella (2003), Caselli and Morelli (2004), Besley (2005), Dal Bó, Dal Bó and Di Tella (2006), Polborn (2006). Institutional failures to efficiently resolve collective disagreements include the possibility of status quo bias (e.g., Romer and Rosenthal 1978), delay to reform (e.g., Alesina and Drazen 1991, Fernandez and Rodrik 1991), and dynamic inefficiency due to the threat of losing political control (e.g., Alesina and Tabellini 1990, De Figueiredo 2002, Besley and Coate 2007).

is evidence that they fail to appreciate the equilibrium effects of the change in the payoff matrix.

This failure may be driven by two forces: subjects may fail to understand that others will behave differently under the two games, but subjects may also fail to understand that they themselves will behave differently under the two games. We show that subjects underappreciate the extent to which others will respond to the change of game: subjects who choose to play the prisoners' dilemma expressed a lower belief that the behavior of others would be different across games. In addition, relying on the estimation of a simple structural model of voting, we also show that some subjects may fail to understand that they will themselves behave differently across the different games.

Our emphasis on the potential inability of voters to fully appreciate equilibrium effects relates to long-standing concerns about the soundness of voter demands that, although plausible, remain relatively unexplored empirically, at least from the perspective of equilibrium. To be sure, writers going back to Adam Smith emphasized the limitations of the general public to grasp the implications of market equilibrium considerations. In modern times, North (1990) surmised that voters might misperceive the relative merits of different policies and institutions, and hence demand suboptimal institutions. A vast literature in political science relying on surveys has devoted attention to several factors shaping voter behavior, such as partisan bias (see Bartels 2008 for a survey). Scholars have also studied the role of insufficient voter information and shown it to have likely swung important elections (Bartels 1996). Caplan (2007) surveys a literature more pointedly focused on divergences between voter and expert opinion, and provides additional evidence of a gap between the anti-market and anti-trade leanings of popular opinion and the advice stemming from the economics profession. In this paper we are concerned with a very specific aspect of voter misperception, namely the inability to appreciate equilibrium effects, which may underlie some of the many

shortcomings that scholars have attributed to voters.

The gap between voters and experts would not be of concern if policy-makers were to follow the advice of experts and educate the public when appropriate. But as Blinder and Krueger (2004, p.328) emphasize, even on matters admitting a technical answer “*the decisions of elected politicians are heavily influenced by public opinion,*” a fact they corroborate by reference to the “*tremendous resources that politicians devote to assessing public opinion*” and with evidence from political science. Consistent with this view, by focusing on the rigors of electoral discipline, most formal theories of electoral politics display politicians not as educators, but as catering to voter policy positions, even when voters are likely wrong, as in the literature on pandering (Canes-Wrone, Herron and Shotts 2001, Maskin and Tirole 2004).

The hypothesis that voters may on average misjudge the policy options before them is at odds with that in voting models studying the virtues of elections at aggregating preferences and information (see among many others Grofman, Owen and Feld 1983, Austen-Smith and Banks 1996, Feddersen and Pesendorfer 1997). We show that people may have systematic bias in the evaluation of policies; they may focus on the direct effect that a policy has on their own welfare while holding behavior constant, and pay less attention to the indirect effect that a policy has through its impact on subsequent behavior.

Our study connects with a small body of work emerging in political economy that emphasizes behavioral aspects broadly understood. Examples are the study of the impact of cognitive dissonance on voting (Mullainathan and Washington 2009), the analysis of collective action with time-inconsistent voters (Bisin, Lizzeri and Yariv 2011, Lizzeri and Yariv 2012), the behavior of voters that face learning difficulties and fail to extract the right information from the strategies played (Eyster and Rabin 2005, Esponda and Pouzo 2010) and failures on pivotal voting (Esponda and Vespa 2012). In this project we study a simple set-

ting with complete information, which therefore lacks the inferential complexities of some of the previous studies of voting. In our setting, elementary predictions of equilibrium behavior would suffice to advise players to vote correctly – the difficulty is posed by the fact that basic policies, by constraining each individual and thus lowering payoffs *ceteris-paribus*, may in fact achieve welfare gains in equilibrium by changing behavior.

This paper also relates to the growing experimental literature studying the choice of self-regulatory institutions (see Dal Bó (2011) for a survey). While recapping that survey would be excessive, a few findings in that literature are worth highlighting here. One emerges in the context of common-pool problems where players would do better by reducing extraction rates, and where they can put extraction rules to a vote. Walker, Gardner, Herr and Ostrom (2000) find that not all voters propose efficient extraction rules, and that the voting protocol affects the ability to reach a welfare-increasing decision. This failure is enhanced in contexts where subjects are heterogeneous (Magreiter, Sutter, and Dittrich 2005). In addition, Dal Bó (2011) offers evidence that the way in which subjects comprehend their strategic situation could affect their ability to select institutions. In this paper we specifically focus on the underappreciation of equilibrium effects and show it to impair the ability of groups to resolve social dilemmas through democratic means. Like most of these papers, we focus on choices involving an unfamiliar option, which matches institutional reforms or policy choices that are not frequently available. Examples are constitutional changes, privatizations, sweeping health-care reform, or decisive action against global warming.

The plan for the paper is as follows. In the next section we describe the experimental design. In section 3 we establish a benchmark by demonstrating that the alternative game does lead to higher payoffs than the prisoner’s dilemma. In section 4 we present our results and substantiate the underlying mechanisms. In section 5 we make further use of our data to rule out alternative explanations, and we conclude in section 6.

2 Experimental design

We begin by explaining the basic structure of experimental sessions in all six treatments and then describe the differences across the treatments.

Part 1 of the experiment divided subjects into groups of six. Every subject played against every other one in the group exactly once in part 1, resulting in five periods of play in this part of the experiment. The game played varied by group. Groups were randomly assigned to play the Prisoners' Dilemma (henceforth, PD) or the Alternative Game (henceforth, AG), detailed in Table 1. The exchange rate was \$1 per 3 experimental points.

After Part 1, new groups of six were formed randomly for Part 2, which included another five periods of play. At the beginning of Part 2, the game to be played in the next five periods was chosen. One of the main treatment variables is the way in which this choice was made as described below. After the choice of game for Part 2, but before Period 6, subjects reported their beliefs about how a randomly selected opponent in a similar experiment would act in each of the two games. As in Part 1, every subject interacted with every other subject exactly once (Periods 6 to 10).

The two main treatment variables are the game that subjects played in Part 1 and the mechanism used to choose the game for Part 2 – see Figure 1. The treatments labeled Control, Random Dictator, Majority and Majority Once had the subjects play the PD game in part 1, while Reverse Control and Reverse Random Dictator had the subjects play the alternative game in Part 1.

In the control treatments (Control and Reverse Control), the game for Part 2 of the experiment was chosen at random by the computer. This choice was done once at the beginning of Part 2, and applied for all players and all periods (i.e. all subjects in a given group played the same game in all periods in Part 2). The treatments Random Dictator and

Reverse Random Dictator differed from the controls by asking all subjects to choose between the two games at the beginning of Part 2 and then implementing for the group the choice of a randomly selected subject. In the Majority treatment, the game chosen by the majority of the group before Period 6 was implemented for all periods in Part 2. Ties were randomly broken by the computer. In the Majority Repeated treatment, subjects voted for a game before each period of Part 2. In this treatment, beliefs were not elicited so as not to affect voting behavior in future periods. In all the other treatments, the belief elicitation always occurred after voting, so as not to affect voting. Subjects were informed of the implemented game and not the voting distribution.

At the end of the experiment, subjects played a p-beauty contest (Nagel 1995) to assess their strategic sophistication in simultaneous-move games and filled out a questionnaire providing basic demographics (gender, political ideology, class, major and SAT scores).

We recruited 384 student subjects from UC Berkeley and 384 from Brown University to participate in the experiment. Table 2 shows the number of subjects from each university in each of the six treatments. Sessions lasted around half an hour and earnings ranged from \$16.75 to \$37 with an average of \$27.81 (earnings included a \$5 show-up fee). The Appendix Table 1 displays summary statistics of demographics and beliefs.

3 Benchmark: Does the alternative game lead to higher payoffs than the prisoners' dilemma?

In order to establish whether voters demand efficient policies as captured by the alternative game, we first need to establish that the alternative game leads to higher payoffs than the PD. Clearly, this is the prediction from game theory: subjects have a dominant strategy to defect in the PD and to cooperate in the alternative game. Hence, any solution concept that

assumes players are rational predicts defection in the PD and cooperation in the alternative game (e.g., Nash equilibrium or rationalizability). Thus, the standard theoretical prediction is that play will match the unique Nash equilibrium in each game, and that the alternative game, displaying higher equilibrium payoffs, represents the efficient policy. If subjects played, but also chose, games according to theory, the alternative game should always be selected over the PD. This prediction is in fact quite robust to players having beliefs about the behavior of others that are not exactly pinned down by Nash equilibrium.

Denote with b_k the beliefs a person has about others probability of cooperating in game $k = PD, AG$, and let $\Delta b \equiv b_{AG} - b_{PD}$. Assuming that subject care linearly about monetary payoffs and that they will play the dominant strategy in each game, the expected utility differential from the two games is:

$$EU_{PD} - EU_{AG} \equiv \Delta EU = -6\Delta b + 3. \quad (1)$$

Thus, a rational subject should prefer the alternative game if it increases the probability of cooperation, relative to the PD, by more than fifty percentage points ($\Delta b > \frac{1}{2}$), rather than the hundred points predicted by Nash equilibrium.

But do subjects play close enough to the Nash outcome in each game so that payoffs and cooperation are greater in the alternative game than in the PD? The answer is yes. Figure 2 shows the evolution of cooperation and payoffs as a function of the randomly chosen game for Part 2 in the Control and Reverse Control Treatments. The figures show that while there are no significant differences in behavior or payoffs in Part 1 by allocation of game in Part 2 (consistent with its allocation being random), behavior and payoffs differ significantly by game in Part 2. Cooperation and payoffs go up when moving from the PD to the AG, and down when moving from the AG to the PD. In the Control condition, the shift from the

PD to the AG raises cooperation rates from 16% to 92% (see Appendix Table 2). While this increase is lower than the 100% predicted by Nash equilibrium, it is well above what a rational player would require to prefer AG over PD. The changes in behavior and payoffs are large even in the first interaction in Part 2 (Period 6), and are significant at less than 1% if we consider all periods and significant at less than 5% if we only consider Period 6.

Similar comparisons hold for the other treatments, where the choice of payoff matrix is not random (see Figure A1 and Appendix Tables 2 and 3).

Another way to see that behavior across games differs in the direction predicted by theory is to compare the cooperation rates across the two games in the 5th period, when the players have already gained experience. If we pool across all treatments where the subjects start to play the PD and the AG respectively, we find that the cooperation rate in the PD is 15.5% while that in the AG is 95.3%. The corresponding average payoffs are 5.62 and 7.65, respectively. Thus in period 5 the AG yields 36% higher payoffs.

In conclusion, behavior and payoffs across the two games vary in the direction predicted by standard game theory. Thus, voting against the alternative game will result in lower payoffs in practice as well as in theory. Having established that people play games according to theory, the next question is whether they choose games accordingly.

4 The demand for bad policy

While voting for the alternative game is unambiguously better for the subjects, a slight majority of subjects (53.60%) across treatments voted for the PD game. The lowest share of subjects voting for the PD game is 50% under Reverse Random Dictator and the largest is 60.83% under Majority Once – see Table 3. These shares are all significantly different from the 0% that could be expected if subjects chose games according to theory.

The main result of the paper is that a majority of subjects demanded the bad game or policy. This resulted in a majority of subjects (54.55%) having to interact in a game that led to lower payoffs than they would have achieved had they voted for the alternative game. The tendency of subjects to support bad policy is remarkably stable across treatments that vary the decision mechanism and timing – we will compare the voting shares across treatments later in the paper.

In addition, we find that there is a significant demand for the bad policy even after subjects gained experience. Figure 3 shows the evolution of votes under Majority Repeated. The percentage of subjects voting for the PD decreases from 50.83% in Period 6 to 28.33% in Period 10. As this lower percentage can only rarely yield a majority for the PD, the percentage of subjects playing the PD decreases from 45% in Period 6 to 10% in Period 10. Although the proportion of subjects choosing the PD decreased significantly with repetition, even after several periods more than a quarter of voters continued to choose the wrong game.

Note again, that none of the other explanations for the implementation of bad policies are applicable in the simple environments of this experiment. As such, the responsibility on the failure to vote for good policies can only be attributed to the subjects, the citizens of this environment. But, what explains that such a large share of subjects demand bad policies?

4.1 Mechanism: failure to appreciate equilibrium effects

The reason that many subjects fail to vote for the alternative game is that they fail to understand or predict the effect that the payoff matrix has on behavior. Figure 4 shows the distribution of the belief of cooperation difference between the the alternative and the PD game. We find that in average subjects underestimate the effect of the game change in behavior. The average difference in belief of cooperation between the alternative and the PD game is 35% in Random Dictator and Majority Once while the true difference is of

almost 76%. Similarly, the average belief difference is 30% in Majority Once while the true difference in behavior is 63%.

Moreover, subjects who underestimate more the effect of the game on behavior are more likely to vote for PD. This is clear from Figure 5, which shows the average elicited belief of cooperation in each game by vote of the subject. In all three treatments in which beliefs were elicited, subjects who voted for the PD expressed a lower belief that the behavior of others will be different across games. That is, subjects who voted for the PD have a lower estimate of the effect of the payoff matrix on behavior. Notice that if you expect behavior to be independent of the payoff matrix, then voting for the PD would be optimal.

The relationship between the difference in the beliefs of cooperation and voting is highly statistically significant across treatments and robust to including personal characteristics of the subjects – see Table 4. The *Belief Diff* variable denotes the difference in the belief about the probability of cooperation of other subjects under the alternative game relative to the PD. This OLS regression shows that an increase in the belief of cooperation of 100% (as predicted by theory) would decrease the probability of voting for the PD by around 50%.

But the correlation of beliefs and voting does not necessarily show that belief differences are a cause of voting. The reason is that people with different beliefs could also differ in dimensions that directly affect voting and are not observed by us.²

To show that beliefs have a causal effect on voting, we exploit exogenous variation in the beliefs held by a subject that is caused by the behavior of the players encountered in periods 1 and 2. The identity and behavior of a subject's opponent in the first two periods is exogenous and cannot be correlated with any personal characteristic or past behavior of the subject (even the partner in period 2 cannot have played with anybody that has played

²Endogeneity may also occur if subjects examine the games further when being asked to report their beliefs, and report beliefs that justify their past voting choice. Costa-Gomes and Weizsäcker (2008) show evidence compatible with the idea that subjects re-examine strategic situations during the elicitation stage.

with the subject in question). A subject who observes more defection in the first two periods while playing a PD should have a greater belief that changing the game affects behavior than a subject that observed more cooperation. Based on this idea, we use the observed behavior of the other players in periods 1 and 2 (measured as a cooperation rate of either 0, 0.5, or 1) as an instrument for beliefs. The approach is admittedly demanding, since by the time beliefs are elicited three more periods of play have occurred. We restrict attention to the three treatments where beliefs were elicited (Random Dictator, Reverse Random Dictator and Majority Once). Panel A in Table 5 shows that the cooperation rate observed in the first two periods has the expected positive effect on beliefs in all three treatments, but while the instrument is strong for Majority Once, it is very weak for the other two.

Panel B in Table 5 shows the second-stage results. For the treatment for which we have an instrument, Majority Once, we can see that subjects who expect the change of games to have a greater impact on cooperation are less likely to vote for the PD. Having a valid instrument in this treatment, we can now ask whether the instrumented estimate differs significantly from the reduced form estimate. If not, then one cannot reject the hypothesis that beliefs are exogenous. A Hausman test cannot reject the null of belief exogeneity.

4.2 Structural approach

The experimental data presented thus far substantiate a failure of reform driven by voters' inability to fully appreciate equilibrium effects. More precisely, we have shown that many subjects underestimate the effect that a policy change will have on the behavior of others. But it is also possible that subjects do not anticipate that their own behavior will change as well.

We now write a simple model where individuals have one of two types depending on the way they think about their best response, conditional on their beliefs about the likely actions

of others in each game. Responsive (R) types know that they will best-respond and play the dominant action in each game, while Inertial (I) types believe that they will keep playing the same action they have been playing.

Consider the finite N -player normal-form game $G = (A_1, A_2, \dots, A_N; u_1, u_2, \dots, u_N)$, where A_i is the finite set of Player i 's actions, and $u_i : A_1 \times A_2 \times \dots \times A_N \rightarrow R$ is Player i 's payoff function. Let $a = (a_1, \dots, a_N)$ denote the full action profile, and let $a_{-i} = (a_1, \dots, a_{i-1}, a_{i+1}, \dots, a_N)$ denote the action profile excluding player i 's action. Let m_{-i} denote a profile of mixed strategies used by players other than i in game G . Let $a_i(m_{-i})$ be the best response for Player i when others are playing m_{-i} .³ We are interested in players' preferences over playing G and the related game $G' = (A_1, A_2, \dots, A_N; u'_1, u'_2, \dots, u'_N)$, which has the same actions and players as G but possibly differs in payoffs. We call Player i a *Responsive type* if in every game G , whenever she believes that her opponents use strategies m_{-i} , she chooses $a_i(m_{-i})$. This definition implies that when contemplating any two games G and G' that have dominant strategies for Player i , a Responsive type of Player i expects to play her dominant strategy in each. In the context of our experiment, this type expects to defect in the PD and to cooperate in the AG. Then, a Responsive type who believes that her opponents use strategies m_{-i} in G and m'_{-i} in game G' chooses game G whenever $E_{m_{-i}} u_i(a_i(m_{-i}), a_{-i}) \geq E_{m'_{-i}} u'_i(a_i(m'_{-i}), a_{-i})$, and chooses game G' otherwise. In other words, a Responsive type prefers game G if the expected payoff when best-responding to the expected behavior of others is higher in G than in G' .⁴

For the purposes of our experiment, we insist that a Responsive type who chooses G over G' also chooses G over G' in any mechanism under which Player i 's choice is pivotal with positive probability in some equilibrium. This, for instance, requires a Responsive player

³In the event that Player i does not have a unique best response to m_{-i} , let $a_i(m_{-i})$ be any selection.

⁴Not only do Responsive types best-respond to their beliefs about others' strategies, but they know that they do so: they prefer a first game to a second if best-responding to their beliefs about their opponents' strategies in the first gives higher expected payoffs than doing so in the second.

who would choose G over G' as dictator to vote for G over G' in a majoritarian election.

Many authors have proposed models of players with limited strategic sophistication who fail to play Nash equilibrium in different games. Because this is not our focus in this paper, we define types who differ from the Responsive type only through their preferences over games. Specifically, we consider players whose preferences over games depend upon a status quo that consists of action profile a in game G , (a, G) .

One way that a player might systematically differ from a Responsive type is by failing to recognize how she will adjust her action following a change in the payoffs of the game. We label this particular type an *Inertial type*. More formally, we call Player i who believes that her opponents use strategies m_{-i} in G and m'_{-i} in game G' an Inertial type if from reference action a_i in game G she chooses G over G' whenever $E_{m_{-i}}u_i(a_i, a_{-i}) \geq E_{m'_{-i}}u'_i(a_i, a_{-i})$ and chooses G' over G whenever the opposite inequality holds. An Inertial type prefers a first game to a second game if and only if the expected payoff from playing her reference action is higher in the first game than in the second game given the beliefs she has about the behavior of others in each game.

We postulate the existence of a share s of Responsive types, and $1 - s$ of Inertial types. Denote with $\Delta u^R(b_i)$ the difference $E_{m_{-i}}u_i(a_i(m_{-i}), a_{-i}) - E_{m'_{-i}}u'_i(a_i(m'_{-i}), a_{-i})$, and with $\Delta u^I(b_i)$ the difference $E_{m_{-i}}u_i(a_i, a_{-i}) - E_{m'_{-i}}u'_i(a_i, a_{-i})$, where the expected payoff differentials depend on the beliefs $b_i = (m_{-i}, m'_{-i})$ held by player i that she will face a mixing m_{-i} in game G and mixing m'_{-i} in game G' . Each one of these differences $\Delta u^t(b_i)$ tracks the intensity of preference of the respective types $t = R, I$ for the game G over G' . Now letting $G = PD$ and $G' = AG$, for the purposes of empirical identification of the share s , we assume that a Responsive (Inertial) type votes for the PD game with a probability that depends on the payoff differential $\Delta u^R(b_i)$ ($\Delta u^I(b_i)$). To account for empirical errors, we will assume that such probability is given by a logistic cdf with parameters (μ, σ) . Thus, a player i with

type t votes for PD with a probability $F(\Delta u^t(b_i), \mu, \sigma)$. It follows that the probability of a PD vote by a player i , given a share s of Responsive types is,

$$P(v = PD|b_i, s) = sF(\Delta u^R(b_i), \mu, \sigma) + (1 - s)F(\Delta u^I(b_i), \mu, \sigma),$$

Given a profile of votes $\mathbf{v} = [v_1, \dots, v_N]$ where $v_j = 1$ denotes a vote for PD by subject j , and $v_j = 0$ a vote for AG, we have that the overall probability of such a profile is,

$$\prod_{i=1}^N P(v = PD|b_i, s, \mu, \sigma)^{v_i} (1 - P(v = PD|b_i, s, \mu, \sigma))^{1-v_i},$$

which yields the log-likelihood,

$$L(s, \mu, \sigma | \mathbf{v}, \mathbf{b}) = \sum_{i=1}^N \left\{ \begin{array}{l} v_i \ln [sF(\Delta u^R(b_i), \mu, \sigma) + (1 - s)F(\Delta u^I(b_i), \mu, \sigma)] \\ + (1 - v_i) \ln [s(1 - F(\Delta u^R(b_i), \mu, \sigma)) + (1 - s)(1 - F(\Delta u^I(b_i), \mu, \sigma))] \end{array} \right\}.$$

We estimate the parameter s , by maximizing $L(s, \mu, \sigma | \mathbf{v}, \mathbf{b})$ given the voting data \mathbf{v} and the elicited beliefs \mathbf{b} . Clearly, in this estimation we take beliefs to be exogenous – this is a maintained assumption with some support from the exogeneity test performed earlier in relation with the instrumental variables findings. We model the expected utility differentials $\Delta u^R(b_i)$ and $\Delta u^I(b_i)$ assuming a linear utility for money as in expression (1). That expression, however, reflected the assumption that subjects would plan to use dominant strategies in each game, and could only be wrong about the likely actions of others. In order to accommodate Inertial types, we now augment expression (1) by considering that each individual i anticipates to cooperate with a probability $b_{i,k}^o$ in each game $k = PD, AG$. The expected

utility differential from playing the PD instead of the alternative game now reads,

$$EU_{PD} - EU_{AG} \equiv \Delta EU = -6.\Delta b_i + 4 - 2b_{i,PD}^o - b_{i,AG}^o.$$

The key aspect differentiating the types is that the term $-2b_{i,PD}^o - b_{i,AG}^o$ is 1 for Responsive types and is either zero or -3 for Inertial types depending on whether they respectively defected or cooperated in period 5. Thus, $\Delta u^R(b_i) = -6.\Delta b_i + 3$ and $\Delta u^I(b_i) = -6.\Delta b_i + 4 - 3c$, where c is an indicator variable for whether the subject cooperated in period 5. We pool the data for the three conditions where beliefs were elicited, namely Random Dictator, Reverse Random Dictator, and Majority Once, for a total of 408 observations.

The estimate of the share of Responsive types s , presented in Table 6, is 67%. The Wald test for the share of Responsive types being less than 100% yields p-values of 0.042 and 0.057 depending on whether the standard errors are clustered respectively at the individual or group level. The estimates for the parameters (μ, σ) of the logistic distribution are 0.8 and 1.33, which is reasonable given the size of the payoff differentials in the games. Taken together, these findings support the notion that a fraction of the players choose their vote not realizing that their own play will adjust following a policy change. The point estimate suggests that a full third of the players are in that situation. This is striking, given that all that is required is to forecast that one will play a different, dominant, strategy in a 2x2 game following the change in policy. That inability, in combination with the distorted expectations about the likely behavior of others documented earlier, drives a collective failure to democratically resolve social dilemmas.

5 Ruling out alternative mechanisms

The variation of treatments allows us to rule out some alternative mechanisms. One possibility is that the PD garners a majority in the Random Dictator and Majority treatments because a status quo bias causes people to stick with their initial game even if it is a sub-optimal one (such a bias could stem from a reluctance to try new, risky things). Under this account, they would never abandon an efficient game if they got to play it from the beginning. This account is falsified by the results from Reverse Random Dictator, where the PD garnered 50% of the vote. This vote share is only 3 points away (and statistically indistinguishable) from that under Random Dictator. In other words, our experiment does not show a reform failure tied to a systematic tendency toward insufficient reform. While we do see insufficient reform in the Random Dictator treatment, in the case of the Reverse Random Dictator treatment we have a tendency to excessive reform.

The Random Dictator treatments allow us to rule out some type of pivotal thinking as a source of the demand for bad policy. Under majority rule, the vote of a citizen matters only if pivotal. Two types of reasoning may dilute a subject's incentive to vote in a considered way. First, the subject may have expectations that the majority will be too small or too large, leaving her with a negligible chance of being pivotal and therefore with no incentives to carefully consider how to vote. Second, even if the chance of pivotality is not negligible, a subject may take the event of being pivotal as a sign that a large share of the subjects are not rational (if they were rational, they would vote for the alternative game). Thus, those subjects may not respond to the change in game as theory predicts and voting for the alternative game may be a bad idea. Under Random Dictator, pivotality has a clear, and not so negligible chance of $1/6$ th. Moreover, the event of being pivotal does not depend on the votes of others and hence it cannot constitute a signal of how others may play in the

alternative game. Therefore, the majoritarian 52.98% of votes for the PD under Random Dictator cannot be explained by the previous pivotality concerns. The share of votes in favor of the PD is greater under Majority Once (60.83%), but the difference is not statistically significant.

6 Conclusion

We have experimentally identified a collective failure to democratically resolve social dilemmas driven by two combined mechanisms. First, players have distorted expectations about the likely change in the behavior of others following a game change, or policy reform. Second, a non-trivial share of players appear to even fail to appreciate that their own behavior will differ across games. In short, a majoritarian proportion of people demand bad policies, and this demand for bad policy is linked to a failure to appreciate the equilibrium effects of policy change. Understanding how people think about policy choice is crucial for our knowledge of how societies choose to regulate themselves. An important example concerns citizens' demand for regulations that would curb socially harmful activities like pollution and the emission of greenhouse gases affecting global warming.

Of course, identifying a tendency to underestimate equilibrium effects in the laboratory does not necessarily mean that we have identified an important source for bad policy in the field. One could hope that public discourse and political competition results in voters learning about the total effect of policies. However, a vast literature in economics and political science—both theoretical and empirical—has considered politicians as reflecting, more than shaping, the positions of the voters. This includes the Downsian democracy paradigm, the citizen-candidate models, and the literature on pandering. In the context of all of these mainstream approaches, the notion that voters may underappreciate the equilibrium effects

of policies becomes a serious threat to efficient policy choice.

7 References

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Table 1: The Two Games

Prisoners' Dilemma				Alternative Game			
		Other's action				Other's action	
		C	D			C	D
Own action	C	9	3	Own action	C	8	2
	D	11	5		D	7	1

Note: Payoff in experimental points (3 experimental points equal \$1).

Table 2: Number of Subjects by Treatment and Place

	Berkeley	Brown	Total
Control	60	60	120
Reverse Control	60	60	120
Random Dictator	84	84	168
Reverse Random Dictator	60	60	120
Majority Once	60	60	120
Majority Repeated	60	60	120
Total	384	384	768

**Table 3: Voting for PD shares by Treatment
(Period 6)**

Treatment	Vote PD	Play PD
Random Dictator	52.98%	46.43%
Reverse Random Dictator	50.00%	55.00%
Majority Once	60.83%	75.00%
Majority Repeated	50.83%	45.00%
Total	53.60%	54.55%

Table 4: Correlates of voting for PD (Dependent Variable: Vote for PD)

	Treatment									
	All (1)	RD (2)	Reverse RD (3)	Majority Once (4)	Majority Repeated (5)	All (6)	RD (7)	Reverse RD (8)	Majority Once (9)	Majority Repeated (10)
Male	-0.086 [0.050]*	-0.126 [0.068]*	0.091 [0.102]	-0.207 [0.083]**	-0.272 [0.100]**	-0.09 [0.046]*	-0.147 [0.062]**	0.049 [0.096]	-0.16 [0.084]*	-0.257 [0.115]**
Year	0.02 [0.020]	-0.007 [0.032]	-0.003 [0.037]	0.081 [0.039]**	-0.023 [0.043]	0.033 [0.023]	0.017 [0.036]	-0.006 [0.047]	0.086 [0.038]**	-0.019 [0.056]
Ideology	0.036 [0.011]***	0.035 [0.018]*	0.038 [0.017]**	0.045 [0.022]*	-0.004 [0.016]	0.037 [0.012]***	0.04 [0.020]*	0.035 [0.022]	0.039 [0.023]*	0.005 [0.017]
Economics	0.032 [0.062]	0.051 [0.062]	0.175 [0.174]	-0.052 [0.151]	-0.086 [0.149]	0.031 [0.065]	0.023 [0.068]	0.123 [0.175]	0.059 [0.181]	-0.174 [0.152]
Pol.Science	0.049 [0.101]	-0.194 [0.222]	0.24 [0.170]	0.093 [0.119]	-0.102 [0.164]	0.105 [0.094]	0.031 [0.190]	0.261 [0.199]	0.039 [0.145]	-0.081 [0.164]
Brown U.	0.071 [0.052]	0.07 [0.080]	0.059 [0.111]	0.149 [0.073]*	0.043 [0.116]	0.093 [0.052]*	0.096 [0.068]	0.058 [0.120]	0.17 [0.100]	0.138 [0.141]
Belief Diff.	-0.005 [0.000]***	-0.005 [0.000]***	-0.004 [0.001]***	-0.005 [0.001]***		-0.005 [0.001]***	-0.005 [0.001]***	-0.005 [0.001]***	-0.004 [0.001]***	
Beauty N.	-0.001 [0.001]	-0.001 [0.002]	0 [0.002]	0 [0.002]	0.003 [0.002]	0 [0.001]	-0.001 [0.002]	0 [0.003]	0 [0.002]	0.001 [0.002]
Math SAT						0 [0.000]	0 [0.001]	0 [0.001]	-0.001 [0.001]	-0.001 [0.001]
Verbal SAT						-0.001 [0.000]	-0.001 [0.001]	-0.001 [0.001]	0 [0.001]	-0.001 [0.001]
Constant	0.538 [0.080]***	0.622 [0.122]***	0.413 [0.151]**	0.43 [0.185]**	0.589 [0.185]***	1.184 [0.286]***	0.804 [0.556]	1.262 [0.632]*	1.215 [0.459]**	1.546 [0.540]***
Observations	408	168	120	120	120	346	140	102	104	97
R-squared	0.18	0.24	0.12	0.24	0.11	0.22	0.29	0.14	0.26	0.12

Note: Columns (1) and (6) do not include Majority Repeated. Year denotes year in college. Ideology from 0 to 10 from most liberal to most conservative.

Belief Difference denotes the difference between beliefs of cooperation under alternative and PD games.

Robust standard errors in brackets clustered by part 1 group: * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Instrumenting for Beliefs

Panel A: First Stage (dependent variable: Belief Difference)						
	Treatment					
	RD (1)	Reverse RD (2)	Majority Once (3)	RD (4)	Reverse RD (5)	Majority Once (6)
Average other coop in periods 1 & 2	-7.122	15.008	-42.89	-8.915	21.166	-47.546
	[10.489]	[21.063]	[7.552]***	[10.032]	[19.966]	[6.701]***
Personal Characteristics Included	N	N	N	Y	Y	Y
Constant	38.944	15.63	52.572	23.745	0.439	35.767
	[5.609]***	[20.935]	[4.740]***	[21.358]	[19.130]	[16.855]**
Observations	168	120	120	168	120	120
R-squared	0	0.01	0.14	0.07	0.06	0.17
F of excluded instrument	0.46	0.51	32.25	0.79	1.12	50.35
Panel B: Second Stage (dependent variable: Vote for PD)						
	Treatment					
	RD (1)	Reverse RD (2)	Majority Once (3)	RD (4)	Reverse RD (5)	Majority Once (6)
Belief Difference	-0.031	-0.022	-0.007	-0.026	-0.021	-0.006
	[0.040]	[0.020]	[0.003]**	[0.026]	[0.017]	[0.003]**
Personal Characteristics Included	N	N	N	Y	Y	Y
Constant	1.656	1.154	0.83	1.112	0.669	0.449
	[1.432]	[0.589]*	[0.110]***	[0.650]*	[0.318]**	[0.186]**
Observations	168	120	120	168	120	120

Note: Personal characteristics include gender, year of studies, ideology, Economics and Political Science concentrations and number from beauty contest game. Belief Difference denotes the difference between beliefs of cooperation under alternative and PD games. Robust standard errors in brackets clustered by part 1 group: * significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Structural estimates

s (share of Rational types)	0.67
	[0.17]
Wald test $s \neq 1$; p-value:	0.06
Logistic distribution parameters	
μ	0.80
	[0.15]
σ	1.34
	[0.35]

Note: Pooled sample from Random dictator, Reverse random dictator and Majority once treatments. Robust standard errors clustered by part 1 group in brackets.

Appendix Table 1: Summary Statistics

	Obs.	Mean	Std. Dev.	Min	Max
Male	768	0.43	0.50	0	1
Year	768	2.70	1.21	1	5
Ideology	768	3.54	2.14	0	10
Economics	768	0.15	0.36	0	1
Political Science	768	0.05	0.21	0	1
Brown U.	768	0.50	0.50	0	1
Beauty Contest Number	768	36.67	21.35	0	100
Math SAT	662	723.95	71.77	400	800
Verbal SAT	644	700.19	77.45	400	800
Belief of C in PD	408	44.26	25.79	0	100
Belief of C in ALT	408	77.74	26.02	0	100
Belieff Difference	408	33.47	-41.11	100	100
Earnings	768	27.81	3.27	16.75	37

Appendix Table 2: Cooperation comparison between games by treatment

	Control			Reverse Control			Random Dictator			Reverse RD			Majority Once		
	Part 1	Part 2		Part 1	Part 2		Part 1	Part 2		Part 1	Part 2		Part 1	Part 2	
Periods	All	6	All	All	6	All	All	6	All	All	6	All	All	6	All
Part 2 Game															
Alternative	27%	88%	92%	99%	92%	93%	28%	94%	96%	95%	94%	95%	37%	97%	94%
PD	21%	30%	16%	95%	38%	30%	26%	21%	15%	93%	58%	36%	31%	33%	21%
Diff. p-value	0.288	0.001	0.000	0.319	0.001	0.000	0.786	0.000	0.000	0.661	0.001	0.000	0.421	0.000	0.000

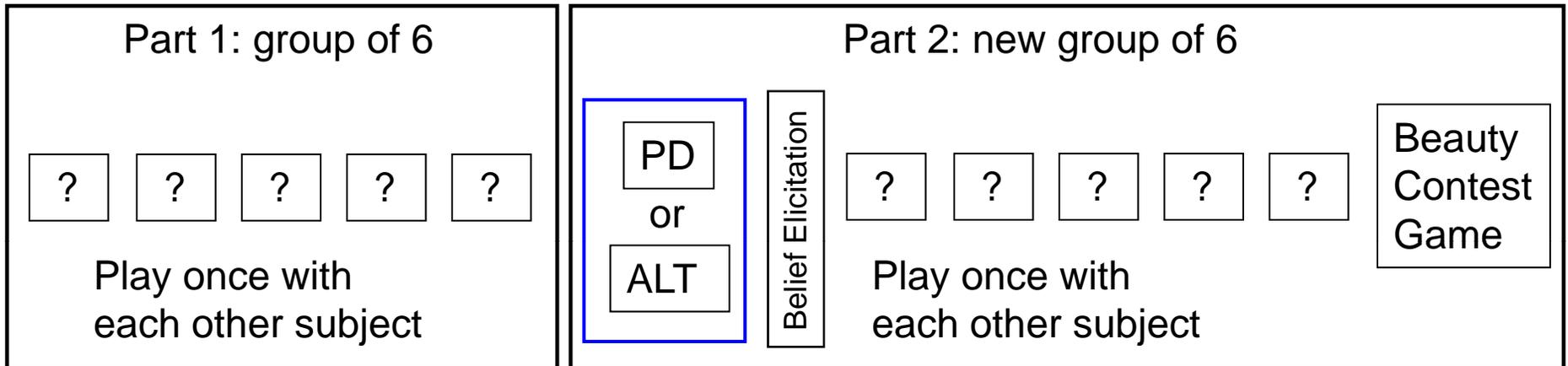
Note: p-values calculated using Wald tests with s.e. clustered at session level.

Appendix Table 3: Payoff comparison between games by treatment

	Control			Reverse Control			Random Dictator			Reverse RD			Majority Once		
	Part 1	Part 2		Part 1	Part 2		Part 1	Part 2		Part 1	Part 2		Part 1	Part 2	
Periods	All	6	All	All	6	All	All	6	All	All	6	All	All	6	All
Part 2 Game															
Alternative	5.81	7.18	7.44	7.79	7.42	7.51	6.04	7.61	7.75	7.70	7.61	7.66	5.95	7.77	7.58
PD	6.12	6.20	5.65	7.75	6.53	6.20	6.16	5.82	5.59	7.48	7.30	6.44	6.42	6.33	5.85
Diff. p-value		0.031	0.000		0.044	0.000		0.000	0.000		0.078	0.001		0.009	0.003

Note: p-values calculated using Wilcoxon/Mann-Whitney non-parametric test.

Figure 1: Experimental Design:



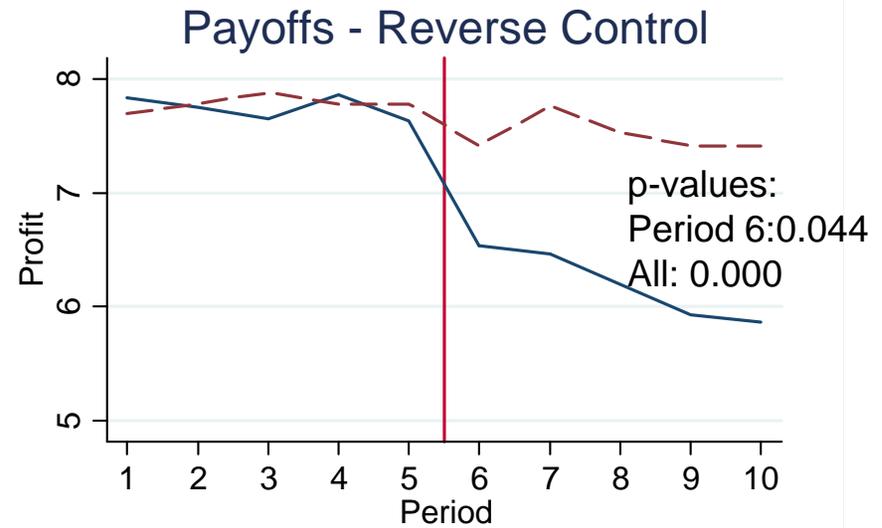
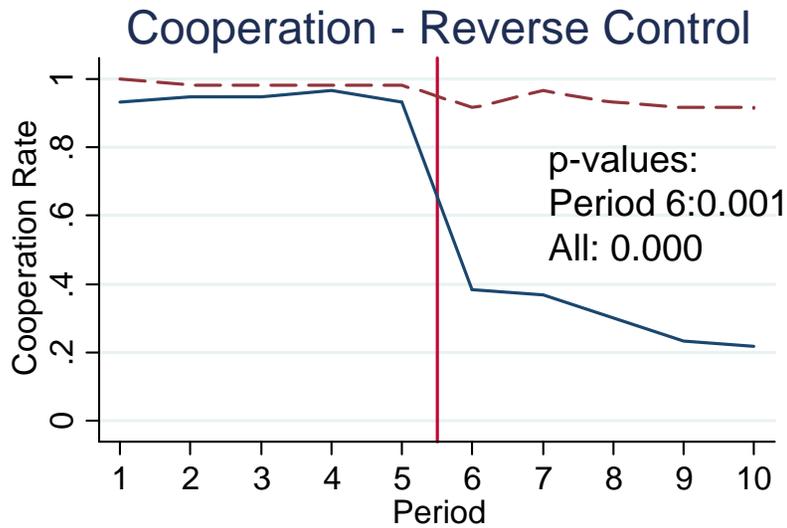
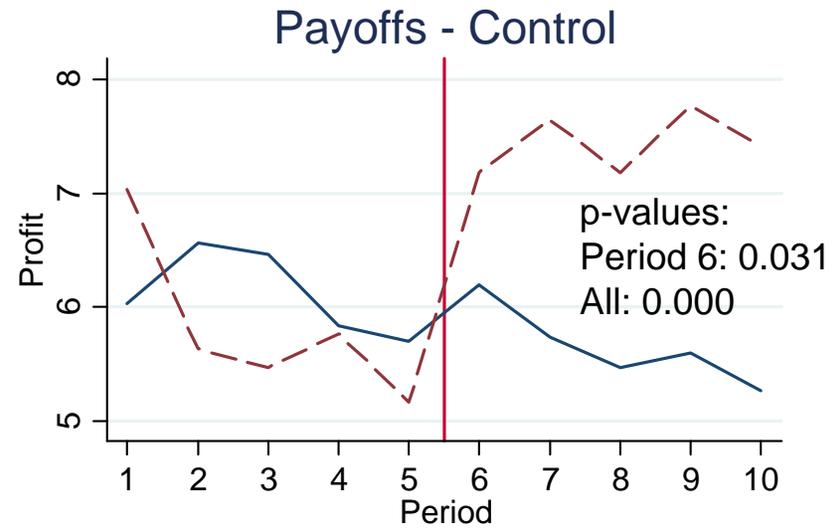
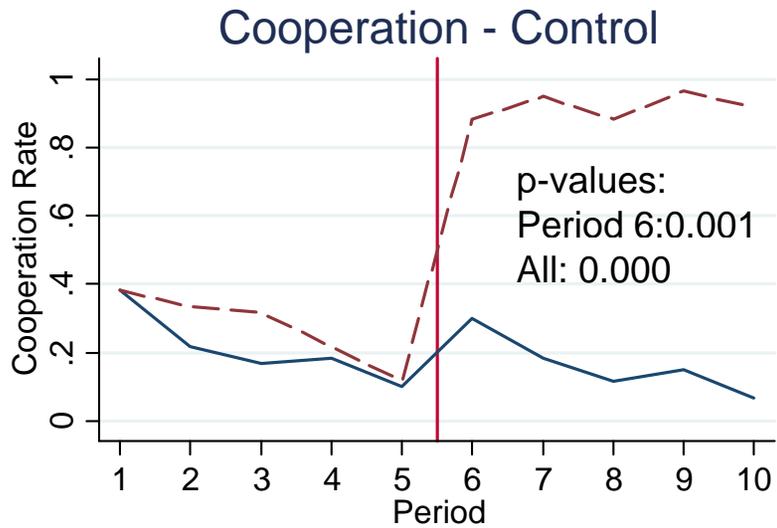
Questionnaire

- Gender
- Ideology
- Class
- Major
- SATs

Treatments:

1. Control: PD first, game chosen randomly
2. Reverse Control: ALT first, game chosen randomly
3. Random Dictator: PD first, random dictator
4. Reverse RD: ALT first, random dictator
5. Majority Once: PD first, majority voting
6. Majority Repeated: PD first, majority voting before each game in part 2

Figure 2: The Power of Nash Equilibrium:



— PD - - - Alternative

Figure 3: Evolution of Voting and Chosen Game in Majority Repeated

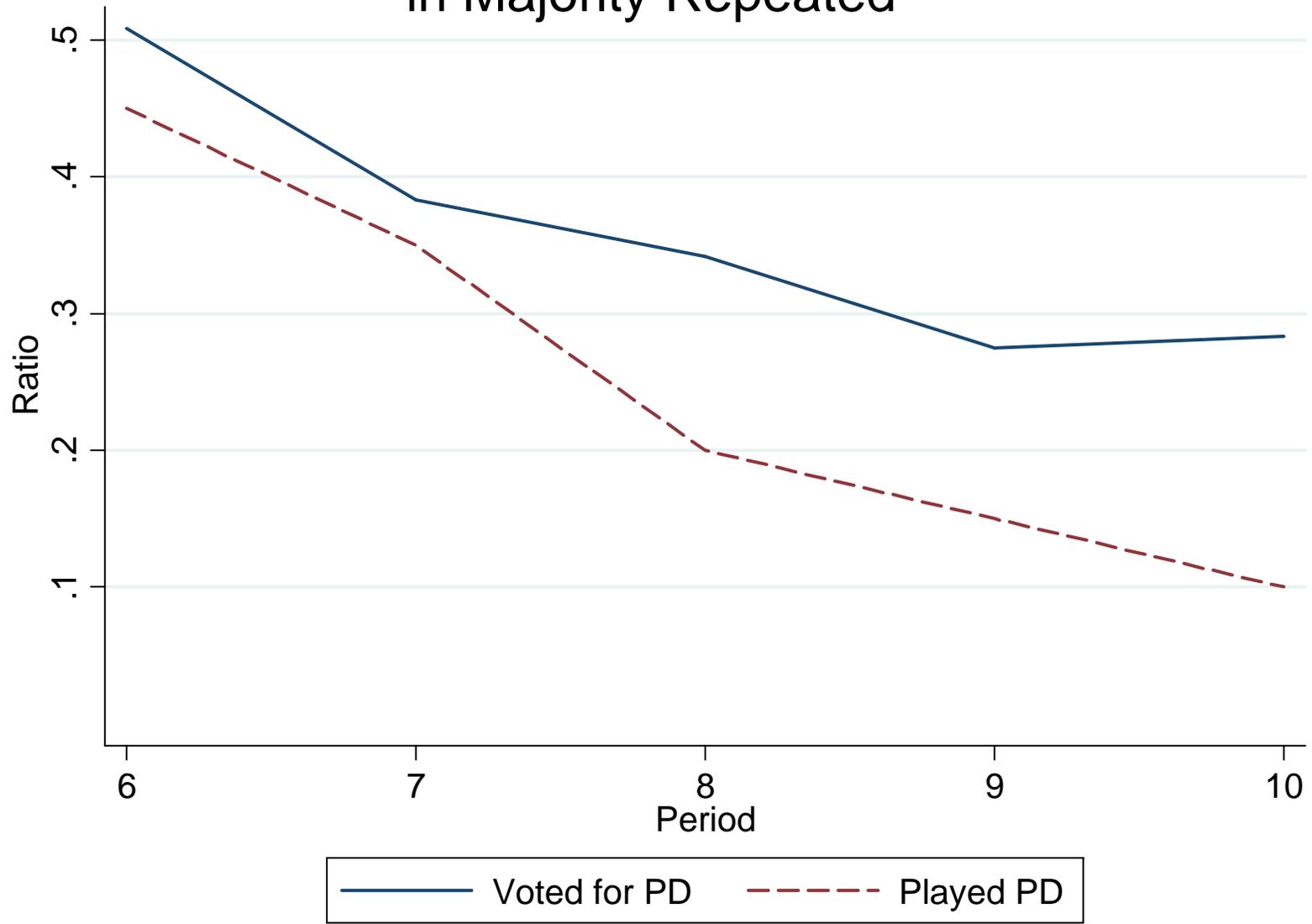


Figure 4: Distribution of Beliefs of Cooperation Difference Between Games

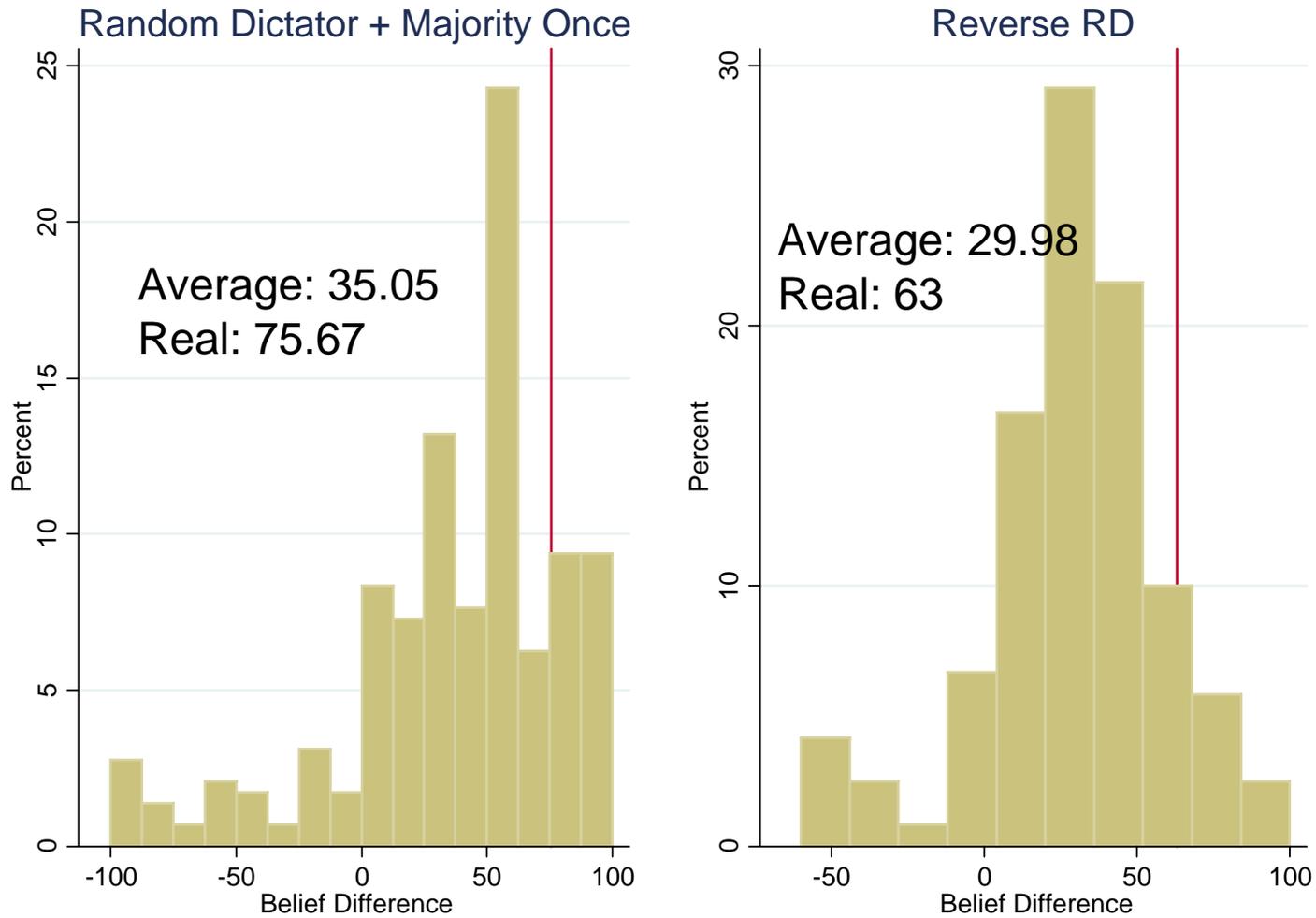


Figure 5: Beliefs of Cooperation and Voting

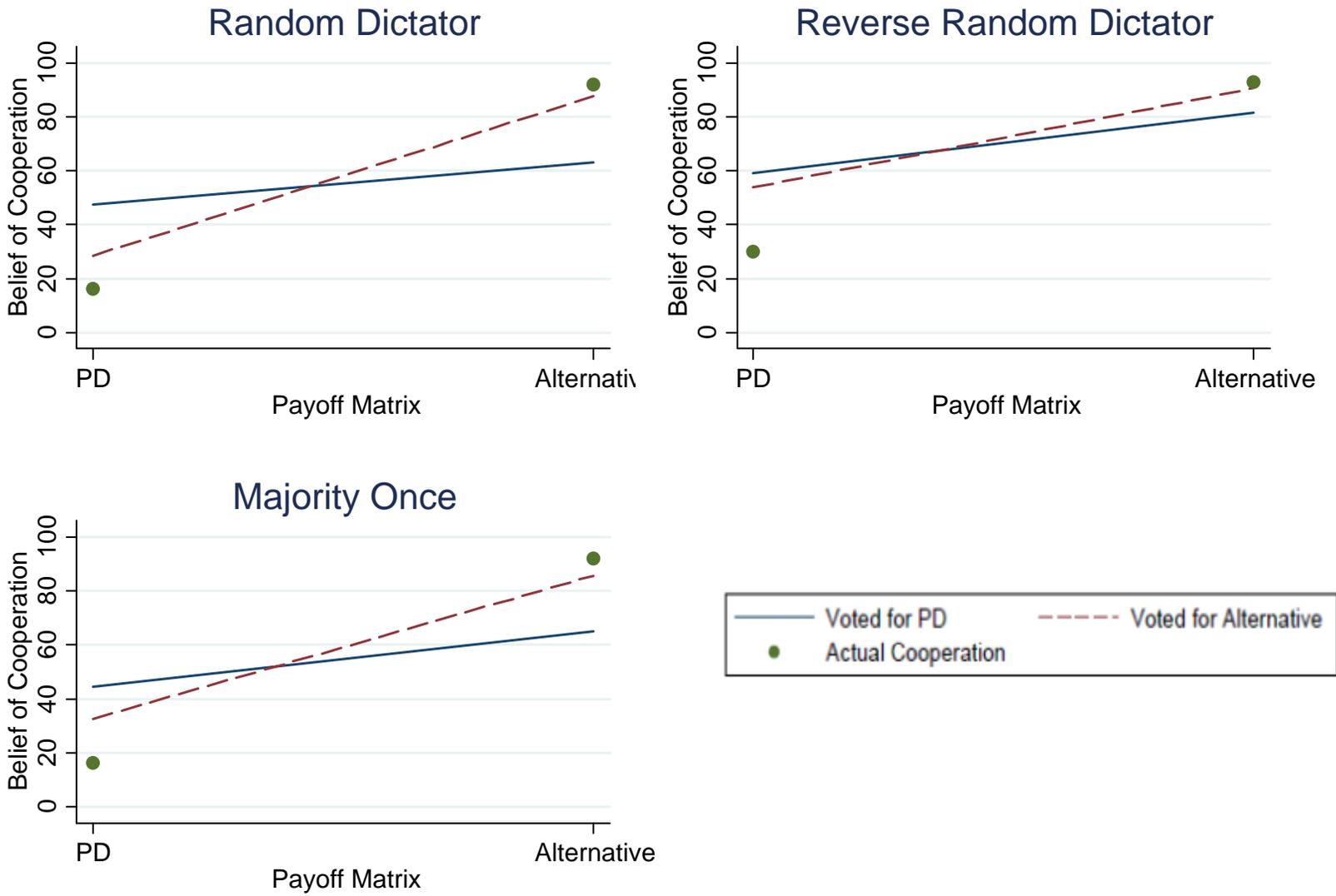


Figure A1: Comparing PD and Alternative Game

