The Democracy Effect: a weights-based identification strategy

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Abstract

Dal Bó, Foster and Putterman (2010) show experimentally that the effect of a policy may be greater when it is democratically selected than when it is exogenously imposed. In this paper we propose a new and simpler identification strategy to measure this democracy effect. We derive the distribution of the statistic of the democracy effect, and apply the new strategy to the data from Dal Bó, Foster and Putterman (2010) and data from a new real-effort experiment in which subjects' payoffs do not depend on the effort of others. The new identification strategy is based on calculating the average behavior under democracy by weighting the behavior of each type of voter by its prevalence in the whole population (and not conditional on the vote outcome). We show that use of these weights eliminates selection effects under certain conditions. Application of this method to the data in Dal Bó, Foster and Putterman (2010) confirms the presence of the democracy effect in that experiment, but no such effect is found for the real-effort experiment.

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I. Introduction

Previous research has suggested that democratic institutions may affect the response to policies. For example, in experimental settings, Tyran and Feld (2006), Ertan, Page, and Putterman (2010), Sutter, Haigner, and Kocher (2010), and Markussen, Putterman and Tyran (2014) among others find that punishments and rewards have greater impact on contributions to a public good when they are allowed democratically.[2] However, as pointed out by Dal Bó, Foster and Putterman (2010), it is difficult to interpret this difference in behavior as a clear evidence that democratic institutions affect behavior directly. The reason is that democracy allows for selection into policies. For example, the availability of punishments after a public good game may have a greater effect when that availability was chosen democratically than when it is chosen randomly simply because subjects who are predisposed to employ punishments may be more likely to vote for such punishments if given the opportunity.

Dal Bó, Foster and Putterman (2010) provided two identification strategies to get around the selection problem and showed that democratic choice, net of selection, increases the effect on cooperation of a fine on unilateral defection in a prisoners' dilemma game. Their identification strategies were based on having all subjects vote and then randomly overriding votes in some groups. Having information on votes of all subjects allowed them to compare behavior controlling for each person's vote (therefore controlling for selection) and show that there is a significant direct effect of democracy when the fine is in place.

A shortcoming of their identification strategy is that one must know how subjects voted even when their votes are overridden. Outside of the laboratory one may not be able to override votes. The researcher may, for example, only have access to vote data from democratic groups or societies, not from otherwise comparable groups in which

[2] More generally, other papers have compared behavior between democratically chosen environments and those imposed both in the lab and in the field. See, for example, Frey (1998), Bardhan (2000), Potters, Sefton and Vesterlund (2005), Olken (2010), Baldassarri and Grossman (2011), Hamman, Weber and Woon (2011), Corazzini et al (2014), Markussen, Reuben and Tyran (2014), Mellizo, Carpenter and Matthews (2014), and Engl, Riedl and Weber (2018). Note that not all of these papers focus on the democracy effect that we study. Democracy can also affect behavior through the implementation of different policies and allowing for sorting into policies. Here we study the direct effect of democracy (for simplicity the "democracy effect") which consists of the impact of democracy on behavior after controlling for the chosen policies and sorting.

rules are imposed exogenously. Moreover, within the laboratory it may be that the process of voting and having one's vote overridden has a different effect than just having the rule imposed without having previously voted.[3] It thus would be useful to be able to test for a direct democracy effect even without information on how each subject in a non-democratic group or society would have voted.

In this paper we devise a method that does just that. Our alternative identification strategy consists of comparing behavior under non-democracy with behavior under democracy but weighting the behavior of voters according to their proportions in the population and not conditional on the election outcome. The basic insight is that the source of the selectivity bias is that "yes" voters are overrepresented, relative to the population, in groups that choose a particular policy. Similarly, "no" voters are overrepresented in groups that reject the policy. If voting decisions are independent across members of a society, a "yes" voter in a majority "yes" group is comparable to a "yes" voter in a majority "no" group. Thus reweighting the behaviors under each policy according to the proportion of yes and no voters in the population will yield an unbiased estimate of the effects of the policy controlling for democratic selection.

We then apply this method to two different settings. First, we employ the data from Dal Bó, Foster and Putterman (2010), which considered a policy that transformed a prisoners' dilemma game into a coordination game. Consistent with the results presented by Dal Bó, Foster and Putterman (2010) we find a significant democracy effect using the new methodology. The second experiment consisted of choosing a payment scheme for a task. The task involves adding sets of five randomly generated two-digit numbers. In the first part of the experiment subjects were paid a fixed amount regardless of the number of correct answers. In the second part, subjects could be paid as in the first part or by a piece rate (their payment was determined by their own performance only, independent of the performance of the other subjects). The payment formula was chosen by the computer for some groups, but for other groups the formula was chosen by simple majority. The question is whether the effect of introducing piece rate payments on performance is larger

[3] These may be the reasons why, despite the wide recognition of the importance of measuring the direct effect of democracy, the identification strategy proposed in Dal Bó, Foster and Putterman (2010) has only been used in a couple of experiments (e.g. Kamei 2016, Chen 2015, Marcin, Robalo and Tausch 2016, and Schories 2017).

if that introduction is chosen democratically. In this case, we found that there is no democracy effect: helping to choose the payment scheme of one's group did not affect the number of correct answers provided. This result suggest that the existence of a democracy effect may be linked to strategic considerations that are present in the coordination game but not in this real-effort task.

The next section revisits the simple formal model in Dal Bó, Foster and Putterman (2010) to explain the main hurdle to identifying the existence of a democracy effect, describes the solutions provided by Dal Bó, Foster and Putterman (2010), and presents the new identification strategy. Section III derives the proposed statistic and its distribution, and studies its performance through Monte Carlo simulations. Section IV applies the new identification strategy to the data from Dal Bó, Foster and Putterman (2010) and data from the new real-effort experiment.

II. Strategies to identify the effect of democracy

As in Dal Bó, Foster and Putterman (2010), we consider a simplified game in which individuals are (1) matched in groups, (2) learn the mechanism $M \in \{D, N\}$ (democratic or not) used to select the environment in which they will interact $E \in \{A, B\}$ (these environments may involve different payoff matrices, rules of interaction or games), (3) vote $v \in \{a, b\}$ (where a denotes a vote for environment A) if they are in the democratic mechanism, (4) learn the chosen environment, and (5) interact in that environment.

A subject's choice in the environment will depend formally on the mechanism M, the environment E, and his or her type μ. We write the choice of action of the subject as $C(M, E, \mu)$. The type μ includes any personal characteristic that is unobserved to the researcher but that may be correlated with both the subject's choice and his or her voting decisions. The type μ is assumed to be iid with density function $f(\mu)$.

In this framework, an individual's vote can only depend on his or her type, as he or she is randomly matched with the others and does not know either their type or how they will vote: $v = v(\mu)$. We say that there is a democracy effect if the choice function depends on the mechanism after controlling for the environment and the subject's type: $C(D,E,\mu) \neq C(N,E,\mu)$.

II.a. Why the usual comparisons do not identify a democracy effect:

As discussed in Dal Bó, Foster and Putterman (2010), a naïve way of testing the existence of a democracy effect, which has been used in the literature, is to consider the expected difference in behavior by mechanism given the environment *E*:

(1) $E(C|D,E) - E(C|N,E) = \int [C(D,E,\mu) f(\mu|D,E) - C(N,E,\mu) f(\mu|N,E)] d\mu$

where $f(\mu|M,E)$ is the conditional density of the type given the selection mechanism and the selected environment. While finding that equation (1) is different from zero is consistent with a democracy effect, it does not identify a democracy effect. As discussed in Dal Bó, Foster and Putterman (2010), the reason is that the environment *E* is informative about the type of the subjects when it was democratically selected but not when it was selected randomly: $f(\mu) = f(\mu|N,E) \neq f(\mu|D,E)$. In other words, subjects that end in a particular environment democratically may not be comparable to those that end in that environment randomly. There may be a selection effect. In particular, the difference (1) may be non-zero even if there are no differences in behavior by mechanism: $C(D,E,\mu) = C(N,E,\mu)$ for all *E*, μ. We could thus falsely claim to find a democracy effect if we just compared behavior across mechanisms for a given environment.

To gain more intuition about the impact of the selection effect into equation (1), it is useful to decompose the elements in that equation. Note that the terms in the left hand side of equation (1) can be written as follows:

$$E(C|M,E) = P(v=a|M,E) \int C(M,E,\mu)\ f(\mu|M,E,v=a) d\mu +$$
$$P(v=b|M,E) \int C(M,E,\mu)\ f(\mu|M,E,v=b) d\mu$$

Note also that the relationship between the vote and the type does not depend on the mechanism or the implemented environment if the types are independent across subjects, that is $f(\mu|D,E,v) = f(\mu|N,E,v) = f(\mu|v)$. In other words, once one knows how somebody voted or would have voted, the environment and mechanism are no longer informative about the subjects' types. Finally, note that given that environment *E* was chosen, the probability that a subject voted or would have voted for *a* is not independent of the mechanism used to choose the environment; however, the probability of voting for a in the non-democratic condition is equal to the probability of voting for a in the population: $P(v = a|D, E) \neq P(v = a|N, E) = P(v = a)$.

Given these facts, we can now write the expected behavior under the two mechanisms as follows:

$$(2)\ E(C|D, E) =$$

$$P(v = a|D, E) \int C(D, E, \mu)\ f(\mu|v = a) d\mu + P(v = b|D, E) \int C(D, E, \mu)\ f(\mu|v = b) d\mu$$

$$(3)\ E(C|N, E) =$$

$$P(v = a) \int C(N, E, \mu)\ f(\mu|v = a) d\mu + P(v = b) \int C(N, E, \mu)\ f(\mu|v = b) d\mu$$

These two equations clearly show why $E(C|D, E)$ and $E(C|N, E)$ could be different even if $C(D, E, \mu)$ and $C(N, E, \mu)$ are not different. The integrals in (2) and (3) would not differ if there were no democracy effect: $C(D, E, \mu) = C(N, E, \mu)$. What would differ is the probability that a subject voted (or would have voted) for A or B. Under environment A, for example, subjects who voted for A are overrepresented under democracy relative to non-democracy: $P(v = a|D, A) > P(v = a|N, A) = P(v = a)$. Similarly, under environment B: $P(v = b|D, B) > P(v = b|N, B) = P(v = b)$. Therefore, if we are focusing on environment A, $E(C|D, A)$ overweighs subjects who voted for environment A relative to groups for which the environment A was randomly imposed on them: $P(v = a|D, A) > P(v = a)$. Since subjects that voted for A may behave differently than those that voted for B, the average behavior under democracy cannot be directly compared to the average behavior under non-democracy to identify the democracy effect.

II.b. The identification strategies in Dal Bó, Foster and Putterman (2010): controlling on types

With a clear representation of the identification problem it is possible to characterize solutions. We start by reviewing the solutions provided in Dal Bó, Foster and Putterman (2010) and then provide a new identification strategy based on reweighting the prevalence of the different types of subjects.

The identification strategies introduced and used in Dal Bó, Foster and Putterman (2010) are based on the fact that they observe the subjects' vote under both mechanisms. In one of their strategies, instead of looking at equation (1), they look at a comparison for each of the possible votes (a and b). It is straightforward to show that once we control by the environment and vote, the difference in behavior across mechanisms can be calculated as follows:

$$(4) \qquad E(C|D,E,v) - E(C|N,E,v) = \int [C(D,E,\mu) - C(N,E,\mu)] f(\mu|v) d\mu.$$

Equation (4) can only be non-zero if, for some positive measure set of types, behavior differs by mechanism (democratic versus not) for subjects with the same vote. By comparing subjects that voted in the same way the selection problem is avoided. Dal Bó, Foster and Putterman (2010) also introduced an additional identification strategy which is based on comparing the behavior of groups with the same distribution of votes, in particular groups with the same number of Yes and No votes.[4]

Note, however, that both of these identification strategies require that subjects vote under both mechanisms. In most applications we will not observe how subjects would have voted under and exogenous mechanism, limiting the applicability of their identification strategies. We also may be concerned that part of the democracy effect that is measured, even when we can observe overridden votes, may reflect the response to having one's vote overridden rather than not the effect of not having had the opportunity to vote at all.

II.c The new identification strategy: reweighting types

We now develop a novel identification strategy that does not require that we observe voting in the exogenous treatment. In addition to assuming that subjects' types are independently drawn, we only need to assume that the distribution of types in the population from which the groups are selected for the exogenous condition is the same as that for those selected for the democratic condition. This equality is ensured by the random allocation of subjects to mechanisms.

[4] This identification strategy does not require assuming that votes are independent across subjects.

As discussed before, the comparison of equations (2) and (3) fails to identify the democracy effect because the distribution of types across mechanisms differs once one controls for the environment selected. In other words, if we study behavior for environment *A*, there will be an over representation of subjects with types that lead to vote for *A* in the democratic treatment relative to the random one.

The proper comparison is between behavior under the non-democratic mechanism, $E(C|N,E)$, and the following properly weighted average behavior under democracy:

(5) $P(v=a)\int C(D,E,\mu)\ f(\mu|v=a)d\mu + P(v=b)\int C(D,E,\mu)\ f(\mu|v=b)d\mu.$

Note that this average uses as weights the unconditional voting probabilities $P(v=a)$ and $(v=b)$, instead of the conditional ones $P(v=a|D,E)$ and $P(v=b|D,E)$ as in equation (2). By comparing (5) with (3), we see that under the null of no democracy effect it must be the case that:

(6) $$P(v=a)E(C|D,E,a)+P(v=a)E(C|D,E,b)=\ E(C|N,E),$$

Where $E(C|D,E,v)=\int C(D,E,\mu)\ f(\mu|v)d\mu$ denotes the expected behavior under democracy conditional on being in environment *E* and the subjected having voted for *v*. Denote the reweighted expected behavior in the left-hand side of equation (6) as $E_{RW}(C|D,E)$, where RW denotes "reweighted."

In conclusion, to test whether there exists a democracy effect under a particular environment, we must compare the average behavior under non-democracy with a properly weighted measure of the behavior of those voting for one or the other environment, where the weights are estimates of the percentage of subjects voting for each environment in the whole population.

III Estimation methodology

In this section we describe how to perform the statistical test based on the identification strategy described in the previous section and provide results from simulation exercises that support the estimation methodology.

III.a. The statistic and its distribution

The terms in equation (6) using data from an experiment with both a democratic and non-democratic treatment can be estimated by taking averages. Denote by $S_{D,E,v}$ and $n_{D,E,v}$ the set and number of subjects who faced the democratic mechanism *D*, participated in environment *E* and voted for *v*. Similarly, denote by $S_{N,E}$ and $n_{N,E}$ the set of subjects who faced the non-democratic mechanism and participated in environment *E*. Denote by S_D and n_D the set and number of subjects in the democracy mechanism and by n_N the number of subject in the non-democratic mechanism. Denote by v_i a subjects' vote and by C_i a subject's choice of action in the environment. For simplicity, denote $P_a = P(v = a)$ and $P_a = P(v = a)$.

The right hand side of equation (6) is estimated as $\hat{E}(C|N,E) = \frac{1}{n_{N,E}} \sum_{i \in S_{N,E}} C_i$, while the left hand side is estimated as $\hat{E}_{RW}(C|D,E) = \hat{P}_a \hat{E}(C|D,E,a) + \hat{P}_b \hat{E}(C|D,E,b)$ where $\hat{P}_a = \frac{1}{n_D} \sum_{i \in S_D} 1\{v_i = a\}$, $\hat{P}_b = 1 - \hat{P}_a$, and $\hat{E}(C|D,E,v) = \frac{1}{n_{D,E,v}} \sum_{i \in S_{D,E,v}} C_i$. The proposed statistic is

$$(7)\ E = \hat{E}_{RW}(C|D,E) - \hat{E}(C|N,E).$$

To perform statistical tests on the existence of a democracy effect we must know the distribution of *DE*.

Proposition 1: If subjects' types are iid and there is no democracy effect, then $E(DE) = 0$.

The intuition behind proposition 1 was described in section II.c.: from equation (6) we see that under the assumption of no democracy effect the expected reweighted average behavior under democracy must coincide with the expected behavior under non-democracy. The proof is provided in the appendix.

To be able to do hypothesis testing, it is important to know that the distribution of our statistic converges. The next proposition provides a central limit theorem for our case.

Proposition 2: If subjects' types are iid, $P_a \in (0,1)$, n_D and n_N grow proportionately with n, and there is no democracy effect, then $\sqrt{n}DE_n$ converges in distribution to a Normal distribution as *n* goes to infinity.

The proof is provided in the appendix.

Knowing that the distribution of the statistic converges, we can use jackknife or bootstrapping procedures to properly estimate the standard errors of the statistic *DE*.

Note that we have assumed that the researcher observes voting and choices at the individual level. This assumption makes it straightforward to estimate $\hat{E}(C|D,E,v)$, numbers that are crucial to this new identification strategy. While this assumption is appropriate for most laboratory experiments in the democracy effects literature, it may not be appropriate for field applications where the researcher only observes voting and choices at the group level. Fortunately, we can still use this identification strategy by calculating $\hat{E}(C|D,E,v)$ from group level data. For example, we can regress group average behavior on vote share for a given environment, and use those estimates to calculate $\hat{E}(C|D,E,v)$.[5]

III.b. Simulations: performance of the statistic and the importance of independence

Simulation results allow us to show the shortcomings of the naïve comparison of behavior between democratic and non-democratic mechanisms and the advantage of the proposed estimator for the democracy effect. The simulations consider an experiment with a hundred groups with 5 subjects each. Subjects interact in an environment, either A or B, with the other members of the group. Groups differ on how this environment is chosen. Half of the groups are randomly assigned a democratic decision mechanism and the other half are assigned a random mechanism (half of these groups will participate in environment A and half in environment B). Groups selected for the democratic

[5] For example, if C_j denotes the average behavior in group *j*, and vs_j denotes the vote share for environment A in group *j*, then we can regress $C_j = \alpha + \beta vs_j + \varepsilon_j$. We can then calculate $\hat{E}(C|D,E,a) = \hat{\alpha} + \hat{\beta}$ and $\hat{E}(C|D,E,b) = \hat{\alpha}$.

mechanisms vote for one of the two environments and the environment with a majority of votes is implemented.

We assume that each subject has an independent type μ_i with distribution N(0,1). Subjects with type greater than zero vote for environment A while those with negative types vote for environment B. The behavior of the subjects only depends on their own type. It does not depend on the environment (for simplicity) and it does not depend on democracy (so that the null of no democracy effect holds). We assume that subjects' choice are between zero and one and a subject's choice corresponds to the normal CDF at the subject's type (for example, the choice is $c_i = \frac{1}{2}$ for a subject with $\mu_i = 0$).

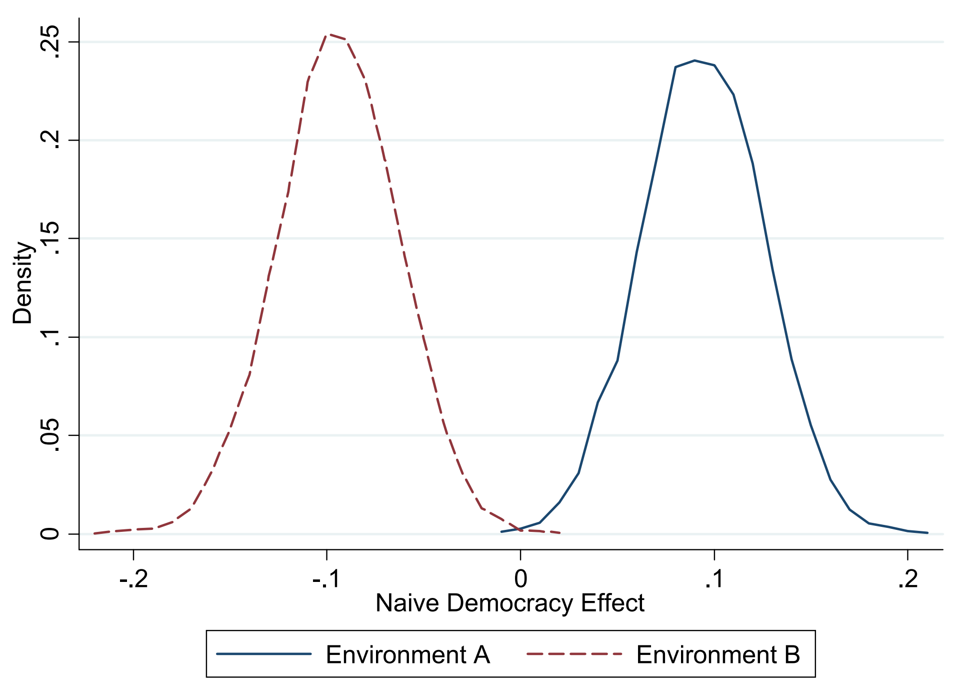
Table 1: The Performance of the Democracy Effect Statistic - Simulation Results

Panel a: i.i.d. types				
	Naïve Democracy Effect		Democracy Effect	
Environment	Average	% Rejection of Null	Average	% Rejection of Null
A	0.094	85.40%	0.0003	5.68%
B	-0.094	85.53%	-0.0003	5.57%
Panel b: correlated types				
	Naïve Democracy Effect		Democracy Effect	
A	0.18	93.80%	0.046	14.80%
B	-0.179	94%	-0.047	15.88%

Note: Results from five thousand simulations. The Naïve Democracy Effect statistic consist of comparing behavior under democracy and non-democracy conditional on chosen environment: $\hat{E}(C|D,E) - \hat{E}(C|N,E)$. The Democracy Effect statistic is as in equation (7). "% Rejection of Null" calculated for rejection of null at 5% significance level. Bootstrap standard errors are calculated at the group level with one thousand replications.

Table 1, panel a, shows the results of 10,000 iterations under the previous assumptions. The first 3 columns show the shortcomings of just comparing behavior under democracy and non-democracy to assess whether there is a democracy effect. Although these simulations assume that there is no democracy effect, a naïve comparison of the estimates suggests that there is an average positive effect of democracy under environment A and a negative effect under environment B. The null of no effect is rejected in the vast majority of the cases. As discussed above, under democracy there is selection into each environment: subjects who voted for A have high types and, hence, also make higher choices, resulting in higher average choices in democratic A environments than in non-democratic A environments. Figure 1 shows the distribution of the naïve democracy effect given the simulations for each environment. These distributions are not centered at zero despite the absence of a democracy effect in the data generating process.

Figure 1: Distribution of the Naïve Democracy Effect under Independent Types and in an Environment without a Democracy Effect



The last 2 columns of Table 1, panel a, show the performance of the proposed estimator of the democracy effect. It can be seen that the democracy effect is in average very close to zero, consistently with the null of no democracy effect. Moreover, the null of no democracy effect is rejected at the 5% level in less than 6% of the cases. The proposed estimator results in a rejection of the null in the correct proportion. Figure 2 shows the distribution of the democracy effect obtained from the simulations. It can be clearly seen that regardless of the environment (A or B), the democracy effect estimator is distributed with mean around zero (the true democracy effect in this case).

Figure 2: Distribution of the Democracy Effect Statistic under Independent Types and in an Environment without a Democracy Effect

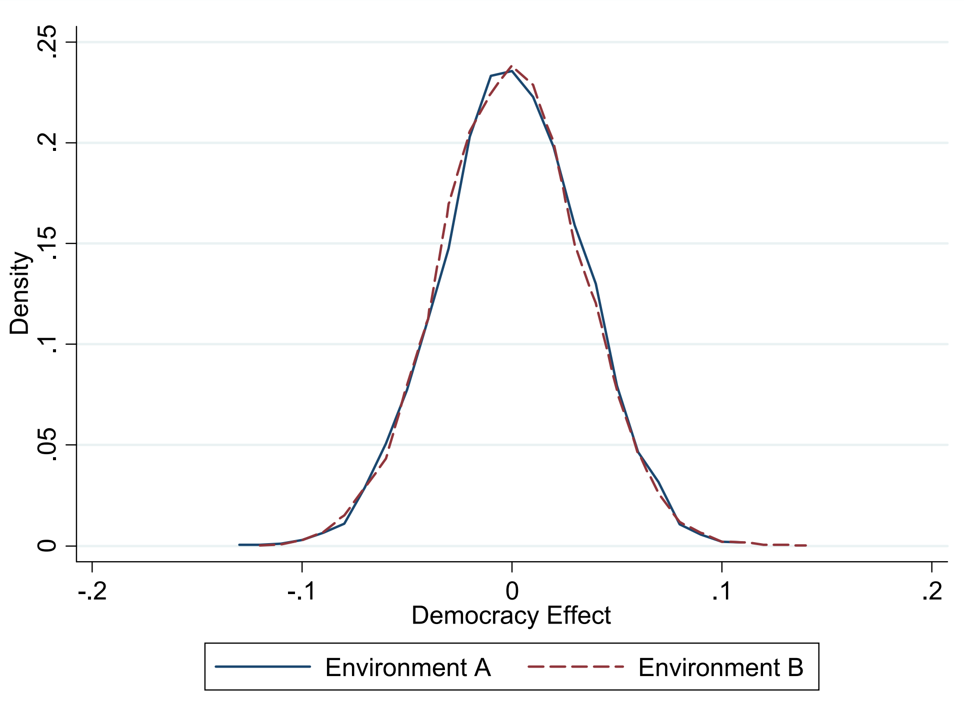


Table 1, panel b, shows simulation results when we relax the assumption of independent types. We assume instead that each subject's type is formed by adding two components. One component is independently distributed with normal distribution N(0,1/2) and the other component is the same for all subjects in a group and is also distributed N(0,1/2). As it was the case for independent types, the naïve measure of democracy effect rejects the null too many times. More importantly, as shown in the last 2 columns in panel b, our proposed statistic for the democracy effect also fails: the null of no democracy effect is rejected at the 5% level 15% of the cases. The reason is that the subjects' types are not independent inside a group. The simulated distribution of the statistic for the case of correlated voters' types is displayed in Figure 3.

Figure 3: Distribution of Democracy Effect Statistic with Correlated Types in an Environment without Democracy Effect

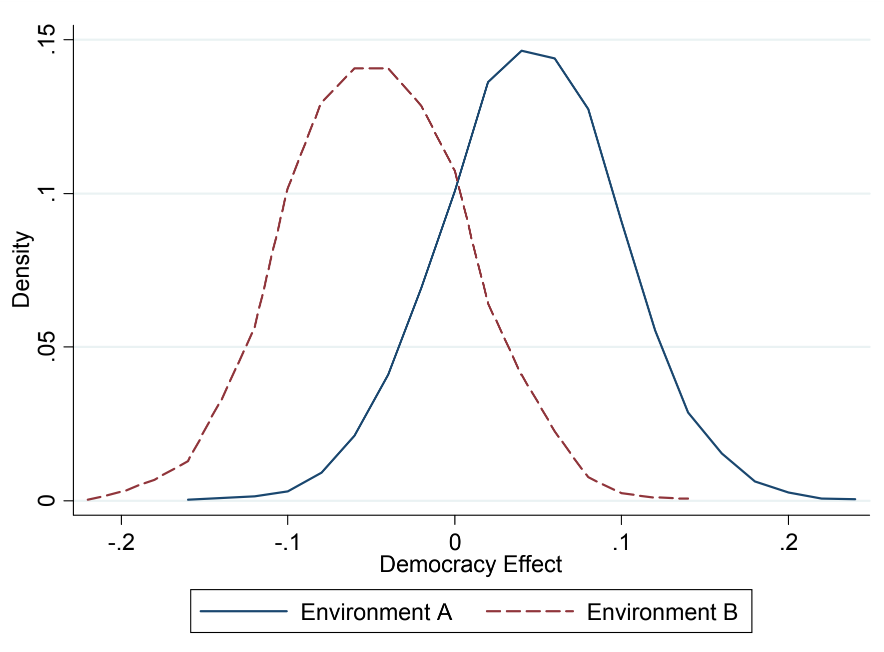


Table 1, panel b, and Figure 3 make it clear that, with correlated types, it is not enough to reweight the behavior of A and B voters to represent the average in the population. When types are not independent, the expected average behavior of an A or B voter depends on the types of the other members of the group. In this case, an A voter in a group for a majority for A is more likely to have a high value of his or her type than an A voter unconditional on the outcome of the vote. This simulation results make it clear that the performance of our proposed statistic for the democracy effect hinges on the independence of types.

IV Application 1: The democracy effect in solving social dilemmas

In this section we apply the weights-based identification strategy introduced in sections II and III to estimate the democracy effect with the data presented by Dal Bó, Foster and Putterman (2010). That paper studies whether a modification of the prisoners' dilemma that would allow players to reach an efficient outcome has a greater impact on behavior when the modification is democratically chosen.

IV.a Experimental design

In each experimental session, subjects were randomly divided into groups of four for the entire session. Each session consisted of two parts. In each of the ten periods in part 1, subjects were randomly matched in pairs to play the prisoner's dilemma game in Table 2 (Initial payoffs). At the beginning of part 2 of the experiment, the payoffs could be modified to the coordination game in Table 2 (Modified payoffs). Then, subjects played 10 rounds with random rematching. Notice that the initial game has a unique Nash equilibrium under the assumption of material payoffs, while the modified game is a coordination game with mutual cooperation dominating mutual defection.

Table 2: Stage Games - Dal Bó, Foster and Putterman (2010)

Initial payoffs			Modified payoffs		
	Other's action			Other's action	
Own action	C	D	Own action	C	D
C	50	10	C	50	10
D	60	40	D	48	40

Whether the payoffs were modified in part 2 was determined as follows. First, subjects voted on whether to modify payoffs at the beginning of part 2. Second, the computer randomly chose whether to consider the votes in each group. If the computer considered the votes, then the majority wins and in case of a tie the computer breaks the tie. If the computer did not consider the votes in a group, then it randomly chose whether to modify payoffs or not in that group.[6]

Following the terminology from section II, in this experiment we have two mechanisms: Democracy denotes that votes were considered, Random denotes that votes were not considered. We have two possible environments: initial and modified payoffs. Subjects could vote Yes for modification or No against modification.

[6] The subjects' computer screens informed them whether the computer randomly chose to consider the votes and whether payoffs were modified. Some groups for which the votes were not considered were informed of whether a majority had voted for modification or not. Since this information did not affect behavior we aggregate both types of sessions here. For more detail on the experimental design see Dal Bó, Foster and Putterman (2010).

IV.b Results

In the democratic treatment, 53% of the subjects voted to modify payoffs. Dal Bó, Foster and Putterman (2010) study the independence of votes in each group and cannot reject the null that votes are independent. This is consistent with the assumption needed to employ the new identification strategy introduced in section II.

Table 3 provides the data that allow us to measure the effect of democracy using the weights-based identification strategy. The first two columns report the percentage of subjects cooperating by vote in the democracy groups under the initial and the modified payoffs (period 11). It is clear that subjects cooperated more under the modified payoffs and that subjects that voted Yes were more cooperative than those that voted No. For groups with the Random mechanism, we also observe greater cooperation rate under the modified payoffs. We do not distinguish here subjects that voted Yes or No in the Random mechanism as that information is not necessary for the new identification strategy while it was essential for the identification strategy used in Dal Bó, Foster and Putterman (2010).

If we focus on the groups that ended with modified payoffs, we see that those groups whose payoffs were modified democratically had a cooperation rate of 70% while those that ended there randomly had a cooperation rate of 47.9%. As discussed in section II, this difference (22.1%) is not a measure of the democracy effect. As anticipated subjects that voted yes are overrepresented in groups that democratically chose to modified payoffs (75% of subjects that voted Yes against 53.13% in the general population) and that Yes voters are more cooperative than No voters. To calculate the democracy effect we need to reweight the behavior of Yes and No voters using the proportions in the general population. This correctly weighted average is presented in column (4) and its comparison with the behavior from exogenously imposed modification results in a democracy effect of 13.33% under modified payoffs. This effect is statistically significant at the 5% level (*p*-value = 0.0396, two-sided).[7]

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[7] If we only consider the sessions from the first experiment in Dal Bó, Foster and Putterman (2010) without the information treatment, then we obtain *p*-value=0.07.

Table 3: The effect of the democracy in overcoming social dilemmas

	Mechanism						
	Democracy				Random	Democracy Effect	Standard Errors[p-values]
Environment	Voted Yes	Voted No	All	Properly weighted average			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Initial	24 (25)	14.55 (55)	17.5 (80)	19.57	18.06 (72)	1.51	6.7 [0.8215]
Modified	80 (60)	40 (20)	70 (80)	61.25	47.92 (192)	13.33	6.48 [0.0396]**

Notes: Data from Dal Bó, Foster and Putterman (2010). Numbers in parentheses indicate the number of subjects. Bootstrapped SE. The number of bootstrap iterations is 10,000. ** indicates significance at the 0.05 level.

Note that similar calculations can be done for the initial payoffs without finding any significant democracy effect. This is not surprising as this game has a unique Nash equilibrium under the assumptions of monetary payoffs and a large majority of subjects chooses to defect.

V. Application 2: Incentives, effort and the democracy effect

In the previous section we considered the effect of democracy on behavior in environments with strategic interaction between the subjects. Strategic interaction may be important because democratic choice and strategic play both have a collective aspect—both voting and the strategic play affect the payoffs of others. It is thus instructive to ask whether a democracy effect is observed when, once the environment is chosen, ones play does not affect others. In this section we thus consider a real-effort experiment in which some subjects had the chance to democratically choose a remuneration policy before performing a set of tasks. We apply the weights-based identification strategy introduced in section II to measure the effect of democracy on the subjects' performance.

The idea that choosing the remuneration can affect effort is related to the literature suggesting that worker participation in the work place can affect productivity (see Levine and Tyson 1990; Bonin, Jones, and Putterman 1993; and Black and Lynch

2001). A recent experimental literature has also shown that allowing workers to choose their own compensation may contribute to high productivity (Charness, Cobo-Reyes, Jiménez, Lacomba and Lagos 2012; Mellizo, Carpenter, and Matthews 2014).

V.a Experimental design

In each experimental session, subjects participated anonymously through computers. Upon arrival, instructions were read aloud (instructions used in the experiment are available in Appendix B). All subjects were given pens and scrap papers. Each session consisted of two parts and subjects were paid only for their performance on one of the two parts (this was chosen randomly at the end of the session). In part 1 subjects were asked to add as many sets of five randomly generated two-digit numbers as possible in 20 minutes. This is a standard real-effort task in the literature – as in Niederle and Vesterlund (2007). During the task, subjects were allowed to take breaks and browse the internet. This was done for the subjects to have an opportunity cost of exerting effort in performing the summation task which may increase the response to the payment formula.[8] In part 1, subjects were paid 8 pounds regardless of the number of correct answers.

In part 2, subjects were randomly assigned to a group of three and, as in part 1, they were asked again to add as many sets of five randomly generated two-digit numbers as possible in 20 minutes. As in part 1, subjects were allowed to browse the internet during the 20 minutes assigned to the task. Payments were determined following one of two possible payment formulas: Formula A which consists of a fixed amount of 8 pounds; and Formula B which consists of 25 pence per correct response. Although each subject belonged to a group of three persons, their payment was determined by their own performance only, independent of the performance of the other two subjects in the group.

How the payment formula was chosen depended on the treatment: democractic or random. In the democratic treatment, each subject voted for one of the two formulas to be used in their group. The formula obtaining a majority of votes in the group was used in

[8] See Araujo et al (2015) for evidence of little response to incentives without internet breaks and Corgnet, Hernán-González, and Schniter (2015) for evidence that the availability of internet increases the effect of incentives in a real-effort task in the lab.

that group. In the random treatment, the computer randomly chose a formula to be used in the group. Since we use the weights-based identification strategy described in section II, we did not ask subjects to vote in the random treatment.

A total of 216 students at the University of York (UK), most of who were undergraduate students, participated in the experiment.[9] Twelve sessions were conducted. The duration of the experiment was less than 80 minutes, and the average earnings (including a participation fee of 3 pounds) are 12.5 pounds.

V.b Results

In this experiment, 56% of the subjects voted for formula B. An important requirement of the procedure, as noted, is that votes are independent within groups. This condition is verified in two ways. First, as Figure 4 shows, there is little difference between the observed cumulative distribution of votes for B in a group (solid line) and the distribution that would arise if subjects decided their votes independently of each other (binomial, depicted as a dashed line). This difference is not statistically significant (*p*-value = 0.5310).[10] Second, a random-effects analysis of voting does not reject the null of no random effects at the group level, suggesting that voting decisions are independent within groups (*p*-value = 0.1493).

[9] The subjects were recruited by email using *hroot* (Hamburg Registration and Organization Online Tool) - see Bock et al. (2015). No subject participated in more than one session. Subjects interacted through individual computer terminals using the z-Tree software (Fischbacher 2007).

[10] Since the theoretical distribution is not continuous, following Dal Bó, Foster and Putterman (2010), we do not use the usual Kolmogorov-Smirnov test but a modification proposed by Anthony N. Pettitt and Michael A. Stephens (1977). We calculate the *p*-value by Monte Carlo simulation under the null that the number of votes for B in a group follows a binomial distribution with probability of success equal to the observed one (0.5556).

Figure 4: Cumulative Distribution of Votes by Group

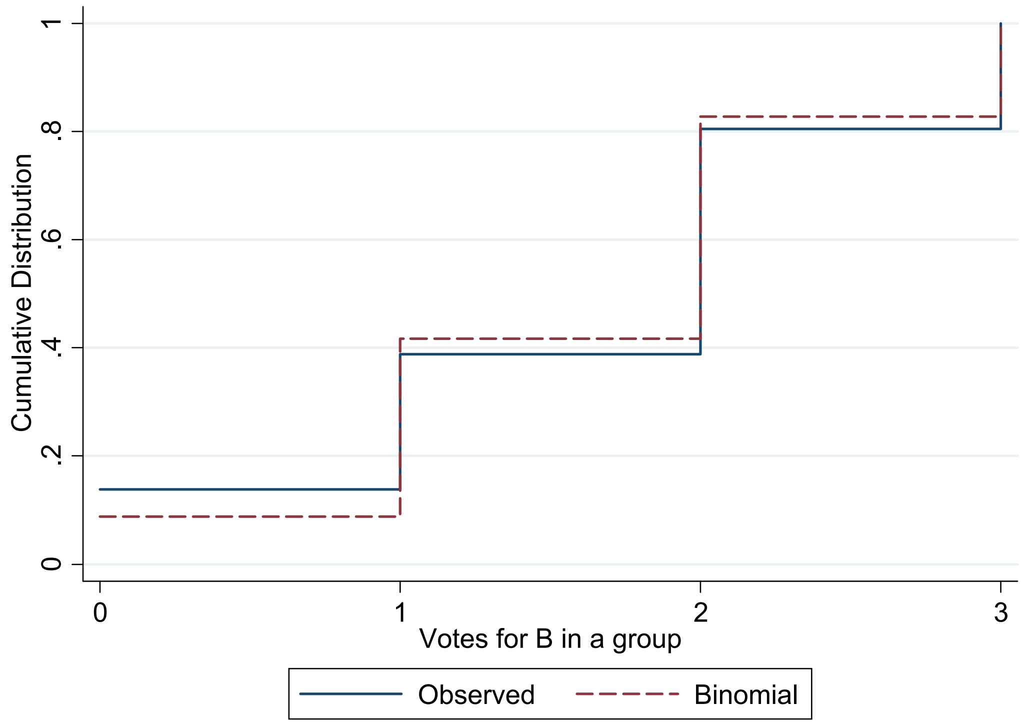

Table 4 provides a summary of the results of the experiment. The first two columns report the average number of correct responses in part 2 by vote in the groups with the democratic mechanism under formula A and formula B.

Note that greater incentives to exert effort (formula B) result in a greater number of correctly answered questions. Under the random institution, formula B results in 41.03 correct questions in average versus 20.87 for formula A. This difference is significant (p-value of less than 0.001).[11] For the groups under the democratic mechanism, we can compare behavior conditional on individual voting. As seen in Table 4, the average number of correct responses under formula A is smaller than under formula B for those who voted formula A (p-value = 0.0014) and for those who voted formula B (p-value less than 0.001). In summary, the experimental design resulted in task performance responding to incentives in this experiment. Consistently with the performance results,

[11] According to an individual-level Mann-Whitney test (two-sided).

we observe that subjects facing steeper incentives work harder: subjects spend significantly less time browsing the internet under formula B than under formula A.[12]

There is also evidence of selection into formula B, as those who voted for B perform significantly better under B than under A (p-value of less than 0.001). It is this selection into formula B that requires that we reweight the observations in order to obtain an unbiased estimate of the democracy effect.

The last two columns of Table 4 present the democratic effects based on the properly weighted average as defined in sections II and III. We do not find a significant democratic effect in raising performance. The democratic effect is negative under formula A and positive under formula B but it is not statistically significant in either case.

Table 4: The effect of the democracy on work performance (Number of correct answers)

	Mechanism						
	Democracy				Random	Democracy Effect	Standard Errors[p-values]
Environment	Voted A	Voted B	All	Properly weighted average			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Formula A	17.97 (33)	19.67 (9)	18.33 (42)	18.92	20.87 (45)	-1.95	4.06 [0.6301]
Formula B	36.2 (15)	51.25 (51)	47.83 (66)	44.56	41.03 (63)	3.53	2.82 [0.2112]

Notes: Numbers in parentheses indicate the number of subjects. Bootstrapped SE. The number of bootstrap iterations is 10,000.

Interestingly, the naïve calculation of the democracy effect is positive and significant for formula B (for formula B the naïve democracy effect is 6.8 with a p-value of 0.0312).[13] That is, the naïve measure of the democracy effect would have lead as to believe that there is a significant and positive effect while this is mainly due to the effect of selection: B voters perform better under that formula and also are overrepresented in groups which voted for that formula.

[12] For example, under the random treatment, subjects under formula A spend 458 seconds browsing the internet on average, versus only 7 seconds under formula B (p-value of less than 0.001).
[13] The naïve democracy effect is not statistically significant under formula A (p-value of 0.378).

While a previous experimental literature has suggested that workers' performance may improve when they are given the opportunity to choose the policies under which they work, our experiment has a different result in that democracy does not significantly raise the subjects' performance after controlling for the chosen policies and selection into policies. One possible reason for this difference is that some of those papers (like Charness et al 2012) focus on the total effect of democracy (that is the effect without controlling for the chosen policy and selection).

However, Mellizo, Carpenter and Matthews (2014) do find a direct effect of democracy on productivity in a real-effort experiment. In that case, the choice of compensation was between revenue sharing and a tournament. These compensation schemes result in payoffs that depend on other subjects' performance, introducing strategic interaction to the effort decision contrary to what is the case in our real-effort experiment. It could be that strategic interaction is necessary for the democracy effect to be important. Maybe a "social multiplier" through strategic interaction is required: the effect may operate, in part, by affecting subjects' expectations about the behavior of other which in turn may affect their own behavior. This channel is not present in our real effort experiment where payoffs are independent of the performance of others.

It is certainly an area for further research to examine the importance of democratic collective decision-making with other tasks studied in the literature, as the impact of mechanisms on intrinsic motivations may largely depend on the various factors including the characteristics of tasks (Gneezy, Meier and Rey-Biel, 2009).

VI. Conclusions

Many papers had found evidence consistent with democracy having a direct impact on behavior. However, it is difficult to prove such an effect due to the endogeneity generated by democratic choice: groups that voted for a particular policy or institution may be different from the average group. Dal Bó, Foster and Putterman (2010) overcome this identification problem by using identification strategies that require observing how subjects under the non-democratic mechanisms would have voted. They find that a democracy effect exists in their laboratory experiment. Since the use of their identification strategies is limited by the requirement that votes be observed even for those under the non-democratic mechanism, we provide here an alternative identification strategy without that restrictive requirement.

The new identification strategy presented here consists of calculating the average behavior under democracy by weighting the behavior of subjects that voted one way or another with their prevalence in the general population. We apply this new identification strategy to the data from Dal Bó, Foster and Putterman (2010) and find, consistently with their results, that there is a direct effect of democracy. We also apply the new identification strategy to a new real-effort task experiment in which payment to subjects depends only on their own performance and the choice of incentives. We find that the effect of introducing piece rate payments relative to a flat rate does not depend on whether they were introduce democratically or not. This suggests that the effect of the democracy effect may depend on the existence of strategic interaction such that optimal behavior may depend on beliefs about others (as in the coordination game in Dal Bó, Foster and Putterman 2010).

We expect that this new identification strategy, due to its less demanding requirements, will allow for the study of the democracy effect in a greater number of settings. Not only can be applied in laboratory experiments but it also may be useful in field experiments and even in observational data.

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Appendix A: Proofs

Proof of proposition 1: The democracy effect statistic under environment E is:

$$DE(E) = \hat{P}_a \hat{E}(C|D,E,a) + \left(1-\hat{P}_a\right)\hat{E}(C|D,E,b) - \hat{E}(C|N,E)$$

$$= (P_a+\varepsilon)(E(C|D,E,a)+\psi) + (1-P_a-\varepsilon)(E(C|D,E,b)+\xi) - (E(C|N,E)+\tau)$$

where ε, ψ, ξ and τ are errors arising from sampling with expectation equal to zero given that $E\left(\hat{P}_a\right) = P_a$, $E\left(\hat{E}(C|D,E,v)\right) = E(C|D,E,v)$ and $E\left(\hat{E}(C|N,E)\right) = E(C|N,E)$. Therefore,

$$E\big(DE(E)\big) = [P_a E(C|D,E,a) + (1-P_a)E(C|D,E,b) - E(C|N,E)] + E(\varepsilon\psi) - E(\varepsilon\xi).$$

Note that the terms in brackets is zero under the assumption of independent types given formula (6) in section II. It remains to be shown that $E(\varepsilon\psi) = E(\varepsilon\xi) = 0$.

Given the law of iterated expectations we have that $E(\varepsilon\psi) = E\big(\varepsilon E(\psi|\varepsilon)\big)$, and it is enough to show that $E(\psi|\varepsilon) = 0$. This is the case when types are independent as own behavior depends only on ones types after controlling for the environment and the mechanism. In the same way we can prove that $E(\varepsilon\xi) = 0$.□

Proof of proposition 2: For simplicity of exposition, we focus on environment *A*. For a sample of size *n* with $n = n_D + n_N$, the democracy effect statistic is

$$DE(A) = \left(\frac{\sum_{i\in S_D} 1\{v_i = a\}}{n_D}\right)\left(\frac{\sum_{i\in S_{DAa}} C_i}{n_{DAa}}\right) + \left(\frac{\sum_{i\in S_D} 1\{v_i = b\}}{n_D}\right)\left(\frac{\sum_{i\in S_{DAb}} C_i}{n_{DAb}}\right) - \frac{\sum_{i\in S_N} C_i}{n_N},$$

where S_D denotes the set of subjects under democracy (D), and S_{DAv} denotes the set of subjects under democracy who ended under environment *A* and voted for *v*.

Note that the last term is the average of iid random variables, and by the central limit theorem, converges to a Normal distribution. Since the last term is independent of the first two ones, it remains to be shown that the first two terms also converge to a Normal distribution as the number of observations increases.

To prove this, we start by defining the following random variables: $a_i = 1\{v_i = a\}$, $b_i = 1\{v_i = b\}$ are random variables denoting the vote of subject i, and $A_i = 1\{i \in S_{DAa} \cup S_{DAb}\}$ is a random variable denoting that subject *i* is under environment *A*. We can now define some averages: $\bar{a} = \frac{\sum_{i \in S_D} a_i}{n_D}$, $\bar{b} = \frac{\sum_{i \in S_D} b_i}{n_D}$, $\overline{Aa} = \frac{\sum_{i \in S_D} A_i a_i}{n_D}$, $\overline{Ab} = \frac{\sum_{i \in S_D} A_i b_i}{n_D}$, $\overline{C_{Aa}} = \frac{\sum_{i \in S_D} C_i A_i a_i}{n_D}$ and $\overline{C_{Ab}} = \frac{\sum_{i \in S_D} C_i A_i b_i}{n_D}$.

The first two terms of the democracy effect can be written as $\bar{a}\frac{\overline{C_{Aa}}}{\overline{Aa}} + \bar{b}\frac{\overline{C_{Ab}}}{\overline{Ab}}$. There are in effect three complications associated with constructing an asymptotic distribution for this expression. The first is that that A_i and C_i are *m-dependent* given that the realization of the voting is the same for all m subjects in a group, the second is that the random variable a_i is correlated with $C_i A_i a_i$ and with $A_i a_i$ and similarly for b_i, and the third is that a_i and b_i are perfectly negatively correlated. All three issues are addressed by Theorem 4 in Hoeffding and Robbins (1948) extended to the case of six correlated m-dependent variables. This extension of Theorem 4 is as follows. Assume a sequence m-dependent random vectors in $\mathbb{R}^6$ such that $Ex_{ij} = 0$, and $E|x_{ij}|^3 < \infty$ for $i = 1,2,\dots,6$ and $j = 1,2,\dots,n$. Assume that the function $H\left(\frac{\sum_j^n x_{1j}}{n}, \frac{\sum_j^n x_{2j}}{n}, \dots, \frac{\sum_j^n x_{6j}}{n}\right)$ has a total differential at $(0,\dots,0)$. Then as $n \to \infty$, $H(.)$ has a limiting normal distribution. Note that the constraint that the random variables have expectation equal to zero is done without loss of generality as the random variables can be centered by subtracting their expected value. That is, if $Ex_{ij} = X_i \neq 0$, then we define $\hat{x}_{ij} = x_{ij} - X_i$ and $\widehat{H}\left(\frac{\sum_j^n \hat{x}_{1j}}{n}, \frac{\sum_j^n \hat{x}_{2j}}{n}, \dots, \frac{\sum_j^n \hat{x}_{6j}}{n}\right) = H\left(\frac{\sum_j^n x_{1j} - X_1}{n} + X_1, \frac{\sum_j^n x_{2j} - X_2}{n} + X_2, \dots, \frac{\sum_j^n x_{6j} - X_6}{n} + X_6\right)$ and we work then with the function $\widehat{H}(.)$.

Note that in our case all the random variable are between 0 and 1, and, hence, the cube of their absolute value is less than infinity. Finally, it is straightforward to show that $\bar{a}\frac{\overline{C_{Aa}}}{\overline{Aa}} + \bar{b}\frac{\overline{C_{Ab}}}{\overline{Ab}}$ has a total differential at the expected value of its arguments (what is crucial here is that $E(A_i a_i) \neq 0$ and $E(A_i b_i) \neq 0$ given that $P_a \in (0,1)$, it would fail if everybody voted in the same way). Thus, the conditions for Theorem 4 in Hoeffding and

Robbins (1948) hold, the first two terms converge to Normal distributions and the proposition follows. □

Appendix B: Experimental Instructions

B.1. Voting Treatment:

Welcome

This is an experiment on decision-making. Please turn off all of your electronic devices (e.g., mobile phone).

Each of you will be paid at the end of the experiment. How much you will be paid depends on what you do during the experiment. All interactions in the experiment will be anonymous and through computers. Your identity will be kept private.

During the experiment you are not allowed to communicate with other participants. If you have a question, please raise your hand.

This experiment has **two** parts. Each part lasts 20 minutes. Your payment is determined potentially based on your own performance in Part 2. At the end of the experiment, Part 1 or Part 2 will be randomly (with a probability of 50%) selected by the computer for payment.

Part 1

In this part, you have 20 minutes to answer a series of questions. For each question, you will be asked to add five randomly generated two-digit numbers as seen in the screen. Once you add five two-digit numbers in a question, fill the blank with your answer and click the "OK" button, you will move on to the next addition question: five two-digit numbers will be randomly re-generated.

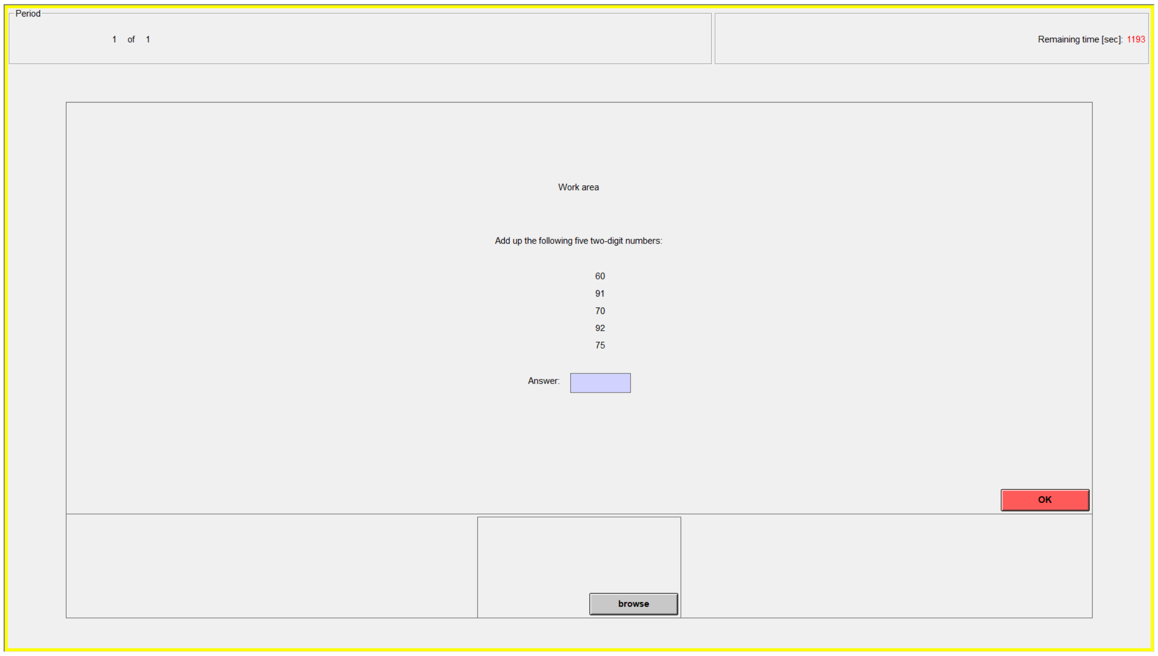
Your payment will be calculated with the following formula in Part 1:

A fixed amount of 8 pounds

Note that your payment will be fixed. It will **not** depend on the number of your own correct answers or the responses of others. You will be paid in total 11 pounds (including the £3 show-up fee) if Part 1 is selected for payment.

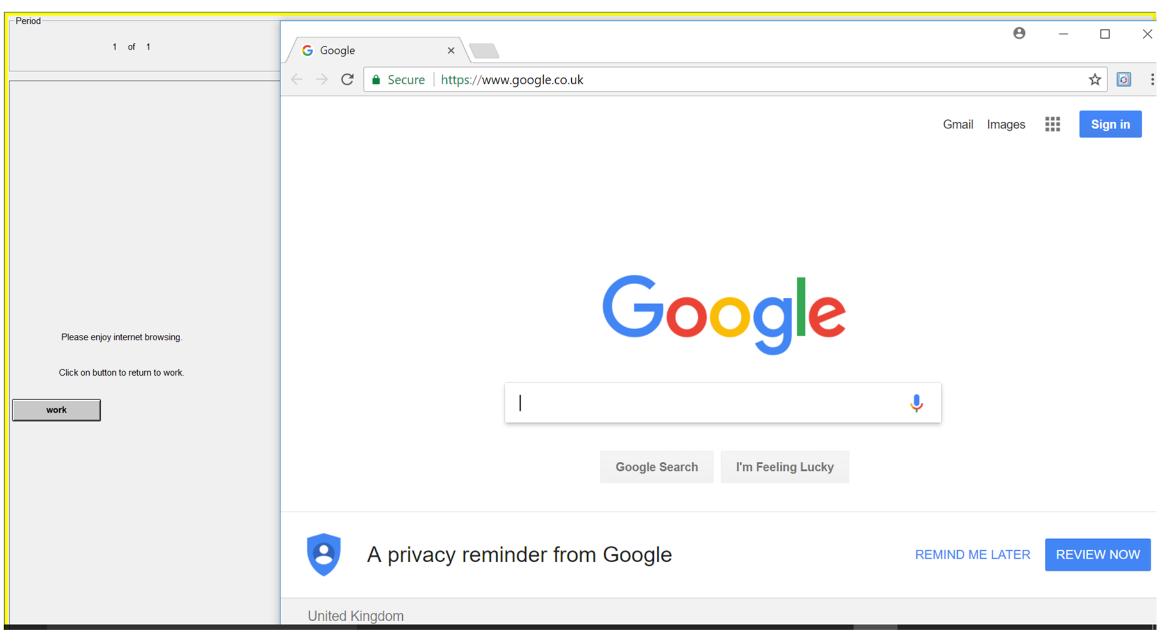
During the experiment, you can take a rest anytime by using Google Chrome (internet browser). You can do whatever you want to do (e.g., check emails, read news) with the internet browser. As shown in the screen image below, there is a "browse" button at the bottom of the screen. If you click on the "browse" button, the addition question will disappear (your screen will become blank) and Google Chrome will pop up. Click on the "work" button if you would like to get back to work. Google Chrome will disappear and your screen will be switched to the work site (the question you were working on) if you click on the "work" button. Notice that the timer will not stop when you are browsing.

[Screen image of the addition task:]

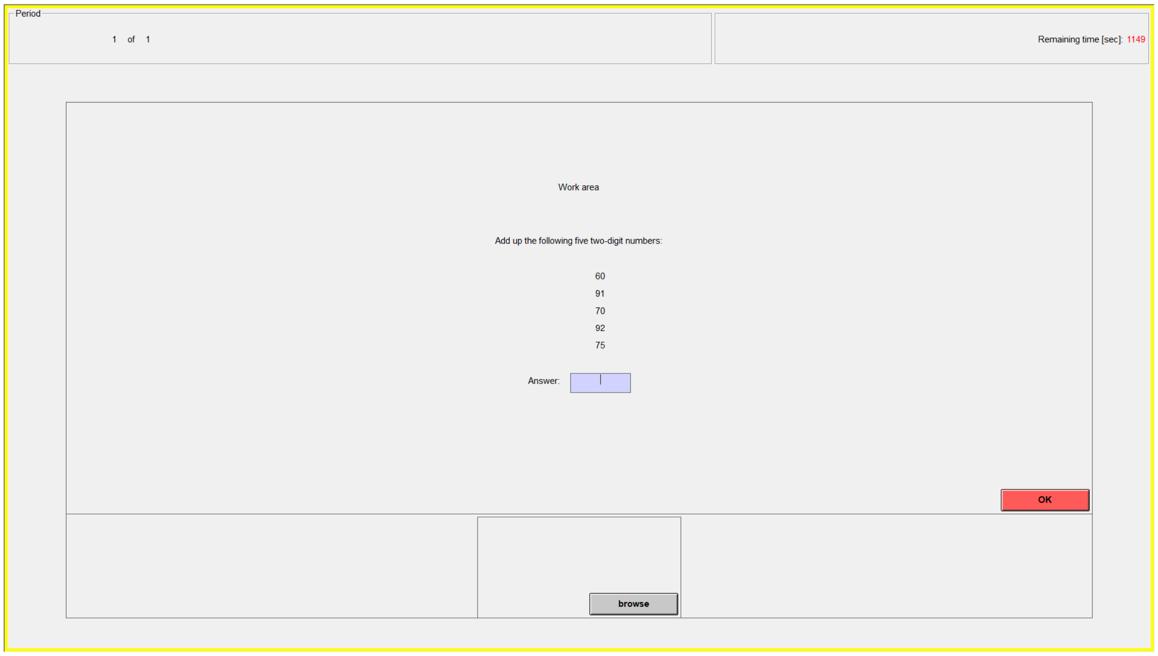


Note: The numbers shown above are for illustrative purpose only.

[If you click on the "browse" button, Google Chrome will pop up and the addition task will disappear:]



[When you click on the "work" button, the google chrome window will disappear and your screen will return to the addition task you were working on before:]



Once the 20 minutes are over, you will be informed of the number of your correct responses in the addition task and will move on to Part 2. Any questions?

Part 2

Part 2 consists of a voting stage and an addition-task stage that lasts for 20 minutes. In this part, participants will be <u>randomly divided into groups of three</u>. This means that you will be randomly assigned to one group with two other participants. You will not know who these participants are, nor will they know who you are.

Before answering the adding questions, you will vote which formula to be used for payment calculation in your group. There are two possible formulas:

Formula A: a fixed amount of 8 pounds
Formula B: 25 pence per correct response

Formula A is the same formula used in Part 1. In contrast, you will be able to earn money dependent on your answers in Formula B. Note, however, that your payment in Formula B will depend on <u>the number of your own correct answers</u>; and it will not depend on the responses of others.

All three persons in your group will use the formula that **receives a majority of votes (i.e., 2 or 3 votes)**. You will be informed of the formula chosen by your group.

In Part 2, you will have 20 minutes to solve as many addition problems as you can. As in Part 1, you can take a rest by browsing internet (see the screen images on the previous pages). Notice that, as in Part 1, the timer will not stop when you are browsing.

If Part 2 is randomly selected for payout, you will be paid a show-up fee of £3 and an amount depending on the selected payment formula and the number of your own correct responses (if Formula B was selected).

Any questions?

B.2. Random choice of formula:

Welcome

This is an experiment on decision-making. Please turn off all of your electronic devices (e.g., mobile phone).

Each of you will be paid at the end of the experiment. How much you will be paid depends on what you do during the experiment. All interactions in the experiment will be anonymous and through computers. Your identity will be kept private.

During the experiment you are not allowed to communicate with other participants. If you have a question, please raise your hand.

This experiment has **two** parts. Each part <u>lasts 20 minutes</u>. Your payment is determined potentially <u>based on your own performance in Part 2</u>. At the end of the experiment, Part 1 or Part 2 will be randomly (with a probability of 50%) selected by the computer for payment.

Part 1

In this part, you have 20 minutes to answer a series of questions. For each question, you will be asked to add five randomly generated two-digit numbers as seen in the screen. Once you add five two-digit numbers in a question, fill the blank with your answer and click the “OK” button, you will move on to the next addition question: five two-digit numbers will be randomly re-generated.

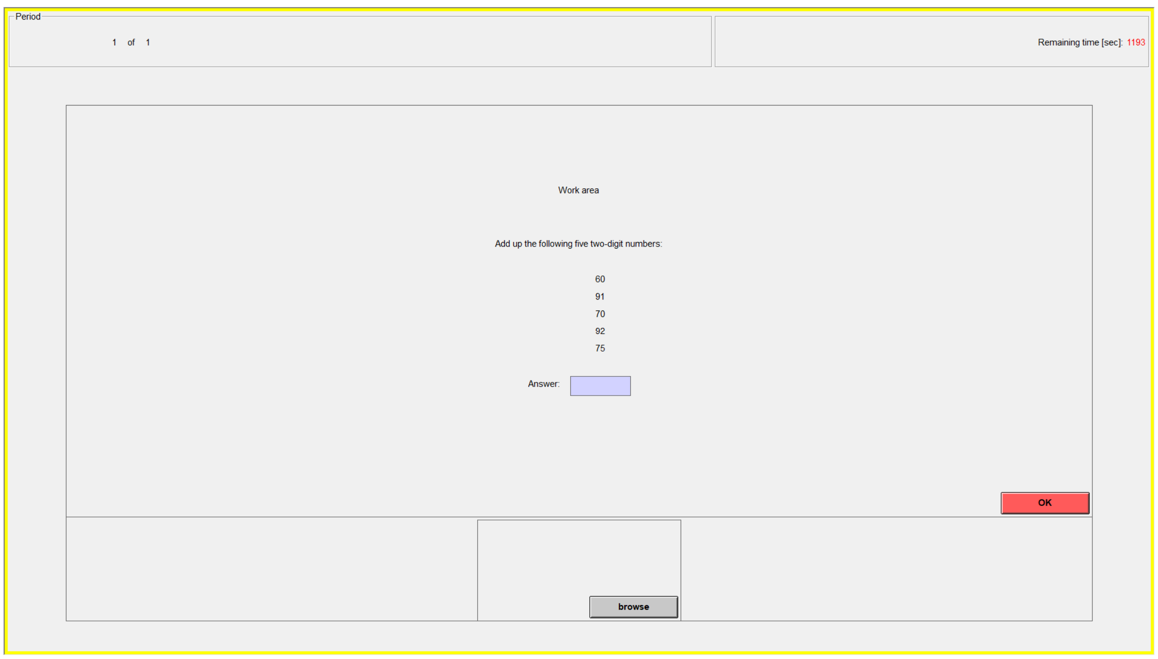
Your payment will be calculated with the following formula in Part 1:

A fixed amount of 8 pounds

Note that your payment will be fixed. It will **not** depend on the number of your own correct answers or the responses of others. You will be paid in total 11 pounds (including the £3 show-up fee) if Part 1 is selected for payment.

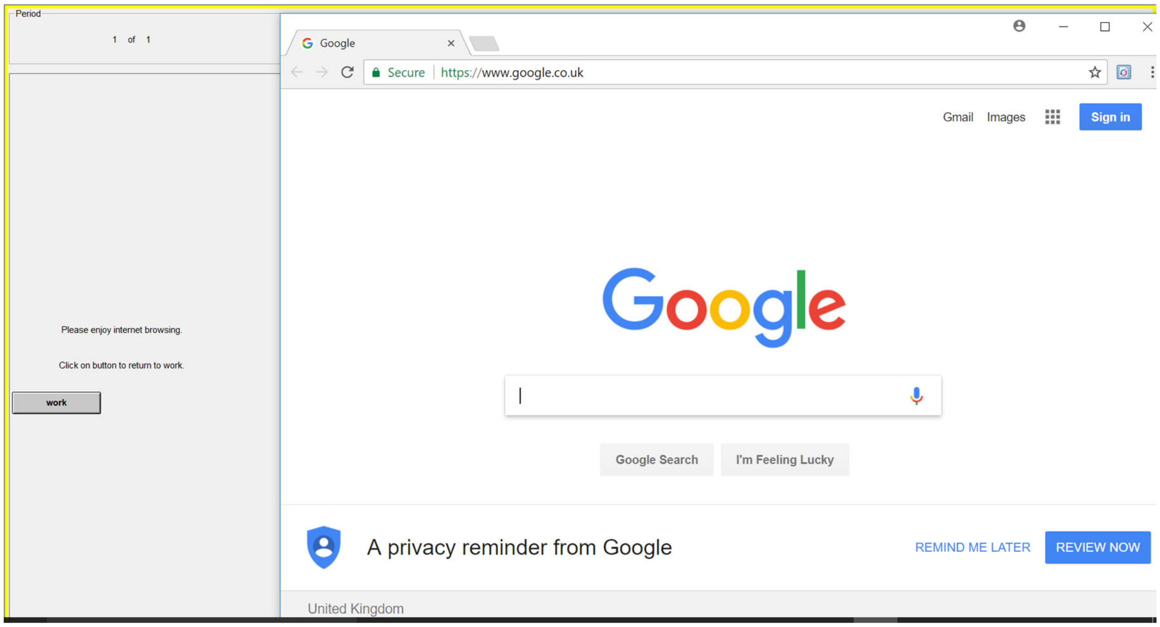
During the experiment, you can take a rest anytime by using Google Chrome (internet browser). You can do whatever you want to do (e.g., check emails, read news) with the internet browser. As shown in the screen image below, there is a “browse” button at the bottom of the screen. If you click on the “browse” button, the addition question will disappear (your screen will become blank) and Google Chrome will pop up. Click on the “work” button if you would like to get back to work. Google Chrome will disappear and your screen will be switched to the work site (the question you were working on) if you click on the “work” button. Notice that the timer will not stop when you are browsing.

[Screen image of the addition task:]

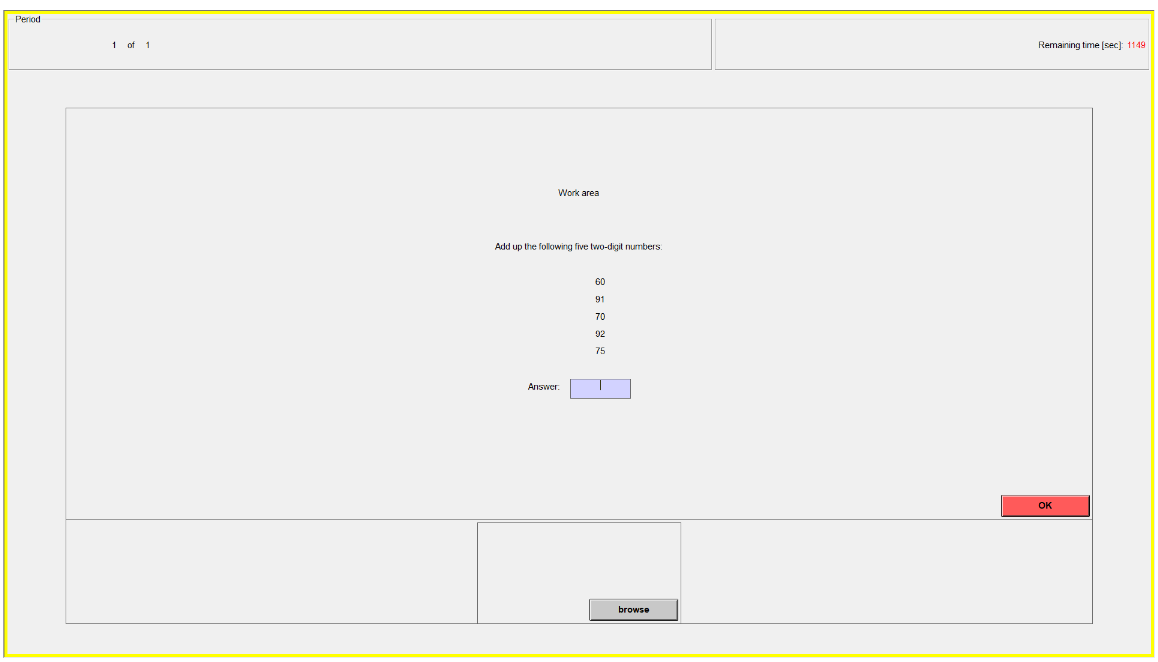


Note: The numbers shown above are for illustrative purpose only.

[If you click on the “browse” button, Google Chrome will pop up and the addition task will disappear:]



[When you click on the "work" button, the google chrome window will disappear and your screen will return to the addition task you were working on before:]



Once the 20 minutes are over, you will be informed of the number of your correct responses in the addition task and will move on to Part 2. Any questions?

Part 2

In this part, participants will be randomly divided into groups of three. This means that you will be randomly assigned to one group with two other participants. You will not know who these participants are, nor will they know who you are.

Before answering the adding questions, the computer will randomly (i.e., with a probability of 50%) choose the formula to be used in your group. There are two possible formulas:

> **Formula A: a fixed amount of 8 pounds**
> **Formula B: 25 pence per correct response**

Formula A is the same formula used in Part 1. In contrast, you will be able to earn money dependent on your answers in Formula B. Note, however, that your payment in Formula B will depend on the number of your own correct answers; and it will not depend on the responses of others.

You will be informed of which formula the computer randomly selected. All three persons in your group will use the same payment formula.

In Part 2, you will have 20 minutes to solve as many addition problems as you can. As in Part 1, you can take a rest by browsing internet (see the screen images on the previous pages). Notice that, as in Part 1, the timer will not stop when you are browsing.

If Part 2 is randomly selected for payout, you will be paid a show-up fee of £3 and an amount depending on the selected payment formula and the number of your own correct responses (if Formula B was given).

Any questions?