

## **Can exit prizes induce lame ducks to shirk less? Experimental evidence**

**Leif Helland**, BI Norwegian Business School, Norway\*

**Jon Hovi**, University of Oslo and CICERO, Norway

**Lars Monkerud**, BI Norwegian Business School, Norway

### **Abstract**

Elected representatives serving their final period face only weak incentives to provide costly effort. However, overlapping generations (OLG) models suggest that exit prizes sustained by trigger strategies can induce representatives in their final period to provide such effort. We evaluate this hypothesis using a simple OLG public good experiment, the central treatment being whether exit prizes are permitted. We find that a significantly higher number of subjects in their final period contribute when exit prizes are permitted. However, this result does not originate from use of trigger strategies. More likely explanations include gift-exchange and focal-point effects.

### **JEL classification**

C7; C9; H4

### **Keywords**

Overlapping Generations; last period effects; legislatures

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\* Address for correspondence: leif.helland@bi.no.

## Introduction

Legislative parties consist of overlapping generations of elected representatives. Having finite biological and political lives, such representatives eventually reach their final legislative period. When a representative realises that she has reached her final period, she becomes a lame duck.

We use the term lame duck liberally, meaning representatives that know they are in their final period. In a strict sense the term lame duck characterizes a representative in the time between electoral defeat and exit from the legislature. In the US Congress lame duck periods are fairly short; they last from the announcement of electoral loss until replacement by a winner a few weeks later. In parliamentary systems lame duck periods may last considerably longer: from when a representative is denied re-nomination prior to election until replacement takes place several months later. Of course, a representative might be in her last period without recognizing it; for example, due to sudden and unexpected death or electoral defeat. In such cases she is not a lame duck, and does not face special incentives problems. Since in this study we use an experimental setup in which re-election is exogenously given, a subject's lame duck period unambiguously corresponds to the final period in which it makes a decision.

Re-election concerns can then no longer discipline her actions. This creates a tension; while a party's remaining (and future) representatives derive benefits from continued effort by lame ducks, a lame duck motivated by self interest will decline to provide costly effort unless she is incentivized to do so.

Whereas only few studies have focused on final-period behaviour in assemblies outside the United States,<sup>1</sup> a voluminous body of research has considered final-period behavior in the US Congress.<sup>2</sup> These US studies have provided mixed results that to some extent contradict predictions derived from agency models. In particular, most studies have found some degree of final-period attendance shirking, but only scant evidence of deviant voting (ideological shirking). Scholars often ascribe the latter finding to selection bias; only candidates with preferences akin to those of their constituencies will likely be elected. It is less clear why constituencies apparently do not select representatives that also continue to attend in their final period (Rothenberg and Sanders 2000).<sup>3</sup> The choice of operational definitions impact on findings and may help explain the mixed results; for example, scholars differ over questions such as how ideological deviations should be measured and when the final period begins.

This article serves a three-fold purpose. First, few previous studies have considered the important question of how political parties might use attractive retreat positions and other post-office perks to influence final-period behavior. We contribute towards filling this gap. Second, we do so by using a novel experimental approach. We believe that the study of final-period behavior can profit from experimental control, because such control allows for explicit regulation of rewards, containment of reputational concerns to the legislative game, unambiguous separation of the final period from preceding periods, and elimination of selection bias. Although many previous public goods experiments have used automatons, our approach is novel in that the automatons mimic an indefinite time horizon in the

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<sup>1</sup> Notable exceptions include Besley's and Larcinese's (2011) study of attendance shirking in the British parliament, as well as Lien's (2008) study of attendance shirking in the Norwegian parliament.

<sup>2</sup> Bender and Lott (1996) provide a critical review of the field. Lott (1990) deals explicitly with post-elective office employment. More recent contributions include Rothenberg and Sanders (2000), Parker and Powers (2002) and Parker (2004).

<sup>3</sup> Our research is related to the literature on end-game effects. See Brañas-Garza and Espinosa (2011) for many references to this literature.

laboratory. Finally, underlying many previous studies of final-period behavior is Alchian's and Demetz's (1972) framework (cf. Crain, Leavens and Tollison 1986), which treats incentive schemes as exogenously given. Taking the common view that control of procedures and institutions constitutes a defining characteristic of majority assemblies, this is clearly unsatisfactory (Krehbiel 1990, Shepsle 1986). Our experimental design allows incentive schemes to be determined endogenously through majority choice.

In the public goods OLG model, sufficiently large exit prizes supported by trigger strategies can sustain a fully cooperative equilibrium in which even lame ducks contribute.<sup>4</sup> The absence of exit prizes, in contrast, means no equilibrium exists in which lame ducks contribute. However, partially cooperative equilibria, in which all other representatives contribute, may still exist (Shepsle and Nalebuff 1990, Cremer 1992, Hammond 1975). With or without exit prizes, other equilibria also exist. In particular, playing the stage-game equilibrium (in which no player contributes) in every period is always an equilibrium.<sup>5</sup>

To fix ideas, one may think of exit prizes as attractive retreat positions. Examples include a position as director or board member of a public company, an ambassador post, a leading position in a national or sub-national administration. Related options include campaign support (financing and endorsement) for a top international position. Political parties commonly award such positions to retired top politicians.<sup>6</sup>

We examine the impact of exit prizes in a simple OLG experiment. Our treatment is whether exit prizes are permitted. We treat electoral support as exogenously given, and representatives are re-elected a fixed number of times.<sup>7</sup>

In each period subjects choose simultaneously whether to contribute to the provision of a public good.<sup>8</sup> We may think of such contributions as costly effort needed for the production of constituency services.<sup>9</sup>

As a simplifying assumption we let the quantity of the public good increase linearly in the number of contributions. Monetary payoffs reflect the assumption that representatives value the public good, but dislike effort. By design, the exit prize

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<sup>4</sup> Salant (1991:255) suggests, but does not analyze, the effect of exit prizes in the generic two-player public goods OLG game. Exit prizes in a purely distributive OLG game are analyzed in Alesina & Spear (1988). In their model electoral competition is endogenous. There is a growing literature on the distribution of agenda setter rights and other valuable positions in legislative assemblies with overlapping generations of legislators (Muthoo and Shepsle 2007, 2004; Shepsle, Dickson and Houweling 2002; Diermeir 1995). The incentive schemes considered in this literature deviate slightly from ours, in that members gradually obtain more valuable positions in equilibrium (a "seniority system"). In these models (as in ours) electoral competition is exogenous. The impact of exit prizes is not considered.

<sup>5</sup> Kandori 1992 characterizes folk theorems for OLG games with varying information about the history of the game.

<sup>6</sup> Political scientists debate whether attractive retreat positions serve primarily as rewards for services rendered, or as instruments for cementing partisan power in the longer run, referred to as "patronage" (see for instance Pappas 2009; and Kopecký and Scherlis 2008, with references). We do not see a fundamental conflict here. As long as attractive retreat positions remain scarce, party leaders can use them both to discipline current legislators and to entrench power in the longer run. And even if such positions were to be used mainly for long-run entrenchment, party leaders selecting among otherwise comparable candidates should avoid candidates with a history of shirking.

<sup>7</sup> This is clearly a simplifying assumption. It may well be that behavior would change in the experiment if re-election was endogenous (and linked to individual contributions or aggregate production). Exogenous re-election allow us to focus more clearly on our main question; can exit prizes incentivize lame ducks.

<sup>8</sup> This binary choice is invoked as a simplifying assumption; alternatively, we could have let subjects contribute part or all of a given endowment.

<sup>9</sup> Shepsle and Nalebuff (1990) provide an alternative interpretation, in which aggregate efforts produce "power".

offsets the lame duck's cost of contributing. Moreover, awarding the prize is cost free for remaining subjects.<sup>10</sup>

Few previous experiments addressing the ability of trigger strategies to support high contribution levels in an OLG public-goods game have been carried out. Notable exceptions include Van der Heijden et al. (1998) and Offerman, Potters and Verbon (2001). Both of these experiments consider a game with two players per period, in which only the newborn player takes an action. In contrast, we consider a strategically richer OLG game with three players per period, all of whom take actions. In keeping with the two mentioned studies, the subjects in our experiment face a binary choice (contribute vs. not contribute). Van der Heijden et al. (1998) study the impact of the game's information structure on contribution levels and on the variability of contributions. They find that knowledge of the game's history explains neither the level of contributions nor their variability. Offerman, Potters and Verbon (2001) consider the impact of recommending the use of a trigger strategy. They find that such a recommendation increases the contribution level and causes many subjects to use trigger-like strategies. However, because of a remaining group of unconditional defectors, the fully cooperative outcome fails to materialize.

Scholars continue to discuss how one might best mimic an indefinite horizon in OLG experiments. Van der Heijden et al. (1998) use a fixed and publicly known last period. Because of this the cooperative outcome cannot be sustained as an equilibrium, which implies serious problems for studying the impact of trigger strategies. Offerman, Potters and Verbon (2001) let subjects play a single OLG game ("no reincarnation") with a constant and publicly known periodic stop-probability. As they note, with this procedure the number of subjects in the lab imposes a publicly known upper bound on the number of periods in the game. Therefore, the game is not indefinitely repeated in a proper sense. OLG experiments conducted in the context of monetary economics rely on a third approach (Lim et al. 1994, Marimon and Sunder 1993), where subjects enter repeatedly in a longer game ("reincarnation"). The reincarnation approach entails that the experimental game becomes much more complicated than the OLG models that motivated it (OLG models do not admit "reincarnation").

We use a novel design in which automatons replace one subject in the penultimate period and two subjects in the ultimate period of a five-period game (see table 1).<sup>11</sup> The automatons are programmed to follow a trigger strategy. This design solves several problems encountered in previous OLG experiments. It also reduces the set of equilibria - an attractive feature for our purposes. The reader might object that the automatons provide a powerful focal point (Schelling 1960) that will likely impact on behavior. Our response is two-fold. First, previous research shows that providing a focal point in the form of a strategy recommendation fails to bring out clear trigger-like behavior in OLG public-goods experiments, and our own results suggest that the (even stronger) focal point provided by our automatons also fails to generate trigger-like behavior. Second, while no-one has thus far come up with a procedure capable of perfectly mimicking an indefinite horizon in finite OLG

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<sup>10</sup> We have resisted the temptation to motivate our experiment more broadly, say, by referring to a richer array of organizations with end-period challenges. In most organizations awarding a bonus for good behavior entails costs for the remaining players. Legislatures are special in this regard. Remaining legislators are not competing for the positions (= bonuses) concerned, so they do not incur a direct cost by awarding such a position to an outgoing representative. In addition, awarding a position to an outgoing representative (rather than to an outside competitor) entails only modest audience costs. Retired legislators will likely be as competent as outside competitors for such positions. Furthermore, the positions in question are usually long term, so current legislators will unlikely compete for them in the near future. Interesting discussions of end-game effects in bureaucracies are provided by Horn (1995).

<sup>11</sup> Increasing the number of periods further (say, to ten) would either reduce the number of independent observations or require a considerably larger number of subjects.

experiments, our automatons provide a nice and novel way of circumventing this serious problem. We describe our automatons in detail in the section on experimental design.

Our research question is: Do exit prizes induce lame ducks to contribute to the public good? We find that a significantly higher number of outgoing subjects contribute in the treatment with exit prizes than in the baseline treatment without exit prizes. However, our results suggest that this result does not derive from the use of trigger strategies.

The next section depicts the OLG public-good model underlying our experiment. The third section outlines our experimental design. The fourth section contains descriptive statistics. The fifth section presents the main results. Finally, the sixth section concludes.

## Model

Consider a T-person overlapping generations game under the standard assumptions of rational and self-regarding players. Each period corresponds to a stage game with Prisoners Dilemma (PD) payoffs, in which  $n = 3$  players move simultaneously. Information is complete and (almost) perfect.<sup>12</sup>

It is common knowledge that the game ends in period T. In period T the stage game is played by two automatons and a human; in period T-1 the stage game is played by two humans and an automaton. In periods  $t = 1, 2, \dots, T-2$  the stage game is played by three humans. A player is either in position A, B or C. The player in position A plays her first stage game, the player in position B plays her second (and next last) stage game, while the player in position C plays her last stage game. The automaton in T-1 plays in position A, while the two automatons in period T play in positions A and B, respectively.

### No prize

The material payoff of player  $i$  in stage game  $t$  is:

$$v_{it} = z(1 - d_{it}) + \beta \sum_{j=1}^n d_{jt} z \tag{1}$$

In equation 1,  $z > 0$  denotes player  $i$ 's (exogenously given) endowment (players are assumed to have identical endowments).  $d_{it}$  is a dummy that takes the value 1 if  $i$  contributes to the public good in stage  $t$ , and 0 otherwise. We assume that the material payoffs satisfy  $(1/n) < \beta < 1$ . This means that the payoffs conform to a Prisoners Dilemma game. Since  $\beta < 1$ , setting  $d_{it} = 0$  is a dominant strategy in the stage game. The unique stage game equilibrium is thus  $d_{it} = 0 \forall i \in T$ . Since  $\beta > (1/n)$ ,  $d_{it} = 1 \forall i \in T$  Pareto-dominates the stage game equilibrium.

Let the state  $R_t$  be:  $d_{it} = 1$  for  $i = (A, B)$  in all periods  $t' < t$ . A "grim trigger" strategy for the game can now be defined as follows: set  $d_{it} = 1$  if in position A or B and state is  $R_t$  and set  $d_{it} = 0$  otherwise. The automatons used in the no-prize treatment were programmed to follow this grim trigger.

On the equilibrium path of the grim trigger, a player in position A nets  $(6\beta + 1)z$ ; a player in position B nets  $(4\beta + 1)z$ ; and a player in position C nets  $(2\beta + 1)z$ . By a single deviation the player in A nets  $(\beta + 3)z$ ; the player in B nets  $(\beta + 2)z$ ; while the player in C nets  $3\beta z$ . It follows that the player in position A cannot profit by a single

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<sup>12</sup> Almost perfect information means that the history up to period  $t$ , but not including period  $t$ , is common knowledge.

deviation from the equilibrium path of the grim trigger (by not contributing) if  $\beta \geq (2/5)$ . Similarly, the player in position B cannot profit by such a deviation if  $\beta \geq (1/3)$ . Finally, the player in position C cannot profit by deviating from the equilibrium path (by contributing) if  $\beta < 1$ .

Once the punishment path has been triggered, no player can profit by a single deviation from this path, since such a deviation would only reduce her payoff by  $(1 - \beta)z$ . It follows that grim trigger is subgame perfect if  $\beta \geq (1/3)$ .

Given the way the automatons are programmed, only one other equilibrium exists. In this equilibrium all players set  $d_{it} = 0$ .

### Prize

Consider now the game with exit prizes. This game differs from the previous game in only one respect. After the players have made their contribution decisions and these decisions have been announced, players in positions A and B vote on whether to award a prize  $\theta z$ , with  $\theta > 0$ , to the player in position C. The prize is awarded only if both of the players in positions A and B vote "yes". The material payoff to players in positions A and B is identical to the one described in equation (1). The material payoff to the player in position C is:

$$w_{it} = v_{it} + e_t \theta z \quad (2)$$

In equation (2)  $e_t$  is a dummy that indexes the outcome of the vote in stage game  $t$ .  $e_t$  takes the value 1 if both of the players in A and B vote "yes", and 0 otherwise. Let state  $S_t$  be:  $d_{it} = 1$  for  $i = (A, B, C)$  and  $e_t = 1$ , in all periods  $t' < t$ . A grim trigger for the game with prize is defined as follows: If (a) in state  $S_t$  set  $d_{it} = 1$ ; (b) in state  $S_t$ , in position A or B, and  $d_{it+1} = 1$  for  $i = (A, B, C)$ , vote "yes". Otherwise set  $d_{it} = 0$  and do not vote "yes". As noted, the automatons in our prize treatment were programmed to follow this grim trigger.

On the grim-trigger equilibrium path the player in position A nets  $(9\beta + \theta)z$ ; the player in B nets  $(6\beta + \theta)z$ ; while the player in C nets  $(3\beta + \theta)z$ . Consider first a single deviation  $d_{it} = 0$  from the equilibrium path of the grim trigger. Such a deviation nets  $(2\beta + 1)z$  for the player in position C; it nets  $(2\beta + 2)z$  for the player in position B; and it nets  $(2\beta + 3)z$  for the player in position A. So, the player in position C has no incentive to deviate if  $\beta \geq (1 - \theta)$ , while the player in position B has no incentive to deviate if  $\beta \geq ((2 - \theta)/4)$  and the player in position A has no incentive to deviate if  $\beta \geq ((3 - \theta)/7)$ . If  $\theta \leq (2/3)$ , then  $(1 - \theta) \geq ((2 - \theta)/4) \geq ((3 - \theta)/7)$ . This means that if  $\theta \leq (2/3)$  and the player in position C has no incentive to deviate by playing not contribute, then nor will players in positions B or A have an incentive to deviate.

Next, consider the possibility of a single deviation from the equilibrium path of the grim trigger consisting of players A or B voting "no" to awarding the prize. By voting "no" a player in position B nets  $z$ , which is clearly less than what he nets if he deviates by setting  $d_{it} = 0$ . By voting "no", a player in A nets  $2z$ , which is clearly less than what he nets if he deviates by setting  $d_{it} = 1$ . Thus, players in positions A and B (who have a choice as to what kind of deviation to perform), will never deviate by voting "no" while continuing to contribute.

Finally, consider a single deviation from the punishment path of grim trigger, consisting of  $d_{it} = 1$ . A player in position C nets  $\beta z$  by deviating, while he nets  $z$  if he abstains. A player in position B nets  $(\beta + 1)z$  by deviating, while he nets  $2z$  if he abstains. Finally, a player in position A nets  $(\beta + 2)z$  by deviating, while he nets  $3z$  if he abstains. Clearly, no player has an incentive to deviate from the punishment path of grim trigger by setting  $d_{it} = 1$ .

The remaining question is whether players in positions A or B can profit by deviating from the punishment path of the grim trigger by voting "yes". The answer is clearly no. A single "yes" is insufficient to change the outcome from no-prize to

prize, so the deviation cannot be profitable. Because voting is cost free, players in A and B do not have a strict incentive to coordinate on a "no" vote. On the other hand, neither do they have a strict incentive to coordinate on a "yes" vote.

Summing up, in the prize treatment the grim trigger is subgame perfect if  $\theta \leq (2/3)$  and  $\beta \geq (1-\theta)$ . Given the way the automatons are programmed, there is only one other subgame-perfect equilibrium. In this equilibrium players in positions A, B and C always set  $d_{it} = 0$  and never vote "yes". In the grim-trigger equilibrium, even a single defection will effectively end cooperation and will thus likely have a definitive effect on the full behavioral path of the group. Moreover, players in positions A and B will have a strong incentive to vote "yes" as long as the players stay on the equilibrium path.

## Design

In our experimental design  $T=5$ ,  $n=3$ ,  $z=100$  and  $\beta=0.6$ . This applied both to the treatment with prize and to the baseline treatment without prize. In the treatment with prize,  $\theta=0.5$ . We specified payoffs in an "experimental currency" called schillings. Every contribution (whether made by a subject or by an automaton) increased the sum of payoffs by 180 schillings, which were distributed equally among the three subjects who were active players in that period (60 schillings per subject), irrespective of the subjects' own decisions. In the prize treatment, lame ducks that were awarded a prize received an additional 50 schillings. We communicated the resulting payoff structure to the subjects via the instructions (available upon request).

We conducted both the prize treatment and the baseline no-prize treatment with three subjects in periods 1, 2 and 3, two subjects and one automaton in period 4, and one subject and two automatons in period 5 (the final period). A game required 5 subjects altogether (cf. table 1). We played four games of the no-prize treatment, and four games of the prize treatment. No subject played more than one game, so this required 40 subjects; 20 subjects in the baseline no-prize treatment, and 20 subjects in the prize treatment. To expand the number of observations, we repeated this basic design with 40 additional subjects. The required 80 subjects were recruited (by e-mail) from undergraduate classes at the BI Norwegian Business School.<sup>13</sup> Since the lab had a capacity of 20 subjects we conducted four sessions, two with the no-prize treatment, and two with the prize treatment. The experiment was conducted on two separate days. Each day we ran one session without prize and one with prize (in that order).

Table 1: Structure of interaction

	<i>Position A</i>	<i>Position B</i>	<i>Position C</i>
Period 1	Subject III	Subject II	Subject I
Period 2	Subject IV	Subject III	Subject II
Period 3	Subject V	Subject IV	Subject III
Period 4	Automaton	Subject V	Subject IV
Period 5	Automaton	Automaton	Subject V

For each session we invited 24 subjects in total. Whenever more than 20 subjects showed up, we withdrew excess subjects through random draws. A withdrawn subject received a show-up fee of 150 NOK (approximately 23 USD). In contrast,

<sup>13</sup> Informed consent to participation was obtained by positive responses to written e-mail invitations with a detailed description of the experimental situation. All procedures – including the execution of the experiment, the recording and management of data and presentation of results – were performed in compliance with applicable laws and regulations on subjects' privacy rights. The experiment was endorsed by the School's committee for coordinating surveys among students.

participating subjects received no show-up fee. We randomly distributed the 20 subjects participating in a given session to the four groups. Also, we randomly assigned numbers from I to V to the five subjects in each group. These numbers determined subjects' starting positions. We communicated the programming of automatons to the subjects through the instructions.

We informed subjects that they would receive their payoffs in cash once their session was over and that schillings would be converted to NOK using the exchange rate 1 schilling = 0.55 NOK. In order to restrict social pressures, subjects entered a separate room one at a time to collect their earnings.

Because it was impossible to earn more than NOK 1000 in this experiment, the payoffs were tax free by Norwegian tax laws. As there was no show-up fee for participating subjects, their minimum theoretical earning was 99 NOK (approximately 15 USD; in the unlikely event that a subject would be the sole contributor in all three periods).

During the experiment all interaction took place via a computer network.<sup>14</sup> Before starting the experiment, the administrator distributed the instructions and read them aloud (to make them public knowledge). The administrator then asked the subjects several control questions to check that they understood the instructions and the payoff structure. We also conducted a test round (without monetary payoffs) to allow subjects to become familiar with the software. After the test round we randomly rematched groups and starting positions, in order to avoid reputation building based on test-round behavior.

To compensate for the fact that participants acting as subject I could make a decision in only one period, we offered those participants additional earnings corresponding to twice the average per period payoff in the group. Similarly, because participants acting as subject II could make a decision in only two periods, we offered these participants additional earnings corresponding to the average per period payoff in the group. The compensation scheme was made public knowledge.

The sessions with prize replicated the sessions without prize, except in the following respect: Once the subjects had taken their contribution decisions, and these decisions were made known to other subjects in their group, the subjects/automatons in positions A and B voted on whether to award the subject in position C a prize of 50 schillings. Awarding the prize required two votes in favor.

## **Descriptive statistics and analytical choices**

In this section we present descriptive statistics for the two types of decisions subjects made during the experiment (contribute vs. not contribute and award prize vs. not award prize).

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<sup>14</sup> The experiment was programmed in Z-tree (Fischbacher 1999).



Table 2: Average percentage of contributions, contingent on position (A/B or C) and treatment (prize or no prize)

Position	<i>Decisions</i>		<i>Subjects</i>		<i>Groups</i>	
	Prize	No prize	Prize	No prize	Prize	No prize
A or B	69.6 (56)	46.4 (56)	70.3 (32)	48.4 (32)	69.6 (8)	46.4 (8)
C	75 (40)	15 (40)	75 (40)	15 (40)	75 (8)	15 (8)

Percentage of decisions, averages over within-subject decision percentages, averages over within-group decisions percentages (N).

Table 2 shows the average percentage of contributions, depending on position (A/B vs. C), and treatment (prize vs. no prize).

Table 3: Average percentage of votes cast by subjects in position A or B to award the prize to the outgoing subject (position C) in the prize game (N)

<i>Decisions</i>	<i>Subjects</i>	<i>Groups</i>
80.4 (56)	82.8 (32)	80.4 (8)

Table 3 shows the average percentage of subjects in positions A or B voting in favor of awarding a prize to the outgoing subject (position C) in the prize treatment. Tables 2 and 3 show little difference between, on one hand, averages based on within-subject means, and, on the other hand, total averages or averages based on group means (the latter two must necessarily yield identical results). As shown in table 2, our data leave us with a maximum of 56 decision-level observations, 40 subject-level observations or 8 group-level observations for contribution decisions within a treatment-position combination. Similarly, table 3 shows that further analysis of subjects' propensity to award a prize can be based on a total of 56 decision-level observations, 32 subject-level observations or 8 group-level observations.

We report results for group-level observations, which we believe bring us closest to the requirement of independent observations. First, we expect dependencies to exist across different decisions made by the same subject; in fact, it would be strange if no learning or adjustment took place. Second, we expect dependencies to exist across subjects in the same group, because of strategic interaction and because subjects in a given group acquire information about the behavior of other players in that group. Third, Kruskal-Wallis tests reveal significant across-group differences in contribution levels and voting patterns between subjects operating under the same experimental condition (i.e. the same combination of treatment and position). This indicates that within-group dependencies actually exist (tests are available upon request).

We use only non-parametric hypothesis tests: Mann-Whitney U-tests (MWU) and Kruskal-Wallis (KW) tests for comparisons among group measures that vary only between groups (e.g. treatment effects and unobserved differences between individual groups); Wilcoxon signed-rank tests (WSR) and Friedman tests for comparisons of group measures that may vary within groups (e.g. differences in contributions between different portions or periods of the game); one-sample location tests (OSL) for comparing certain outcomes with model equilibria. Finally, we use the two-tailed  $p < 0.10$  as the criterion for a significant effect.

## Results

We organize the discussion in two parts. First, we demonstrate that lame ducks contribute more often in the prize treatment than in the no-prize treatment. Second, we explore possible explanations for this finding.

### ***Do prizes induce lame ducks to contribute?***

Both treatments in our experiment include an equilibrium in which no player contributes. However, only the prize treatment also includes an equilibrium in which players in position C contribute. Thus, our main hypothesis is that the fraction of position C players that contribute is on average at least as high in the prize treatment as in the no-prize treatment.

Table 2 shows that, in the no-prize treatment, only 15 per cent of the subjects in position C contribute on average. By contrast, in the prize treatment 75 per cent of the subjects in position C contribute. This 60 percent difference between the two treatments is highly significant in a MWU test ( $Z=-3.41$ ,  $p<0.000$ ), and thus supports our main hypothesis.<sup>15</sup>

Also, the average percentage of votes to award the prize to the outgoing subject in the prize treatment seems quite high in substantive terms ( $P=80.4$ ; see table 3).<sup>16</sup>

To check the robustness of our finding that the prize treatment boosts contributions from the outgoing subject, we check whether a similar difference over treatments exists for positions A and B as well, and whether the difference for position C is stable across periods. A first and overall test can be conducted by comparing overall contribution levels for positions A and B in the prize treatment and the no-prize treatment, respectively (i.e. the difference between  $P=69.6$  and  $P=46.4$  in table 2). It turns out that no significant difference exists here (MWU test,  $Z=-1.45$ ,  $p=0.15$ ). Figure 1 provides additional evidence.

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<sup>15</sup> However, all four percentages in the rightmost column of table 2 are significantly off the mark in relation to any possible equilibrium outcome: OSL tests for  $H_0: P = 0$  and  $H_0: P = 100$  for the prize-AB, prize-C and no-prize-AB conditions and  $H_0: P = 0$  test for the no-prize-C condition all generate p-values less than 0.10.

<sup>16</sup> Invoking the harsher criterion that data should conform not only to a model's directional predictions but also to its point predictions, we note that this percentage is significantly off the mark in relation to both of the model's equilibrium outcomes (specifically, OSL tests of  $H_0: P = 0$  and  $H_0: P = 100$  both produce  $p = 0.01$ ).

Figure 1: Average number of contributions, contingent on position, treatment and period. N=8 for each data point

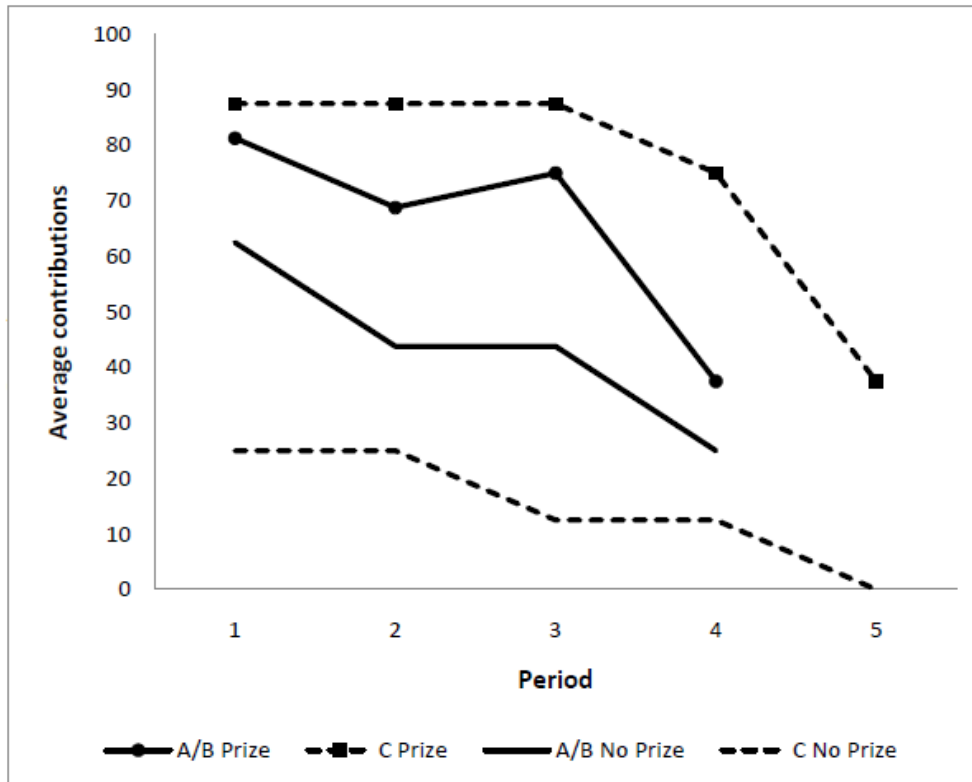


Figure 1 shows that in periods 1 through 3, across-treatment differences for subjects in position C are two to three times larger than the corresponding differences for subjects in positions A or B. In period 4 the across-treatment difference for subjects in position C is a sizeable 5 times larger than the corresponding difference for subjects in positions A or B. In the final period no subject in position C contributes in the no-prize treatment, whereas almost 40 per cent of the subjects in position C contribute in the prize treatment. The pattern of gradually diminishing contributions over time that revealed in Figure 1 resembles the pattern usually observed in non-OLG public goods games (without punishments). It is generally thought that such patterns are due to heterogeneity in social preferences.<sup>17</sup>

Table 4: Mann-Whitney U tests for differences over treatments implied by Figure 1

Position	Treatment	$\chi^2$	p-value
A or B	Prize*	9.15	0.03
	No prize*	9.00	0.03
C	Prize**	9.23	0.06
	No prize**	2.33	0.67

Total N=16 for each test

Table 4 displays MWU tests of the across-treatment differences in contribution levels, conditioned on positions and periods. Whereas a significant difference across treatments exists for the A/B condition in period 3, the other period-by-

<sup>17</sup> Ledyard (1995) provides a rich survey of public goods experiments. See for instance Fehr & Fischbacher (2002) for more recent work on social preferences in public goods experiments.

period across-treatment differences are not significant for this condition. In contrast, all period-by-period across-treatment differences are significant for the C condition. These results make it evident that position C subjects behave differently in the prize and no-prize treatments.

Figure 1 also shows a marked tendency for the average number of contributors to decrease over the five periods. Table 5 presents Friedman tests for differences over the five periods. The results suggest that this tendency is significant for all experimental conditions except the C position in the no-prize game, in which contribution levels consistently remain low throughout.

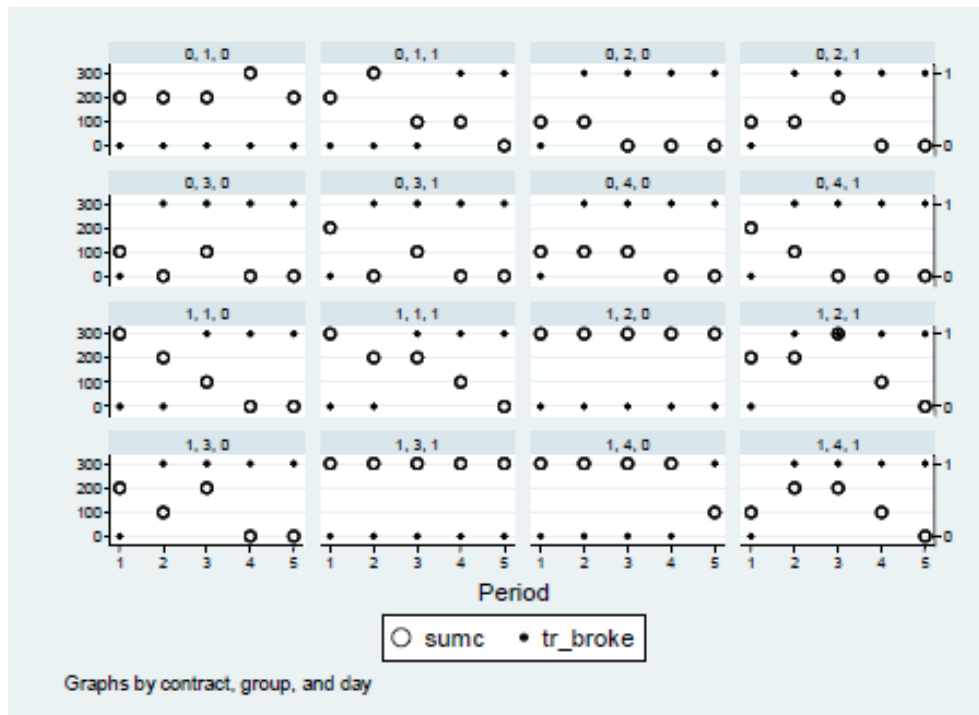
Table 5: Friedman tests for differences over periods implied by Figure 1

Position		Period				
		1	2	3	4	5
A or B	Z-value	-1.47	-1.17	-1.83	-0.45	-
	p-value	0.14	0.24	0.07	0.65	-
C	Z-value	-2.38	-2.38	-2.84	-2.38	-1.78
	p-value	0.02	0.02	0	0.02	0.07

\*Total N=32, \*\*total N=40.

That contribution levels decrease over the five periods is not necessarily incompatible with subjects using trigger strategies to sustain cooperation. We now check if subjects stick to the trigger's punishment phase whenever a deviation from the cooperative equilibrium path has occurred. To do this we condition contribution levels on whether the trigger's punishment phase has been activated. When this phase has been activated, the trigger instructs subjects to cease contributing and, if relevant, cease voting in favor of awarding a prize, for the remainder of the game. Figure 2 conveys a first impression of the results.

Figure 2: Total group contributions (hollow circles, left y-axis) and whether trigger is broken or not (right y-axis black discs) over periods, contingent on contract availability (1=Prize; 0=No Prize), group (1-4), and day (0=First day; 1=Second day)



Total group contributions (hollow circles, left y-axis) and whether trigger is broken or not (right y-axis black discs) over periods, contingent on contract availability (1=Prize; 0=No Prize), group (1-4), and day (0=First day; 1=Second day)

The figure displays the evolution of contributions over the five periods, for the 16 groups in the experiment. The first two rows in the figure show play in the eight groups with no prize, whereas the last two rows show play in the eight groups with prize. Hollow circles indicate the sum of contributions for the group (including contributions from the automatons). These are plotted on the left y-axis. Black discs, plotted on the right y-axis, indicate whether the punishment path of the trigger is activated. The main picture is that contributions tend to fall once the punishment phase is activated, though not instantly (as it should according to theory). While only one group in the no-prize treatment manages to stay on the trigger's equilibrium path for all five periods, two groups manage to do so in the prize treatment (and one additional group stays on the equilibrium path until the final period). Six groups in the no-prize treatment activate the punishment path by period two, compared to three groups in the prize treatment. In all, prizes clearly delay activation of the punishment path.

Table 6 displays average contribution levels in the two experimental treatments for the part of the game following a deviation from the cooperation path of the trigger strategy, conditioned on position.

Table 6: Mean contribution levels over treatments in non-cooperative parts of the game, contingent on position (N)

<i>Position</i>	<i>Prize</i>	<i>No prize</i>
A or B	41.3 (5)	34.3 (7)
C	56.9 (6)	7.1 (7)

Does the behavioral pattern correspond to what we would expect if subjects were to use trigger strategies? First, OSL tests for all but one of the entries (P) in table 7 (the exception is position C contributions in the no-prize treatment) reveal that actual outcomes differ significantly from what one should expect if the trigger had not been activated. Specifically, for these entries tests of  $H_0: P=100$  (i.e. the cooperative equilibrium outcome of an unbroken trigger) all show p-values less than 0.10, which suggests that activation of the trigger at the very least entails a reduction of contribution levels. These results are consistent with the conjectures provided by game theory. Still, OSL tests of  $H_0: P=0$  (i.e. the equilibrium outcome once the trigger has been broken) are also consistently significant ( $p < 0.10$ ) for all table entries, save for contributions from the C position in the no-prize treatment ( $P=7.1$ ,  $p=0.50$ ). In particular, contribution levels in the prize-treatment's C position ( $P=56.9$ ) are way off the equilibrium mark in substantive terms.<sup>18</sup>

### **Gift exchange, efficiency, or focal point effect?**

Why do we observe such a clear breach of equilibrium behavior in our data? In addressing this question we are constrained by our experimental design, and our answers can therefore be suggestive only. We focus on three possible explanations. First, outgoing subjects might be aiming for creating a gift-exchange relationship. By contributing, a position C subject makes a costly sacrifice for the benefit of the group. The remaining subjects can return this favor - at no cost - by awarding a prize to the outgoing subject. Moreover, this holds regardless of whether the cooperative equilibrium path of the trigger has already been abandoned. Previous research from other settings show that gift exchange may provide a stronger motivation for effort than hard incentives do.<sup>19</sup> Note that the presence of a gift-exchange relationship would undermine the credibility of the trigger's punishment phase.

Second, the high observed rates of "yes" votes and of awarded prizes might reflect efficiency concerns. Specifically, awarding a prize increases the subjects' aggregate monetary payoffs, at no cost to those subjects that award the prize.

There is an ongoing debate in the experimental economics literature regarding the extent to which efficiency concerns impact on behavior.<sup>20</sup>

<sup>18</sup> Although the p-value is a "mere" 0.06, this result is most likely influenced by a shortage of observations as some groups (2 out of 8) manage to stay on the cooperative path, as well as by the fact that group-level measures become more unstable as aggregates are based on fewer periods/decisions (only those following a switch to the punishment path of the trigger strategy, which, as noted above, tends to come relatively late in the prize treatment).

<sup>19</sup> Gift-exchange relationships have been studied experimentally by Fehr, Gächter and Kirchsteiger (1997), and by Fehr, Kirchsteiger and Reidl (1993, 1998). In Fehr, Klein and Schmidt (2001) the contract design is endogenous (i.e. subjects choose a contract from a menu of available contracts). Fehr and Fischbacher (2005) provide a brief overview of gift-exchange experiments.

<sup>20</sup> For a seminal paper dealing with this question in a social preferences framework, see Charness and Rabin (2000). Engelman and Strobel (2002) provide a review and some new experiments, which Fehr, Naef and Schmidt (2005) criticize. Güth, Kliemt and Ockenfels (2000) provide an application dealing

Finally, across-treatment differences in contribution and "yes-vote" levels may reflect a focal-point effect (Schelling 1960): In addition to introducing an altered strategic environment, the prize treatment arguably creates a strong focus on cooperative behavior as such.

We utilize the comparative-statics features of our experimental design, first with respect to contributions from the outgoing player and then with respect to "yes-votes". Specifically, our comparisons exploit the fact that subjects' actions (contributions, voting decisions) cannot in any way be decisive for outcomes (awarding the prize) when (1) the trigger's cooperative equilibrium path has been abandoned and (2) automatons have entered the group as players (periods IV and V).

Table 7 lists mean contribution levels for different experimental conditions in which one or two - but not all - of the three mentioned explanations can be operative. If the trigger drives the subjects' behavior, one should observe low contribution levels and little across-treatment differences in cases where the trigger's cooperative equilibrium path has been abandoned. However, the two MWU-tests reported in table 7 show that contribution levels in such cases are in fact significantly higher in the prize treatment (42 to 70) than in the no-prize treatment (0 to 17). Moreover, while the results for the upper row in table 8 (i.e. for the early periods of the game) are clearly at odds with trigger behavior, as well as with behavior driven by efficiency concerns, it is consistent with both the gift-exchange hypothesis and the focal-point hypothesis (i.e. the prize treatment creates a "cooperative focus"). In other words, this particular test cannot distinguish between the two latter hypotheses.

Table 7: Mean contribution levels for outgoing subjects (position C) in periods where non-cooperative behavior has occurred, by treatment and period

<i>Periods</i>	<i>Prize</i>	<i>No prize</i>	<i>Z-value</i>	<i>p-value</i>
2 and 3	70 (5)	16.7 (6)	1.86	0.06
4 and 5	41.7 (6)	0 (7)	2.38	0.02

Z-statistics and p-values for the Mann-Whitney U test (N).

Therefore, it makes sense to compare the entries in the bottom row of table 7. Whereas our two treatments differ in more than one respect in early post-defection periods (II and III), a nice feature of our design is that the treatments do not differ in more than one respect in later periods (IV and V). In our experiment it is public knowledge that automatons will necessarily vote against awarding a prize in periods IV and V, assuming that the cooperative equilibrium path has already been abandoned. Hence, it would be pointless to aim at a gift exchange in these periods. However, the two experimental treatments differ in the focus they create on cooperation. The substantial and significant difference in contribution levels at this point (42 vs. 0) provides a strong indication of a focal-point effect.

Finally, if the gift-exchange hypothesis is correct, one should expect sharply declining contributions from outgoing subjects as opportunities for gift-exchange behavior wither: The pattern for the player in position C of the prize treatment shown in figure 1 is suggestive, with contribution levels decreasing from 87.5 in the first three periods to 50.0 in period IV and 37.5 in period V. However, it is not clear

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directly with public goods provision. Hsu, Anen and Quartz (2008) explore the neurological correlates of equity and efficiency concerns.

to what extent consistently cooperative groups manage to keep the aggregate at relatively high levels. The relevant pattern and test for group aggregates of contributions in situations where the cooperative equilibrium path has been abandoned concern the levels presented in the far left column of table 7. It turns out that a substantial 40 percentage point difference exists in contribution levels between (1) periods where outgoing subjects can be decisive (70) and (2) periods where they cannot be decisive (30, rather than the listed figure of 41.7, which includes a group that cooperates through periods 2 and 3). However, although sizable, this effect is not significant in a WSR test at conventional levels ( $S=3.0$ ,  $p=0.25$ ,  $N=5$ ).<sup>21</sup>

We now turn to voting patterns in the prize treatment. Table 8 lists the group mean of votes cast in favor of awarding a prize to the outgoing player, conditioned upon whether the latter chooses to contribute. Table 8 includes only cases where voting occurs after the trigger's cooperative equilibrium path has been abandoned in periods 1, 2 or 3.<sup>22</sup>

Table 8: Mean share of votes in favor of awarding the prize to the outgoing subject (position C) in periods where non-cooperative behavior has occurred before period 4, by contribution from the outgoing player (N)

<i>Contribution from C</i>	<i>No contribution from C</i>	<i>Total</i>
0.8	0.63	0.75
(5)	(2)	(7)

The overall share of votes cast in favor of awarding a prize after abandoning the cooperative equilibrium path is high (0.75,  $N=7$ ) and significantly different from zero in a simple OSL test ( $p=0.02$ ). While at odds with the hypothesis that subjects play trigger strategies, this finding is consistent with the focal-point hypothesis. However, it is also consistent with the gift-exchange hypothesis and the efficiency-concerns hypothesis. While having the right "reciprocal sign" according to the gift-exchange hypothesis, the difference between the contribute condition and the not contribute condition is insignificant. Table 9 shows that only two group-level units have observations in both conditions. The average percentage of "yes-votes" is 0.63 in the not contribute condition, and increases only moderately to 0.80 in the contribute condition for the same two groups. Needless to say, applying the appropriate WSR test with only two group-level units proves futile ( $S=0.5$ ,  $p=1.00$ ).<sup>23</sup>

In essence, our experiment fails to provide statistically strong support for the gift-exchange hypothesis. However, as we explained in our analysis of contribution patterns, it seems that outgoing subjects do not expect a prize to be awarded

<sup>21</sup> Five groups in the prize treatment have observations both in early periods (II and III) and in late periods (IV and V). In other words, three out of the eight groups are naturally excluded from the analysis: the two groups that manage to stay on the cooperative equilibrium path, and the one group that abandons this path in period IV, so that it plays under the broken trigger only in period V.

<sup>22</sup> We include decisions in period 1, since the trigger strategy may shift to the punishment path not only as a consequence of non-cooperative play in any previous period, but also as a consequence of any departure from a pattern of across-the-board contributions in the current period of play. Also, when looking at voting behavior in post-defection parts of the game we completely disregard patterns in period 4 since any level here is consistent with both the gift-exchange and the efficiency-concerns hypotheses: A voting decision in this situation is not only costless, it is also haphazard since subjects are completely disenfranchised (the outcome of no prize assured by the automatons inevitable exercise of its veto powers).

<sup>23</sup> Departing from our analytic choice of utilizing the group level as our unit of analysis and/or from the sounder practice of applying within-unit tests (WSR) where this is feasible, we also note that the listed difference of  $0.80-0.63=0.17$  in vote share averages between the two situations is not significant in the more lenient MWU test between groups ( $Z=-0.63$ ,  $=0.53$ ,  $N=7$ ) nor in an MWU test with individual decisions as units of analysis ( $\text{diff.}=0.80-0.67=0.13$ ,  $Z=-0.63$ ,  $p=0.53$ ,  $N=26$ ).



regardless of whether they contribute. In the prize treatment, outgoing subjects usually choose to contribute rather than to keep their endowment hoping that remaining subjects will nevertheless award them a prize (see table 6).

In conclusion, outgoing subjects contribute far more often in the prize treatment than in the baseline no-prize treatment. However, our data indicate that this result does not originate from use of trigger strategies. Several statistical tests reveal that behavior differs significantly from what one would expect if trigger strategies were to drive behavior. In the prize treatment, the focal-point effect likely explains the observed behavior; in fact, this is the only alternative explanation for which we find statistically significant evidence. Although there is also some evidence that subjects engage in gift-exchange behavior, the small number of observations in our design makes statistically significant evidence for this proposition infeasible.

## **Conclusion**

Many scholars consider that lame ducks will continue to service their party only if being incentivized. According to the model motivating our experiment, remaining party representatives can discipline lame ducks by credibly promising them exit prizes (such as attractive retreat positions or other post-office perks) if they do not shirk.

We have examined this proposition experimentally. Novel use of automatons allowed us to sidestep the fundamental problem of mimicking parties with indefinite lives in the lab. In one treatment remaining subjects could award cost-free exit prizes to the lame duck. The other treatment did not permit such prizes. In line with the model's prediction, we found that a significantly higher proportion of outgoing subjects contribute in the prize treatment than in the baseline no-prize treatment.

Closer inspection, however, revealed that many outgoing subjects who chose to contribute received a prize irrespective of whether the trigger's punishment path had been activated. While inconsistent with the incentive scheme provided by trigger strategies, this behavioral pattern may be explained in several ways. We have indicated three explanations.

First, the strategic situation generates a gift-exchange relationship. Sheer decency suggests that an outgoing player who contributes should be awarded a prize regardless of play in previous periods. The fact that awarding a prize entails zero cost for remaining players makes this moral imperative particularly compelling. Second, because failure to award a prize would leave money on the table, efficiency concerns imply that a prize should be awarded regardless of previous play. Finally, the prize treatment makes cooperative behavior a powerful focal point, which might explain the observed behavior.

Our data cast considerable doubt on efficiency concerns as a likely explanation, while not allowing us to discriminate between gift-exchange and focal-point effects. The presence of either of these two motives, however, might undermine the credibility of the trigger strategy that supposedly sustains the proposed incentive scheme.

The environment we study is very simple: The structure of the game is public knowledge; legislative (mis)behavior cannot impact on re-election prospects or on future job prospects (through reputational concerns); end-game behavior is automated; there is no ambiguity with respect to lame-duck periods; all interaction is anonymous; and rewards and punishments are fully controlled by the majority of the party. Clearly, it bears little resemblance to the environment of real-world legislative politics. This, however, is hardly an objection. Our environment was designed to control the impact of such other factors, so as to obtain a rigorous test of one particular hypothesis about legislative behavior: Are lame ducks disciplined by endogenously determined exit prizes? Our experiment indicates that they are, although in ways inconsistent with the standard theory of rational, self-regarding

players. This, in turn, suggests that standard theory may be too restrictive to provide a proper understanding of legislative behavior, and that further experiments could be useful to guide future modelling. If subjects in a simple setup like ours deviate systematically from standard theory, one may also expect legislators operating in vastly more complex environments to deviate.

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