Non-Consequentialist Voting*

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April 19, 2010

Abstract

Standard theory assumes that voters' preferences over actions (voting) are induced by their preferences over electoral outcomes (policies, candidates). But voters may also have non-consequentialist (NC) motivations: they may care about how they vote even if it does not affect the outcome. When the likelihood of being pivotal is small, NC motivations can dominate voting behavior. To examine the prevalence of NC motivations, we design an experiment that exogenously varies the probability of being pivotal yet holds constant other features of the decision environment. We find a significant effect, consistent with at least 12.5% of subjects being motivated by NC concerns.

JEL classification: C91, D01, D63, D72

Keywords: Elections, Democracy, Expressive voting, Social preferences, Extended preferences

*We thank Robert J. Aumann, Christoph Engel, Alex Gershkov, Sergiu Hart, Ori Heffetz, Ilan Nehama and Eyal Winter for valuable comments and suggestions. Ari Eichler, Guy Sopher and Matan Zilcha provided excellent research assistance. Moses Shayo thanks the Authority for Research & Development at the Hebrew University for financial support.

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1 Introduction

Social choice and political economy models typically assume that voters are exclusively motivated by preferences over electoral outcomes (policies, candidates). An alternative conjecture is that voters may also care about the act of voting itself, even if it does not affect the electoral outcome. Call this motivation "non-consequentialist" (NC). NC motivation is not necessary to explain voting behavior that is inconsistent with maximization of economic self interest. Voters may well have consequentialist other-regarding preferences.[1] The key implication of having a NC component in voters' preferences is that as the probability of being pivotal in an election decreases, the consequentialist motivation diminishes in importance and the NC motivation comes to dominate voting decisions. This paper experimentally investigates this implication.

Various NC motivations have been discussed in the economics, political science and moral philosophy literatures. Here are three examples.

(a) **Expressive voting**. A vote is not a choice between outcome t_1 and outcome t_2 but is, rather, a choice between expressing a preference for t_1 and expressing a preference for t_2 (Brennan and Lomasky 1993). Brennan and Buchanan (1984) compare voting to cheering for sports teams: "neither the act of voting nor the direction of a vote cast can be explained as a means to achieving a particular political outcome, any more than spectators attend a game as a means of securing the victory of their team" (p. 187).

(b) **Moral reasons and duties**. People care about the morality of their actions, which is sometimes evaluated independently of their consequences. A prominent example of what may be viewed as a moral NC motivation in the economics literature is rule utilitarianism (Harrod 1936, Harsanyi 1977, Coate and Conlin 2004, Feddersen and Sandroni 2006. Laffont 1975 proposes a similar "Kantian economics"). Rule utilitarians have a preference for acting according to the rule that, if followed by everyone (perhaps in a certain reference group), maximizes social welfare (or the welfare of the reference group). A second example is deontological duties, namely duties to perform an act independently of its consequences, such as not to lie, not to kill people etc. (see e.g. Williams 1973, Nagel 1986, Harel 2005).

[1] Voters may e.g. be motivated by altruism (Orr 1976), concerns for relative position (Corneo and Grüner 2000) or social identity (Shayo 2009).

(c) **Self-image concerns**. People want their actions to be consistent with their self-image. For example, they may think of themselves as "charitable". When the cost of acting "charitably" is low, such an action becomes optimal. Rich people may thus vote for a policy that would transfer income from the rich to the poor, when they believe that the probability of their vote affecting the outcome is low. This example is due to Tullock (1971).

The existence of voters with NC motivations can have important implications for the equilibrium properties of common voting mechanisms. For example, under NC motivation, even if there are only two candidates and all voters have identical preferences over these candidates, majority rule may deliver poor outcomes. One may imagine a situation where all voters prefer peace to war: if pivotal, any voter would choose the dovish candidate. However, voters also prefer to express nationalist sentiments by casting their votes for the hawkish candidate. Given that no voter is likely to be pivotal the latter motivation may prevail, leading all voters to vote for the hawkish candidate.

In this paper we set out to empirically examine the existence and prevalence of NC motivations. Our focus is on *how* people vote rather than on whether and why they turn out to vote. Specifically, we seek to examine whether a lower probability of being pivotal increases the likelihood of voting in accordance with NC considerations.

As we discuss in section 3, identifying a causal effect of the probability of a given voter being pivotal – which we denote by p – on voting behavior in field data is extremely difficult. This is primarily due to the endogeneity of p in actual elections. Furthermore, even if such a causal effect can be established, it may well be driven by other mechanisms. At a more fundamental level, the fact that under standard voting mechanisms voters may have incentives to vote strategically makes it hard to infer preferences from voting decisions. To overcome these problems, we employ an experimental approach.

We design an experiment in which subjects vote over the allocation of a fixed sum of money (roughly \$50) between four subjects. We examine behavior under four different (exogenously determined) probabilities of being pivotal. In all treatments, subjects are faced with at least two alternatives. The first of these alternatives is an equal division of the \$50 among all group members, while the second is a division heavily tilted towards the voter making the decision. The equal division is designed to represent the morally superior alternative – an assertion we verify empirically by eliciting impersonal and disinterested judgments. The unequal division is designed to be the alternative

most subjects would choose when pivotal: it maximizes the decision maker's monetary payoff, yet is not entirely selfish and is comparable to typical offers in the dictator game. As we point out in our theoretical discussion, at a sufficiently low p, a voter with a moral NC motivation switches from the consequentially-optimal to the morally-optimal alternative.

Our experiment is designed to rule out several confounding factors which may hinder identifying the effect of p on voting decisions. Specifically, we are able to exogenously vary p while keeping group size constant and without creating intra-group disparities in the ability to affect the outcome. By varying the number of alternatives we can also address the possibility that the shift in behavior is due to subjects voting erratically at low p's where stakes are small.

Results suggest a significant effect of p on voting behavior, consistent with the existence of NC motivations among at least 12.5% of our subject population. The proportion of NC-motivated individuals is slightly higher when focusing on subjects who have turned out to vote in actual elections. Interestingly, the effect only sets in at rather low probabilities ($p = 0.001$), and does not seem to increase when p is further reduced.

This paper is closely related to two main strands of the literature. The first is the literature on expressive voting, beginning with Fiorina (1976) and Brennan and Buchanan (1984). See Hamlin and Jennings (2009) for a recent review. This literature includes several attempts to identify expressive voting experimentally, with mixed results. Carter and Guerette (1992) and Fischer (1996) examine whether donation to a charity is more likely the lower is the probability that the decision will be implemented while Tyran (2004) studies the relation between voting for a proposal to donate and subjective expectations regarding the approval rate of the proposal. Eichenberger and Oberholzer-Gee (1998) compare decisions in variants of the dictator game to analogous decisions under majority rule. Feddersen et al. (2009) examine voting over two possible distributions of monetary rewards, where the number and identity of eligible voters vary from round to round, enabling direct variation of p within subjects. Finally, Kamenica and Egan (2010) study support for income redistribution, comparing decisions made by dictators (who decide for the whole group) to the assertions of powerless subjects regarding how they would decide if they could determine the outcome, as well as to voting under majority rule.

Our experimental design differs from the above studies in several respects. First and foremost, in all these studies except Carter and Guerette (1992), variations in p

are achieved by varying other relevant features of the decision environment such as the rules for determining the outcome (e.g. dictator vs. majority voting) or the number of eligible voters. It might therefore be hard to disentangle the effect of p from the effects of these other features.[2] Second, all the above experiments except Eichenberger and Oberholzer-Gee (1998) assume rather than measure what the moral standard is in the particular experimental situation.[3] Third, all the experiments except Eichenberger and Oberholzer-Gee (1998) present subjects with only two alternatives. Thus, the only deviation allowed from the "selfish" option is to the (presumably) moral one. As we show below, this fails to take into account the possibility that subjects' tendency to make mistakes may vary with p. Finally, three of the above mentioned studies use a within-subject design, which opens up the possibilities of repeated-games effects, reciprocity, consistency-seeking etc.[4] We elaborate on these issues in section 3.1.

A second strand of the literature related to this paper is the study of social preferences. An important branch of this literature attempts to explain behavior in simple games in terms of consequentialist preferences over the final allocation of resources (e.g. Fehr and Schmidt 1999, Charness and Rabin 2002).[5] Our results suggest that some individuals may also be motivated by preferences over the strategies themselves, and that more research effort should be directed at understanding these NC motives.

[2] Carter and Guerette's (1992) direct manipulation of p comes at the cost of turning the decision from a group decision into a purely individual one. Tyran (2004) varies the majority required for accepting the proposal. His main finding, however, is that *within* any specific treatment (and regardless of whether higher approval means the subject is more likely to pay a private cost for supporting the proposal), subjects who expect the proposal to receive higher approval also support it more. This is an interesting result, but the variation in expectations underlying it is not exogenously controlled.

[3] Our approach differs from Eichenberger and Oberholzer-Gee (1998) in that they measure the moral standard using a survey (asking e.g. what a fair dictator *ought* to give), while we use a revealed preference approach.

[4] A minor difference with our experiment is that none of the above studies measure the actual political participation of the subjects and only one (Kamenica and Egan 2010) measures relevant political attitudes. Measuring these variables helps us evaluate the randomization procedure and enables us to check if the results are not driven by people who do not vote in real world elections.

[5] A second prominenet approach introduces concerns for reciprocity (Charness and Rabin 2002 combine elements of both approaches). The present paper explicitly rules out the possibility of reciprocity (see point 5 in section 3.1). However, there is a sense in which reciprocity concerns can be seen as NC concerns akin to the rule-utilitarian concerns in example (b) above, see Sugden (1984). A more closely related model to our paper is Andreoni's (1990) model of warm glow. As formulated by Andreoni, warm glow is a consequentialist motivation. In his model, an individual receives a warm glow payoff from making a particular donation g_i (irrespective of whether anyone benefits from the donation) so that g_i acts like a private good. Thus, the individual receives no warm glow if no donation is actually made. Nonetheless, it seems that part of the intuitive appeal of the warm glow concept, as well as its empirical implications (e.g. no crowding out of private donations by public provision) stem from its capturing a NC motivation, albeit in situations where one's decision is always implemented.

The properties of a model that allows players to have preferences over strategies and not only over outcomes are studied by Segal and Sobel (2007). However, Segal and Sobel assume that when, given a strategy profile of the other players, two of player i's strategies lead to the same distribution of expected utility from outcomes for all players, then player i must be indifferent between these two strategies. In contrast, our notion of NC motivation relates to situations where a player is not indifferent between two actions even though they do not affect the outcome (i.e. the voter is not pivotal given the way others vote). We now make this notion a bit more precise.

2 Theoretical framework

Consider the following simple setting. There is a set of outcomes (policies, candidates) T, and a set of individuals who vote directly over these outcomes (or possibly a subset of the outcomes). To focus on the decision how to vote (rather than whether to vote), assume that everyone votes. Individuals vote privately, simultaneously and only once. Thus the set of available actions for each individual i is $A_i \subseteq T$. The analysis below focuses exclusively on the decisions of a single agent.

Adapting Segal and Sobel's (2007) framework to our setting, each individual has preferences $\succcurlyeq_i^{out}$ over T, as well as preferences over A_i. The latter depend on the "context" in which the game is played, which is given by some profile of actions $a^* = (a_i^*, a_{-i}^*)$. Given context a^*, individual i has preferences $\succcurlyeq_{i,a^*}$ over A_i. The statement $a_i \succ_{i,a^*} a_i'$ says that given the context a^*, individual i would prefer to vote for a_i rather than to vote for a_i'.

Standard models assume that preferences over actions are induced by preferences over outcomes. In other words, if $t(a)$ is the outcome elected when the profile of votes is a, then $a_i \succcurlyeq_{i,a^*} a_i'$ iff $t(a_i, a_{-i}^*) \succcurlyeq_i^{out} t(a_i', a_{-i}^*)$. In this case we say that the voter has a purely consequentialist motivation.

Definition 1 *Voter i is said to have a NC motivation if there exists a context a^* in which $t(a_i, a_{-i}^*) = t(a_i', a_{-i}^*)$ but $a_i \succ_{i,a^*} a_i'$.*

Intuitively, a voter in a presidential elections who is certain that candidate A is going to win regardless of her vote, might still not be indifferent between voting for candidate A and voting for candidate B (e.g. for one of the reasons mentioned in the introduction).

We focus on the special case where voter i's preferences over actions can be represented by a utility function of the form:

$$u_{i,a^*}(a_i) = v_i\left(t(a_i, a^*_{-i})\right) + \gamma_i g(a_i) \tag{1}$$

where $\gamma_i \geq 0$; $v_i(t)$ is voter i's (consequentialist) payoff from policy t; and $g(a_i)$ is a (NC) payoff from voting for a particular alternative a_i, where $g(\cdot)$ is not constant. To simplify the exposition we take $v_i(\cdot)$ and $g(\cdot)$ to be positive-valued functions.[6]

The existence of voters with NC motivations can have important implications for the equilibrium outcomes of common voting mechanisms. However, in the present paper we are concerned with the more basic question of the implications of NC motivations for individual behavior. To this end, we take the probability p that i will be "pivotal" as exogenously given and positive. We say that individual i is pivotal when her vote determines the outcome, i.e. when $t(a)$ coincides with a_i.[7]

Notice first that if $\gamma_i = 0$ (i.e. the voter has no NC motivation), then voter i should vote for the policy that maximizes $v_i(t)$, regardless of p. If it turns out that she is pivotal, then voting in this way is strictly better; while if she is not, it doesn't hurt.[8]

Consider now the case where the voter has a NC motivation ($\gamma_i > 0$). Let t_{-i} be the policy elected if i is not pivotal and denote by $U_i(a_i)$ voter i's expected utility from voting for $a_i \in A_i$. We have:

$$U_i(a_i) = pv_i(a_i) + (1-p)v_i(t_{-i}) + \gamma_i g(a_i) \tag{2}$$

Taking t_{-i} as given, maximizing expected utility means maximizing $pv_i(a_i) + \gamma_i g(a_i)$. Thus, as p shrinks the NC term becomes the dominant factor determining voting be-

[6] Equation (1) represents a stronger form of NC motivation than required in Definition 1. In particular, it makes three assumptions. First, if $g(a_i) \neq g(a'_i)$, the separability implies that in *any* context such that $t(a_i, a^*_{-i}) = t(a'_i, a^*_{-i})$, an individual with a NC motivation ($\gamma_i > 0$) is not indifferent between voting for a_i and a'_i. Second, the NC ranking of actions is context-independent: $g(\cdot)$ depends only on a_i (cf. Sugden 1984). Finally, equation (1) assumes that everyone in society agree on what "the right thing to do" is, and individuals only differ in the weight (γ_i) they assign to this NC motivation. All the theoretical results below hold if we allow people to disagree on the NC ranking of the alternatives. However, our experiment is designed to test this stronger form of NC motivation, in which g is the same for all individuals. Our estimates of the prevalence of NC motivation will hence be a lower bound on the prevalence of NC motivations in general.

[7] We use p rather than p_i to save on notation throughout the paper. In the actual experiment, every voter has her own exogenous p_i and expected utility is: $U_i(a_i) = p_i v_i(a_i) + \sum_{j \neq i} p_j v_i(a_j) + \gamma_i g(a_i)$. However, since the second term is taken as given by i, this does not change any of the results.

[8] Note the importance of the exogeneity of p. Under alternative mechanisms, voters may well have incentives not to vote for the policy that maximizes $v_i(t)$ even if they have no NC motivations.

havior. Consider any two alternatives, say t_1 and t_2, where voting for t_1 yields a higher NC payoff: $g(t_1) > g(t_2)$. Suppose that other things equal, voter i prefers policy t_2 to be implemented, i.e. $v_i(t_2) > v_i(t_1)$.[9] This voter would prefer voting for t_1 over voting for t_2 iff:

$$p \leq \gamma_i \frac{g(t_1) - g(t_2)}{v_i(t_2) - v_i(t_1)} \equiv p^*.$$

This yields a simple testable implication.

Observation 1 *Suppose* $A_i = \{t_1, t_2\}$ *and* $g(t_1) > g(t_2)$.
(a) If $\gamma_i = 0$ *then the voter's choice does not vary with* p.
(b) If $\gamma_i > 0$ *then there exists a threshold level* $p^* > 0$ *such that* $U_i(t_1) \geq U_i(t_2)$ *iff* $p \leq p^*$. *In particular, if* $p^* < 1$ *(i.e.* t_2 *maximizes utility when* i *is pivotal) the voter will shift from* t_2 *to* t_1 *when* p *becomes sufficiently small.*

2.1 Allowing for mistakes

Observation 1 suggests that NC motivation can be inferred from the sensitivity of simple choices between two alternatives – one of which plausibly maximizes individuals' utility when pivotal, the other plausibly maximizing a NC payoff – to variations in p. This idea underlies previous attempts to study NC voting in the lab. But suppose that individuals sometimes make mistakes which result in erratic voting. In particular, it seems natural to assume that mistakes are more likely when the resulting utility loss is small. A simple way to model this is to use a Luce-type choice model:

$$prob(i \text{ votes } t_j) = \frac{U_i(t_j)}{\sum_{t_k \in A_i} U_i(t_k)} \tag{3}$$

In this case, observed choices might respond to p even if voters have no NC motivation. Consider again the two-alternative case and suppose as before that $v_i(t_2) > v_i(t_1)$. Assume however that $\gamma_i = 0$. If voter i is pivotal, then $prob(i \text{ votes } t_1) = v_i(t_1)/\left(v_i(t_1) + v_i(t_2)\right) < 0.5$.[10] As p decreases the utility loss from voting for t_1 also decreases and voting for this option becomes more likely (in the limit, as p approaches

[9] Obviously, if $v_i(t_1) \geq v_i(t_2)$ then voter i always votes for t_1.

[10] More generally, from equations (2) and (3) we have:

$$prob(i \text{ votes } t_1) = \frac{pv_i(t_1) + (1-p)v(t_{-i})}{p\left(v_i(t_1) + v_i(t_2)\right) + 2(1-p)v(t_{-i})}.$$

This probability is decreasing in p.

0, the probability of voting for t_1 approaches 0.5). To sum up:

Observation 2 *Suppose* $A_i = \{t_1, t_2\}$, $\gamma_i = 0$ *and* $v_i(t_2) > v_i(t_1)$. *Then if individual choice is governed by equation (3), the probability of* i *voting for* t_1 *is higher the lower is* p.

Thus, an observed shift from one (plausibly consequentially-optimal) alternative to another (plausibly NC-optimal) alternative as a result of lower p may simply be due to individuals voting more erratically when the stakes are very low.

Consider however the case of three alternatives. Again, the probability of voting for a given alternative may respond to p even if there are no NC considerations. In this case, the probability of voting for *any* alternative tends to 1/3 as p approaches zero.[11]

Observation 3 *Suppose* $A_i = \{t_1, t_2, t_3\}$, $\gamma_i = 0$ *and individual choice is governed by equation (3). Then for any alternative* $t_j \in A_i$, *if* $prob(i \text{ votes } t_j) < 1/3$ *when* $p = 1$, *then* $prob(i \text{ votes } t_j)$ *is higher the lower is* p.

In other words, if what drives the response to reductions in p is the tendency to vote erratically as stakes become low, we should observe an increase in the likelihood of voting for *any* alternative that is rarely chosen under high p, and not just for the NC-optimal one.

Drawing on these observations, our empirical investigation seeks to examine whether a reduction in p causes some voters to shift from a consequentially-optimal alternative to a NC-optimal alternative, but not to other alternatives.

3 Experimental Design

Is there a causal effect of p on voting behavior due to NC motivations? In Appendix A we present some anecdotal evidence from US presidential elections that could be consistent with such an effect. However, credibly identifying this effect in survey or election data is a serious challenge. First is the issue of endogeneity: the likelihood of being pivotal in actual elections depends on how people vote, making it hard to infer the effect of p on voting behavior. An instrumental-variables approach seems unlikely to be

[11] In this case, equations (2) and (3) yield:

$$prob(i \text{ votes } t_j) = \frac{pv_i(t_j) + (1-p)v(t_{-i})}{p\left(v_i(t_1) + v_i(t_2) + v_i(t_3)\right) + 3(1-p)v(t_{-i})}.$$

If $prob(i \text{ votes } t_j) < 1/3$ when $p = 1$, then this probability is decreasing in p.

feasible since any variable that affects p may well affect voting behavior in ways other than through its effect on p. Second, even if we could identify a causal effect of p on voting decisions, the mechanism underlying this effect might not necessarily involve NC motivations. Voters may behave differently when they expect to be pivotal, even if they have no NC motivations (e.g. they may seek more information about the candidates). Incentives for strategic voting may also differ between narrow elections and landslides. And differences in the likelihood of being pivotal across states or districts can also affect the actions taken by politicians (in the US for instance, candidates allocate significantly more resources to "battleground states"). Finally, in national elections, p is typically extremely small for all voters.[12] Thus, even if we had a reliable measure of p and a method of isolating and disentangling its causal effects, there might simply not be enough meaningful variation in p to generate an identifiable effect.

Laboratory experiments, while sacrificing some external validity, offer the possibility of exogenously manipulating p while tightly controlling the actions and information available to voters, in order to study the basic mechanisms of NC voting.

In this section we detail our experimental setup and design. Advantages and limitations are discussed in section 3.1.

We conduct an experiment in which subjects vote over the allocation of a sum of money. Eighteen subjects participate in each session. Two are randomly assigned the role of "observers" and sixteen are designated "regular participants". The sixteen regulars are randomly assigned into four groups of four. Subjects do not know the roles of other subjects, nor do they know who else is in their group. Each group is endowed with a sum of NIS 200 (NIS is the Israeli currency; NIS 200 are worth slightly more than $50 at the time of the experiment) and subjects vote over how to allocate it between the group members. Each group holds a single election. Thus, a regular subject makes a single decision throughout the experimental session. An observer makes four decisions, one for each group, as detailed below. There is no interaction between subjects and their decisions are made simultaneously. All decisions are anonymous and payments are made in private.[13]

[12] Gelman et al. (2009) estimate that the highest probability (across states) that a single vote would determine the national election outcome in the 2008 US presidential elections was approximately 1 in 10 million. The average voter had an estimated p of 1 in 60 million.

[13] Some details about procedures. The experiment was conducted at a computer lab at The Hebrew University of Jerusalem in the spring and fall of 2009. Subjects were recruited from the pool of students at the University. Each subject participated in only one session and had no prior experience with our experimental setup. At the begining of a session each subject is randomly assigned to a cubicle with a computer screen and given written instructions. An administrator reads the instructions aloud before

We first describe the voting rules and then the alternatives subjects choose from.

The voting rules are as follows. Each group receives a fixed total number of ballots, divided between the group members and the observers. Each subject then records her vote for one of the available alternatives on her ballot(s). Subjects cannot split their vote, and the same decision is registered on all the ballots allocated to a given subject. After all subjects have cast their votes, the allocation of the NIS 200 between the group members is determined by a random draw of one ballot from the pool of ballots allocated to the group.

Experimental treatments vary in the total number of ballots and in the allocation of ballots between group members and observers. The four treatments are described in Table 1. In each treatment, all group members receive the same number of ballots. For example, in the first treatment (top row), each of the four group members receives 24 ballots out of 100, while each of the two observers receives two ballots. Overall, the experiment allows us to observe choices of group members when the probability that they will be pivotal is 24%, 2%, 0.1% or 0.01%. Notice that this design gives us substantial freedom in setting the probability of being pivotal. In particular, we are not constrained by the number of subjects it is feasible to recruit for an experiment.

[Table 1 about here]

Turning to the alternatives that subjects choose from, we use two conditions. In the first condition, a group member chooses between an equal division of the NIS 200 allocated to the group (NIS 50 per group member) and a division that gives her 70% of the pie (NIS 140) and each of the other group members NIS 20. In the second condition, we add a third alternative, which gives the decision maker NIS 140, but gives the other group members nothing.[14]

Observers choose from all the possible alternatives presented to the subjects (e.g. in the two-alternative condition: an equal division; a division that gives "participant

the experiment starts to make sure the rules of the experiment are public information. Subjects are then asked to fill out a quiz to verify their comprehension of the procedure (the quiz and sample instructions are in appendix B). A session lasts about an hour. All subjects receive a show-up fee of NIS 10 (approximately $2.5). In addition, group members receive their share of the NIS 200 as determined by the election outcome in their group. Observers receive a fixed payment of NIS 50 (plus the show-up fee). Subject anonymity is kept not only vis-à-vis other subjects but also vis-à-vis the experimenters: a specific subject's name cannot be linked with the decision she made during the experiment. Payments are made upon presentation of cards denoting the computer terminal a subject was assigned, and cannot be linked to any identifying personal information.

[14] In all treatments, the order in which the alternatives are presented is randomized across subjects.

x" NIS 140; a division that gives "participant y" NIS 140 and so on). Each observer makes four decisions – one for each group.

Table 2 summarizes the 4 by 2 design of the experiment, and shows the number of sessions held in each cell (for a total of 20 sessions with 360 subjects).

[Table 2 about here]

After all subjects complete their vote, the lotteries that determine the results of the vote for each group is performed. Before learning the result, subjects complete a questionnaire that includes demographics as well as questions measuring political participation and attitudes towards inequality and poverty. After completing the experiment, subjects are informed of the winning allocation in their group and the number of ballots that supported each proposed allocation. They are then asked to leave the room. Payments are made in private (see footnote 13 for details).

3.1 Discussion

Our experimental design has the following features.

1. The probability of being pivotal p is exogenously determined. This rules out the main problem with naturally occurring data, namely the endogeneity of p with respect to voting behavior. Furthermore, as mentioned above, p is not constrained by the number of participants. In principle, this allows an examination of the causal effect of p on voting behavior at any value of p. We can thus examine at some detail when (if at all) NC motivation becomes important and whether the response to p is monotonic. In practice, however, the parameters chosen for the experiment (p's from 0.0001 to 0.24) mean that we cannot identify NC motivations that are either too strong ($p^* > 0.24$) or too weak ($p^* < 0.0001$) to generate a shift in behavior in the domain we study.

   While several features of our setting are common to many democratic institutions (e.g. the secret ballots; the fact that several people are involved in the decision and that decisions are made only once), the procedure we use to aggregate the votes is obviously quite different from most real-world election mechanisms. However, our main goal is not to replicate actual elections but to isolate the effect of p on behavior. Using majority voting would make it difficult to precisely control

the probability of a particular subject being pivotal, not least due to the typical multiplicity of equilibria. Furthermore, given the manipulability of majority rule, variations in the decision environment could be associated with variations in incentives for strategic voting. Such incentives are absent from our procedure.[15] Finally, even if strategic voting could somehow be addressed, attempting to influence p by varying the size of the electorate or the required majority introduces confounding factors (a change in the size of the electorate might affect behavior through other channels).

2. In all experimental treatments we offer at least two alternatives. The first – an equal division of the NIS 200 – is plausibly perceived as the moral choice and hence maximizes the NC payoff (see point 4 below). The second alternative – 140 to self and 20 to each of the other group members – plausibly represents the allocation most subjects would choose when pivotal. Notice that this alternative does not represent a purely selfish division. The share given to the three other subjects (30%) is designed to be slightly higher than typical offers in two-person dictator games (around 25%).[16] In terms of our theoretical framework, the first alternative is meant to maximize the NC payoff $g(\cdot)$ and the second alternative to maximize utility of most subjects when they are pivotal.

3. Choices are made under two conditions: one with two alternatives and one with three alternatives. This feature follows directly from the theoretical discussion in section 2.1. It allows us to examine whether an increased propensity to vote for the "moral" option as p decreases is due to the effect of a NC motivation or to subjects making more mistakes when p is small. To this effect, in some treatments we add a third alternative – 140 to self and nothing to others – which seems morally inferior to the other alternatives in that it allocates zero to other group members, while it entails no gain in terms of one's own monetary payoff.

4. Choices made by the observers are impersonal and disinterested. Hence they provide a natural way to empirically gauge which of the alternatives is regarded as

[15] This point is quite general and pertains not only to majority rule (the Gibbard–Satterthwaite theorem applies to *any* non-random voting system).

[16] One might expect the share offered to three others (in a situation with high probability of the decision being implemented) to be significantly higher than that given to one other person. However, Fisman et al. (2007) report that the total amount given in a dictator game with giving to two others is not very different from giving to one other person. On average, subjects in their experiment give 25% of expenditures to two others, compared with 21% to one other.

morally superior in our experimental context (Harsanyi 1953).[17] Given that the money to be allocated is essentially a windfall unrelated to effort, we hypothesized that impartial observers would judge the equal division to be the moral alternative.

5. Group members make a single decision. This feature is crucial as it suppresses a multitude of confounding factors that can arise if subjects vote several times. These include repeated games effects; reciprocity and conformity effects that may be triggered by observing others' behavior in previous rounds; the possible desire to be consistent when making multiple decisions; and the possible overreaction to the experimental treatments when the same subject is being exposed to different treatments.[18]

6. The probability of being pivotal is easy to understand and requires no computation. This minimizes concerns about a possible discrepancy between perceived and actual probabilities (see e.g. Duffy and Tavits 2008).

7. Group size is constant in all treatments, as is the number of people participating in the elections. In other words, we can observe the effect of variations in p holding the size of the relevant populations – both the voting population and the affected population – constant.

8. Variations in p are not associated with intra-group differences: group members are all given the same number of ballots. This avoids various confounding effects that may stem from a differential treatment of group members. For example, having more power to decide the outcome may lead high-p subjects to behave more generously towards the relatively helpless low-p subjects.[19] Conversely, intra-group differences might generate resentment by group members who are given relatively little influence over the electoral outcome, leading perhaps to selfish (or even spiteful) behavior.

[17] In Harsanyi's view, a major feature of value judgments concerning social welfare is that they are "nonegoistic impersonal judgments of preference" (p. 434). This also echoes Adam Smith's notion of the impartial spectator.

[18] The last two effects may be present even if there is no feedback between decisions. A secondary implication of a single decision design is that the stakes in each decision are considerably higher than they would be if total payment to subjects were held fixed but subjects made many decisions.

[19] Camerer (2003) suggests a similar explanation for differences between dictator and ultimatum offers: Proposers in ultimatum games "expect Responders to stick up for themselves, whereas they behave more fair-mindedly in dictator games because Recipients cannot stick up for themselves" (p. 56).

Despite the within-group symmetry, our experiment might still be subject to the "responsibility-alleviation effect" which states that shifting responsibility for an outcome to an external authority dampens internal impulses toward honesty, loyalty, or generosity (Charness 2000). In our setting, a lower p shifts responsibility to observers. Thus, if this concern is valid, it would lead to *less* support for the equal division as p is reduced and hence would bias our estimates of the prevalence of NC motivations downward. One could hence address this concern by interpreting our estimates as lower bounds on, rather than point estimates of, the prevalence of NC motivations.

9. The information subjects have on the available alternatives is constant across p-treatments. In particular, it is not the case that when p is low, subjects tend to become less informed (see e.g. Caplan 2007). Also, since p is exogenous, it does not convey any information about other voters or about the alternatives (see e.g. Feddersen and Pesendorfer 1996).

10. Ballots are secret and we take great care to guarantee subject anonymity. Beyond ruling out social pressure and post-experiment exchanges, this suppresses possible interactions between p and social image concerns (Andreoni and Bernheim 2009).

11. Finally, we measure the political participation of our subjects – and in particular whether they have voted in actual elections. This allows us to associate behavior in the lab with "real-world" political behavior. Importantly, we can examine whether the results are sensitive to excluding from the analysis subjects who did not vote in the last elections despite being eligible.[20]

4 Results

360 subjects participated in the experiment. Table 3 reports descriptive statistics by p-treatments. There are 108 subjects in each of the two "extreme" treatments ($p = 24/100, 1/10,000$) and 72 in each of the intermediate treatments. All treatment groups consist of around 54 percent females and have roughly similar age distributions centered around 24 years, except that the 1/1,000 group is slightly older. Overall, 80% of the

[20] Students in Israel are typically several years above voting age (18) and parliamentary elections have been held three months before the first session. Assuming they are Israeli citizens, all of our subjects who did not vote in the elections were eligible to vote according to their age.

subjects are Jewish, 17% Muslim, and 2% Christian. The treatment groups are fairly similar in ethno-religious composition, although the 1/1,000 group has a somewhat higher proportion of Jewish subjects. The groups are generally very similar in terms of study major as well as measures of political participation and attitudes towards inequality and poverty (see table notes for details on these variables). It is noteworthy that 90% of the subjects have voted in actual elections.[21] Within each p-treatment, exactly one half of the subjects are assigned to the two-alternative condition and the other half to the three-alternative condition. There are no significant differences in any of the above demographics between the two-alternative and three-alternative treatment groups. In the analysis below we report results both with and without demographic controls.

[Table 3 about here]

We start with the behavior of the observers, depicted in Figure 1. For each probability of the observer being pivotal, the figure shows the proportion of votes for an equal division of the pie between group members, as well as the 95% confidence interval for the within-subject proportion. The main result is that, as expected, observers overwhelmingly vote for an equal division (87% of observer voting decisions in total). This lends support to our interpretation of the equal division as the morally-superior alternative.[22]

[Figure 1 about here]

It is noteworthy that observers' support for equal division does not decrease with p. If anything, observer behavior presents the opposite pattern: voting for an equal division when having a high likelihood of determining the outcome, but occasionally voting for other alternatives when p is low. In other words, it is not the case that lower p is automatically associated with voting for a more egalitarian allocation, irrespective of payoffs (e.g. that being powerless in itself causes people to prefer equality).[23] However,

[21] The turnout rate in the entire eligible population residing in Israel in the 2009 elections was estimated at 72.1%. See Central Bureau of Statistics press release: http://www.cbs.gov.il/hodaot2009n/24_09_024b.pdf

[22] Out of 160 observer decisions, 21 are for non-equal allocations: 19 for the (140,20) allocation and 2 for the (140,0) alternative. This is consistent with our conjecture (section 3.1 point 3) that the (140,0) alternative is morally inferior to the other two alternatives.

[23] An OLS regression of voting for an equal division on a dummy variable for the two lower-p treatments (46/100 and 2/100 combined) with standard errors clustered at the individual level yields a negative coefficient of -.1625 with p-*value*=0.095. Probit yields an identical marginal effect with p-*value*=0.043 (note fixed effects are not feasible as each observer only faces one p).

since we only have data on eight or twelve observers at each p, it is hard to make strong claims regarding their response to this variable. The main lesson here is the overall tendency of observers to choose the equal split.

We can now turn to the main results. Figure 2 shows the overall behavior of group members. For each probability of being pivotal, the figure depicts the proportion of subjects who voted for the equal division, with the 95% (binomial exact) confidence interval. The first thing to note is that even at the relatively high $p = 0.24$, 29% of the subjects choose an equal split. Moving from a 0.24 probability of being pivotal to a 0.02 probability has virtually no effect on the proportion of subjects voting for the equal division. However, once p becomes sufficiently small the proportion of subjects voting for an equal division rises to 42%. Interestingly, this proportion is the same whether p is 1/1,000 or 1/10,000.[24] Recalling Observation 1, this is consistent with 12.5% of our subject population having a NC motivation with a cutoff p^* between 0.02 and 0.001. Of course, the data do not allow us to speculate on whether the remaining 87.5% include subjects with NC motivations and a p^* outside the domain of p we study.

[Figure 2 about here]

Figure 3 breaks the data by whether the subject was presented with two or three alternatives. The figure reveals no consistent difference between the two conditions. This suggests that the tendency for equitable voting to increase as p decreases is not due to subjects tending to make more mistakes when p is low. Subjects do not deviate from the selfish alternative towards just any alternative. Rather, they move towards the moral alternative. Choice of the (140, 0) alternative is very rare and occurred only 4 times out of 160 in the treatments with three alternatives (two of which in the higher-p treatments).[25]

[Figure 3 about here]

Table 4 reports OLS estimates of the effect of p on the probability of voting for an equal division among group members.[26] The first two columns essentially replicate the

[24] Equality of proportions between the two lower-p treatments (combined) and the two higher-p treatments (combined) is rejected at the 2 percent level (two-sided z test). Equality of proportions between the two extreme treatments (24/100 vs. 1/10,000) is rejected at the 7 percent level. Due to fewer observations, we cannot reject equality between the two intermediate treatments (2/100 vs. 1/1000). Regression analysis is reported below.

[25] Choice of (140, 0) occured twice in the $p = 1/1,000$ treatment, once in the $p = 2/100$ and once in the $p = 24/100$ treatment.

[26] We report OLS estimates for ease of interpretation. Results from probit estimations are very similar. Given that there is no significant difference between the two lower-p and two higher-p treatments,

results seen in Figures 3 and 4. The first column includes only a dummy for the two lower-p treatments and the second column adds a dummy for the treatment with a third alternative. Consistent with Figure 2, a probability of being pivotal $p \leq 0.001$ increases the probability of voting for the moral option by 12.5 percentage points, relative to treatments with $p \geq 0.02$. And as suggested by Figure 3, presenting subjects with a third, morally inferior, alternative does not have any appreciable effect.

[Table 4 about here]

In the third column we add controls for demographics and political participation. As expected from the random assignment, the estimated effects of the experimental treatments are essentially unchanged.[27] Consistent with previous studies, economics and business students are more likely to vote for the alternative that maximizes one's own monetary payoff.[28] Perhaps more interesting, there seems to be some suggestive evidence that subjects who are more active politically are also more likely to vote for an equal share of the pie, but this association is rather weak.[29]

Columns (4) and (5) repeat the estimation for subjects in the three-alternatives treatments only. If the effect of p is due to subjects making more mistakes as p diminishes, we should expect an attenuated effect on the likelihood of voting for the equal option under three-alternatives. The results indicate that, if anything, the effect is larger.

Finally, this paper focuses on how people vote rather than on turnout decisions. Thus, our results might arguably be less informative if they are obtained from a population that does not in fact turn out to vote. In columns (6)-(8) we restrict our sample only to those voters who report that they in fact voted in elections. The results are

we present results with a single dummy variable for low p.

[27] We do not include attitudes towards inequality and poverty due to the risk of endogeneity. In practice, including these variables does not change the estimated effects of the lower-p treatments (standard errors are slighlty smaller in columns 3 and 5). Further, these variables are not significantly associated with voting for an equal division (Inequality is never significant while Poverty is significant only in column 5 (p-$value$=0.08) and only when not additionally controlling for inequality).

[28] See e.g. Marwell and Ames 1981; Frank et al. 1993; Klor and Shayo 2010. No significant effect is found for other majors.

[29] The weak association of revealed preference for equal division and political participation is consistent with results reported by Dawes et al. (2008). Dawes et al. find that subjects with a revealed preference for increasing total payoffs in a modified (Andreoni-Miller 2002) dictator game are more likely to participate in politics than are subjects with selfish preferences. However, subjects with a preference for equal payoffs are no more likely to participate in politics than are subjects with selfish preferences. In our setting, baseline voting for equal division might be due to either a consequentialist preference for equal payoffs or to a consequentialist altruistic regard for others (or both).

essentially the same as in the first three columns (point estimates of the effect of the p treatments are larger, but not statistically significantly so).

5 Conclusion

Does lower probability of being pivotal increase the likelihood of voting in accordance with NC considerations? Our results suggest the answer is yes. The prevalence of voting for an egalitarian allocation – the allocation chosen by disinterested observers – is 12.5 percentage points higher when the probability of being pivotal is sufficiently reduced. This result cannot be attributed to changes in the decision environment associated with variations in p, nor to subjects making more mistakes as p decreases.

Our experiment is not designed to identify all NC motivations. It does not identify non-egalitarian NC motivations, nor does it identify egalitarian NC motivations which are either too strong or too weak to generate a shift in behavior in the domain of p explored. It may thus underestimate the prevalence of NC motivations.

From a positive perspective, our results suggest that more research should be directed at characterizing NC motivations and understanding their effects in democratic processes. From a normative perspective, the presence of NC motivations provides a potential source of inferior equilibria, as some individuals may vote (for NC reasons) for parties or candidates they would not choose if pivotal. While the properties of various voting mechanisms have been extensively studied under the assumption of purely consequentialist motivations, we know rather little about their properties in the presence of NC motivations. We hope future research will fill these gaps.

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APPENDIX

A Voting for minor candidates

This appendix briefly documents one empirical example of voting behavior that, although somewhat exotic, is plausibly indicative of NC motivation.[30] In the 2008 American presidential elections it was pretty clear that either Barack Obama or John McCain would win the elections. Nevertheless, 1,960,153 voters cast their votes for other candidates (or indeed cast blank votes). It seems reasonable to conjecture that many of these voters did not think that their vote would affect the outcome of the election, and were motivated by other considerations (e.g. expressing displeasure with the political system or support for some specific cause). Figure A1 presents some evidence on this type of voting in American presidential elections. On the vertical axis we measure the share of votes in any given state cast for neither the Democratic nor the Republican candidate. On the horizontal axis we have the actual difference in vote shares between Democratic and Republican candidates.[31] Interestingly, and consistent with a NC interpretation of these votes, in most election years the proportion of votes cast for non-major candidates tends to be lower in states where the elections are closer. Obviously, for reasons discussed in the paper (p. 8), this relationship may well be spurious.

[Figure A1 about here]

[30] We are grateful to Robert Aumann for suggesting this example. The data used in this appendix are from the Office of the Clerk of the House of Represenative, available at: http://clerk.house.gov/member_info/electionInfo/index.html

[31] The exceptionally high proportion of votes cast for non-mainstream parties in 1992 and 1996 is largely due to Ross Perot. It is of course impossible to say whether Perot voting was predominantly NC in nature. In the 1992 election season some polls suggested a nonnegligible probability that Perot could win.

B Sample experiment instructions and quiz

[Translated from the Hebrew original]

An Experiment in Decision-Making

This is an experiment in decision-making. During the experiment you will make decisions and so will the other participants. Your decisions and the decisions of others will determine the payment you will receive according to the rules that will be explained later. This payment is **in addition** to the payment for participating in the experiment (NIS 10) that every participant receives.

You will be paid in cash at the end of the experiment exactly according to the rules.

The experiment will be conducted by a computer. All decisions you make will be performed by clicking appropriate commands.

You are asked to keep absolutely quiet during the experiment and not to talk with other participants. Please turn off your cellular phones. If you have a question of any kind, please raise your hand and one of the supervisors will approach you.

At this stage we wish to explain the rules that determine the payments in the experiment. We will start the experiment only when the rules are clear to everybody.

In this experiment there are 18 participants. Each participant was assigned randomly to a computer terminal. Please keep the card with the terminal number that was assigned to you. The payment at the end of the experiment will be made only upon presenting this card. Please verify again at this time that the card you have matches the terminal number.

At this stage the computer will select two participants who will serve as "observers". Each observer will receive at the end of the experiment a fixed amount of NIS 50 (in addition to the NIS 10 for participating in the experiment). The remaining sixteen participants will be divided into four groups of four participants each, and their payment will be determined according to the rules explained below.

The assignment of roles and into groups is made by the computer and is random. No participant can know who among the other participants belongs to his group, or who was assigned to be an observer.

All decisions are anonymous and confidential.

Please note: the data concerning the participants' decisions are kept only on the basis of the number of the computer terminal, so that even the experimenters cannot link the decisions made with the names of participants.

Decision-Making

Each one of the four groups is allocated NIS 200. This sum will be divided at the end of the experiment among the four participants of the group in accordance with their votes and the observers' votes as detailed below.

Allocating Ballots

Each group is allocated 100 "ballots". These ballots are divided among the four group members and the two observers as follows

- Each group member gets 2 ballots.
- Each observer gets 46 ballots.

Dividing the NIS 200 Among Group members

On the computer screen of each group member there will appear two options of dividing the NIS 200. Each option will specify exactly how much money the same group member will receive at the end of the experiment and how much money the other participants in his group will receive. Each of the group members will be asked to choose one of the two options of allocating the money.

On the computer screen of each of the observers will be presented **all** the options of dividing the money that were offered to the four members of the group. Each observer will be asked to choose one option.

Please note: as a group member you will only be asked to make a single choice. This choice will be entered on all the ballots you were allocated.

After all observers and group members have finished voting, the computer will draw **one ballot** from the one hundred ballots of the group members and the observers. The draw will be random: each one of the hundred ballots has an equal chance of being selected.

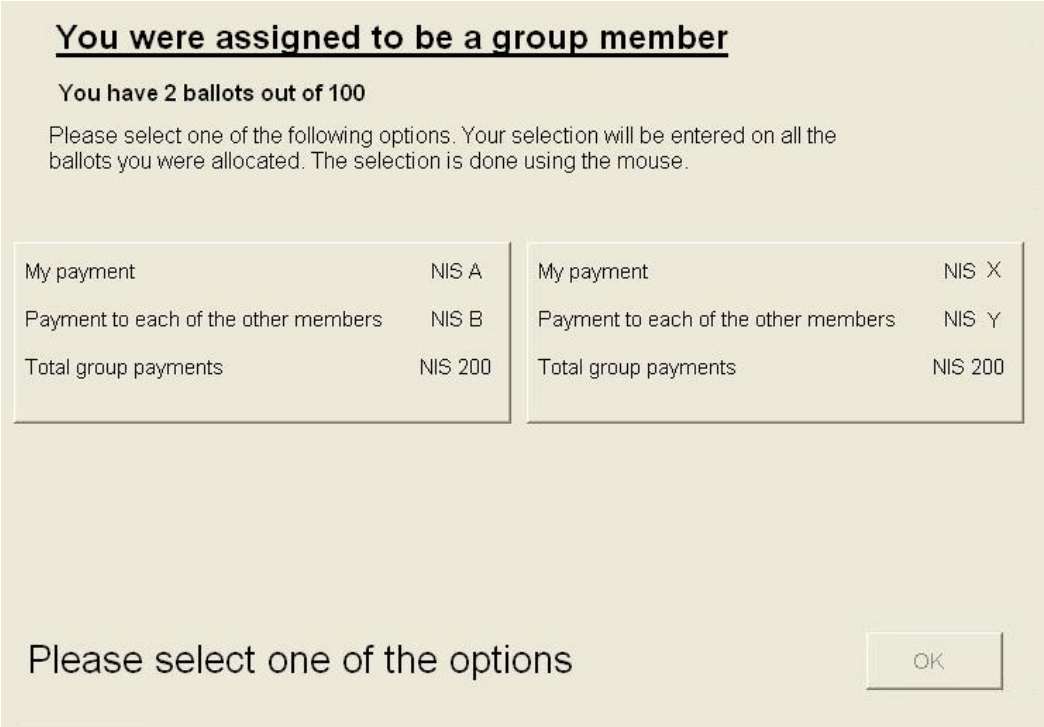
The division of the money that was entered on the selected ballot will exclusively determine the way the money is divided at the end of the experiment. The ballots that have not been selected have no effect on the final division.

At the end of the experiment all participants will be informed of the winning allocation and also of the numbers of ballots that supported each of the options (including the non-winning ones).

Please note: a participant who was assigned to one of the four groups will make during the entire experiment **only one decision**: how to allocate the money between him and the other members of his group. In contrast, a participant who was assigned to be an observer will make **four** decisions: one for each of the four groups.

Examples

Example A: Suppose that you were assigned to one of the groups. The screen that describes your choices will look like this:



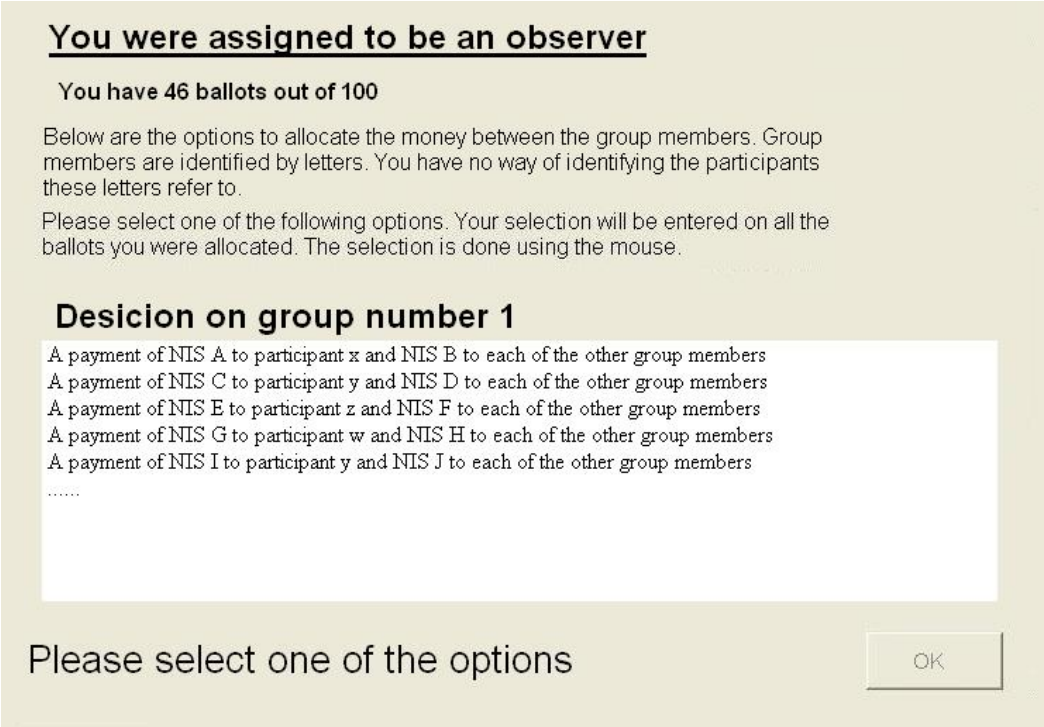
(In the experiment itself, numbers will be presented instead of the letters X, Y, A, B).

If you select the allocation presented on the right (NIS X to you and Y to each of the other participants) then there are at least 2 ballots (out of 100) with this allocation entered on them. The other 98 ballots have the allocations chosen by the three other group members and the two observers. (Note: there may be more than 2 ballots with the allocation you chose if another participant also chose an identical allocation).
After all members of your group and the two observers made their decisions, the computer will draw one ballot out of the 100 ballots. If your ballot was selected then at the end of the experiment you will receive NIS X and the other participants will receive NIS Y.

Similarly, if you selected the allocation presented on the left (NIS A to you and B to each of the other participants), then there are at least 2 ballots with this allocation entered on them. If one of these ballots is selected then at the end of the experiment you will get NIS A and the other participants in your group will get NIS B each.

We wish to emphasize: you will make the decision only once. After you confirmed your decision you will not be able to change it and there will be no more decisions to make.

Example B: Suppose that you were assigned to be an observer. In this case you will be asked to make a decision for each one of the four groups, one after the other. Accordingly you will see four decision screens, one for each group.
For example, the screen pertaining to group number 1 will look like this:



[In the experiment itself there will be numbers instead of letters (A,B,C…) and the number of options may change].

The ballots with your decision will be added to the ballots of group no. 1, and the lottery will take place in accordance with the rules explained above.

As mentioned earlier, the payment to observers does not depend on their decisions and it is fixed in advance (NIS 50 + a participation fee of NIS 10).

After the lottery you will be asked to fill out a questionnaire and this will end the experiment.

At the end of the experiment each participant will see on his screen the detailed results of the vote. That is, each participant will see the winning allocation as well as the number of ballots that supported each one of the other allocations (including those that did not win).

As stated above, at the end of the experiment we will pay you in cash the complete sum according to the above rules.

Participants who need a signature confirming their participation will get it in addition to the above payments.

Payments will be made **discreetly** at the end of the experiment and you have no obligation to tell anyone how much you earned in the experiment.

As mentioned above, all decisions are anonymous and discreet. The data concerning the participants' decisions are kept only on the basis of the number of the computer terminal so that even the experimenters cannot link any of the decisions made with the names of participants.

Terminal no. _____

Questionnaire

In order to verify that you understand the instructions, please answer the following questions.

When you have finished, raise your hand and one of the supervisors will approach you.

1) How many members are there in each group? _____

2) What is the amount of money that is allocated among members of the group?_____

3) Assume that Itamar was assigned to one of the groups.

   How many ballots does Itamar have? _____

   How many ballots participate in determining the allocation of the money among the members of the group? _____

4) How is the ballot that determines the actual allocation of the money selected?

   _____

Table 1: Distribution of Ballots

Total	Group member	Observer
100	24	2
100	2	46
1,000	1	498
10,000	1	4998

Note: Four group members and two observers vote over the allocation within each group.

Table 2: Experimental Design

		Alternatives (self, others)	
		t_1 = *(50, 50)* t_2 = *(140, 20)*	t_1 = *(50, 50)* t_2 = *(140, 20)* t_3 = *(140, 0)*
Probability of group member being pivotal	24/100	3	3
	2/100	2	2
	1/1,000	2	2
	1/10,000	3	3

Note: The table shows the number of sessions in each treatment. There are eighteen subjects per session: sixteen group members and two observers. Alternative *(x,y)* represents NIS x to the group member making the decision and NIS *y* to each of the other group members.

Table 3: Descriptive Statistics

		Differences by *p*-treatment			
	24/100 mean	2/100 vs. 24/100	1/1000 vs. 24/100	1/10000 vs. 24/100	F-stat (all=24/100)
	(1)	(2)	(3)	(4)	(5)
Female	0.537	-0.009	-0.023	0.037	0.246
	{0.501}	[0.076]	[0.076]	[0.068]	(0.864)
Age	23.759	0.741	1.102	-0.500	3.928
	{2.647}	[0.518]	[0.518]**	[0.464]	(0.009)
Jewish	0.787	0.060	0.102	-0.074	3.275
	{0.411}	[0.061]	[0.061]*	[0.054]	(0.021)
At least one major in:					
Humanities	0.380	-0.019	-0.005	0.009	0.048
	{0.488}	[0.074]	[0.074]	[0.066]	(0.986)
Economics, Accounting, Business	0.398	0.005	-0.023	0.065	0.552
	{0.492}	[0.075]	[0.075]	[0.067]	(0.647)
Social Sciences (w/o econ)	0.352	0.009	0.009	0.000	0.011
	{0.480}	[0.073]	[0.073]	[0.066]	(0.998)
Voted in elections	0.907	-0.005	-0.005	-0.046	0.498
	{0.291}	[0.047]	[0.047]	[0.042]	(0.684)
Political activities	2.213	0.009	0.023	-0.037	0.046
	{1.192}	[0.175]	[0.175]	[0.157]	(0.987)
Attitudes towards:					
Inequality	4.287	0.130	0.255	0.204	0.223
	{2.153}	[0.348]	[0.348]	[0.311]	(0.880)
Helping the poor	3.426	0.199	0.255	0.056	0.308
	{1.925}	[0.305]	[0.305]	[0.273]	(0.820)
N	108	72	72	108	

Notes: Standard deviations are shown in braces in column 1. Standard errors are reported in brackets in columns 2–4. p-values for F-tests are reported in parentheses in column 5. The last row shows total number of subjects in each treatment.

Study-major dummies = 1 if at least one of the participant's study fields is in the indicated category. Since most Israeli undergraduate students major in two fields of study, a subject may be counted in up to two of the study major categories. Subjects who are counted in none of the reported three categories include nonstudents as well as students in law, the life sciences, the natural sciences, and other small groups.

Political activities is the number of political activities the subject participated in from the following five: voting in elections, signing a petition, joining in boycotts, attending lawful demonstrations, joining unofficial strikes.

Attitude items are adapted from the World Value Survey (Inequality) and the General Social Survey (Helping the poor). Both ask subjects how they would place their views on a scale from 1 to 10. For the Inequality item, 1 = "Incomes in Israel should be more equal" and 10 = "We need larger income differences as incentives for individual effort". For the Helping the poor item, 1 = "The government should do everything possible to improve the standard of living of all the poor in Israel" and 10 = "Improving the standard of living of the poor is not the government's responsibility: people should take care of themselves".

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Table 4: The effect of *p* on voting behavior

Dependent variable = voted for equal division

	All			Three alternatives		Excluding non-voters		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low *p* treatments (1/1000 or 1/10000)	0.125***	0.125***	0.126***	0.138**	0.139**	0.135***	0.135***	0.133**
	(0.041)	(0.040)	(0.043)	(0.050)	(0.053)	(0.043)	(0.043)	(0.047)
Three alternatives		-0.025	-0.010				-0.016	0.001
		(0.040)	(0.043)				(0.043)	(0.048)
Female			0.034		0.039			0.041
			(0.054)		(0.058)			(0.055)
Age			-0.002		0.014			-0.001
			(0.007)		(0.012)			(0.008)
Jewish			-0.073		-0.094			-0.043
			(0.063)		(0.106)			(0.090)
Economics, accounting, business			-0.134***		-0.102**			-0.124**
			(0.039)		(0.045)			(0.048)
Political activities			0.055*		0.040			0.051
			(0.028)		(0.047)			(0.033)
Constant	0.294***	0.306***	0.321	0.275***	-0.038	0.283***	0.291***	0.254
	(0.026)	(0.035)	(0.210)	(0.030)	(0.323)	(0.025)	(0.034)	(0.217)
Observations	320	320	320	160	160	284	284	284
R^2	0.017	0.018	0.059	0.021	0.049	0.020	0.020	0.056

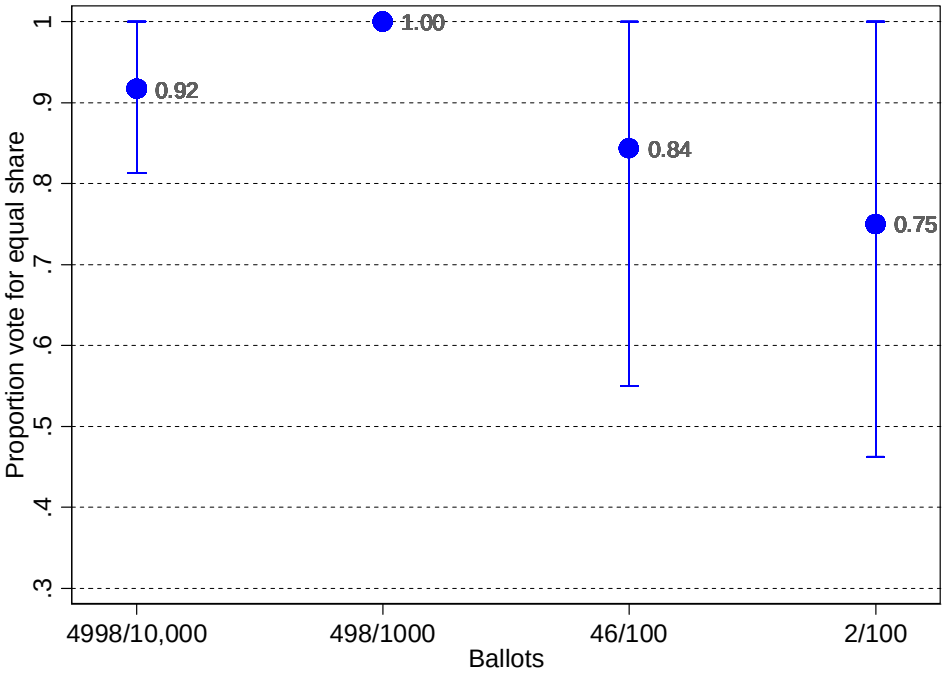
Notes: OLS, Standard errors clustered by session in parentheses. Dependant variable is whether or not the subject voted for the (50,50) alternative. Columns (1)-(3) report results using all group members. Columns (4)-(5) report results from the three-alternative treatments only. Columns (6)-(8) report results using only group member who have voted in actual elections. See notes to Table 3 for details on the demographic variables.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

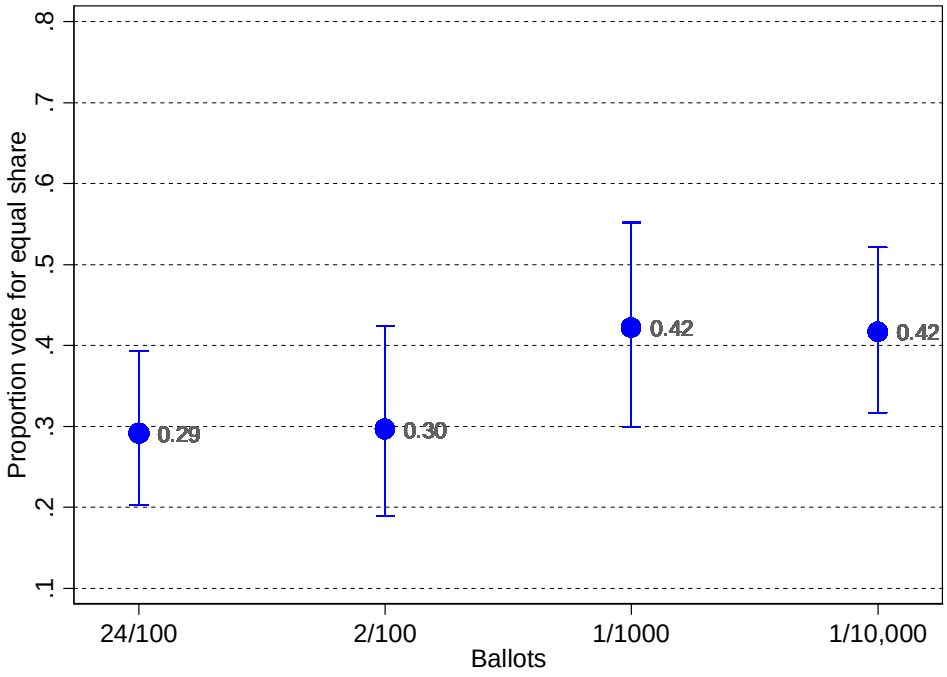
* Significant at the 10 percent level.

Figure 1: Observers' votes for equal division



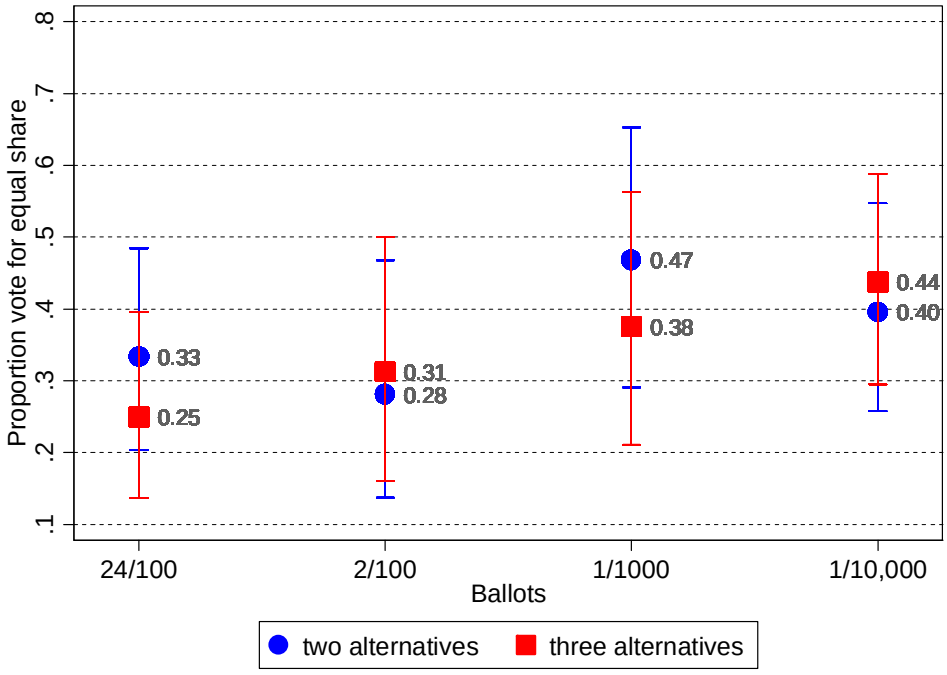
Note: Means of within-subject means. Based on 40 subjects, each making 4 decisions. Capped ranges indicate 95 percent confidence intervals (truncated at 1).

Figure 2: Group members' votes for equal division



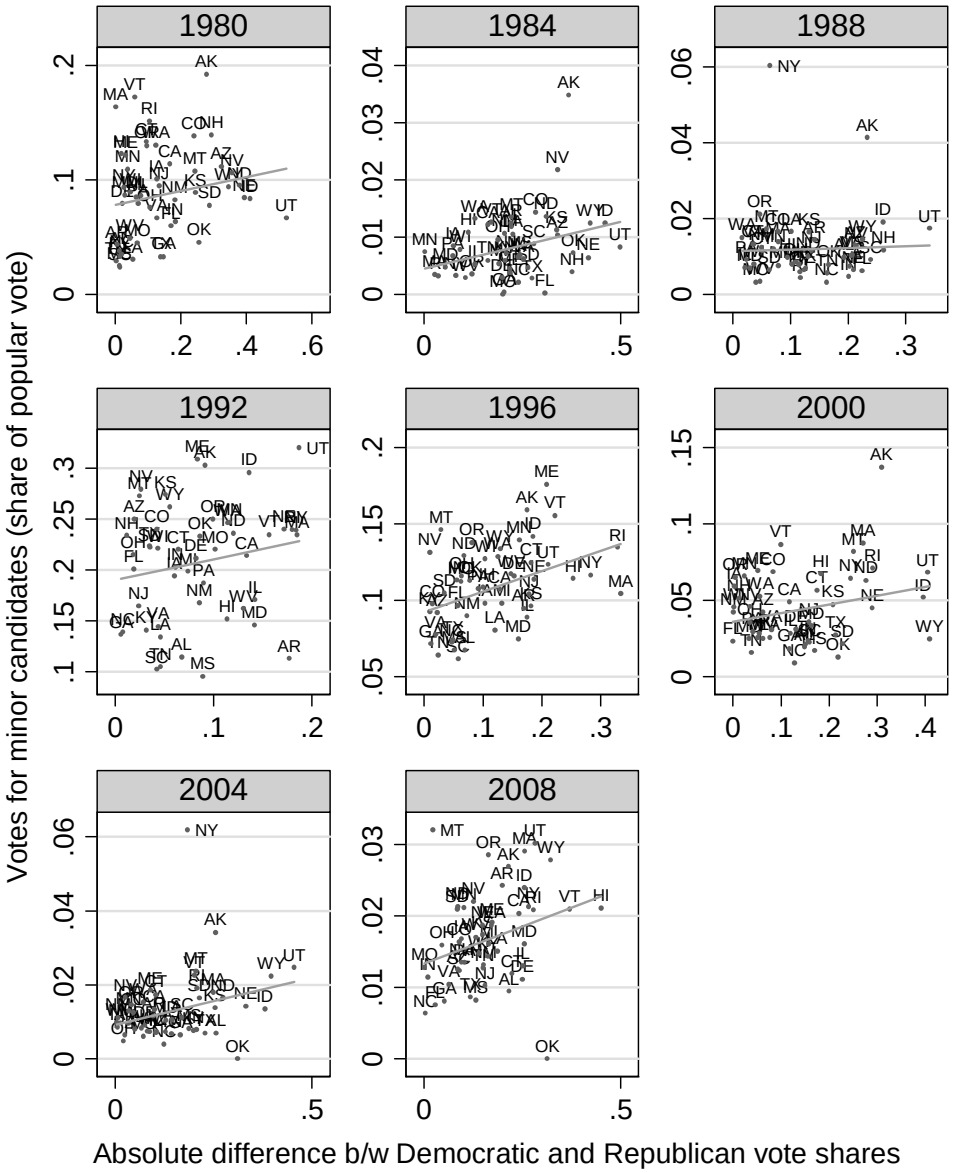
Note: Based on 320 observations. Capped ranges indicate 95 percent confidence intervals.

Figure 3: Group members' votes by number of alternatives



Note: Based on 320 observations. Capped ranges indicate 95 percent confidence intervals.

Figure A1: Votes for non-major candidates, U.S. presidential elections



Note: The figure shows the share of the popular vote cast for neither the Democratic nor the Republican candidate. This includes votes for minor candidates as well as blank votes.