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JEL Classification: D02, D31, D72, J16, O10

Keywords: Power, patronage, inequality, institutions, gender differences

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1 Introduction

There are many real-world settings where agents compete for power: such as government, firms, and criminal enterprises. Winning such competitions requires a base of support; and such support is often obtained through patronage. Take Tammany Hall, for instance, which courted New York's newly arrived immigrants with jobs, social services, firewood, and coal. Likewise, the Medici plied Florence's prominent families with generous loans and sweetheart business deals; and gained favor with the general citizenry by building churches, giving to the arts, and distributing food to the poor.[1] This paper uses a laboratory experiment to study competitions for power — and the role of patronage in such competitions.

We construct and analyze a new game — the "chicken-and-egg game" — in which agents compete for power and can engage in patronage. In this game, (finitely-lived) chickens correspond to (finitely-tenured) positions of power and the eggs laid by chickens are the game's currency. The game is played by a group of subjects over multiple rounds. Each round, an election takes place to determine the owner of a newborn chicken. Each subject chooses whether to be a voter or run in the election as a candidate.[2] Prior to voting, candidates can pledge eggs from their existing stock of chickens to voters in return for their votes.

We run the chicken-and-egg game in the laboratory with groups of six subjects, who play for thirty rounds. Seven main results emerge.

First, power distributes unequally — and tends to accumulate in a single person's hands. The number of chickens reaches a steady state in round 6; from that point on, we refer to subjects who own at least 80 percent of chickens as "lords." Lords are extremely common, arising in 40.1 percent of all rounds. Other subjects, furthermore, tend to behave like their vassals — in the sense that they take lords' handouts the majority of the time, rather than run or vote against them.

Second, lords' power is relatively stable. 53 percent of lord tenures are 9 rounds or more.

[1] See Golway (2014) on Tammany Hall and Hibbert (1974) on the Medici.

[2] Our experiment therefore relates to the literature on citizen-candidate models (see Osborne and Slivinski, 1996 and Besley and Coate, 1997).

The average lord tenure is 10.1 rounds.

The emergence and stability of lords reflects a basic force at work in our setting that tends to concentrate power: powerful subjects (i.e., those with chickens) can pledge eggs to voters, which helps them win elections and amass more power. We estimate that winning an election increases the chances of winning future elections by anywhere from 12.8 to 15.8 percentage points.[3]

Third, lords' power is not perfectly stable. 24.9 percent of lord tenures are 4 rounds or less. In 52.1 percent of groups where a lord emerges, the first lord is toppled and replaced by another lord.

The fragility of lords reflects the presence of a countervailing force that tends to disperse power: a preference among voters for underdogs. We find that, after controlling for pledge size, candidates with more chickens receive fewer votes. Our post-experiment survey suggests that voters favor underdogs in part because they care about equity, and in part out of a desire to induce competitive elections, in which candidates have a strong incentive to pledge eggs.

Fourth, we observe substantial wealth inequality as well as power inequality. The wealthiest group member ends the game with 35.5 percent of all eggs on average. While there is considerable wealth inequality, it is less pronounced than power inequality because the powerful transfer eggs to the less powerful. Lords, for instance, give away 28.4 percent of their eggs on average. Such generosity may be a response to voters' propensity to topple lords — especially those who are stingy.

Fifth, some groups are substantially more unequal in power and wealth than others. For instance, in the top quintile of groups — as ranked by their wealth Gini coefficients — the wealthiest subject acquires 52.6 percent of total wealth, compared to 22.6 percent in the bottom quintile. We suspect that group differences are driven by different norms regarding what is fair. In line with this view, subjects in low-inequality groups vote for underdogs more

[3]Relatedly, there is an empirical literature that measures incumbency advantage in various electoral settings. For example, Ansolabehere et al. (2000) estimate a 7-10% incumbency advantage in 1980s and 1990s US House elections (see also Gelman and King, 1990 and Levitt and Wolfram, 1997).

often and report greater concern with equity in our post-experiment survey.

Sixth, in a second treatment, where we eliminate patronage by knocking out the ability to pledge eggs, inequality almost vanishes. Lords never arise and the wealthiest group member captures a much smaller share of the surplus (20.5 percent of all eggs, on average, compared to 35.5 percent in the baseline). Furthermore, we do not see a "power begets power" dynamic. In contrast to the baseline, winning an election does not increase the chances of winning subsequent elections — in fact, it slightly *reduces* the chances.

Finally, in our baseline treatment, there are striking gender differences in outcomes. Women are less powerful and less wealthy. In the tail of the distribution, the differences are particularly dramatic. For instance, women are lords only 32 percent as often as men. These differences in outcomes come about because of small gender differences in style of play, which are compounded by the game's "power begets power" dynamic.[4]

2 Literature Review

Most closely related to our paper is Acemoglu and Robinson's theory of the evolution of political institutions (see Acemoglu and Robinson, 2005, 2012). Acemoglu and Robinson (hereafter, AR) argue that political institutions determine the distribution of economic resources; and economic resources allow agents to shape future political institutions. A vicious cycle can develop where wealthy agents use their resources to amass power; they use their power, in turn, to amass more wealth.[5] Consequently, power and wealth can become concentrated in the hands of a few. A version of AR's vicious cycle arises in our baseline treatment.[6]

Altering one feature of our experiment — the ability to engage in patronage — elimi-

[4] We can attribute gender differences in outcomes to style-of-play differences — rather than gender discrimination — because subjects do not know the genders of other participants.

[5] Zingales (2017) makes a similar argument, with particular reference to political rent-seeking by large firms. Glaeser et al. (2003), likewise, point out that subversion of institutions by the wealthy — specifically, the courts — can exacerbate inequality.

[6] Acemoglu and Robinson's theory fits into a broader literature on institutions as a driver of growth and a determinant of inequality. See, for instance, Glaeser and Shleifer (2002), Rodrik et al. (2004), La Porta et al. (2008); for a review of the literature, see Acemoglu et al. (2005).

nates vicious cycles. This finding is in line with AR's emphasis on "good institutions" as bulwarks against vicious cycles — and suggests that an effective way to reduce inequality may be to curtail patronage systems. The United States, for instance, introduced a series of reforms which were successful in addressing patronage: most notably, the Pendleton Act of 1883, which established a Civil Service Commission, and the Hatch Act of 1939 which forbid bribery of voters and restricted the political activity of government officials.

Importantly, while we allow certain institutions in our experiment to evolve (i.e., who holds power), we take others as fixed. In particular, we impose democratic elections. In so doing, we suppress a force that AR highlight as exacerbating vicious cycles: democratic institutions tend to erode when power and wealth are concentrated.[7] Even absent this force, we observe vicious cycles — an outcome that Acemoglu and Robinson (2008) refer to as "captured democracy."

Our paper also relates to a literature in cultural anthropology on "reverse dominance hierarchies" (see especially Boehm 1993, 1999). Like AR, Boehm sees societies as having a tendency towards authoritarianism; but he argues that the vicious cycle can be broken when those at the bottom of the hierarchy band together to constrain those at the top. One of the key forces leading people to band together is concern for equity. In line with Boehm's idea, subjects in our experiment frequently topple lords; our survey suggests that equity concerns are a primary motivation. Furthermore, lords make substantial transfers to other subjects — presumably to stay in power — which reduces wealth inequality to an extent.

The literature on clientelism is also concerned with vote buying by politicians (see Dixit and Londregan 1996; Wantchekon 2003; Stokes 2005, 2009; Finan and Schechter 2012; and Robinson and Verdier 2013). Issues that have been studied include: whether politicians buy

[7] Take, as an example, the newly formed republics of postcolonial Latin America — many of which modeled themselves explicitly on the United States, adopting presidentialism, bicameral legislatures, and supreme courts. Vicious cycles, nonetheless, led to the emergence of autocrats in most cases — such as Perón in Argentina and Getúlio Vargas in Brazil — who eroded democratic institutions (see Levitsky and Ziblatt, 2018, Chapter 5). Perón, for instance, came to power through vote buying and dispensing political favors. Once in power, he packed the courts with loyal judges who helped keep him in power: by upholding, for example, the conviction of Ricardo Balbín, the leader of the main opposition party (see Acemoglu and Robinson (2012), p. 330).

votes from marginal or core supporters, the policy consequences of clientelism, and why clientelism is associated with poverty and inequality. Our experiment contributes to this literature by showing how clientelism can, over time, lead to concentration of power.

Our paper, of course, fits into an experimental literature on elections (see Palfrey, 2006 for a review). Topics studied include voter turnout, strategic voting, and candidate competition. Our experiment is the first, to our knowledge, to focus on political evolution and vicious cycles.

Finally, we contribute to a literature on inequality, where small differences in initial endowments can lead to large differences in outcomes. For instance, Frank and Cook (2010) argue that the emergence of winner-take-all markets has magnified differences in wealth between stars and other market competitors. Piketty (2014) suggests that a "capital begets capital" process can lead to the entrenchment of a rentier class. Cunha and Heckman (2007) make a "skills beget skills" argument: because of dynamic complementaries in skill formation, small differences in early childhood education can lead to large disparities in later outcomes. Akerlof and Holden (2016) develop a theory in which "social connections beget social connections," leading to the emergence of "movers and shakers" who command large rents. In our paper, inequality stems from a "power begets power" dynamic. Powerful "lords" emerge who play an outsize role in determining the distribution of income.

3 Experimental Design

Subjects in our experiment played a version of the "chicken-and-egg game." The game was played in groups of six over thirty rounds. Subjects were randomly allocated to groups and groups were assigned to either a "baseline" treatment or a "no pledge" treatment. All choices in the game were publicly observable; to preserve anonymity, subjects were given pseudonyms.

Baseline Treatment

In each round of the game, except the final one, an election takes place. The election winner is awarded a chicken, which lays two eggs per round for the next five rounds — or until the end of the game — and then "retires." Eggs are the game's currency and are converted to cash at the end of the experiment.

The outcome of each election is determined by a randomly-selected deciding voter. Candidates can pledge to give some of their eggs to the deciding voter if they win. Elections proceed as follows.

1. Each subject decides whether to be a candidate or a voter. The list of candidates is then publicly announced. In the event that there are no candidates — or no voters — the computer randomly allocates the chicken.

2. Candidates choose how many eggs to pledge to the deciding voter. Candidates can only pledge eggs out of their stock of "fresh eggs" (i.e., eggs laid in the current round). Candidates' pledges are then publicly announced.

3. Voters simultaneously cast votes for candidates. These votes are then made public, and the computer randomly (and publicly) selects a "deciding voter" whose vote determines the election winner.

4. Finally, the election winner gives the pledged amount to the deciding voter. Subjects keep the eggs that they do not give away and accumulate them over the course of the experiment.

In the final round, subjects simply collect the eggs laid by their chickens.

No-Pledge Treatment

The no-pledge treatment differs from the baseline in only one respect: candidates do not have the option to make pledges.

Procedural Details

The experiment was conducted at Nanyang Technological University in Singapore between August 2018 and September 2019 and was programmed in zTree (Fischbacher, 2007). Subjects were recruited by email from the undergraduate population. A total of 456 subjects participated in the experiment over 21 sessions (see Table 1).

Table 1: Treatment Descriptions

Treatment	Sessions	Sample size
Baseline	15	330
No Pledge	6	126
Total	21	456

Randomization into treatments took place at the session level. Each session contained at least three groups of six participants.

At the start of the experiment, subjects received written instructions, which were also read aloud, and played two non-incentivized practice rounds.[8] At the end of the experiment, subjects were asked to complete a non-incentivized survey about their motivations during the experiment.[9]

The eggs subjects accumulated were converted to Singapore dollars at the rate of 5 eggs to $1. Subjects also received a $5 show-up fee. The experiment lasted about 90 minutes and subjects earned an average of $14.20.

Discussion

We will now take a moment to highlight several features of our design.

Elections. Power is acquired in our experiment through elections, so it is natural to think of the experiment as speaking to the democratic process. We like to think of voting in our experiment, though, as simply an act of fealty or support. Under this interpretation, the

[8] Subjects played one practice round as a voter and one as a candidate.

[9] In our first three baseline-treatment sessions, subjects received a different survey with more open-ended questions. The results we report in the paper come from the later version of the survey.

experiment speaks to a wide range of settings — not just those where power is contested through formal elections. The experiment might also speak to military conflicts between warlords or the accumulation of influence within organizations.

Only fresh eggs can be used to make pledges. In our baseline treatment, only fresh eggs (eggs laid in the current round) can be used to make pledges. Consequently, power (chickens) — rather than wealth (eggs) — is what determines a subject's ability to pledge. This design choice highlights that many common forms of patronage (e.g., public-sector jobs) are only possible with political power; we recognize, of course, that the ability to engage in patronage depends upon both power and wealth in most political settings.

Chickens retire after five rounds. Chickens in our game have finite lifespans. Hence, a subject must continually win elections in order to hold onto power. A further implication is that, from round 6 onwards, there are always five living chickens, since each chicken "birth" is offset by a retirement. A subject's power can be measured, from round 6 onwards, by the number of chickens they own out of five.

Fixed number of chickens and eggs. The game is zero-sum, with a fixed surplus of 270 eggs. As such, we will be principally interested in the division of this surplus. There is also a fixed amount of power allocated over the course of the game. We will also be interested in the distribution of this power.

Explicit vote buying. In practice, vote buying is rarely explicit. More commonly, candidates make gifts to voters; and voters reciprocate out of sense-of-duty or fear of retaliation. In our baseline treatment, by contrast, vote buying is explicit. Voters are only eligible to receive eggs if they vote for a candidate. Explicit vote buying simplifies the game. Our game can easily be modified, though, to make vote buying more implicit (e.g., by having candidates make unconditional gifts to voters).

Deciding voter. We chose to have a deciding voter — who receives the entirety of the election winner's pledge — because it reduces strategic complexity. For instance, if the election winner's pledge were divided between the winner's supporters, voters would need to take into account the likely split of the pledge. If, additionally, there were plurality voting, voters

would need to factor in each candidate's chances of winning.

4 Results of the Baseline Treatment

We start by relating the findings of our baseline treatment, where candidates can pledge eggs to voters.

Emergence of Lords

Our first finding is that power tends to concentrate in the hands of a single person. From round 6 onwards, we refer to subjects as "lords" when they own at least four out of five living chickens. 87.3 percent of groups have a lord in at least one round. Across all groups, 40.1 percent of rounds have a lord.

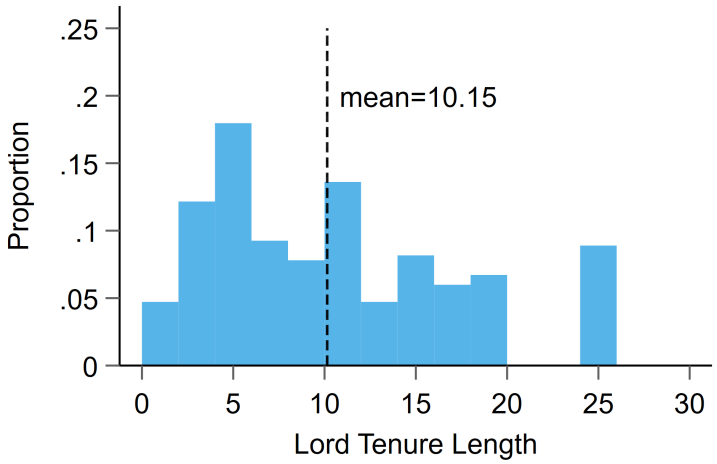


Figure 1: Distribution of Lord Tenures[10]

Furthermore, power is relatively stable. Figure 1 shows how long lords tend to stay in power. Following Clark and Summers (1979), the distribution shown in Figure 1 is weighted by tenure length.[11] While there are some short tenures, 53 percent of tenures are 9 rounds

[10] The distribution is weighted by tenure length. A tenure is defined as a continuous spell as a lord. Some subjects have multiple spells as a lord and therefore appear more than once.

[11] To understand why we weight by tenure length, consider the following example adapted from Clark and Summers (1979). Suppose there are 20 lords with tenures of one round and one lord with a tenure of 20

or more. The (weighted) average lord tenure is 10.1 rounds.[12]

Why do lords emerge? We find that there is a "power begets power" dynamic in the game: that is, having power (i.e., chickens) makes it easier to win elections and acquire more power. We believe that this dynamic accounts for the emergence of lords.

Figure 2 provides suggestive evidence of such a dynamic. It shows that having chickens is positively correlated with winning elections.

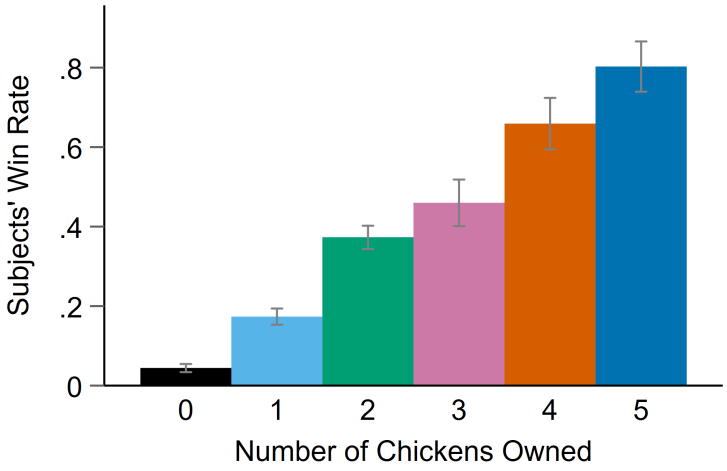


Figure 2: Power and Win Rates[13]

We can exploit randomness in election outcomes to formally test whether such a dynamic exists. In some elections, several candidates receive the same number of votes; and one wins rather than the others purely due to chance. We find that winners of such "balanced" elections win 12.8 percentage points more often in subsequent rounds than equally-popular losers (see Table 2).

rounds. The mean lord tenure is only 1.9 rounds; however, half of all rounds with a lord are accounted for by a 20-round tenure. Hence, focusing on mean tenure underweights long tenures. Clark and Summers (1979) argue that a solution is to look at the distribution of tenures one would expect to observe in a given round. Weighting by tenure length accomplishes this.

[12] Figure A.1, in the Appendix, shows that the prevalence of lords is more-or-less constant over the course of the game. There are two time trends of note in Figure A.1, though. First, candidates pledge slightly less in the final four rounds — most likely because the chickens-to-be-won are less productive (they lay eggs for less than five rounds). Second, there is a decline over time in the number of candidates.

[13] Figure 2 restricts attention to Rounds 6 - 29. Observations are at the subject-round level. Error bars indicate 95% confidence intervals, with errors clustered at the group level. As further evidence of a relationship, in an OLS regression of whether a subject won on number of chickens owned, the coefficient on number of chickens owned is positive and significant ($p = 0.000$).

Alternatively, we can use the first election to test for a "power begets power" dynamic. In the first round, all candidates look the same given that there is no prior history of play and no candidate has eggs to pledge. Consequently, it is (essentially) random which subject, among those who run, wins the first election. We find that first-round winners win 15.8 percentage points more often in subsequent rounds than first-round losers (see Table 2). The only potential concern is that subjects may systematically vote for certain pseudonyms over others; but the results remain similar after including pseudonym fixed effects.

Table 2: Tests of the "Power begets Power" Dynamic

Dep Var: Future Win Rate	Balanced Election	First Election	First Election
Won	0.128***	0.158***	0.153***
	(0.036)	(0.046)	(0.049)
Won × Three-way tie	-0.026		
	(0.094)		
Constant	-0.046**	0.161***	0.184***
	(0.018)	(0.009)	(0.040)
Election Fixed Effects	✓	✓	✓
Pseudonym Fixed Effects			✓
Observations	617	231	231

* 0.10 ** 0.05 *** 0.01, OLS with candidate-election-level observations. Standard errors are clustered at the group level. An election is balanced if two or three candidates tie for first in votes obtained. "Three-way tie" is a dummy for whether, in a balanced election, three candidates tied for first.

As one might expect given the game's "power begets power" dynamic, it is incredibly valuable to win the first election. First-round winners earn 33.1 more eggs on average, and are 36.1 percentage points more likely to become lords than first-round losers (see Table A.2, in the Appendix).[14]

Why does power beget power? Patronage is critical to the emergence of a "power begets power" dynamic. In the no-pledge treatment, the dynamic completely vanishes (see Section 5). This result is intuitive. A chicken gives a subject eggs to pledge; and pledging eggs (pre-

[14]The 33.1 egg boost in earnings is significantly greater (p=0.0039) than the 10 eggs laid by the first chicken. The 36.1 percentage-point boost in the chance of becoming a lord is also significant (p=0.000).

Table 3: Pledging Behavior and Electoral Success

	Won Election
Eggs Pledged	0.621***
	(0.043)
Observations	3724

* 0.10 ** 0.05 *** 0.01. Conditional logit (Rounds 6-29), grouped at the election level, with candidate-election-level observations. We restrict attention to elections with more than one candidate. Standard errors are clustered at the group level.

sumably) helps a subject win further chickens. Indeed, in contested elections, we observe a positive correlation between winning and the amount pledged (see Table 3).

Who becomes a lord? Lords are more likely than other subjects to be male and less likely to major in humanities or arts (see Table A.3). Other characteristics that we measure do not differ systematically between lords and other subjects (year of study, inequity aversion, authority preference).

In Section 6, we will discuss gender differences in outcomes in greater depth.

Emergence of Vassals

When a lord is present, other subjects tend to behave like "vassals": they vote for the lord rather than run or vote against the lord the majority of the time. On average, when a lord runs for election, 50.2 percent of other subjects vote for the lord, compared to 13.9 percent who vote for another candidate and 35.9 percent who challenge the lord. Vassal-like behavior makes it easier for lords to retain power, and thus reinforces the "power begets power" dynamic.

More generally, electoral competition is weaker when power is more concentrated. Figure 3 shows that there are fewer candidates, on average, when the most powerful group member has more chickens.

Why do lords face few challengers? There is an economic case to be made for behaving like a "vassal." First, challengers rarely beat lords. In elections where a lord is challenged, the

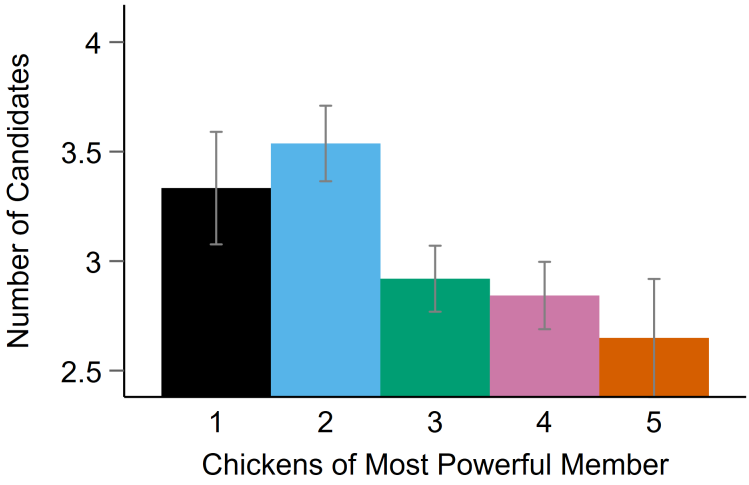


Figure 3: Power Concentration and Run Rates[15]

lord wins 72.3 percent of the time. Second, lords give substantial handouts to subjects who vote for them: subjects who vote for a lord receive 1.04 eggs, on average. Subjects forgo the opportunity to partake of these handouts when they challenge a lord. Our post-experiment survey suggests that these economic considerations were at the forefront of subjects' minds (see Table A.1).

Who behaves like a vassal? Table A.4 examines whether there are traits that correlate with vassal-like behavior. Specifically, in rounds with a lord, we compare subjects who vote for the lord to subjects who oppose the lord. Most of the traits we measure (year of study, major, authority preference, inequity aversion) do not correlate with vassal-like behavior. The one exception is gender. We find that men are more likely than women to oppose lords.[16]

Fragility of Lords

Since "power begets power," one might expect lords to hold onto power indefinitely. However, we find that power is not perfectly stable. While Figure 1 shows that some lord tenures

[15] Figure 3 restricts attention to Rounds 6 - 29. Observations are at the group-round level. Error bars indicate 95% confidence intervals, with errors clustered at the group level. As further evidence of a relationship, in a group-round level OLS regression of number of candidates on chickens of the most powerful group member, the coefficient on chickens of the most powerful group member is negative and significant ($p = 0.000$).

[16] Although male voters do not oppose lords significantly more often than female voters, men are significantly more likely than women to run against lords. Overall, we find that men have higher run rates than women (see Section 6).

are long, it also shows that many are short. 24.9 percent of tenures are 4 rounds or less. Furthermore, power often changes hands. The first lord to emerge is toppled and replaced by another lord in 52.1 percent of groups where at least one lord emerges.

What causes lord tenures to end? One way in which a lord can lose power is by failing to run for election. However, lords run 95.6 percent of the time; and only 15.7 percent of tenures end because a lord fails to run. In the vast majority of cases where tenures end, lords run and lose to another candidate.

Table 4 shows that lords' pledges decline at the end of their tenures and that lords with longer tenures pledge more (on average). These findings suggest that lack of generosity plays a role in ending lords' tenures. That said, lords are rarely out-pledged — even at the end of their tenures. In 82.9 percent of rounds where a lord loses power (despite running for election), the lord pledges strictly more than other candidates. In these rounds, voters oppose lords even though they sacrifice eggs in that round by doing so.

Table 4: Lords' Pledges

Dep Var: Lord's Pledge	(1)	(2)	(3)
Last Round of Lord's Tenure	-0.733***		-0.370**
	(0.192)		(0.180)
Total Length of Tenure		0.306***	0.285***
		(0.063)	(0.062)
Owns Five Chickens	-0.567	-0.542*	-0.656**
	(0.337)	(0.296)	(0.319)
Round	0.023	-0.025	-0.021
	(0.034)	(0.030)	(0.029)
Constant	2.131***	1.126***	1.292***
	(0.312)	(0.239)	(0.278)
Group Fixed Effects	✓	✓	✓
Observations	305	305	305

* 0.10 ** 0.05 *** 0.01. OLS with lord-election-level observations. Standard errors are clustered at the group level. We restrict attention to elections where a lord runs and to lord tenures that eventually end (i.e., prior to Round 30). "Last Round of Lord's Tenure" equals one if it is the final round of the lord's tenure spell, and zero otherwise. "Total Length of Tenure" is defined as the total duration of the tenure spell of which the current round is a part.

Why do voters sacrifice eggs to oppose lords? Voters' appear to have a preference for "underdog" candidates, which motivates their opposition to lords. We find, for instance, that owning chickens hurts — rather than helps — candidates after controlling for pledge size (see Table 5, Column 2).

Table 5: Determinants of Candidate Vote Share

Dep Var: Voted for Candidate	(1)	(2)
Candidate's Number of Chickens	0.425***	-0.096***
	(0.024)	(0.036)
Candidate's Pledge		0.507***
		(0.043)
Candidate Made Largest Pledge		0.482***
		(0.094)
Observations	9464	9464

* 0.10 ** 0.05 *** 0.01. Conditional logits (Rounds 6 - 29), grouped at the voter-election level, with candidate-voter-election-level observations. Standard errors are clustered at the group level.

Subjects also indicated in our post-experiment survey that they were inclined to vote for underdogs (see Table A.1). There appear to have been two reasons for this preference. One reason was concern about equity. In our survey, subjects indicated that they sometimes voted against the candidate with the most chickens because they saw it as fair. Indeed, subjects saw winning chickens as largely a matter of luck, which may have inclined them to undo such lucky outcomes by voting against lords.[17]

Subjects also reported that they supported underdogs because it was in their long-term economic interest. In our survey, they indicated that they voted against the candidate with the most chickens because they thought competition would increase the size of pledges. We

[17] The average response was 6.6 out of 10 to the question: "To what extent do you think winning chickens was a matter of luck?" (see Table A.1). Consistent with our findings, Fehr and Schmidt (1999) have shown that people are willing to act against their economic interest for the sake of equity; and Alesina and Angeletos (2005) and Benabou and Tirole (2006) find that people are particularly concerned with equity when they see outcomes as due to luck.

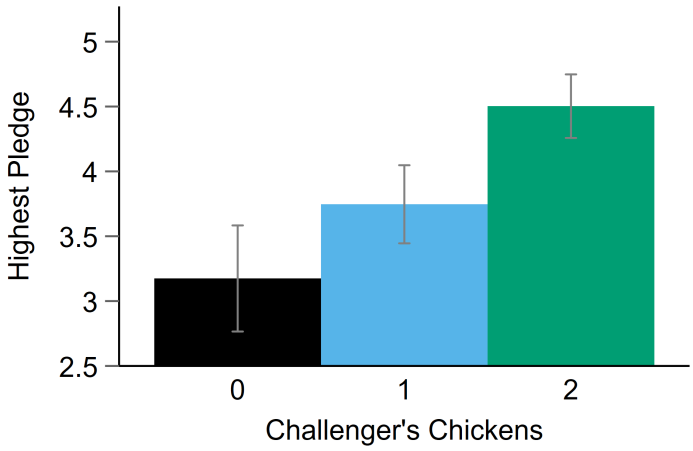


Figure 4: Competition and Pledge Sizes[18]

do, in fact, find that candidates pledged more when competition was greater. Figure 4 looks at rounds where there is a single challenger to the leader. It shows that the largest pledge is strictly increasing in the challenger's size.[19]

Wealth Distribution

Considerable inequality — in power and wealth — emerge in our experiment. We measure a subject's power (wealth) by the total number of chickens won (eggs accumulated) over the course of the experiment. Figure 5 shows that, on average, the most powerful subject acquires 49.5 percent of total power — compared to 2.1 percent for the least powerful subject. The wealthiest subject acquires 35.5 percent of total wealth, compared to 6.2 percent for the least wealthy subject. The figure shows that, while wealth inequality is substantial, it is less pronounced than power inequality. Overall wealth inequality — as measured by the average group Gini coefficient — is 0.32, compared to 0.51 for power (this difference is significant in a paired t-test, $p = 0.000$).

[18] Figure 4 restricts attention to Rounds 6 - 29 and to cases where the top 2 candidates own all five chickens. Error bars indicate 95% confidence intervals, with errors clustered at the group level. As further evidence of a relationship, in an OLS regression of highest pledge on challenger's size, the coefficient on challenger's size is positive and significant ($p = 0.000$).

[19] Table 4 gives additional suggestive evidence that competition increases the size of pledges. It shows that lords with five chickens pledge fewer eggs than lords with four chickens.

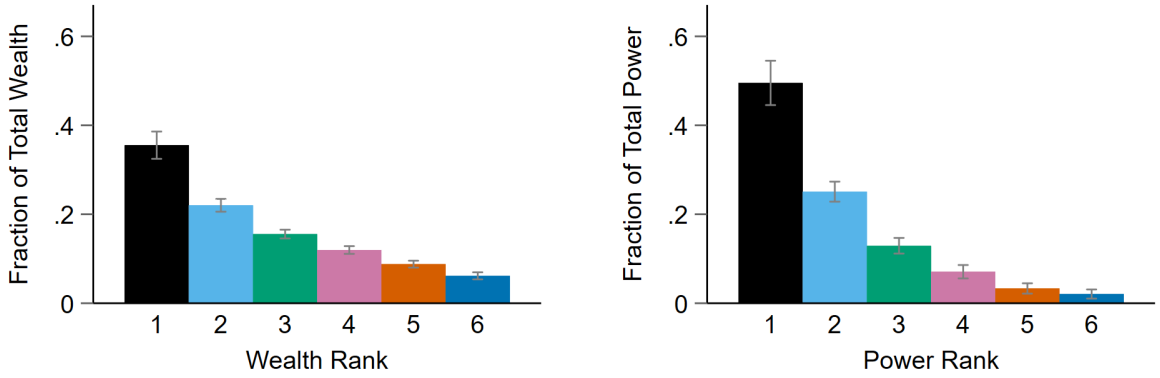


Figure 5: Wealth and Power Inequality[20]

Wealth inequality is less pronounced because the powerful transfer some of their eggs to the less powerful. For instance, lords give away 28.4 percent of their eggs on average. Figure 6 shows that, within groups, less-wealthy subjects obtain most of their eggs from transfers; in contrast, wealthy subjects obtain most of their eggs from their own chickens. In the average group, transfers make up 41.3 percent of total earnings.

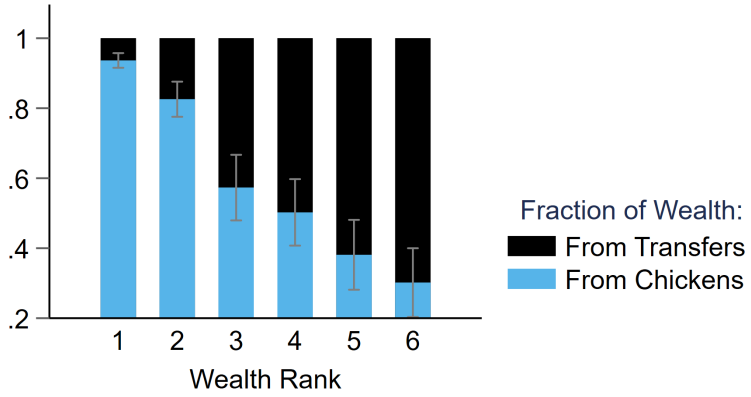


Figure 6: Sources of Wealth, by Subject Rank[21]

Why do people give away eggs? Subjects may give away eggs because they consider it fair. Alternatively, they may give away eggs to acquire or retain power. Power may be its own

[20] In Figure 5, observations are at the subject level. Error bars indicate 95% confidence intervals, with errors clustered at the group level. Ties in rank are broken at random.

[21] In Figure 6, observations are at the subject level. Error bars indicate 95% confidence intervals, with errors clustered at the group level. Ties in rank are broken at random. As further evidence of a relationship, in a OLS regression of fraction of wealth from chickens on wealth rank, the coefficient on wealth rank is negative ($p = 0.000$).

reward; it may also, ultimately, lead to a higher egg payoff.

Figure 7 shows that subjects pledge a larger fraction of their eggs when they have fewer chickens. If subjects were concerned solely with fairness, it seems natural that they would pledge a smaller fraction of their eggs when they have fewer chickens. Hence, Figure 7 provides suggestive evidence that subjects give away eggs, at least in part, because they value power. Likewise, our survey indicates that, while fairness was a concern, pledging was more driven by subjects' desire to win elections (see Table A.1).

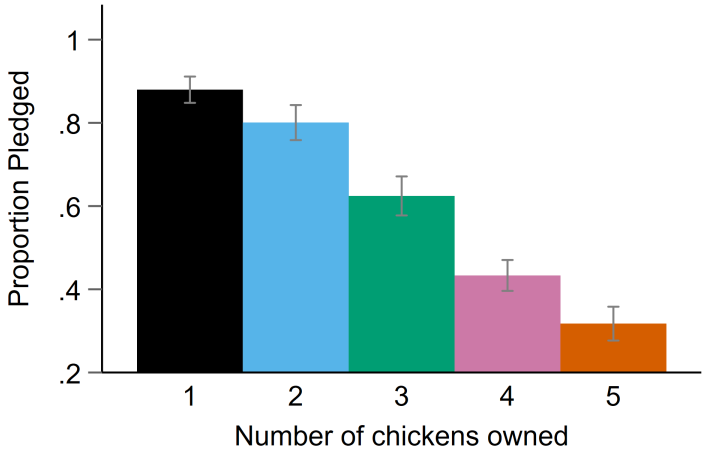


Figure 7: Power and Pledging Behavior[22]

Differences Across Groups

Some groups are substantially more unequal than others (see Figure 8). Suppose we rank groups by their wealth Gini coefficients. In the top quintile of groups, the average Gini coefficient is 0.47 and the wealthiest subject acquires 52.6 percent of total wealth (on average). In the bottom quintile, inequality is much lower: the average Gini coefficient is 0.15 and the wealthiest subject only acquires 22.6 percent of total wealth (on average).[23]

[22] The data in Figure 7 is from Rounds 6 - 29. Observations are at the candidate-round level. Error bars indicate 95% confidence intervals, with errors clustered at the group level. As further evidence of a relationship, in an OLS regression of proportion pledged on chickens owned, with standard errors clustered at the group level, the coefficient on chickens owned is negative and significant ($p = 0.000$), and remains so when we include individual fixed effects ($p = 0.000$).

[23] The wealth Ginis of the top and bottom quintiles are significantly different in a two-sided t-test ($p = 0.000$), as are the power Ginis ($p = 0.000$).

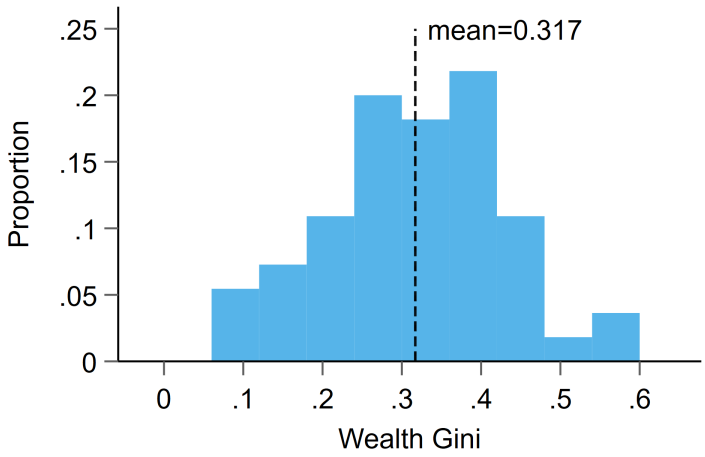
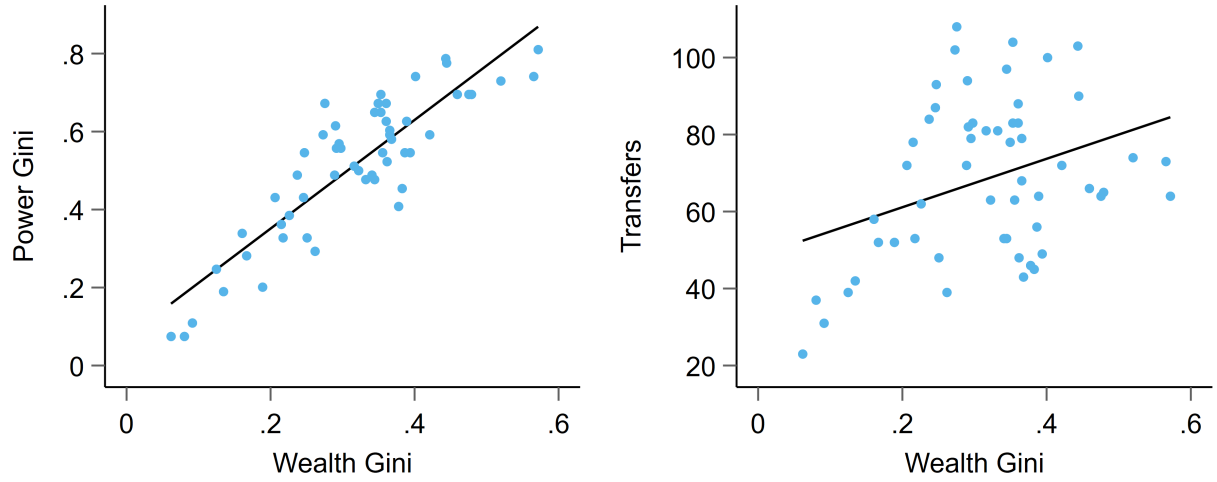


Figure 8: Wealth Inequality Across Groups

Groups in the bottom quintile achieve equal outcomes by distributing power equally rather than by transferring wealth. Figure 9 shows that wealth inequality and power inequality are highly correlated; it also shows that transfers are not particularly large in low-inequality groups (transfers are actually lower than in high-inequality groups).



(a) Wealth Inequality and Power Inequality **(b)** Wealth Inequality and Transfers

Figure 9: Correlates of Wealth Inequality[24]

[24]Figures 9a and 9b depict group-level OLS regressions of (a) power Gini on wealth Gini and (b) transfers on wealth Gini. The wealth Gini coefficients for both regressions are positive and significant: (a) $p = 0.000$, $R^2 = 0.766$; (b) $p = 0.008$, $R^2 = 0.124$.

Why do groups differ? We suspect group differences are driven by different norms regarding what is fair. In line with this view, we find that voters show a greater preference for underdogs in low-inequality groups (see Table 6). We also find, in our post-experiment survey, that subjects in low-inequality groups are more concerned with fairness and less concerned with winning eggs (see Table A.6).

Table 6: Determinants of Candidate Vote Share

	Voted for Candidate
Candidate's Pledge	0.590***
	(0.045)
Candidate Made Largest Pledge	0.538***
	(0.095)
Candidate's Number of Chickens	-0.720***
	(0.097)
Candidate's Number of Chickens × Group's Wealth Gini	1.540***
	(0.242)
Observations	9464

* 0.10 ** 0.05 *** 0.01. Conditional logit (Rounds 6 - 29), grouped at the voter-election level, with candidate-voter-election-level observations. Standard errors are clustered at the group level.

5 The Role of Pledges

We turn now to the results of the no-pledge treatment. Our theory is that patronage gives rise to a "power begets power" dynamic in the baseline treatment, which in turn generates inequality. The no-pledge treatment allows us to test this hypothesis. In the no-pledge treatment, we eliminate patronage by knocking out the ability to pledge eggs. If our hypothesis is correct, we should see less inequality in the no-pledge treatment; furthermore, the "power begets power" dynamic should disappear. This is indeed what we find.

Inequality is dramatically lower in the no-pledge treatment (see Figure 10). The average wealth Gini is 0.08 (versus 0.32 in the baseline) and the wealthiest group member captures just 20.5 percent of the total surplus on average (versus 35.5 percent in the baseline). The

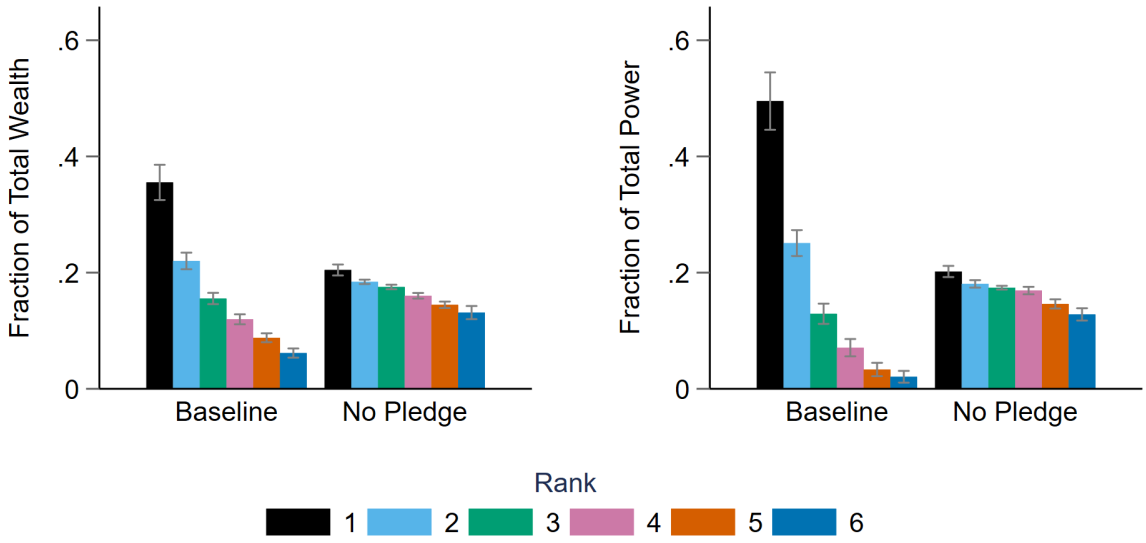


Figure 10: Wealth and Power Inequality, by Treatment[25]

average power Gini is 0.08 (versus 0.51 in the baseline) and the most powerful group member captures just 20.2 percent of total power (versus 49.5 percent in the baseline). Furthermore, in the no-pledge treatment, we never see a lord, compared to 40.1 percent of rounds (87.3 percent of groups) in the baseline treatment.[26]

The "power begets power" dynamic is also absent in the no-pledge treatment. In fact, we find that winning an election *hurts* rather than helps in subsequent rounds — perhaps due to subjects' concern about equity. The winners of "balanced" elections win 6.9 percentage points less often in subsequent rounds of the game than equally-popular losers (see Table 7). Similarly, the first-round winner wins 1.3 percentage points less often in subsequent rounds than first-round losers.

[25] Observations are at the subject level. Error bars indicate 95% confidence intervals, with errors clustered at the group level.

[26] These differences are all significant ($p = 0.000$) under two-sided t-tests.

Table 7: Tests of the "Power begets Power" Dynamic in the No-Pledge Treatment

Dep Var: Future Win Rate	Balanced Election	First Election	First Election
Won	-0.069***	-0.013	-0.012
	(0.023)	(0.011)	(0.010)
Won × Three-way tie	-0.004		
	(0.040)		
Constant	0.213***	0.173***	0.167***
	(0.011)	(0.003)	(0.008)
Election Fixed Effects	✓	✓	✓
Pseudonym Fixed Effects			✓
Observations	317	95	95

* 0.10 ** 0.05 *** 0.01, OLS with candidate-election-level observations. Standard errors are clustered at the group level. An election is balanced if two or three candidates tie for first in votes obtained. "Three-way tie" is a dummy for whether, in a balanced election, three candidates tied for first.

Comparing Inequality Against a Benchmark

It is almost inevitable that some inequality will arise in the game simply due to chance. To assess whether subjects' behavior results in additional inequality — beyond this background level — it is useful to compare our two treatments against a benchmark. Consider a benchmark where subjects are completely passive: they always run for election (or, equivalently, never run for election). The outcome in this case is that, each round, the chicken is randomly allocated and no transfers take place.

Figure 11 compares wealth inequality in the two treatments against this passive benchmark. We obtained a distribution of outcomes for the benchmark by simulating play for 10,000 groups. It is not surprising, given the "power begets power" dynamic, that inequality in the baseline treatment (average Gini of 0.32) is higher than the benchmark (average Gini of 0.22). Interestingly, inequality in the no-pledge treatment is lower than the benchmark (0.08 versus 0.22); in other words, subjects actually reduce inequality rather than contribute to it.[27]

[27] The distributions are all pairwise significantly different ($p = 0.0000$) under Kolmogorov-Smirnov tests.

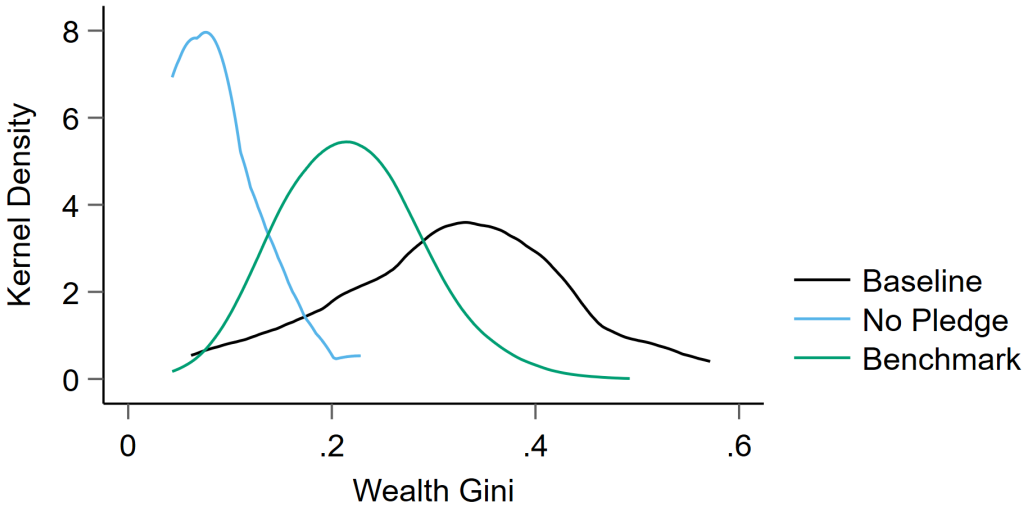


Figure 11: Wealth Gini Distributions by Treatment

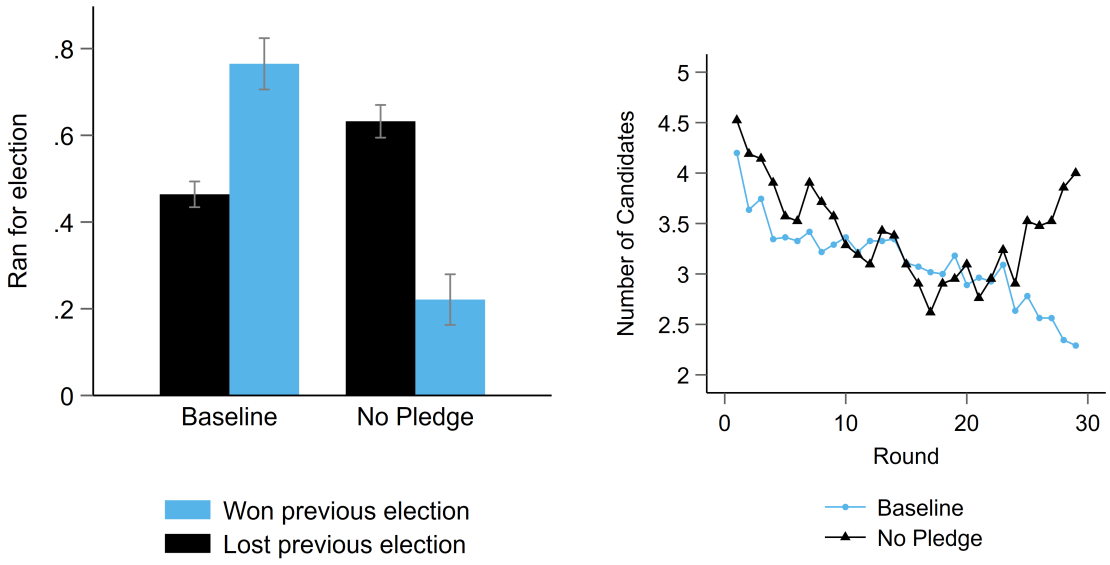
One way in which subjects reduce inequality in the no-pledge treatment is through their voting behavior. In the no-pledge treatment, candidates with more chickens receive fewer votes — in contrast to the baseline treatment where they receive more (see Table 8). Presumably, absent pledging, voters are freer to express an underdog preference.

Table 8: Candidate Vote Shares vs. Candidate Power, by Treatment

Dep Var: Voted for Candidate	No Pledge	Baseline
Candidate's No. of Chickens	-1.598***	0.425***
	(0.224)	(0.024)
Observations	3690	9464

* 0.10 ** 0.05 *** 0.01. Conditional logits (Rounds 6 - 29), grouped at the voter-election level, with candidate-voter-election-level observations. Standard errors are clustered at the group level.

Subjects also reduce inequality in the no-pledge treatment by taking turns at winning. Figure 12a shows that election winners rarely run in the next round: they run 22.1 percent of the time, compared to 63.2 percent for election losers. By contrast, in the baseline treatment, election winners run in the next round 76.5 percent of the time, compared to 46.4 percent for election losers.



(a) Winners vs. Losers

(b) By Round

Figure 12: Run Rates[28]

We speculate that the taking-of-turns reflects, at least in part, a relational contract that restrains subjects from running too frequently. Such a relational contract may be easier to enforce in the no-pledge treatment than the baseline, where patronage can create temptations to renege. Figure 12b shows that run rates increase in the no-pledge treatment towards the end of the game, which we interpret as a breakdown of the relational contract in the final rounds.

6 Gender Differences

There are large gender differences in outcomes in our baseline treatment (see Table 9).[29] On average, women have only 84.7 percent of the wealth of men and only 70.4 percent of the power. The differences are particularly striking in the tail of the distribution. Women are

[28] The data in Figure 12 is from Rounds 1 - 29. In Figure 12a, observations are at the subject-round level and error bars indicate 95% confidence intervals, with errors clustered at the group level.

[29] Seven subjects chose not to disclose their gender (six subjects in the baseline treatment and one in the no pledge treatment). Our results on gender differences are based on the remaining 449 subjects who disclosed their gender.

Table 9: Gender Differences in Outcomes

	Baseline			No Pledge		
	Female mean (sd)	Male mean (sd)	Difference (p-value)	Female mean (sd)	Male mean (sd)	Difference (p-value)
Wealth	41.467	48.971	-7.505***	44.333	45.549	-1.216
	(24.554)	(34.858)	(0.007)	(8.358)	(7.710)	(0.496)
Power	4.007	5.690	-1.683***	4.704	4.930	-0.226
	(4.160)	(6.377)	(0.002)	(0.944)	(0.799)	(0.246)
Was ever a Lord	0.147	0.322	-0.175***	0.000	0.000	0.000
	(0.355)	(0.469)	(0.000)	(0.000)	(0.000)	(.)
Rounds as a Lord	0.793	2.483	-1.689***	0.000	0.000	0.000
	(2.400)	(5.085)	(0.000)	(0.000)	(0.000)	(.)
Was Wealthiest	0.120	0.213	-0.093**	0.167	0.169	-0.002
Group Member	(0.326)	(0.410)	(0.020)	(0.376)	(0.377)	(0.974)

* 0.10 ** 0.05 *** 0.01. For tests of differences, standard errors are clustered at the group level.

only 56.3 percent as likely as men to end the game as the wealthiest group member; they are only 45.6 percent as likely as men to ever become lords; and they are lords only 31.9 percent as often. In the no-pledge treatment, by contrast, there are no significant differences between the genders.

What accounts for differences in the baseline treatment? Subjects cannot be discriminated against for their gender since they are only identified by gender-neutral pseudonyms — such as "Mushroom" and "Spinach." Therefore, differences in outcomes must be due to differences in style of play.

While there are gender differences in style of play, they are small — and they seem to belie the dramatic gender disparities in outcomes. The most notable style-of-play difference is that women run for election less often than men: 12.5 percentage points less often in the first round and 5.5 percentage points less often overall (see Table 10). This is probably not the only style-of-play difference, though, since women win less often when they run for election, controlling for number of chickens owned (see Table 11). For example, women with three chickens — who are on the cusp of becoming lords — have a win rate of only 42.8 percent, compared to 59.9 percent for men. Women in our sample pledge less than men (see Table 10), which we suspect is one reason for their lower win rates. Women with three chickens,

Table 10: Gender Differences in Style-of-Play

	Female mean (sd)	Male mean (sd)	Difference (p-value)
Run Rates			
Round 1	0.633	0.759	-0.125**
	(0.484)	(0.429)	(0.015)
Overall	0.492	0.547	-0.055**
	(0.199)	(0.231)	(0.035)
Proportion Pledged Conditional on Running and Chickens Owned[†]			
1 Chicken	0.854	0.905	-0.051*
	(0.240)	(0.189)	(0.058)
2 Chickens	0.774	0.839	-0.065*
	(0.201)	(0.188)	(0.053)
3 Chickens	0.540	0.610	-0.070**
	(0.173)	(0.239)	(0.046)
4 Chickens	0.342	0.409	-0.067*
	(0.124)	(0.171)	(0.061)
5 Chickens	0.306	0.287	0.019
	(0.128)	(0.157)	(0.680)

* 0.10 ** 0.05 *** 0.01. For tests of differences, standard errors are clustered at the group level.
[†]Data is from Rounds 6 - 29. Observations are at the subject level; variables are averaged for each subject.

for instance, pledge 54 percent of their eggs on average, compared to 61 percent for men.[30]

Given the small size of the style-of-play differences, it is difficult to reach firm conclusions about what drives them. Nor does our survey offer any helpful clues: the responses of men and women are very similar.[31] It is possible that women are less proactive than men about seizing power — just as other work has shown that women are less likely than men to seek out job promotions. For instance, in a laboratory experiment, Small et al. (2007) find that men are nine times more likely than women to ask for higher compensation (see also Babcock and Laschever, 2003; Dittrich et al., 2014; Leibbrandt and List, 2014; Card et al., 2015; Exley et al., 2020). Women have also been shown to shy away from competition (see Niederle

[30]We do not observe any significant differences in how men and women vote. Both genders vote similarly in response to candidates' pledges, and earn similar amounts per round (see Tables A.7 and A.8). Consequently, gender differences in wealth and power do not seem to be driven by gender differences in voting behavior.

[31]None of the survey responses exhibit gender differences at a 10-percent significance level under standard t-tests.

Table 11: Gender Differences in Win Rates

	Female mean (sd)	Male mean (sd)	Difference (p-value)
Win Rates Conditional on Running and Chickens Owned			
0 Chickens	0.158	0.193	-0.035
	(0.224)	(0.266)	(0.300)
1 Chicken	0.319	0.304	0.014
	(0.320)	(0.318)	(0.770)
2 Chickens	0.410	0.522	-0.112**
	(0.351)	(0.340)	(0.037)
3 Chickens	0.428	0.599	-0.170**
	(0.382)	(0.363)	(0.035)
4 Chickens	0.460	0.661	-0.201**
	(0.398)	(0.280)	(0.044)
5 Chickens	0.650	0.719	-0.069
	(0.275)	(0.268)	(0.454)

* 0.10 ** 0.05 *** 0.01. For tests of differences, standard errors are clustered at the group level. Data is from Rounds 6 - 29. Observations are at the subject level; variables are averaged for each subject.

and Vesterlund, 2011 for a review). For instance, Niederle and Vesterlund (2007) find that women — of equal ability to men — are less than half as likely to enter a tournament. One could interpret our findings in these terms. However, women do compete in our experiment: they run for election only slightly less often (14.3 times, on average, versus 15.9 times for men).

We believe that the game's "power begets power" dynamic explains why small style-of-play differences translate into large disparities in outcomes. For instance, because of the game's "power begets power" dynamic, it is quite important to run in the first round. We estimate that gender differences in run rates in the first round alone — while small — account for 12.8 percent of the total gender wealth gap.[32]

Humanities/Arts Majors. Another trait that correlates with outcomes is subject's major. In the baseline treatment, humanities/arts majors are lords only 25.1 percent as often as other subjects (see Table A.9); and they have only 76.1 percent as much wealth (on average).

[32] If women increased their run rates in the first round by 12.5 percentage points — to the level of men — we would expect them to earn an additional $0.125 \times 29.1 \times 0.264 = 0.96$ eggs since: (i) women who win, rather than lose, the first election earn an additional 29.1 eggs, and (ii) candidates have a 26.4 percent chance of winning when they run. The overall gender wealth gap is 7.51 eggs, so 0.96 eggs constitutes 12.8 percent.

Given that humanities/arts majors skew female, the outcomes of humanities/arts majors might be driven by gender; by the same token, gender differences might be driven by choice of major. Table A.10 decomposes the effects of gender and major and shows that both contribute significantly to outcomes. Majoring in humanities/arts has a roughly comparable (negative) effect on outcomes as being female.

We suspect that humanities/arts majors and women experience worse outcomes for similar reasons. Table A.11 shows that humanities/arts majors, like women, run for election less often and pledge a smaller fraction of their eggs.

7 Conclusion

This paper uses a new game — the "chicken-and-egg game" — to study the political process. Our main finding is that patronage, through a "power begets power" dynamic, generates considerable inequality between individuals and between genders.

The chicken-and-egg game can easily be adapted to explore issues beyond those focused on in this paper. For instance, one potential direction for future work could be to study non-zero-sum political conflicts where politicians destroy surplus in pursuit of power. Take, for instance, pork-barrel politics resulting in "bridges to nowhere," or destructive wars between feudal lords. Within the chicken-and-egg game, "bridges to nowhere" could be modeled as inefficient transfers from candidates to voters; wars could be introduced as a technology that gives candidates the ability to destroy others' chickens.

Given that political institutions are a key driver of development and a major determinant of the distribution of resources, it is critical to understand how they evolve and change. We believe that the time is ripe to study political evolution in the laboratory and we see the chicken-and-egg game as a promising vehicle for doing so.

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A Appendix

Table A.1: Survey Results

I. Baseline Treatment

	Mean Response (SD)
Pledging Strategies Ranked by Importance	
(1) I pledged eggs because I wanted to win elections.	7.409 (2.989)
(2) I pledged eggs because I was concerned with fairness.	4.424 (3.440)
Voting Strategies Ranked by Importance	
(1) I voted for the candidate who pledged the most eggs.	6.432 (2.873)
(2) I voted against the candidate with the most chickens because I thought more competition would increase pledges to voters.	5.652 (3.327)
(3) I voted against the candidate with the most chickens because it was the fair thing to do.	4.924 (3.211)
(4) I voted for candidates who pledged a large share of their eggs, even if they did not pledge the most.	4.811 (3.252)
(5) I voted for candidates who voted for me in the past.	4.436 (3.600)
(6) I was easily bored so I voted more or less randomly.	2.443 (2.976)
Running Strategies Ranked by Importance	
(1) I chose whether to be a candidate or voter depending on what I thought would get me the most eggs.	6.833 (2.791)
(2) I sometimes chose to vote because I wanted to support/oppose a particular candidate, even when I thought it would not get me the most eggs.	5.523 (3.514)
(3) I sometimes chose to be a candidate because I wanted to oppose someone I wanted to see lose, even when I thought it would not get me the most eggs.	4.674 (3.519)
(4) I sometimes chose to vote because I felt it was unfair to be a candidate too often or win too many chickens.	4.580 (3.719)
(5) I was easily bored so I chose whether to be a voter or a candidate more or less randomly.	2.466 (2.965)
Luck?	
To what extent do you think winning chickens was a matter of luck?	6.614 (2.826)

Responses are on a Likert scale from 0 to 10.

II. No-Pledge Treatment

	Mean Response (SD)
Voting Strategies Ranked by Importance	
(1) I voted against the candidate with the most chickens because it was the fair thing to do.	6.976 (3.525)
(2) I voted for candidates who voted for me in the past.	6.720 (3.340)
(3) I was easily bored so I voted more or less randomly.	1.632 (2.441)
Running Strategies Ranked by Importance	
(1) I sometimes chose to vote because I wanted to support/oppose a particular candidate.	6.432 (3.342)
(2) I chose whether to be a candidate or voter depending on what I thought would get me the most eggs.	6.256 (3.255)
(3) I sometimes chose to vote because I felt it was unfair to be a candidate too often or win too many chickens.	6.064 (3.512)
(4) I sometimes chose to be a candidate because I wanted to oppose someone I wanted to see lose	3.504 (3.585)
(5) I was easily bored so I chose whether to be a voter or a candidate more or less randomly.	1.336 (2.016)
Luck?	
To what extent do you think winning chickens was a matter of luck?	5.256 (2.932)

Responses are on a Likert scale from 0 to 10. As the result of entering an incorrect ID number, one subject out of the 126 subjects in the no-pledge treatment did not complete the survey.

Table A.2: Consequences of Winning the First Election (Baseline Treatment)

	Total Wealth	Ever Became Lord?
Won First Election	33.070***	0.361***
	(7.639)	(0.089)
Constant	45.866***	0.114
	(5.571)	(0.070)
Election Fixed Effects	✓	✓
Pseudonym Fixed Effects	✓	✓
Observations	231	231

* 0.10 ** 0.05 *** 0.01, OLS with candidate-level observations. Standard errors are clustered at the group level.

Table A.3: Characteristics of Lords

Dep Var: Rounds as a Lord	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	-1.754***						-1.726***
	(0.424)						(0.465)
Humanities/Arts		-1.827***					-1.897**
		(0.523)					(0.726)
Year of study			0.051				-0.067
			(0.275)				(0.285)
Authority Preference				0.190			0.167
				(0.143)			(0.153)
Disadv. Inequity Aversion					-0.106		-0.102
					(0.084)		(0.104)
Adv. Inequity Aversion						0.023	0.054
						(0.112)	(0.112)
Constant	3.459***	3.167***	3.030***	1.997**	3.626***	3.112***	2.920**
	(0.071)	(0.000)	(0.733)	(0.884)	(0.363)	(0.262)	(1.375)
Group Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Observations	324	330	330	330	312	305	290

* 0.10 ** 0.05 *** 0.01. OLS with subject-level observations. Standard errors are clustered at the group level. Authority preference is measured by subjects' responses to the post-experiment survey question: "How much do you value having authority over other people?" Inequity aversion was elicited using a Holt-Laury procedure in our post-experiment survey. Six subjects did not disclose their gender; observations for these subjects are omitted in Columns 1 and 7. Inequity aversion could not be calculated for some subjects whose responses were non-monotonic; observations for these subjects are omitted in Columns 5, 6, and 7. See Table A.5 for a breakdown of subjects' majors.

Table A.4: Characteristics of Vassals

Dep Var: Was a Vassal	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	0.403**						0.476**
	(0.201)						(0.225)
Humanities/Arts		0.055					0.377
		(0.309)					(0.350)
Year of study			0.048				-0.012
			(0.081)				(0.098)
Authority Preference				0.069*			0.064
				(0.038)			(0.056)
Disadv. Inequity Aversion					-0.014		0.010
					(0.047)		(0.052)
Adv. Inequity Aversion						-0.020	-0.020
						(0.035)	(0.036)
Chickens Owned	-2.077***	-2.043***	-2.043***	-2.038***	-2.045***	-1.959***	-1.997***
	(0.372)	(0.357)	(0.361)	(0.352)	(0.365)	(0.381)	(0.373)
Constant	0.086	0.405***	0.319**	-0.007	0.495*	1.188***	0.480
	(0.159)	(0.000)	(0.146)	(0.227)	(0.292)	(0.160)	(0.524)
Election Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Observations	1905	1995	1995	1995	1889	1794	1688

* 0.10 ** 0.05 *** 0.01. Logits with subject-round-level observations. Standard errors are clustered at the group level. We restrict attention to rounds where there is a lord who runs for election, and to subjects that are not lords in those rounds. "Was a Vassal" equals one if the subject voted for the lord in that round, and zero otherwise. Authority preference is measured by subjects' responses to the post-experiment survey question: "How much do you value having authority over other people?" Inequity aversion was elicited using a Holt-Laury procedure in our post-experiment survey. Six subjects did not disclose their gender; observations for these subjects are omitted in Columns 1 and 7. Inequity aversion could not be calculated for some subjects whose responses were non-monotonic; observations for these subjects are omitted in Columns 5, 6, and 7. See Table A.5 for a breakdown of subjects' majors.

Table A.5: Breakdown of Subjects' Majors

	Baseline	No Pledge
Humanities / Arts	29	7
Engineering and Computer Science	147	56
Natural Sciences and Mathematics	57	21
Economics	26	15
Social Sciences (excluding Economics)	19	5
Business and Accounting	57	26
Other Technical and Professional Disciplines	11	0
Total	346	130

16 subjects in the Baseline treatment and 4 subjects in the No Pledge treatment, whose double majors span two categories, are counted twice.

Table A.6: Group's Survey Responses and Inequality (Baseline Treatment)

	Wealth Gini
Voted based on who pledged the most eggs[†]	0.032***
	(0.011)
Voted based on fairness concerns[‡]	-0.026**
	(0.011)
Constant	0.234**
	(0.091)
Observations	44

* 0.10 ** 0.05 *** 0.01, OLS with group-level observations. Each independent variable is the group-average response (on a 0 – 10 scale) to a question in the post-experiment survey (see Appendix B.5). [†]Response to: *"How well do the following statements describe the strategies you followed as a voter? → I voted for the candidate who pledged the most eggs."* [‡]Response to: *"How well do the following statements describe the strategies you followed as a voter? → I voted against the candidate with the most chickens because it was the fair thing to do."*

Table A.7: Gender Differences in Amounts Received as a Voter (Baseline Treatment)

	Amount Received
Female	-0.019
	(0.070)
Constant	0.908***
	(0.047)
Observations	3817

* 0.10 ** 0.05 *** 0.01. OLS (Rounds 6 - 29) with voter-round-level observations. Standard errors are clustered at the group level.

Table A.8: Gender Differences in Voting Behavior (Baseline Treatment)

	Voted for Candidate
Candidate's No. of Chickens	-0.093
	(0.057)
Candidate's Pledge	0.515***
	(0.059)
Candidate made Largest Pledge	0.371***
	(0.132)
Candidate's No. of Chickens × Female Voter	-0.013
	(0.064)
Candidate's Pledge × Female Voter	-0.008
	(0.081)
Candidate made Largest Pledge × Female Voter	0.178
	(0.199)
Observations	9284

* 0.10 ** 0.05 *** 0.01. Conditional logit (Rounds 6 - 29), grouped at the voter-election level, with candidate-voter-election-level observations. Standard errors are clustered at the group level.

Table A.9: Differences in Outcomes by Major

	Baseline			No Pledge		
	Hum/Arts mean (sd)	Non-(Hum/Arts) mean (sd)	Difference (p-value)	Hum/Art mean (sd)	Non-(Hum/Arts) mean (sd)	Difference (p-value)
Wealth	34.966	45.967	-11.001**	42.857	45.126	-2.269
	(22.837)	(31.228)	(0.018)	(7.381)	(8.003)	(0.428)
Power	2.862	5.023	-2.161***	4.714	4.840	-0.126
	(3.573)	(5.622)	(0.004)	(0.756)	(0.873)	(0.665)
Was ever a Lord	0.103	0.249	-0.146**	0.000	0.000	0.000
	(0.310)	(0.433)	(0.016)	(0.000)	(0.000)	(.)
Rounds as a Lord	0.448	1.787	-1.339***	0.000	0.000	0.000
	(1.594)	(4.266)	(0.000)	(0.000)	(0.000)	(.)
Was Wealthiest	0.069	0.176	-0.107**	0.000	0.176	-0.176
Group Member	(0.258)	(0.382)	(0.040)	(0.000)	(0.383)	(0.000)

* 0.10 ** 0.05 *** 0.01. For tests of differences, standard errors are clustered at the group level. A subject is "Hum/Arts" if at least one major is in Humanities or Arts; all other subjects are "Non-(Hum/Arts)."

Table A.10: Impact of Gender and Major on Outcomes

	Wealth	Power	Was ever a Lord	Rounds as a lord	Was Wealthiest
Female	-7.957**	-1.811***	-0.195***	-1.688***	-0.101**
	(3.334)	(0.634)	(0.050)	(0.403)	(0.049)
Humanities, Arts	-11.268*	-2.272**	-0.179**	-1.606***	-0.113*
	(6.104)	(0.953)	(0.069)	(0.515)	(0.067)
Constant	46.326***	5.135***	0.199***	3.448***	0.184***
	(0.556)	(0.106)	(0.008)	(0.067)	(0.008)
Group Fixed Effects	✓	✓	✓	✓	✓
Observations	324	324	324	324	324

* 0.10 ** 0.05 *** 0.01. OLS with subject-level observations. Standard errors are clustered at the group level.

Table A.11: Impact of Gender and Major on Style of Play

	Round 1 Run Rate	Overall Run Rate	Proportion Pledged when Chickens= 1	2	3	4	5
Female	-0.115**	-0.062**	-0.048	-0.081	-0.053	-0.074	0.009
	(0.051)	(0.030)	(0.033)	(0.051)	(0.061)	(0.121)	(0.318)
Humanities, Arts	-0.201*	-0.094**	-0.005	-0.085	-0.030	-0.225**	-0.110***
	(0.105)	(0.044)	(0.041)	(0.133)	(0.135)	(0.105)	(0.000)
Constant	0.771***	0.562***	0.987***	1.000***	0.750***	0.295***	0.250***
	(0.035)	(0.005)	(0.008)	(0.000)	(0.000)	(0.000)	(0.000)
Group Fixed Effects		✓	✓	✓	✓	✓	✓
Observations	324	324	222	161	114	73	42

* 0.10 ** 0.05 *** 0.01. OLS with subject-level observations. Standard errors are clustered at the group level. Proportion pledged is conditional on running and averaged across Rounds 6-29.

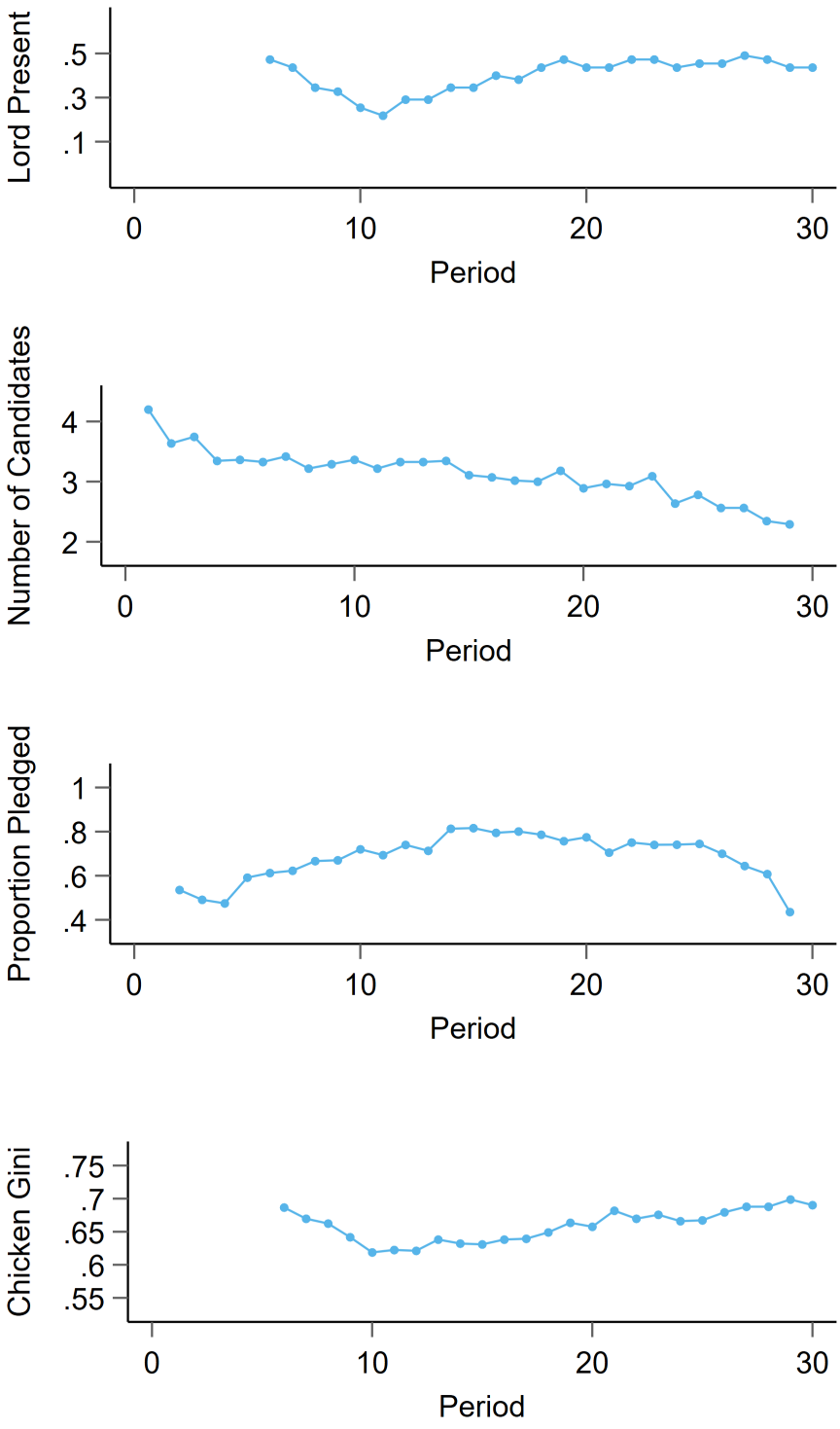


Figure A.1: Time Trends (Baseline Treatment)

B Supplementary materials

B.1 Instructions (Baseline Treatment)

Ground Rules

Welcome to the experiment. Please read the instructions below carefully.

Communication between participants is not allowed. Also, please refrain from using any communication devices. If you have any questions at any time, please raise your hand and an experimenter will come over to see you.

If you need to write anything, please use the paper and pen provided. Please do not write anything on this instruction sheet.

Groups and Privacy

The computer will randomly assign you to a group of *six* participants. You will interact only with the participants in your group. The computer will randomly select an ID for you, such as "Cabbage" or "Potato." You will keep the same ID throughout the experiment.

Your decisions in the experiment will be anonymous, and your anonymity will be strictly preserved. Participants will interact with each other using only their IDs. For example, you may learn that "Cabbage has voted for you"; but you will not be told the real name of "Cabbage."

Chickens and Eggs

In this experiment, you may win *chickens* that lay *eggs* for you. You may give some of your eggs to other participants. At the end of the experiment, your eggs will be converted into dollars at the rate of 5 eggs to $1.

Rounds

The experiment will consist of 30 rounds.

In each round, except the final round, an election will take place. The winner of the election receives a chicken. Chickens lay eggs for five rounds, and then retire.

Your Coop and Your Basket

Your chickens live in your chicken *coop*. At the start of each round, each of your chickens lays two eggs in the coop. You may give some of these eggs to other participants.

At the end of the round, the eggs in your coop are transferred to your egg *basket*.

Details of Elections

In each round except the final round, there is an election to determine who will win a chicken. You will have a choice whether to 1) be a candidate in the election or 2) a voter in the election. One voter will be selected at random by the computer to be the *deciding voter*. The election outcome will be determined by the deciding voter's vote.

The election will proceed as follows:

Step 1: If you are a candidate, you may pledge to give some eggs from your coop to the deciding voter if he/she votes for you.

Step 2: If you are a voter, you will choose whom to vote for after observing the candidate's pledges. The computer will then randomly select the deciding voter.

Step 3: At the end of the election, the election winner's pledge will be transferred to the deciding voter's basket.

If nobody chooses to be a candidate or nobody chooses to be a voter, the computer randomly allocates the chicken to one participant.

Final Round

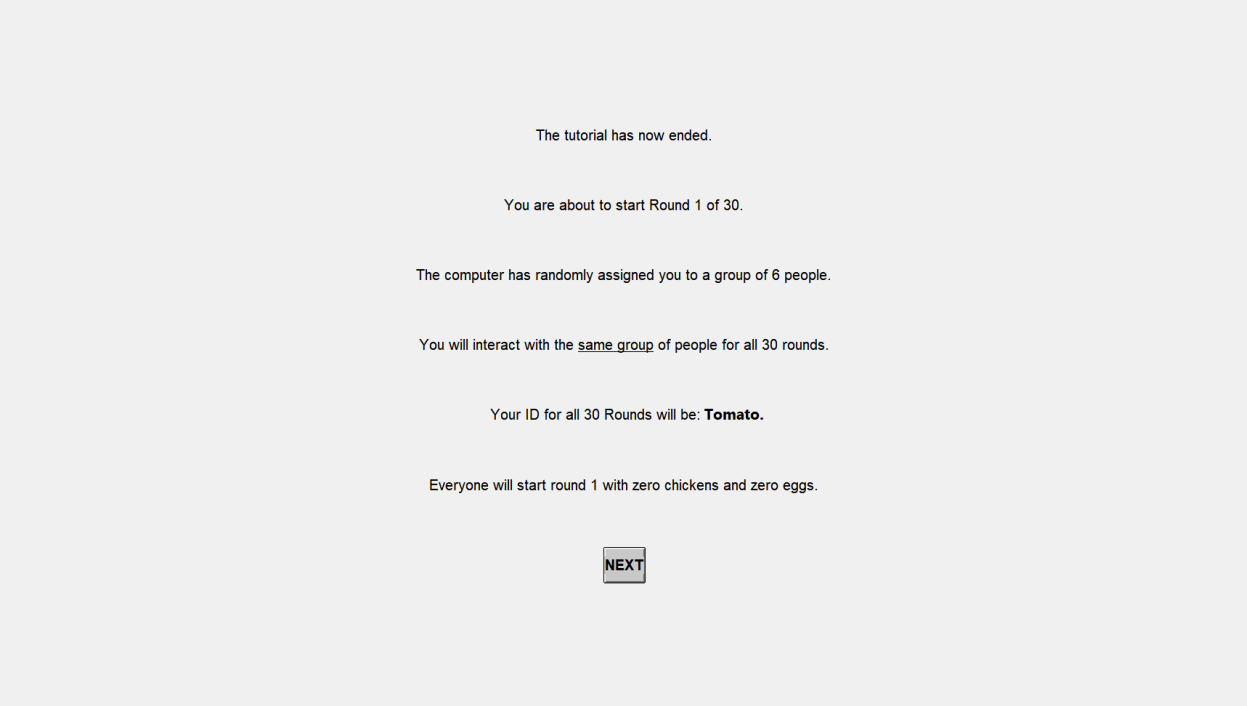
In the final round, there is no election. Each chicken's eggs are immediately placed in its owner's basket.

Payment

At the end of the experiment, the eggs in your basket will be converted into dollars at the rate of 5 eggs to $1. You will also receive a show-up fee of $5. You will be paid privately and confidentially.

You will be asked to fill in a short questionnaire before being paid.

B.2 Screenshots (Baseline Treatment)



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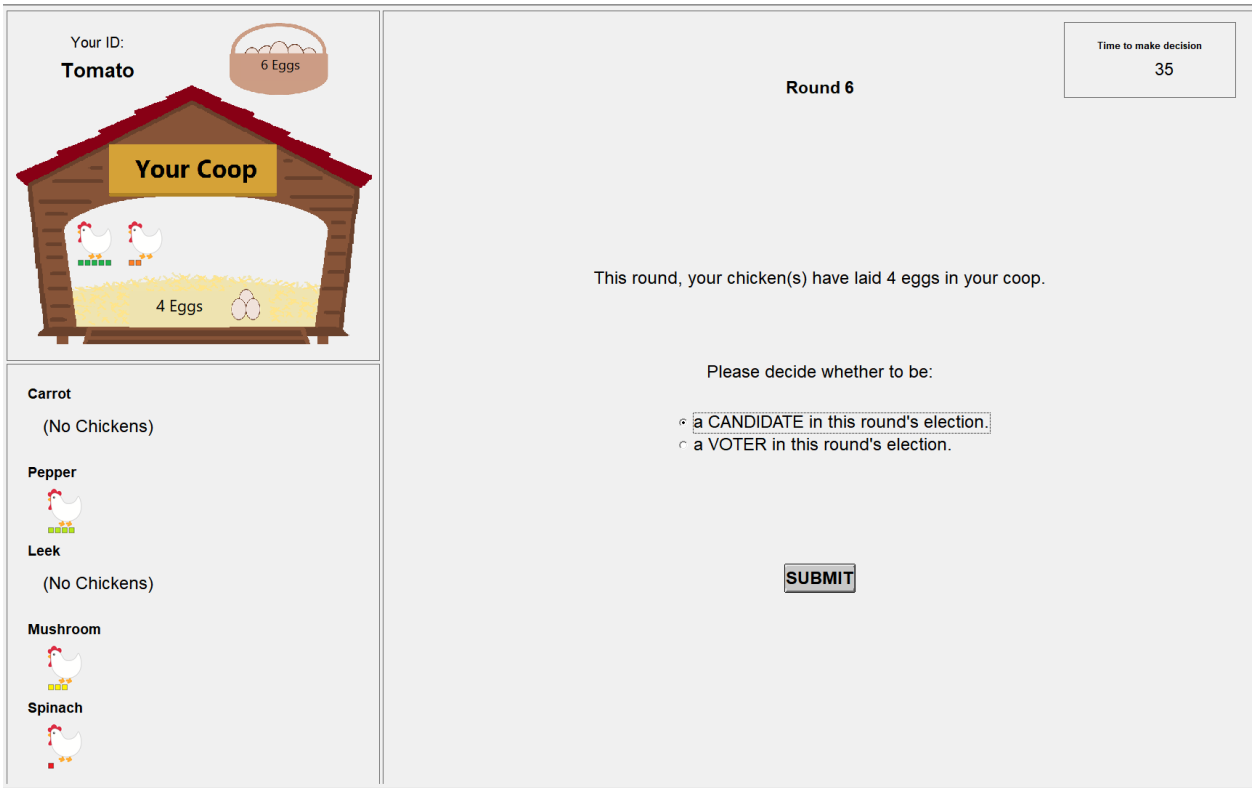
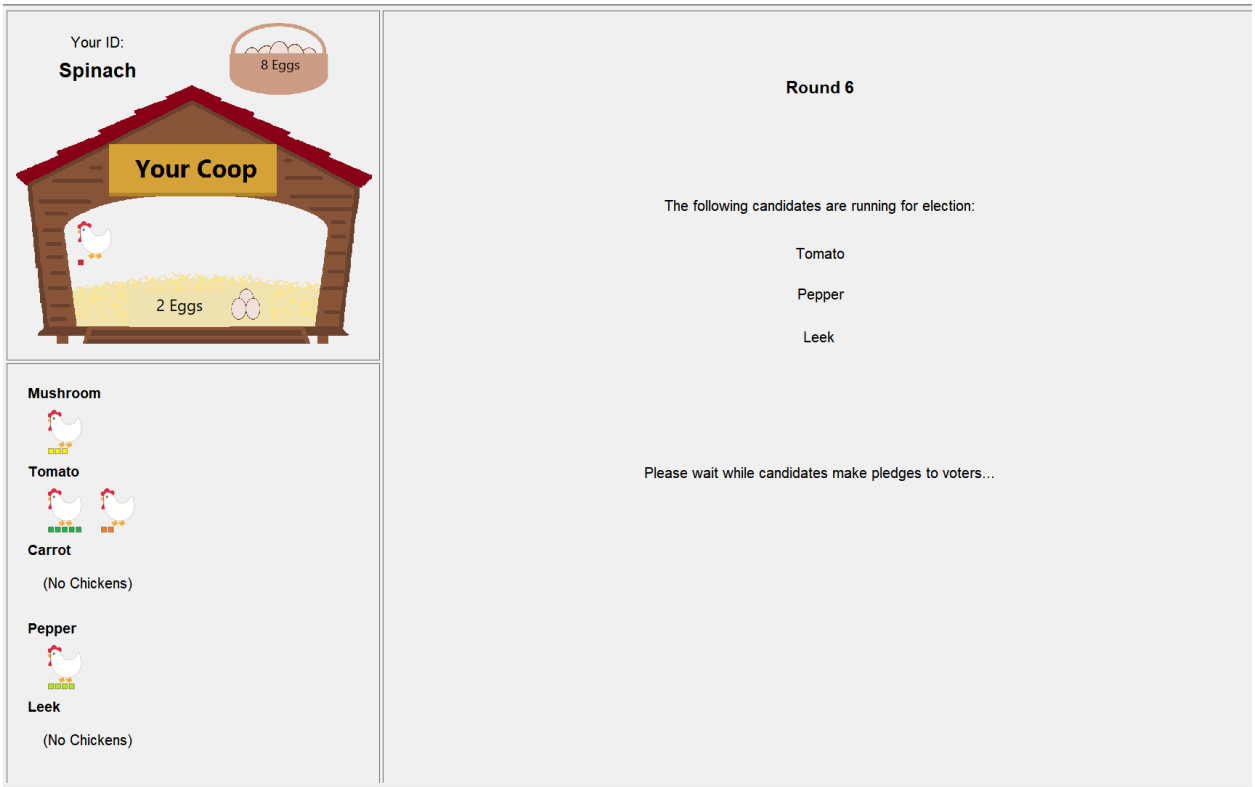
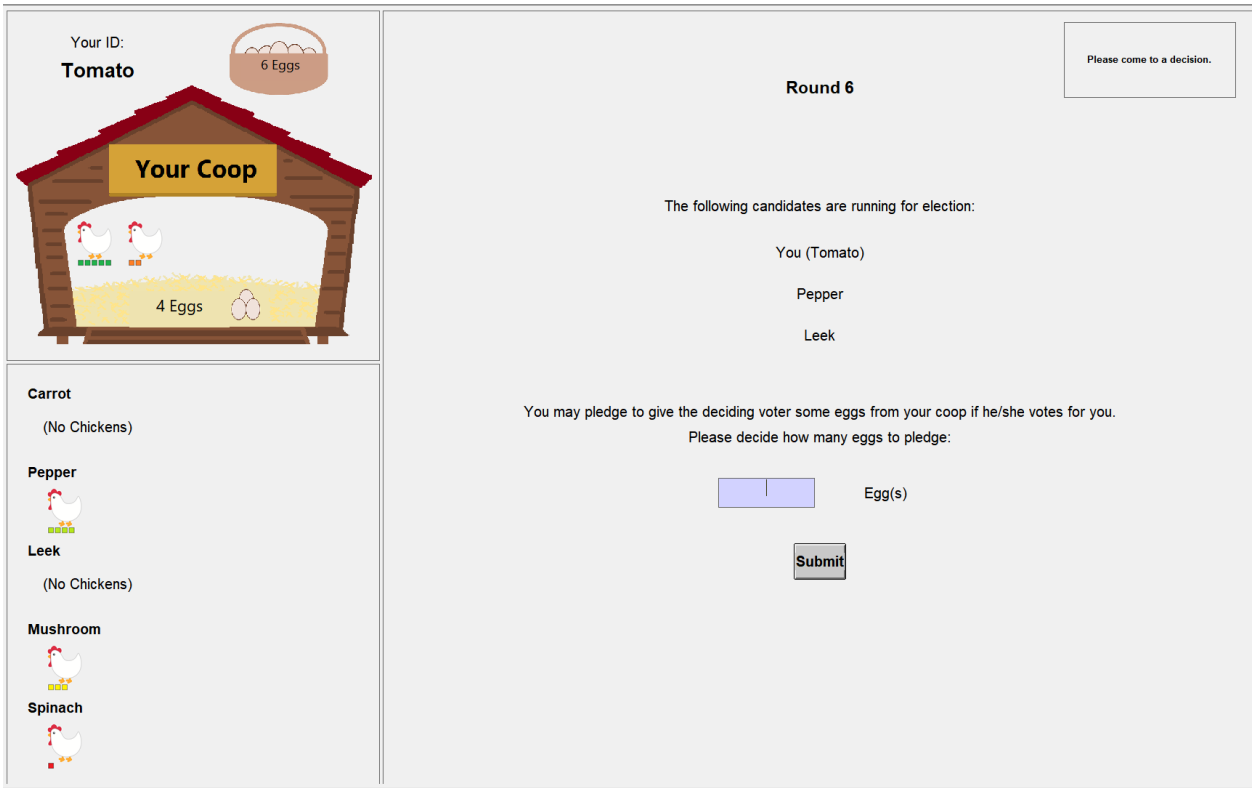


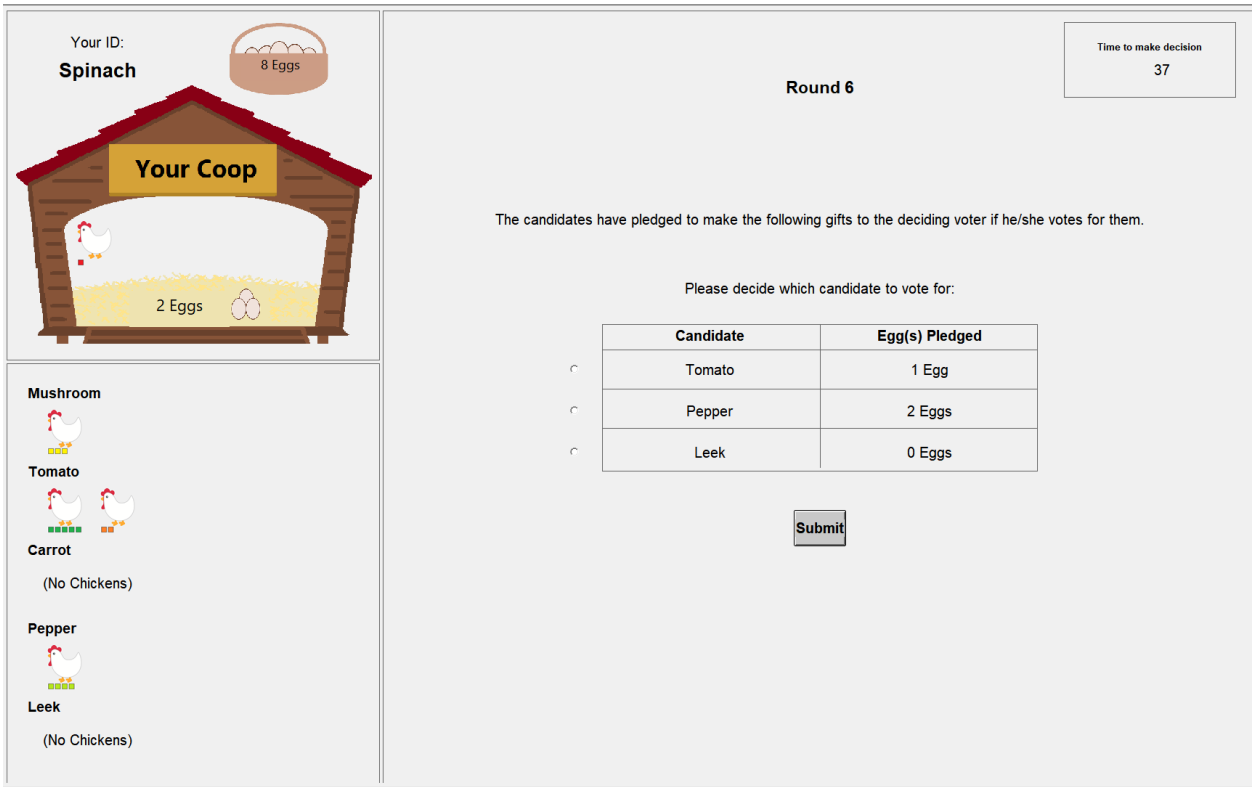
Screen 1



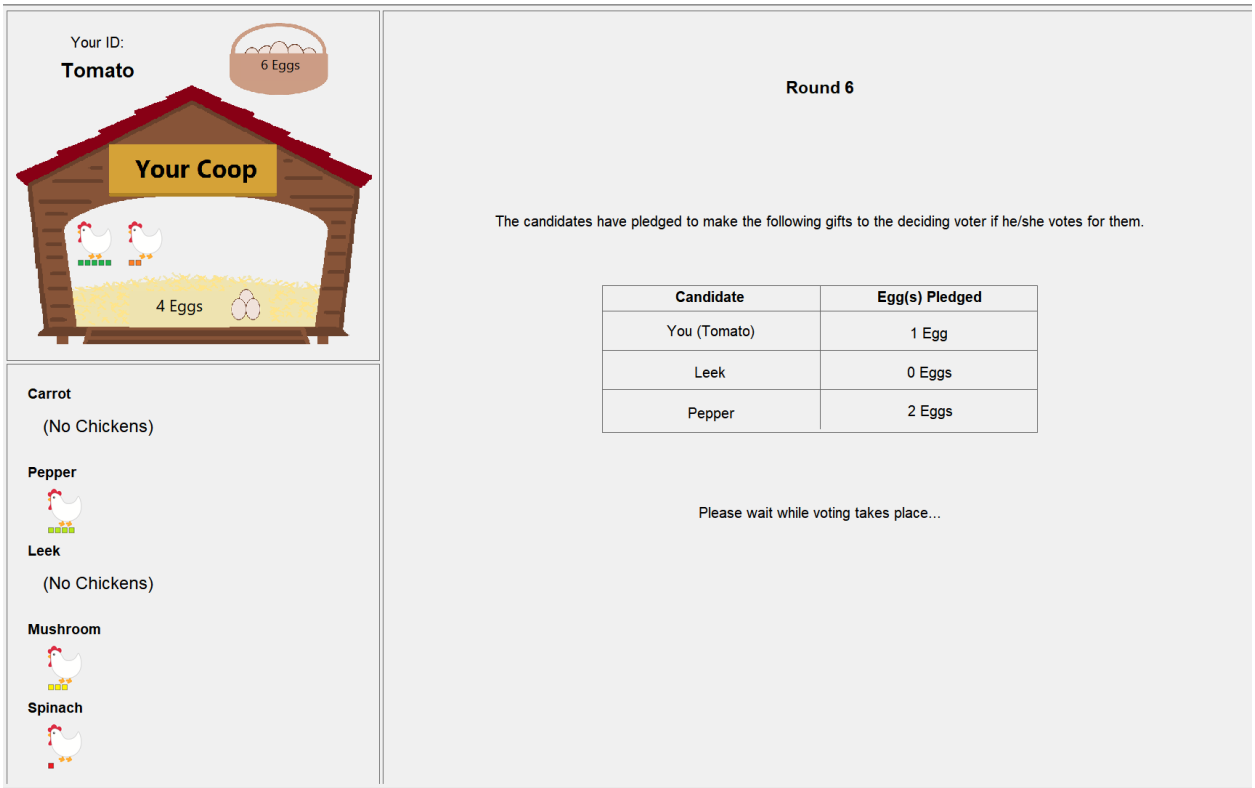
Screen 2 (Voter)



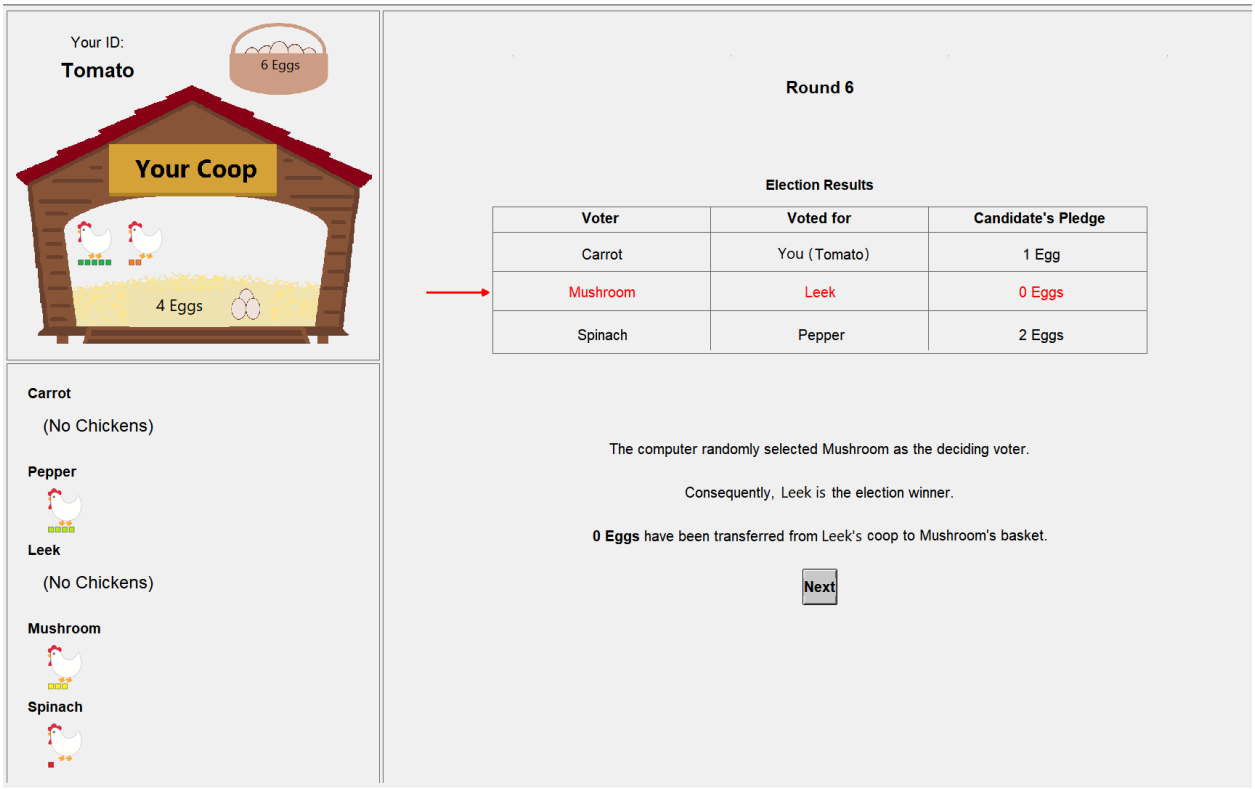
Screen 2 (Candidate)



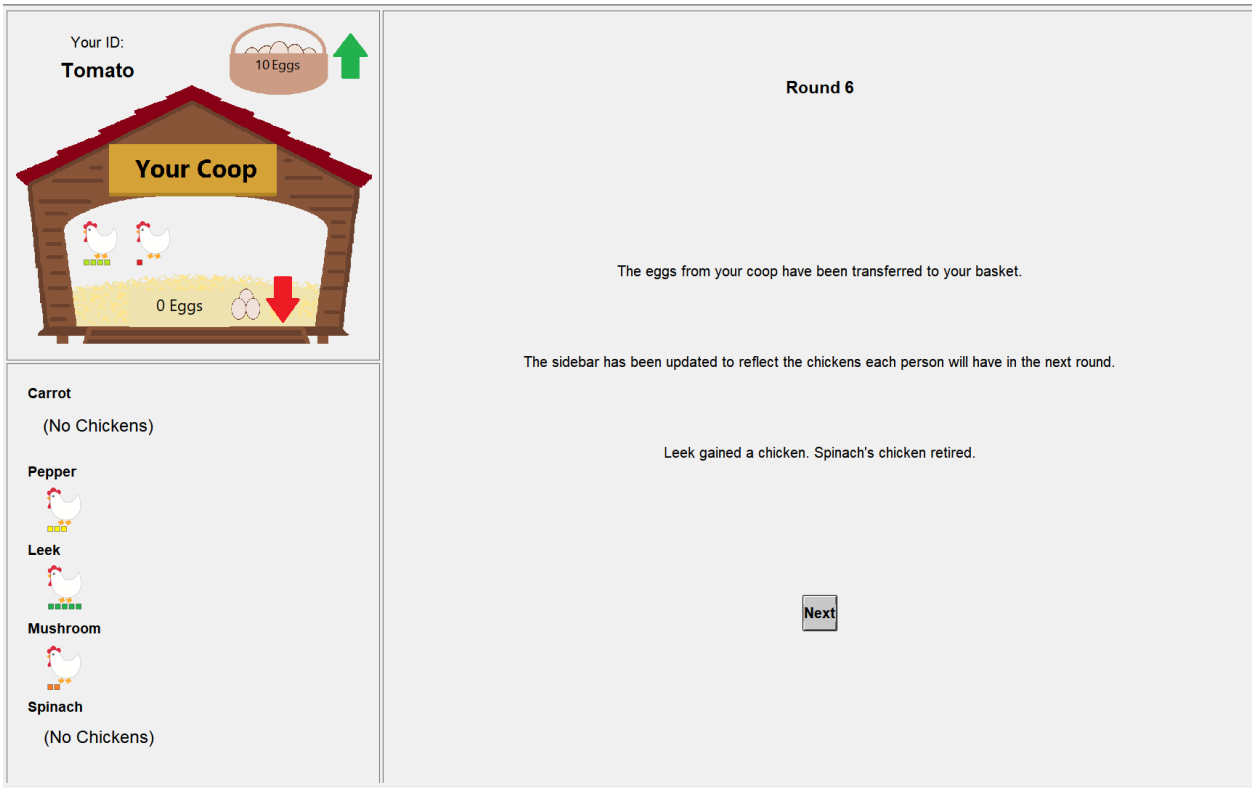
Screen 3 (Voter)



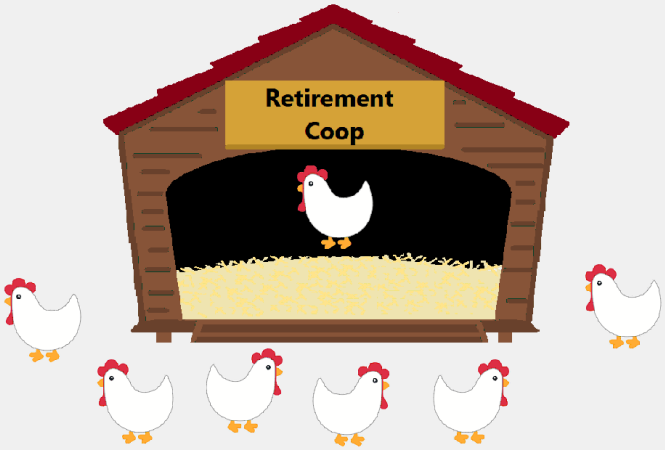
Screen 3 (Candidate)



Screen 4



Screen 5



The experiment is now over.
No chickens were harmed in the making of this experiment.

Next

End Screen 1

Final Egg Totals

	Amount in Basket
You (Tomato)	130.00 Eggs
Mushroom	20.00 Eggs
Carrot	26.00 Eggs
Leek	30.00 Eggs
Pepper	30.00 Eggs
Spinach	34.00 Eggs

Your eggs will be converted into dollars at the rate of 5 eggs to 1 dollar.

Next

End Screen 2

B.3 Instructions (No-Pledge Treatment)

Ground Rules

Welcome to the experiment. Please read the instructions below carefully.

Communication between participants is not allowed. Also, please refrain from using any communication devices. If you have any questions at any time, please raise your hand and an experimenter will come over to see you.

If you need to write anything, please use the paper and pen provided. Please do not write anything on this instruction sheet.

Groups and Privacy

The computer will randomly assign you to a group of *six* participants. You will interact only with the participants in your group. The computer will randomly select an ID for you, such as "Cabbage" or "Potato." You will keep the same ID throughout the experiment.

Your decisions in the experiment will be anonymous, and your anonymity will be strictly preserved. Participants will interact with each other using only their IDs. For example, you may learn that "Cabbage has voted for you"; but you will not be told the real name of "Cabbage."

Chickens and Eggs

In this experiment, you may win *chickens* that lay *eggs* for you. At the end of the experiment, your eggs will be converted into dollars at the rate of 5 eggs to $1.

Rounds

The experiment will consist of 30 rounds.

In each round, except the final round, an election will take place. The winner of the election receives a chicken. Chickens lay eggs for five rounds, and then retire.

Your Coop and Your Basket

Your chickens live in your chicken *coop*.

At the start of each round, each of your chickens lays two eggs. These eggs are put in your *basket*.

Details of Elections

In each round except the final round, there is an election to determine who will win a chicken. You will have a choice whether to 1) be a candidate in the election or 2) a voter in the election.

If you choose to be a voter, you will cast a vote for one of the candidates. The computer will then randomly select a *deciding voter*. The election outcome will be determined by the deciding voter's vote.

If nobody chooses to be a candidate or nobody chooses to be a voter, the computer randomly allocates the chicken to one participant.

Final Round

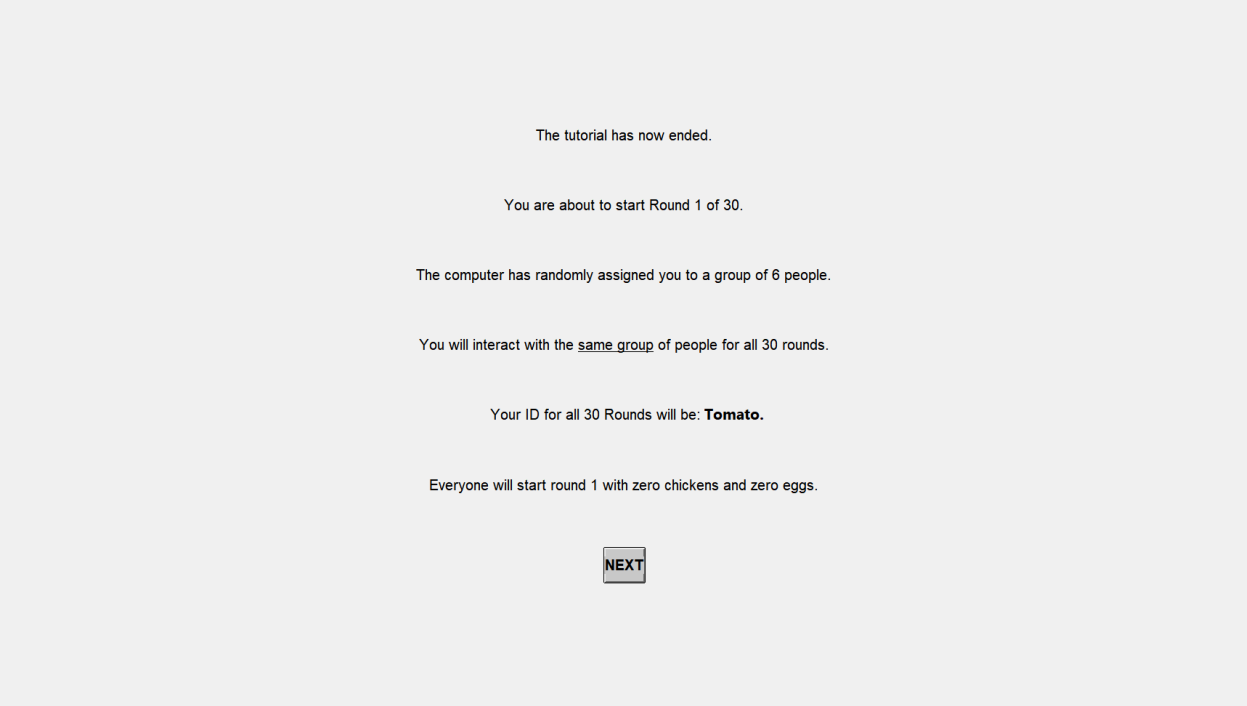
In the final round, there is no election. You will simply receive the eggs laid by your chickens.

Payment

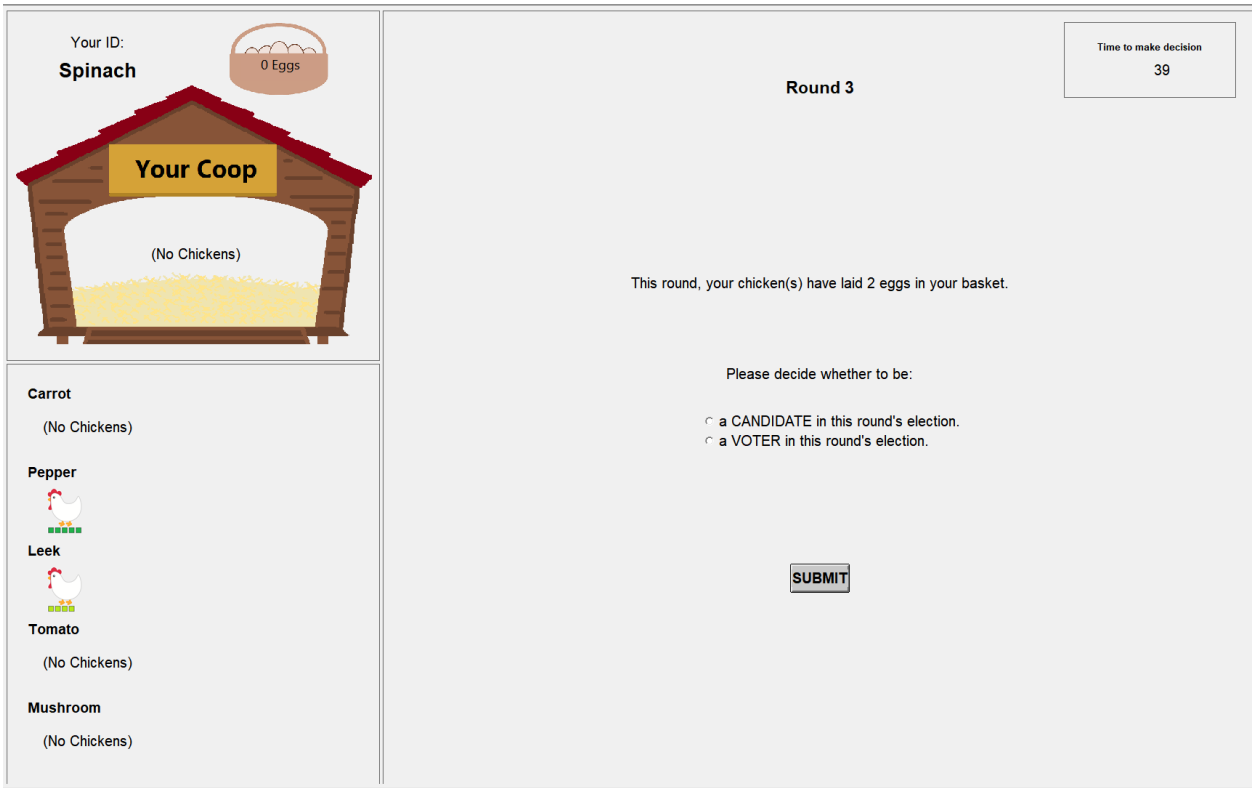
At the end of the experiment, the eggs in your basket will be converted into dollars at the rate of 5 eggs to $1. You will also receive a show-up fee of $5. You will be paid privately and confidentially.

You will be asked to fill in a short questionnaire before being paid.

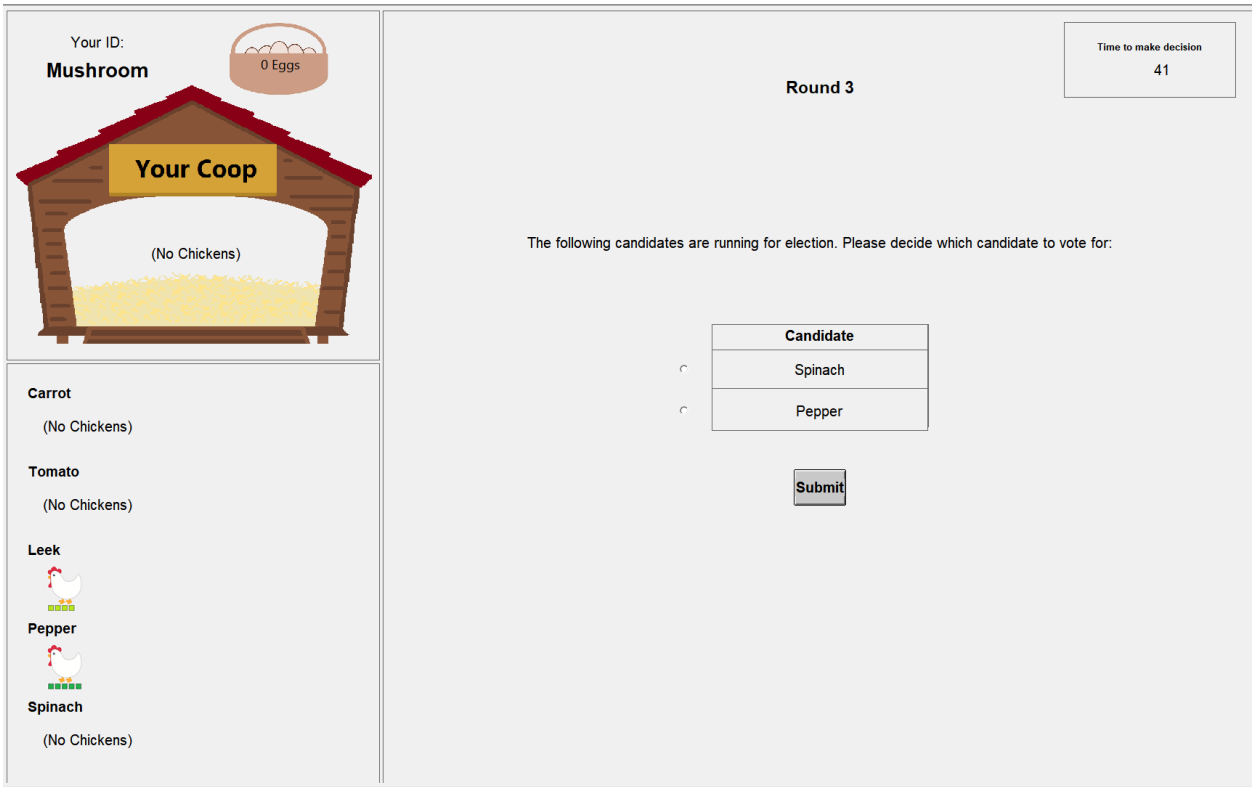
B.4 Screenshots (No-Pledge Treatment)



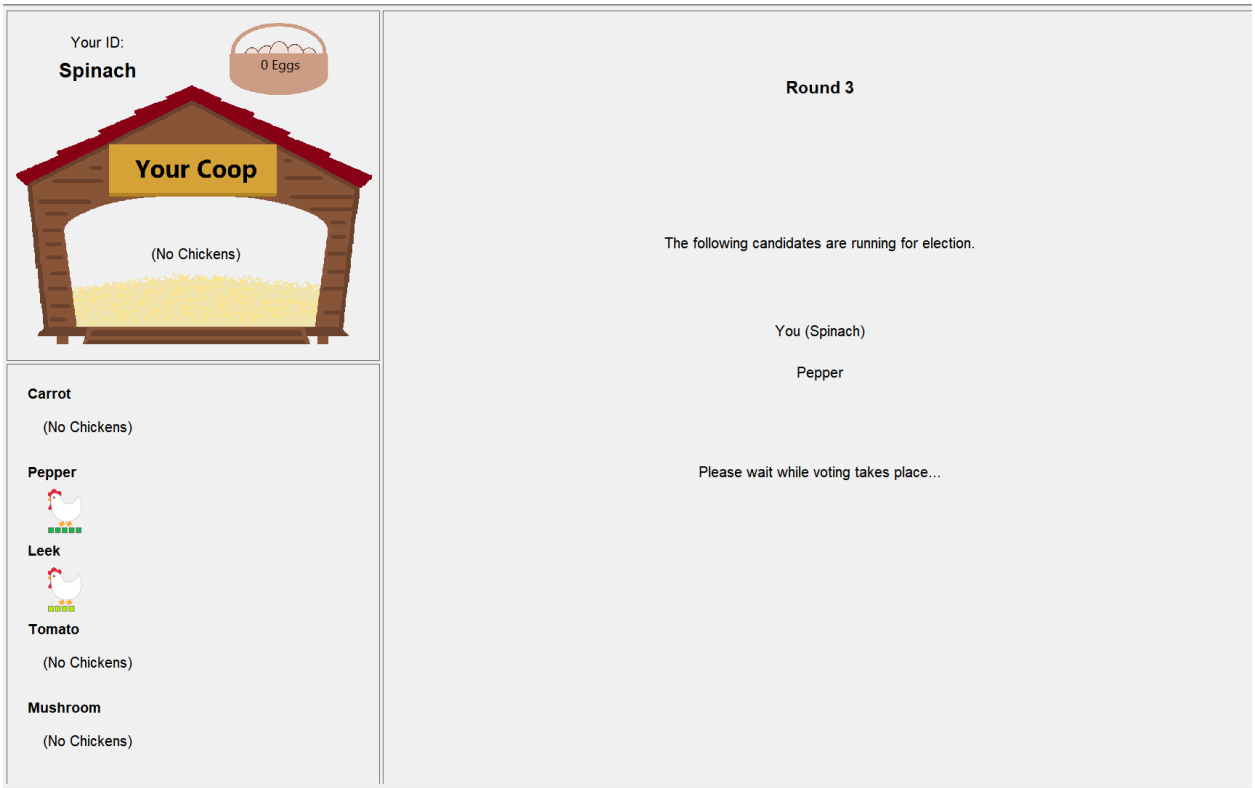
Start Screen



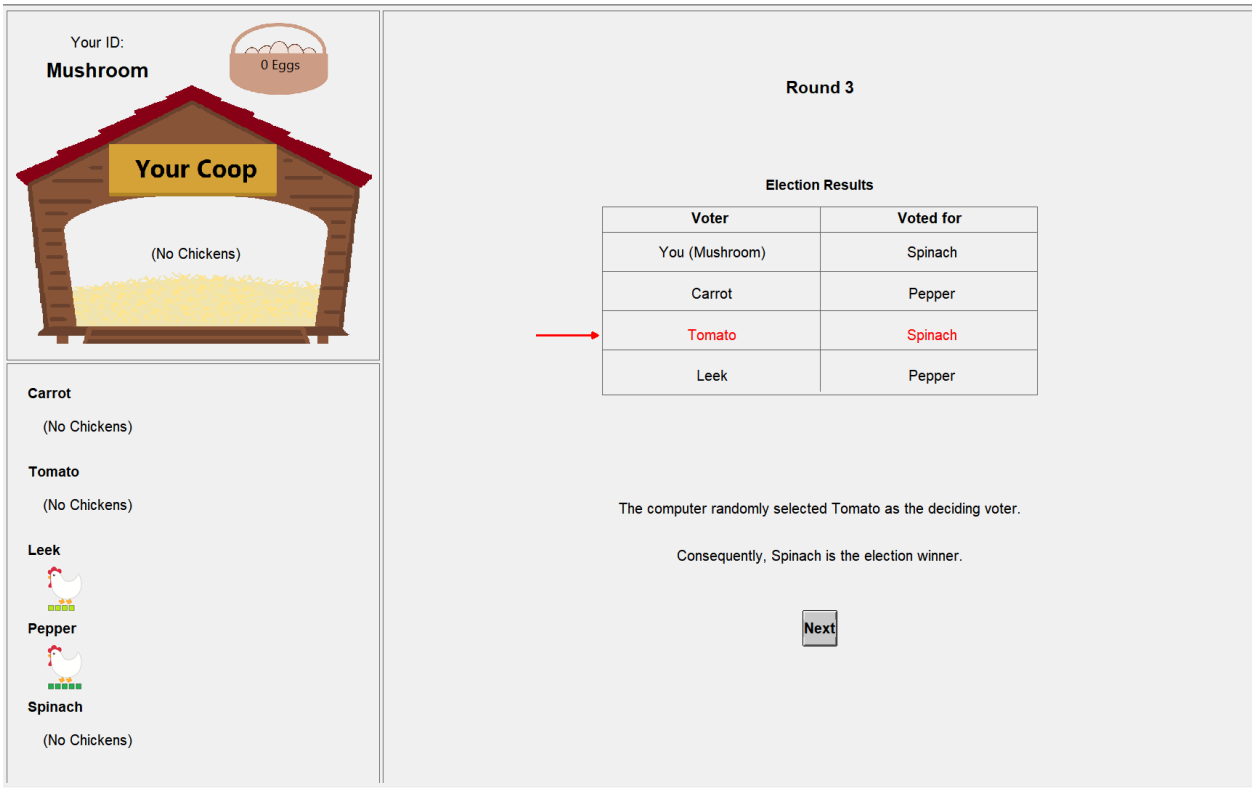
Screen 1



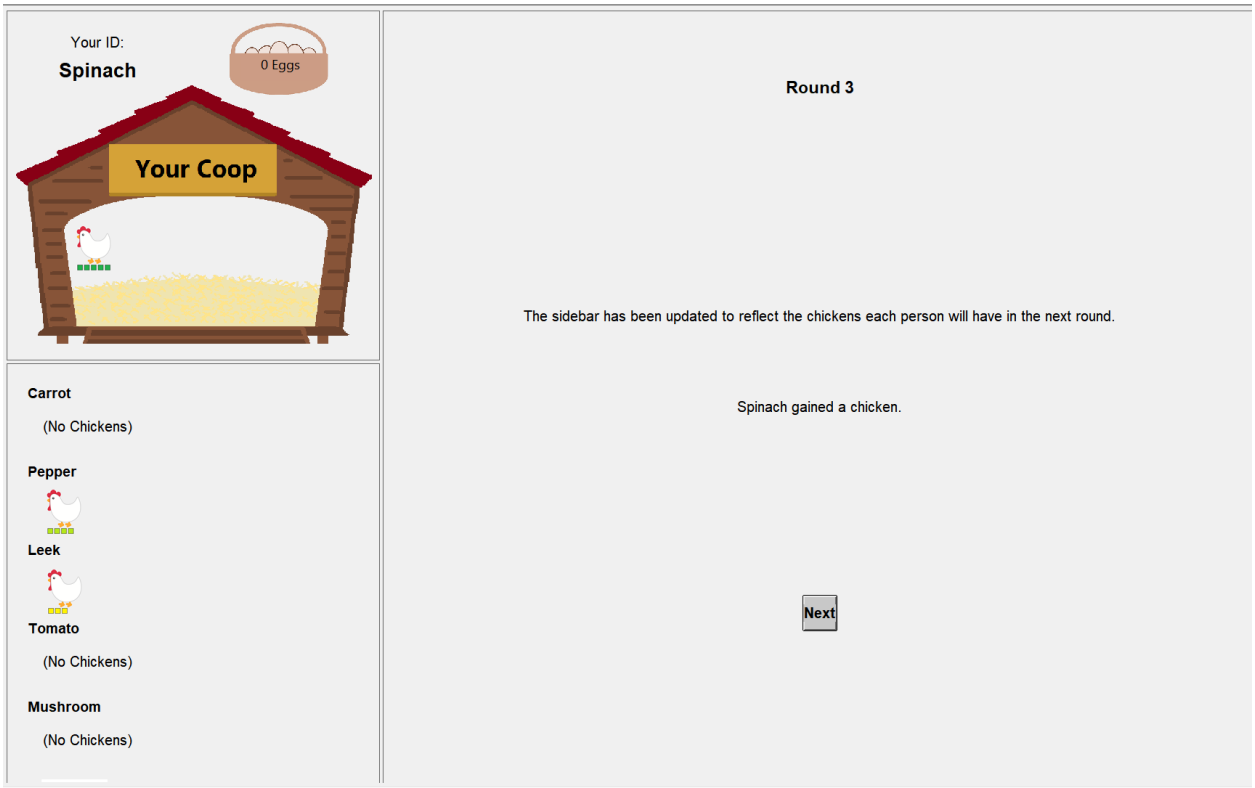
Screen 2 (Voter)



Screen 2 (Candidate)



Screen 3



Screen 4

B.5 Post-Experiment Survey Questions

Demographic questions

What is your age? *(If you would prefer not to answer, please leave it blank.)*
What is your year of study? [1st Year, 2nd Year, 3rd Year, 4th Year, Postgraduate]
What is your nationality?
What is your course of study?
What is your gender? [Male, Female, I'd prefer not to answer, Other (Please describe if you wish)]

Voting behaviour*
How well do the following statements describe the strategies you followed as a voter? *Note: if you never voted, please indicate how you think you would have voted.* [0: Not well at all – 10: Extremely well]

- I voted for the candidate who pledged the most eggs.†
- I voted for candidates who pledged a large share of their eggs, even if they did not pledge the most.†
- I voted against the candidate with the most chickens because I thought more competition would increase pledges to voters.†
- I voted against the candidate with the most chickens because it was the fair thing to do.
- I voted for candidates who voted for me in the past.
- I was easily bored so I voted more or less randomly.

Are there other strategies you followed? If so, please describe below.

Pledging behaviour*
How well do the following statements describe your reasons for pledging eggs when you were a candidate? *Note: if you were never a candidate, please indicate how you think you would have pledged.* [0: Not well at all - 10: Extremely well]

- I pledged eggs because I was concerned with fairness.†
- I pledged eggs because I wanted to win elections†.
- Are there other reasons you pledged eggs? If so, please describe below.†

Running behaviour*
How well do the following statements describe your reasons for choosing whether to be a candidate or a voter in each round? [0: Not well at all - 10: Extremely well]

- I chose whether to be a candidate or voter depending on what I thought would get me the most eggs.
- I sometimes chose to vote because I felt it was unfair to be a candidate too often or win too many chickens.
- I sometimes chose to vote because I wanted to support/oppose a particular candidate.‡
- I sometimes chose to vote because I wanted to support/oppose a particular candidate, even when I thought it would not get me the most eggs.†
- I sometimes chose to be a candidate because I wanted to oppose someone I wanted to see lose.‡
- I sometimes chose to be a candidate because I wanted to oppose someone I wanted to see lose, even when I thought it would not get me the most eggs.†
- I was easily bored so I chose whether to be a voter or a candidate more or less randomly.

Are there other reasons why you chose to be a candidate or voter? If so, please describe below.

Miscellaneous questions
To what extent do you think winning chickens was a matter of luck? [0: Not Luck - 10: Mostly Luck]
How much do you value having authority over other people? [0: Not at all - 10: A lot]
Was there anything unclear about the instructions?

Disadvantageous inequity aversion
In each row below, you will have to choose between hypothetical allocations of experimental Coins between yourself and another. Please select, for each row, which option you prefer.

(1)	Option A: You: 12.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins
(2)	Option A: You: 11.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins
(3)	Option A: You: 10.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins
(4)	Option A: You: 9.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins
(5)	Option A: You: 8.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins
(6)	Option A: You: 7.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins
(7)	Option A: You: 6.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins
(8)	Option A: You: 5.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins
(9)	Option A: You: 4.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins
(10)	Option A: You: 3.5 Coins, Other: 15 Coins	Option B: You: 10 Coins, Other: 26 Coins

Advantageous inequity aversion

In each row below, you will have to choose between hypothetical allocations of experimental Coins between yourself and another. Please select, for each row, which option you prefer.

(1)	Option A: You: 18.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins
(2)	Option A: You: 17.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins
(3)	Option A: You: 16.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins
(4)	Option A: You: 15.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins
(5)	Option A: You: 14.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins
(6)	Option A: You: 13.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins
(7)	Option A: You: 12.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins
(8)	Option A: You: 11.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins
(9)	Option A: You: 10.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins
(10)	Option A: You: 9.5 Coins, Other: 9 Coins	Option B: You: 17 Coins, Other: 5 Coins

*Order of questions was randomised within section.
†Only included in baseline treatment survey.
‡Only included in no-pledge treatment survey.