University of Cyprus
Department of Economics

Working Paper 02-2022

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February 1, 2022

We test in the laboratory four mechanisms whereby group identification might affect redistribution in representative democracies. For voters, group identification can give rise to a preference for own-group payoffs, for electing an own-group candidate, and could be used to assess candidate-sincerity. For candidates, identity might affect the optimal campaign platform. There is evidence to support all four. The influence of own-group pay-offs has been studied before, but the other mechanisms have not. These new mechanisms combine to make redistribution depend on a hitherto unrecognized factor: the political representation of the minority group.

Key Words: Identity, Inequality, Redistribution, Minority Representation, Representative Democracy, Voting Experiment

JEL Codes: C91, D63, D72, D90

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1 Introduction

How do different group identifications, like those of race and ethnicity within an electorate, affect the extent of redistribution in a representative democracy? The question is important because there is a puzzling variation in the degree of redistribution across rich countries and one possible explanation turns on the varying influence of such group affiliations on attitudes to redistribution in more or less heterogeneous societies.[1] For example, the poor in the US may be less inclined to vote for redistribution, because they are racially divided, as compared, say, to the poor in Sweden who are less differentiated in this and other respects.[2] There is some observational data to support this conjecture (see Alesina et al. 1999; Alesina and Glaeser 2004; Alesina, Michalopoulos and Papaioannou 2016; Dahlberg et al. 2012; Luttmer 2001; Rueda 2018). As with all such evidence, however, it is difficult to establish causation and identify the mechanisms of influence. For this reason, we use an experiment in this paper to test whether such group identifications influence through four possible mechanisms the extent of redistribution in a representative democracy.

There is one analogous, *direct* democracy experiment on this question. It finds the poor are less willing to vote for redistribution when they are split in this way than when they are not (see Klor and Shayo, 2010). There is also a conjoint choice experiment on income taxation choices that finds some evidence that White racial resentment weakens progressive preferences (see Ballard-Rose et al, 2017). In contrast, we examine the question with an experiment in a representative democracy setting. This difference is

[1] The puzzle arises because, following Meltzer and Richard (1981), we might expect that greater inequality increases the incentives for the poor to vote for redistribution and so there is likely to be more redistribution in more unequal societies. However, this is typically not revealed in cross country evidence (e.g. see Scheidel, 2017, or in time series evidence on how countries respond to increasing inequality (e.g. see OECD, 2017).

[2] For example, poor whites may be less inclined to vote for redistribution because they identify with rich whites who will be harmed by any redistribution.

important for two reasons. First, the type of democratic decision making that determines redistribution policies in rich countries is representative and not direct. Second, representative democracies create greater scope for the influence of group identification in determining redistribution outcomes. In particular, there are three distinct new mechanisms of group influence on redistribution outcomes that we test; as well as a fourth that is shared with direct democracies.

The first two new mechanisms relate to understanding how the identity of a candidate can contribute symbolic benefits to voters in a representative democracy. It is known, for example, from Bassi et al (2011) in a representative-like democracy experiment that the group identity of a candidate affects voting. Likewise, there are conjoint choice experiments that reach the same conclusion: i.e. sharing a group identity with a candidate yields a symbolic benefit for voters, see Harden and Clark, 2016 and Shockley and Gengkler, 2020. These findings are broadly consistent with social identity theory but little is known about the precise character of these symbolic benefits. We test for two. One is that voters have a preference for own group victory in electoral contests. The other is that voters value candidate sincerity (see Kartik and McAfee, 2007) and they infer something about a candidates true preferences (and hence their sincerity) from their group affiliation.

The third new mechanism arises because, in a representative democracy, redistribution outcomes are generated through the interaction between the demand for and supply of redistribution policies and the supply side emanates from a simultaneous strategic interaction between candidates and also voters: i.e. an electoral contest. In contrast, in direct democracy or conjoint choice experiments, the supply of redistribution policies is, in effect, determined by the experimenter. This is an important difference and the extant experiments in this field miss in effect one side of the potential influence of group identities on redistribution outcomes. The influence of group identities on the supply of redistribution policies in a representative democracy has been studied theoretically (e.g. see Matakos and Xefteris, 2017), but we are the first to test these theoretical predictions.

The evidence on the operation of the three new mechanisms in our experiment combines to yield the conclusion that the extent of minority group political representation among the electoral candidates critically influences redistribution outcomes. This is a wholly new insight.

To see why this is so, it should be noted that there is already considerable evidence that the group identities of legislators are associated with, for example, the generosity in the welfare programmes that emerge from their legislatures (e.g. see Preuhs, 2006, and Clark, 2019, although this association appears to be weaker in racialized contexts). There is also evidence that when there are multiple group identities (e.g. race and gender), this apparent effect from political representation is not simply additive (see Reingold and Smith, 2012). However, the mechanisms responsible in these associations are not always clear. Where they are, they turn on the actual influence that representatives have when contributing to the legislative process through floor speeches, amendments, voting, etc (e.g. see, Bratton Haynie, 1999, Tate, 2003, Preuhs, 2006). Our mechanisms of influence are entirely different. There is no scope in our experiment for the winning candidate to engage in this kind of legislative politics. Our mechanisms combine to affect the agenda of redistribution that emerges from the electoral contest itself (and the strategic elements involved in it), as distinct from anything that might arise in the legislative process of turning the victorious electoral policy platforms into actual policies. In this respect, the character of our insight is breaking new ground. It is also novel because it does not relate to what is referred to as descriptive group representation among legislators in this literature. It refers to the representation of groups among the candidates in an election (and not just the victors who take up positions in the legislature).

Our experiment thus contributes to three literatures. First, we contribute to the literature, mentioned above, that is concerned with how group identification influences redistribution outcomes under democracy. This literature has been primed by the growth of inequality in many rich countries and the fact that group identifications appear to be more salient in the politics of rich countries. For example, the BREXIT vote in the UK, the Trump 2016 victory in the US and the Five Star Movement in Italy have all been associated, albeit sometimes controversially, with the rise of "identity politics" (see e.g. Fukuyama 2018; Gennaioli and Tabellini 2018).[3]

Second, we fill a gap in the experimental literature. There is experimental evidence on how group membership affects behaviour in many domains (e.g. in public goods game, see Chen and Li, 2009; and in trust games, see Hargreaves Heap and Zizzo, 2009), but it has rarely been examined experimentally in relation to elections. To our knowledge, Klor and Shayo (2010) is the only direct democracy experiment to consider voting on redistribution explicitly when there is scope for group identification. In addition, there are conjoint choice experiments that also explore voters preferences. But neither type of experiment addresses what happens in a representative democracy and, as we have suggested, there are potentially significant additional mechanisms in play in a representative democracy.[4]

Our final contribution comes from our conclusion that minority political representation matters for inequality outcomes for reasons that are wholly new to the literature on

[3]For example, Gennaioli and Tabellini (2018) show that if globalization splits society around a nationalist-cosmopolitan cleavage instead of the traditional left-right, this can dampen demand for redistribution despite potential increases in income inequality.

[4]Bassi et al (2011) as noted also study the influence of identity in an experiment where choices are understood in terms of voting but their results, while interesting, are not directly applicable to the standard redistribution problem that we have in mind. In part this is because the structure of the decision problem builds-in a preference for own candidates winning through the subjects pay-offs. But it is also because the structure of pay-offs turns the election/decision problem into a coordination game with multiple equilibria. Elections do not typically have this property in terms of material pay-offs, but the real difficulty is that, in the absence of an accepted theory of equilibrium selection, departure from equilibrium behavior is not well defined in these circumstances.

political representation. This is important in its own right and highlights in a new way the importance of the recent literature concerned with understanding the selection/origins of political candidates in democracies (e.g. see. Dal Bo et al, 2017).

Our experiment is not only original in identifying and testing for the three new mechanisms of group influence in a representative democracy, it is also designed with an interesting internal robustness check. We conduct our analysis under three different trade-offs between efficiency and equity: a) the traditional negative one (where total income falls as it becomes more equal); b) no trade-off (as is the case in Klor and Shayo, 2010); and c) a positive trade-off whereby more equality boosts total income as is currently envisaged by the OECD and IMF (OECD, 2015, Ostry et al., 2014). This enables us to test for the robustness of our results across different equity-efficiency trade-offs. It also provides an interesting insight into how redistribution outcomes might change if the OECD and IMF are proved right about the new shape of this trade-off. In our experiment, such a change in the efficiency-equity trade-off has a smaller effect, relative to the influence of group identities, in a representative as compared with a direct democracy.

In the next section, we develop the theory from which we derive the hypotheses we wish to test. Section 3 explains our experimental design. Sections 4 gives and discusses the results and Section 5 concludes the paper.

2 Background and Hypotheses

The basic idea that we wish to unpack formally and test in a representative democracy experiment has a long history. It is a version of the enduring political maxim Divide et Impera (e.g. see Machiavelli 1520; Madison 1787; Kant 1795). The practical politics of running an empire seems almost always to rely upon this maxim (e.g. see Tharoor 2017); and it has specifically been connected to the growth of inequality in the US through the emergence of dog whistle politics around race (see Haney Lopez 2014). The idea behind divide and rule is that a majority along one dimension (e.g. those who would gain from redistribution) may fail to secure their material interests over the minority (e.g. those who would lose from redistribution) when the majority is itself divided along some other dimension (e.g. race in the US; see also Matakos and Xefteris 2017, 2020).

To examine theoretically the mechanisms through which such criss-crossing of economic status with group identification might affect redistribution decisions, we assume that individual voters belong to one of two groups. There is a numerically dominant, majority group (labelled Green in our experiment) and a minority group (labelled Yellow). Individuals are endowed with a high (=100 points) or a low (=50 points) endowment: i.e. they are either rich or poor in the status quo. The majority group has a mix of rich and poor members while the minority group only has poor members. So, the majority group is on average richer than the minority group. We further assume that each individual has a utility function of the general form given by (1) that they seek to maximise when deciding how to vote in two-person/candidate electoral contests where candidates, identified by their group membership, make (re)distribution proposals for the allocation of the experimental points, possibly away from their endowment levels. That is:

$$U_i = f(P_i, In_j, Ef_j, OGP_j, OGV_j, CS_j) \tag{1}$$

where i refers to the individual and j refers to the redistributive policy proposal.

We assume in (1) that an individual potentially assesses each proposal according to how it affects two types of preferences. First, there are preferences that are independent of any group consideration: there is his or her individual pay-offs (P) and, in so far as the individual has any of the usual social preferences over outcomes, like inequality aversion

(In) and efficiency (Ef), a proposal will be judged by these lights too. Second, there are preferences that depend on group identities: OGP, OGV, CS. We now explain and note how each of these group related preferences generates an hypothesis with respect to voting behaviour.

1. ***OGP = Own Group Pay-off***

People may identify with their group through a concern for own group pay-offs and so judge any proposal, in part, by how it influences OGP. There is some evidence of this motive (e.g. see Coate and Conlin, 2004); and this is, in effect, the mechanism in our framework that is the direct analogue to what Klor and Shayo (2010) find in their direct democracy experiment. In the absence of group identities, OGP cannot motivate. Therefore, when there are group identities and in so far as people are motivated by OGP, they now have a new reason for preferring outcomes that yield a higher OGP over those that yield less than they did when there were no identities. H1 follows.

Hypothesis 1 (OGP): Ceteris paribus, the introduction of Green/Yellow identification moves subjects decisions towards outcomes with higher OGP relative to the choices made when there are no group identities.

Remark 1: OGP depends on the actual distribution proposals and not on who is making them. Thus, for the same proposals OGP varies in the same way whether these proposals arise in a direct democracy or a representative one. Further, for any two given redistributive proposals in a representative democracy, the OGP of each will be same whether they are proposed in Green-Green (GG) or Yellow-Yellow (YY) or Green-Yellow

(GY) electoral contests.

2. ***OGV = Own Group Victory***

People may value an own group member winning the election in a representative democracy and so they could be more inclined to vote for a redistribution policy when it is made by a member of their own group rather than a member of the other group in GY contests. There is some evidence that people do like winning in this sense (e.g. in contest games it is often suggested that people may overinvest in contests because they like winning and such over investment seems to be even larger in contests between groups, see Abbink et al., 2010).

Remark 2: OGV will not arise in a direct democracy because there are no candidates. Further, OGV only affects voting behaviour in electoral contests between candidates in representative democracy that come from different groups (i.e. GY contests). This is because in both GG and YY electoral contests, voting for one candidate or the other does not affect the group identity of the winner: its always a Green or Yellow respectively in these two types of contests. This is the basis for H2.

Hypothesis 2 (OGV): For any pair of proposals in GY contests, i) Greens vote more for a proposal when made by a Green than when the same proposal is made by a Yellow and ii) Yellows vote more for a proposal when made by a Yellow than when the same proposal is made by a Green.

3. ***CS = Candidate Sincerity***

It has been argued that the sincerity of a candidate is positively valued by voters (e.g. see Kartik and McAfee, 2007). To determine whether a candidate is sincere, voters have to answer the following question: does the candidate's proposal reflect their true preferences (or, the preferences of the constituency they are supposed to represent), rather than some opportunistic electoral calculation?

Of course, it is always difficult to know what a person's true preferences are in this sense. But, there is evidence that voters sometimes take demographic group identities, like race and gender, as clues in low information elections as to a candidates preferences (e.g. see Mcdermott, 1998). In our experiment, for example, conditional on the endowments of candidates being inferable, there is an aspect of the candidates preferences that can be inferred: their own selfish interest. In particular, the selfish interest of candidates will incline rich candidates to be less redistributive than poor ones. Thus, it might be thought contests between two rich candidates, on grounds of "representativeness" (see Kahneman and Tversky, 1974), that the candidate proposing the least redistribution is sincere whereas as the one proposing more redistribution is not. The candidate proposing less is, as it were, acting sincerely because this accords with the selfish preferences of a rich person. The converse would be the case for elections between poor candidates. The candidate proposing the most redistribution "represents" better the known (selfish) preferences of the poor for redistribution in such contests and so they are regarded on "representative" grounds as being sincere (and the candidate proposing less is not).

This is important because while voters do not directly observe the endowment of the candidates, they know their group identity and Greens are known to be richer on average than Yellows. Thus, GG contests involve on average the relatively rich, while YY contests involve the relatively poor on average. On this account, this means the candidate proposing the least redistribution in GG contests will attract the CS vote; whereas in YY contests, CS attaches to the candidate proposing the most redistribution.[5]

Remark 3: CS will not arise in direct democracies as there are no candidates. Further, CS only affects voting (if at all) in a representative democracy in GG and YY elections. It will not affect voting in GY elections. This is because if Green proposes less redistribution than the Yellow, then both are acting in accordance with what is the average selfish interest of members of their respective groups. So, CS attaches to both candidates and provides no reason for choosing between them. If on the other hand, Green proposes more redistribution than the Yellow, then both are acting against what is in the average selfish interest of members of their respective groups. Neither candidate, therefore, has a sincerity advantage and so, again, the CS does not provide a reason for choosing between them. H3 follows.

Hypothesis 3 (CS): For any given pair of redistribution proposals, voters prefer less redistribution in GG contests than when the same proposals are made in YY contests.

Remarks 1, 2 and 3 are important for the experimental design to test H1, H2 and H3. In relation to H1, Remark 1 means OGP is the same for any proposal whether made in direct and representative democracy setting. Remarks 2 and 3 imply that, unlike direct democracy, OGV and CS can also influence voting in GY and GG/YY representative democracy elections respectively. Thus, to test for the distinct influence of OGP in all

[5] Indeed, the candidates' sincere preferences might not depend solely on their self-interest, but also on the interests of the other citizens they identify with (group-loyalty). Notice that if group-loyalty is strong, then sincere candidates in GG contests would still propose on average lower redistribution than sincere candidates in YY races. Hence, the prediction that the candidate proposing the least (most) redistribution in GG (YY) contests will attract the CS vote is robust to alternative reasonable specifications of candidates' motives.

types of electoral contests, we focus on distribution decisions where there are no candidates (because OGV and CS cannot arise).

In relation to H2 and H3, Remark 1 means that while OGP could affect any pair of redistribution proposals in group identity contests, it is not an influence that varies with the group identity of the proposers. Thus it is not a reason for people to vote differently, for any given pair of redistribution proposals, depending on whether they are made in a GG, or YY or GY electoral contests. Remarks 2 and 3 mean that OGV only arises when assessing proposals in GY contests and CS only arises as a consideration in GG and YY contests. Thus, we can test for OGV in GY contests because if people do vote differently between any given pair of redistribution proposals depending on the identity of the persons making the proposal, it cannot be because another group related motivation OGP or CS is a source for distinguishing between the proposals of the candidates in these election contests. In the same way, we can test for the influence of CS voting between any pair of proposals when they surface in GG as compared with YY election contests because neither OGP nor OGV should affect the choice between any pair of redistribution proposals in these contests. The former because OGP does not depend on the identity of the proposer and the latter because OGV only arises in GY elections.

4. ***CR = Candidate Responsiveness***

We have two further hypotheses that we test. They concern the candidates choice of redistribution proposals and they test whether candidates adjust their redistributive proposals to the voting differences identified in H2 and H3 when there are group identities. H4 is based on H3 and predicts that strategic candidates will be responsive to the incentives provided by voters' preferences for CS, and propose less redistributive policies in GG than in YY elections.

Hypothesis 4 (CR-W): Candidates propose less redistribution in GG elections than YY ones.

While a potential confirmation of H4 would seem at first sight as strong evidence for CR, this need not be actually so. Indeed, the hypothesized differences in GG and YY contests might arise not only due to strategic behavior, but also due to candidates selecting the policy platforms that they sincerely prefer (see discussion in CS). Hence, while we test H4 as it follows the CR argument, evidence in line with H4 does not strongly support CR. This is why we call it CR-W: the weak test for CR.

To properly test for CR one has to compare the behavior of the Greens (Yellows) in GG (YY) and GY contests. If the differences in GG and YY contests are not driven by strategic considerations, then the Greens (Yellows) should behave similarly in GG (YY) and GY races.

H5 relates precisely to candidate responsiveness in GY elections and it comes from Matakos and Xefteris (2017). Consider first how the incentives for a Green candidate to be more redistributive changes when shifting from a GG contest to a GY one. We know that OGP will be the same across the contests as will the evaluation of selfish pay-offs. So any increase in redistribution will be attractive to the poor on selfish grounds and unattractive to Green voters because it lowers OGPs to the same degree in GG as GY. The change in the incentive to redistribute comes from the shift from CS as a consideration in voting in GG to OGV as a consideration in GY. In GG elections, H3 suggests that an increase in redistribution will at the margin court the danger of losing votes on grounds of candidate sincerity. In GY contests, this disincentive towards redistribution disappears and OGV will, according to H2, solidify the Green support and so help stem the loss of Green votes on OGP grounds when Greens propose more redistribution. The incentive for a Green to redistribute in GY contests is, therefore, stronger than in GG ones and so we should expect more redistribution to be proposed in the former. A similar logic applies when considering how a Yellow might shift their proposal when moving from a YY contest to a GY one. The CS reason to be more redistributive in YY disappears and the weakening effect of less redistribution on OGP of Yellow is tempered by OGV. Thus Yellows will become less redistributive in GY contests than in YY ones. H5 follows. It is an implication of candidates recognising the force of H1, H2 and H3 and so we refer to this as CR-S: the strong test for candidate responsiveness.

Hypothesis 5 (CR-S): In GY contests, Greens propose more redistribution compared proposals in GG contests and Yellows propose less redistribution compared to those in YY contests.

Thus we have three hypotheses concerning how voting behaviour is affected by three different group identity related preferences and two hypotheses related to candidate responsiveness in their supply of redistribution policies to these group related voter effects. These are the mechanisms for group identification to affect redistribution in a representative democracy that we wish to test.

3 Experiment Design

Subjects make 3 types of decision in the experiment: a dictator-like distribution decision (Decision 1); a candidate decision regarding what distribution proposal to make for an election (Decision 2); and a voting decision between two candidate distribution proposals (Decision 3).

The dictator Decision 1 and candidate proposal Decision 2 are always made on a set of 3 distribution options. We label these options here for the sake of convenience in the explanation and analysis as {1,2,3}. Option 2 is always the status quo endowment: rich subjects get 100 and poor ones get 50 experimental points. Option 1 redistributes the endowments towards the rich and away from the poor; and Option 3 redistributes to the poor away from the rich. Thus, when subjects choose a higher option number, they are choosing a more equal redistribution outcome: i.e. the option number is an index of the degree of equalising redistribution chosen in Decision 1 or proposed by the candidate in Decision 2.

The voting Decision 3 is over all the possible pairs of redistribution proposals from the set of 3 options {1,2,3}: i.e. [1,2]; [1,3]; and [2,3]. Again, the number acts as index of the vote for more equal distributions.

Subjects arrive and are formed into populations of 7 people. In each population 2 people are randomly endowed with the 'rich' 100 experimental points and 5 are endowed with the 'poor' 50 experimental points. They then make the first version of dictator decision (= Decision 1a): they rank the options from {1,2,3} in the order that they would most like to implement for their population of 7.[6]

After Decision 1a, subjects are randomly given a Green or Yellow group identity. This randomisation always has 5 Greens (2 of which are rich and 3 are poor) and 2 Yellows (both are poor). Subjects know these details and they then make the dictator Decision 1 again (= Decision 1b) in the knowledge of their group identity and how any redistribution affects group earnings.

[6]We incentivize subjects' dictator decisions using a biased lottery. For each population grouping, one of the three Dictator decisions is selected randomly by the computer and one randomly selected subject's decision in each population of 7 is implemented. The lottery is structured such that with 50% probability the first preference of the randomly selected subject is implemented, with 30% probability their second most preferred option is implemented and with 20% the randomly chosen subjects least preferred option is implemented for the population 7.

Decision	Decision Type	Group ID	Possible Group Motivation	Data for H test
1a	Dictator	No		H1
1b	Dictator	Yes	OGP	H1
2	Proposal	Yes	OGP, OGV, CS	H4, H5
3	Voting GY	Yes	OGP, OGV	H2
	Voting GG, YY	Yes	OGP, CS	H3

Table 1: Experiment Summary.

Subjects then make the candidate proposal Decision 2 in the knowledge of the group identity of their opponent in the electoral contest. So subjects make two decisions for the option set {1,2,3}: what redistribution option to propose in an own group contest; and what redistribution option to propose in a contest with a candidate from the other group.

Finally, subjects vote in Decision 3 on all the possible pairs of proposals where the identity of the candidate making each proposal is identified. There are 4 versions of each pair of redistribution options in these voting decisions. For example, the pair of redistribution proposals [1,2] could have Greens proposing both 1 and 2; Yellows proposing both 1 and 2; Green proposing 1 and Yellow proposing 2; and Yellow proposing 1 and Green proposing 2. Hence for any option set {1,2,3}, there are 12 electoral contests where proposals are distinguished by their redistribution and the group identity of the candidate. Each electoral contest is repeated 3 times with the result being announced after each one. Table 1 sets out the order of these decisions and summarises the likely group influences in each, based on Remarks 1, 2 and 3.

The logic behind this within-subject design in relation to the hypotheses follows from the earlier discussion and is summarised in the last column in Table 1. In the dictator decisions 1a and 1b, there are no candidates and there is no election, so OGV and CS cannot arise. However, when there are group affiliations in 1b, OGP does potentially motivate behaviour, whereas it cannot in 1a because there are no group identities at this stage. To test H1, therefore, we can compare decisions in 1a and 1b: do subjects with group identities shift their decisions to those proposals with higher OGP? We test H2 by examining voting behaviour in Decision 3 in GY contests and we see whether for the same pair of redistribution options votes switch as the group identity of those making the proposals changes from Green to Yellow and vice versa. We test H3 by examining voting behaviour for the same pairs of redistribution proposals in GG elections as compared with YY ones: with the same proposals, do people vote for less redistribution in GG than in YY contests? Finally we test H4 and H5 with candidate proposal choices in Decision 2: e.g. for H5 by comparing Green candidate proposal choices in Decision 2 in GG and GY contests (do they become more redistributive in the latter?) and Yellow candidate proposal choices in YY and GY contests (do they become less redistributive in the latter?).

There are three other aspects of the design that are worth commenting on. First, we have minimal, artificial group identities. One virtue of this is that it enables better control than is possible with natural groups. We are, for example, thereby able to introduce a non-economic source of group identification that has clear mapping on to the economic differences in the population. In particular, the minority group is poorer than the majority one in our experiment and we chose this because it corresponds to many of the interesting non-economic group allegiances that we observe in the world (e.g. over race in society and gender in the workplace). We also opted for an artificial source for the non-economic group identification that criss-crosses with economic status because the influence of such group memberships on behaviour is often clearer/stronger with artificial groups than natural ones (see Lane, 2016). These virtues, of course, have to be weighed against the potential external validity drawbacks of artificial groups (for example, the character of race group relations in the US have a long and varied history that are likely to affect how race group identification influences behaviour). We have opted for the greater control that comes from artificial groups (and see Hargreaves Heap and Zizzo, 2009, for an alternative account of the possible external validity of the artificial group paradigm).

Second, we incentivise subjects dictator decisions (Decision 1) using a biased lottery. For each population grouping, one of the three Dictator decisions is selected randomly by the computer and one randomly selected subjects decision in each population of 7 is implemented. The lottery is structured such that with 50% probability the first preference of the randomly selected subject is implemented, with 30% probability their second most preferred option is implemented and with 20% the randomly chosen subjects least preferred option is implemented for the population of 7. This encourages subjects to rank truthfully the redistribution options in each round. No feedback is given either after each dictator decision or when all dictator decisions have been made; the chosen allocation is only revealed to subjects once the experiment is over and they receive payment information. Decision 2 and 3 are incentivised in the following way. Subjects are told in Decision 2, when they decide what proposal to make, that an election will be selected by choosing randomly two people from their population and this contest will be resolved and implemented using how people vote in that particular contest in Decision 3. The subject whose proposal wins this election contest gains a bonus of 70 tokens. Hence, subjects are incentivised to make winning proposals in Decision 2 and subjects are incentivised to vote in Decision 3 because they know that one of the contests that they vote upon will be randomly selected for implementation. It is worth noting that the binary nature of all potential elections ensures that the sincere revelation of a voter's preference is incentive compatible. That is, there are no incentives for strategic voting since, for any given pair of alternatives, voting for one's preferred option is a weakly dominant strategy. Indeed, a voter's exact preferences on the available alternatives (i.e. the bundles of candidate identities and policy outcomes) might depend on a number of factors, and to be able to detect these factors, it is important that additional strategic/coordination dimensions are absent. Our design secures that voters are always better off by behaving sincerely; and, hence, their voting actions should reflect only their true valuation of each alternative, and not their expectations regarding the behaviour of other voters.

Third, the voting decision was repeated for each possible election contest 3 times in order to allow some learning to occur and potentially influence behaviour. Having cast their votes, subjects receive feedback on the outcomes of each vote - we inform subjects which policy wins, but not how many votes each policy receives. In fact there was little evidence that voting behaviour changed over each round.[7]

The final detail of the experiment concerns the redistribution options {1,2,3}. The subjects made Decisions 1, 2 and 3 on three distinct versions of these {1,2,3} option sets. The versions differ according to whether redistribution affects the size of the pie. In one set, there is negative equity efficiency trade-off (NEE) so that total wealth is higher in option 1 than option 2; and option 2 is higher than option 3. In the no trade-off option set (NoEE), total wealth is the same under each of the redistribution options {1, 2, 3}. In the final positive equity efficiency trade-off (PEE) total wealth grows with redistribution and so it is higher in option 3 than option 2; and option 2 is higher than option 1. The precise options sets in each version of the trade-off are given in Table 2. This is an important detail. The various trade-offs provide a robustness check for our results. OGV and CS operate independently of the trade-off as possible considerations when voting and so we expect to see their effects when we test H2 and H3 across the trade-

[7]We use Jonckheere-Terpstra tests where the null hypothesis is that the frequency of voting for high redistribution in an election type (GG/YY/Green High Yellow Low/ Green Low Yellow High) does not change across repetitions. We find that on aggregate the frequency of votes for high redistribution does not change. When we disaggregated by income and social identity group we do find that Rich Greens in NEE, in GG elections and Green High Yellow Low elections increase their votes for high redistribution ($p = 0.0179$ and $p = 0.0733$). Everywhere else there is no change in behaviour over rounds.

offs. In addition, although the OGP associated with any redistribution will be different depending on the efficiency-equity trade-off, the qualitative prediction of moving towards higher OGP outcomes when there are group identities is independent of the nature of the efficiency-equity trade-off. In fact, with the precise equity efficiency trade-offs in the experiment, the Yellow OGP always increases with a higher proposal number. For Greens, in contrast, OGP increases with a lower proposal number in NoEE and NEE; and in PEE the reverse is the case as Proposal 2 and 3 are not materially different in terms of OGP and both are higher than proposal 1.[8]

The experiment was conducted in the LExEcon Experimental Lab at the University of Leicester. All participants were undergraduate and masters students at The University of Leicester. Subjects were recruited through an online platform that sent out a mass email 48 hours before the experiment to all students who had previously indicated their willingness to participate in future experiments. For all sessions we over recruited on the number of required subjects due to previous problems with attendance, whenever there was a surplus of participants on the day of the experiment participation was decided via a lottery and all those who were not admitted were paid the showup fee and given first

[8]It is worth noting that later group identity decisions may be reinforced by the priming of group identity in earlier decisions in this design. For example, an initial use of group identities in making decisions might, perhaps through cognitive dissonance, prime later use of group identities. This is unavoidable in a within subject design of this kind. We are inclined to be relaxed for three reasons. First, Thus, although the strength of the group prime may change in the course of the experiment, all subjects receive the same group primes at any particular decision. Thus for example, subjects may have had their group identities primed more intensively when making the voting decisions (Decision 3) than when they make their candidate decisions (Decision 2) by virtue of having previously made the candidate decisions (Decision 2). However, this is not obviously worrying since the tests of, say, H2 and H3 depend only Decision 3 votes. Likewise, the tests of H4 and H5 depend only candidate decisions (Decision 2). What matters therefore is that subjects have received the same primes when they make Decision 2 and that they have also received the same primes when they make Decision 3. It is not important for these tests that the prime should be the same for Decision 2 as for Decision 3. Second, the possibility that the prime may strengthen for all in the course of the decisions is not itself a source of worry because many experiments use stronger group primes than our initial minimal groups. Third, when we discuss the robustness of our results in section 5, we also draw on between subject comparisons that are afforded by a Baseline where subjects make the same sequence of decisions without any group identifications. These between subject comparisons, where relevant, largely support those derived in this experiment from the within subject ones.

Option Set	Rich	Poor	Total Wealth	OGP Green	OGP Yellow
NoEE					
1 (More unequal)	120	42	450	366	82
2 (Status Quo)	100	50	450	350	100
3 (More equal)	80	58	450	344	116
NEE					
1 (More unequal)	132	46	494	402	92
2 (Status Quo)	100	50	450	350	100
3 (More equal)	72	53	409	303	106
PEE					
1 (More unequal)	108	38	406	330	76
2 (Status Quo)	100	50	450	350	100
3 (More equal)	76	65	477	347	130

Table 2: Distribution Options

priority for the next experiment. Subjects participated in only one session.

Upon entering the laboratory, subjects were randomly seated at computer terminals which were divided to maximise privacy and remove any opportunities for communication between the subjects either visually or verbally. Both the experiment and the instructions were computerised and programmed using z-Tree experiment software (Fischbacher, 2007). In addition to the computerised instructions, the instructions were read aloud by the experimenter to ensure subjects had common knowledge. Subjects were given the opportunity to ask clarification questions to the experimenter by raising their hand, these were asked and answered in private.

Subjects earned tokens at an exchange rate of £1=20 tokens and were paid in cash in private at the end of the experiment. In total 70 subjects participated. The sessions consisted of 14 subjects. We therefore have 10 independent observations. Average payments was £12.19 again including a £2 show up fee and sessions lasted on average 64 minutes.

4 Results

Since a movement towards the OGP maximising outcome (for Green/Yellow respectively) always increases OGP for Greens/Yellows in our experiment, we test H1 by examining whether the gap between the actual decision and the OGP maximising outcome shrinks with the introduction of group identities. Table 3 gives the aggregate data on this gap when there are no group identities in Decision 1a (column 2) and when there are group identities in Decision 1b (column 3). The gap shrinks significantly with all three efficiency-equity trade-offs (see column 4, where the p values are given for a test on whether the gap is significantly different in Decision 1b than Decision 1a)).

Table 4 disaggregates the same data for the poor (panel A) and the rich (panel B) and, among the poor, for Green Poor (panel C) and Yellows (Panel D). The gap significantly shrinks as predicted by the influence of OGP for the poor in NoEE and NEE and weakly so for the rich in PEE; and among the poor, the Yellow seem to be driving the observed shrinkage in NoEE and NEE with some contribution from the Green Poor in NEE. Result 1 follows from Tables 3 and 4.

Table 3: Wilcoxon Signed Rank: Differences in the Preferences for Redistribution

Distribution	Diff no ID	Diff ID	p-value
NoEE	0.96	0.79	0.0361
NEE	1.01	0.63	0.0024
PEE	0.67	0.56	0.0455

Note: The variable of interest is the absolute difference between a subjects preference for redistribution [1,2,3] minus the OGP maximising option, before and after the introduction of group identities. The null hypotheses for the test is Diff no ID - Diff ID = 0. n=70.

Table 4: Wilcoxon Signed Rank: Differences in the Preferences for Redistribution

	Distribution	Diff no ID	Diff ID	p-value
(A)	NoEE	1.20	0.98	0.0446
	NEE	1.34	0.80	0.0031
	PEE	0.64	0.58	0.3173
(B)	NoEE	0.35	0.30	0.5637
	NEE	0.20	0.20	1
	PEE	0.75	0.50	0.0588
(C)	NoEE	1.60	1.53	0.4837
	NEE	1.47	1.03	0.0578
	PEE	0.93	0.90	0.5637
(D)	NoEE	0.60	0.15	0.0147
	NEE	1.15	0.45	0.0067
	PEE	0.20	0.10	0.4142

Note: The variable of interest is the absolute difference between a subjects preference for redistribution [1,2,3] minus the OGP maximising option, before and after the introduction of group identities. The null hypotheses for the test is Diff no ID - Diff ID = 0. Panel A: n=50, Panel B: n=20, Panel C: n=30, Panel D: n=20.

Result 1(in support of H1/OGP): Subjects decisions are significantly closer to the OGP maximising option when there are group identities. This result is largely driven by the behaviour of the poor, particularly the Yellows, and in aggregate holds across all three efficiency-equity trade-offs

We turn next to H2 (OGV). To address this Figure 1 plots in GY contests the aggregate frequency of Greens and Yellows voting for the high redistribution in any pair of redistribution proposals by the Green/Yellow identity of the candidate proposing the high redistribution. Thus, for example, with the proposal pair (1,2) we want to test whether the likelihood of, say, Greens on average voting for option 2 will be higher when the candidate proposing 2 is a Green rather than a Yellow. Notice, although an individual may

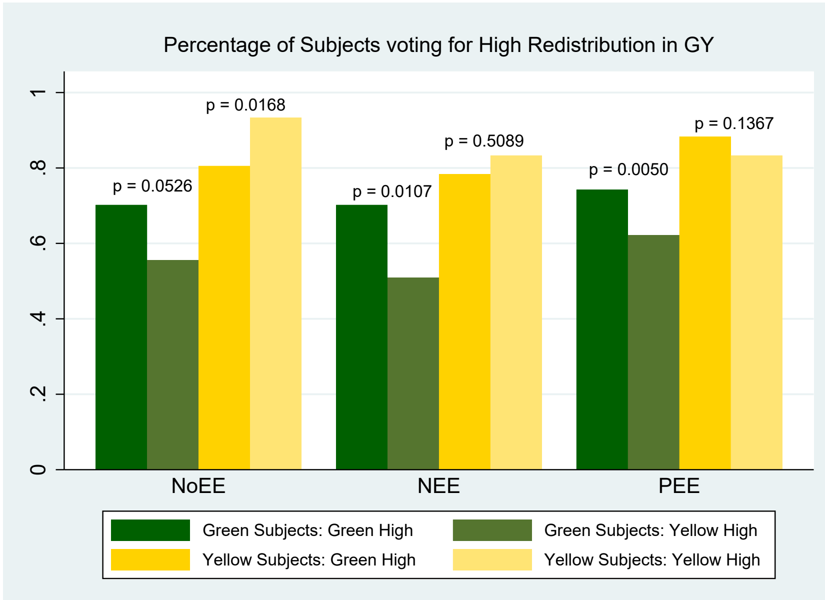

Figure 1: Wilcoxon signed-rank tests: Voting Behaviour Heterogeneous Elections

Note: The variable of interest is the percentage of subjects voting for the high level of redistribution for a given pair of proposals. The null hypothesis states the percentage of votes for high redistribution is equal whether the high redistribution option is proposed by a Yellow or a Green candidate. E.g. bars 1 and 2 show the percentage of Green subjects who vote for the high level of redistribution when this option is proposed by a Green and a Yellow, respectively. We use the average vote for each group, hence $n = 10$ for all tests.

be influenced in their vote between option 1 and 2 by their Decision 2 as a candidate because they have an incentive as a candidate for their proposal to win in such elections, this would not be a reason to change the likelihood of voting for option 2 depending on the identity of the candidate proposing option 2. It is apparent that Greens always vote significantly more for a high distribution proposal when it comes from a fellow Green as compared with when it comes from a Yellow. Likewise, Yellows vote significantly more for high redistribution when the candidates identity is Yellow as compared with when the candidate is a Green in NoEE, but there is no statistically significant difference in the frequency of Yellow voting for the high redistribution in NEE and PEE.

The individual level data is similarly clear. In the comparison of GxYz with GzYx contests (i.e. the contests with the same proposals, x and z, but where candidate identity attached to each switches) we find that there are 406 instances where a voter changes their vote. On 326 of these switches, their vote follows the candidate with whom they share an identity: i.e they continue to vote for their own candidate despite the fact that their candidate is now proposing the opposite redistribution in the contest to the one they voted for before. Such switching could, of course, occur if subjects were making random choices between candidate platforms, but in this case we would observe such own candidate voting with a 0.5 frequency. With 326/406 own candidate switches, we can reject the hypothesis that these switches are driven by random choice behaviour. Likewise, we find 51 of our 70 subjects switch at least once and 42 of these subject switchers always follow their own candidate when switching. Again, we can reject the hypothesis that this is driven by random choice behaviour. The same pattern is found when we further disaggregate by group identity: 258/317 are own candidate switches for Greens and 68/89 are own candidate switches for yellows. Again, we can reject the hypothesis that these own candidate following switches arise from random choice behaviour. Result 2 follows.

Result 2 (in support of H2/OGV): Greens in the aggregate data are more likely to vote for a proposal made by a fellow group member in GY elections than when the same proposal is made by a member of the other group in all equity-efficiency trades-offs. Yellows do the same for Yellow candidates in the aggregate in NoEE. In the individual level data, when subjects switch their votes in a given contest as the identity of the candidates making each proposal switches, these vote switches are skewed towards following their own candidate; and this pattern is significantly different from what would be expected by random choice producing such switches.

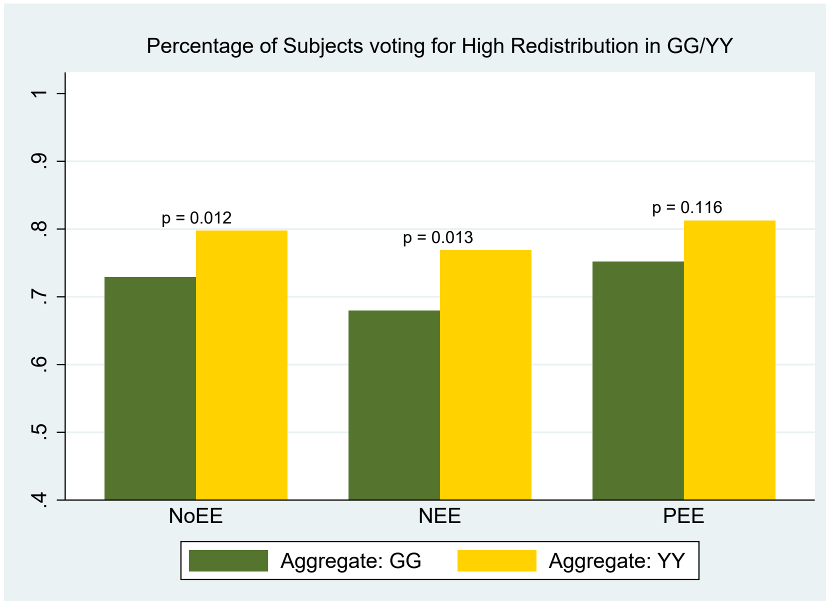

Figure 2: Wilcoxon signed-rank tests: Voting Behaviour Homogeneous Elections

Note: The variable of interest is the percentage of subjects voting for the high level of redistribution for a given pair of proposals in a GG/YY election contest. The null hypothesis states the percentage of votes for high redistribution is equal in GG and YY elections. We use the average vote for each group, hence $n = 10$ for all tests.

Table 5: Wilcoxon signed-rank tests: Voting Behaviour Homogeneous Elections

	NoEE			NEE			PEE		
	Green	Yellow	Agg	Green	Yellow	Agg	Green	Yellow	Agg
GG	60.2	85.6	72.9	59.8	76.1	67.9	65.3	85.0	75.2
YY	69.6	90.0	79.8	69.3	84.4	76.9	71.3	91.1	81.2
p-value	0.0737	0.0801	0.012	0.0825	0.0858	0.013	0.2588	0.3666	0.116

Note: The null hypothesis for all tests states: the percentage of subjects voting for the high level of redistribution in GG elections is equal to the percentage of subjects voting for the high level of redistribution in YY elections. We use average vote for each group, hence $n = 10$ for all tests.

Our next hypothesis relates to the possible influence of the group affiliation of candidates as a signal of candidate sincerity (CS) and the prediction that voters will be more willing to vote for the higher redistribution in any given pair of proposals when they are made by Yellow candidates (i.e. YY elections) than when they are made by Green candidates (i.e. GG elections). Figure 2 plots the aggregate percentage of subjects voting for high redistribution in GG and YY elections for all the pairs of such proposals. The proportion voting for the higher redistribution is always higher in YY than GG elections and significantly so in NoEE and NEE. Table 5 disaggregates this data by the group identity of the voter: the same pattern emerges for both Greens and Yellow voters but the higher proportion in YY elections is only weakly significant at 10% level when disaggregated.

Table 6 gives the individual level regressions on the likelihood of voting for the more redistributive outcomes by contest and overall in GG and YY elections. There is a dummy for YY elections to test for whether there is a significant difference in this propensity at the individual level in these elections compared with GG ones. Since individuals could have an interest in either the more or less redistributive proposal in any contest when they have, as a candidate, made this proposal, we control for this possible source of individual difference in voting with two dummies: Dummy 1 (=1) when the subjects made the lower of the redistributive proposals as a candidate and Dummy 2 (=1) when the subject made the higher of the redistributive proposals as a candidate. In addition, we allow for the possibility that voting behaviour varies in this respect across the different efficiency equity trade-offs through an additional set of dummies. We include in the appendix a version of this regression where we allow also for differences between Green Rich, Green Poor and Yellow. The results are not qualitatively different (see Table A1). The group identity dummies are dropped here because they may also explain the choice of policies as a candidate and so be captured by the Dummies. The key coefficient on YY is positive and significant at 5% level for the regression on all elections (column 4) and in the disaggregation of this regression by electoral contest, it is positive in (1,3) and (2,3) elections and significantly so in the latter. We also note that trade-off interact dummies are not significant, suggesting the YY difference from GG holds across the three

Table 6: Ordered Logit Regressions - Voting Behaviour GG/YY Elections

	(1) Vote12	(2) Vote13	(3) Vote23	(4) All
YY	-0.002	0.274	0.645***	0.344**
	(0.01)	(0.182)	(0.202)	(0.150)
NEE	-0.107	0.043	-0.201	-0.105
	(0.30)	(0.380)	(0.321)	(0.313)
PEE	0.293*	0.174	0.129	0.213*
	(1.81)	(0.164)	(0.155)	(0.120)
NEE*YY	0.023	0.041	0.042	0.049
	(0.08)	(0.279)	(0.277)	(0.236)
PEE*YY	-0.202	-0.053	-0.012	-0.083
	(0.98)	(0.203)	(0.229)	(0.151)
Dummy 1	-1.663***	-1.402***	-0.198	-0.925***
	(6.75)	(0.269)	(0.205)	(0.150)
Dummy 2	-0.226	0.979***	0.976***	0.664***
	(0.87)	(0.228)	(0.187)	(0.115)
Vote12				0.212***
				(0.057)
Vote23				-0.315***
				(0.092)
Constant	0.918*	0.435	0.018	0.490
	(1.74)	(0.517)	(0.533)	(0.510)
N	1,260	1,260	1,260	3,780

Note: Standard errors reported in parentheses are clustered at the subject level. All regressions include both period and group fixed effects. The dummy variable YY indicates an election is between two Yellow candidates. Significance Levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

efficiency-equity trade-offs.; and that the coefficients on own proposal dummies take the expected signs in the regression on all the elections. Result 3 follows.

Result 3 (in support of H3/CS): Subjects are significantly more likely to vote for high redistribution in YY elections than GG ones in the aggregate in NoEE and NEE. At an individual level, subjects are also significantly more likely to vote for high redistribution in YY than GG elections, under all efficiency-equity trade-offs; and this is strongest where the proposal pair is (2,3).[9]

We note that Table 6 also suggests a status quo bias in the sense that in votes involving the status quo option, 2, there is a bias towards this outcome (i.e.. the coefficient in column 4 on the Vote 12 dummy is positive and the coefficient on the Vote23 dummy is negative).

To examine H4 and H5 on candidate responsiveness, Table 7 gives the aggregate evidence for both. The first two rows address H4 by comparing the GG proposals in the aggregate with those in YY elections. The proposals are significantly more redistributive in YY than GG elections with $p < 0.05$ in NoEE and PEE and $p < 0.10$ in NEE. The next rows address H5. They set out how Green proposals in GG compare with Green ones in GY elections and how Yellow proposals in YY compare with Yellow ones in GY elections. Green proposers are more redistributive in GY than GG and these differences are significant in NoEE and PEE but only weakly significant in NEE (p=0.0469, p=0.0926, p= 0.0058 respectively for NoEE, NEE and PEE). In GY, Yellows propose lower redistribution as compared to their proposals in YY. These differences are significant for NEE and PEE (p=0.0208 and p=0.0643 respectively).

[9] We also note that in relation to the question of whether this result tells in favour of CS or a distinct version of group loyalty (see footnote 5), the Yellows are as inclined to vote for less redistribution in GG as Greens and likewise Greens are as inclined as Yellows to vote for more redistribution in YY elections in Table 5. This points in the direction of the CS interpretation

Table 7: Candidate Proposals

	NoEE	NEE	PEE
Green-Green	2.2	2.2	2.3
Yellow-Yellow	2.6	2.6	2.7
p-value	0.0150	0.0673	0.0181
Green-Green	2.2	2.2	2.3
GY Green	2.5	2.5	2.6
p-value	0.0469	0.0926	0.0058
Yellow-Yellow	2.6	2.6	2.7
GY Yellow	2.4	2.2	2.4
p-value	0.1509	0.0208	0.0643

Note: Table contains both Wilcoxon signed-rank tests and Mann Whitney Tests. Null Hypothesis (Wilcoxon): Proposals [1,2,3] in the homogeneous elections GG (YY) do not differ from the proposals in GY elections for Green (Yellow) candidates. Null Hypothesis (M-W): Proposals for redistribution in homogeneous elections GG and YY are equal. We take as the independent observation the group average proposal per experimental session, hence $n = 10$ in each test.

In Table 8, we run individual regressions. GY is the omitted variable and so the coefficients on GG and YY address H4 and H5. The coefficient on GG is significantly negative and that on YY is usually significantly positive (as in H5) and the coefficient on GG is significantly lower than that on YY (as in H4). We also note that the coefficients on NEE and PEE again take the expected sign as does the coefficient on the rich dummy. Finally the interact between efficiency trade-off type and the GG and YY dummies are not significant, suggesting the difference in proposals identified above does not change with the efficiency-equity trade-off. Results 4 and 5 follows.

Table 8: Ordered Logit Models: Candidate Proposals

	(1)	(2)	(3)	(4)
GG	-0.665**	-0.943***	-0.961***	-1.038***
	(2.38)	(0.329)	(0.333)	(0.392)
YY	0.589	1.060**	1.072**	1.021*
	(1.40)	(0.501)	(0.506)	(0.586)
Rich		-1.338***	-1.354***	-1.354***
		(0.297)	(0.299)	(0.299)
Green		-1.267***	-1.287***	-1.291***
		(0.429)	(0.433)	(0.433)
NEE			-0.207*	-0.314**
			(0.109)	(0.155)
PEE			0.315***	0.321
			(0.109)	(0.202)
GG*NEE				0.234
				(0.223)
GG*PEE				-0.027
				(0.290)
YY*NEE				0.093
				(0.438)
YY*PEE				0.063
				(0.311)
N	1,260	1,260	1,260	1,260
Groups	10	10	10	10

Note: The standard Errors in parenthesis are clustered at the subject level. All regressions include both period and group fixed effects. GG/YY dummy represents a homogeneous election contest, the omitted category are GY election contests. Significance Levels: ** $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.*

Result 4 (supporting H4/CR-W): Candidates propose less redistribution in GG elections than in YY ones both in the aggregate and at the individual level and these differences are significant for each possible efficiency-equity trade-off, except in the aggregate data in NEE, where the difference is only weakly significant.

Result 5 (supporting H5/CR-S): Greens are more redistributive in GY than GG and

Yellows are less redistributive in GY than YY in the aggregate and there is similar evidence of these differences at the individual level. These differences are often significant in the aggregate and are significant at the individual level where they hold across all efficiency-equity trade-offs.

Finally, we also note that this *supply-side* difference regarding candidates' redistributive proposals in GG versus YY elections that we document above (see e.g. first two rows of Table 10) is further reinforced by the influence of *CS* in the subjects' voting decisions (see Figure 2). As a result, the next corollary immediately follows.

Corollary 1: Redistribution is increasing in minority (political) representation.

This last finding is a wholly new insight with respect to representative democracy.

5 Discussion and Conclusion

We begin by noting that our results are consistent, where there is overlap, with what is known from other experiments. For example, we find evidence of social preferences that are typically a mixture of efficiency and inequality aversion, as has been found by others (see e.g. Charness and Rabin 2002). This is evident in Decision 1a; and that, as a result, the appetite for redistribution depends predictably on the precise trade-off between equity and efficiency (for example, the numbers voting for the highest redistribution increases as the trade-off moves from NEE to NoEE to PEE). We also find evidence of a status quo bias in voting and a status quo bias has been commonly found in the experimental literature (see e.g. Kahneman and Tversky 1991). Finally, we find evidence of an Own Group Pay-off motive when voting (Result 1) and this has been found before (see Coate and Conlin, 2004). OGP is the mechanism of group influence that potentially operates in both direct and representative democracies and our finding in this respect is also consistent with the Klor and Shayo (2010) on how redistribution preferences change with group identities in a direct democracy. This is because, like them, we find the strongest influence of OGP is on the poor. However, it is worth remarking that we find that the Yellow drive this result more strongly than Green Poor and that there is also some evidence in PEE that Green Rich may behave in the manner suggested by Rueda (2018).

We turn now to our distinctive contribution regarding the specific influence of group identities in representative democracies.

First, voting is influenced by the identity of the candidate making the proposal. This has also been found in Bassi et al (2011) in a non-distribution context but we are the first to identify and test two distinct mechanisms through which candidate identity might influence voting for redistribution. One is an Own Group Victory motive (Result 2). Our finding in this respect is a powerful illustration of Trumps famous claim that "I could stand in the middle of Fifth Avenue and shoot somebody and I wouldn't lose voters" (Reuters, 24 January, 2016). There was no shooting in our experiment, but a significant number of individuals stuck with a candidate of their own identity despite their own identity candidate switching to the opposite policy. In other words, what the candidate proposes to do matters less than their identity; much as Trump suggested his support would not waver even if he committed a crime publicly.

The other mechanism through which candidate identity influences voters is in its use to infer candidate sincerity (Result 3). Candidate sincerity has been found to influence voting in other studies, but we are the first to link its attribution to group identities and assess its influence on redistribution voting. We provide a robustness check on this result in the Appendix, where we report on a baseline experiment where subjects make the same decisions but there are no group identities. This enables between subject comparisons with those in this experiment so as to check on the robustness of the within subject results we report above. The check on Result 3, however, is not straightforward.[10] This is because when comparing, say, how subjects vote between (2,3) with no group identities with how they vote in GG version of the choice (2,3), two new considerations are introduced for voters with identity: OGP as well as CS. Green voters in GG may be less inclined to vote for the more redistributive option in (2,3) because redistribution lowers OGP and they may also be more inclined to vote for the less redistributive option because, in a GG election, they attribute greater CS to the candidate making the less redistributive proposal. However, Yellow voters in GG elections will find that OGP and CS pull in opposite directions. Thus, if Yellow voters vote for less redistribution compared with the baseline where there are no identities, CS must have dominated any contrary OGP effect on behaviour. In an analogous test, in YY elections, if Greens vote for more redistribution than in the baseline equivalent choice, CS must have dominated OGP. This is a tough test for the presence of CS because it requires not just that CS motivate our subjects but that it also overwhelm OGP. Nevertheless, we find that this is the case in several election choices (see Tables A2-A4 in Appendix) and so we find support for Result 3 in this between subject robustness check.

Second, candidates respond to these group voting sensitivities in a representative democracy: in majority candidate elections, GG candidate proposals are less redistributive than the YY proposals in minority candidate elections (Result 4) and GY candidate proposals moderate these tendencies by bringing the G and Y proposals away from the

[10]This is unlike the use of the between subject comparison to check on Result 1, that is straightforward and which provides support for the within subject comparison that we report in Result 1 (see Appendix Table A5).

extremes of the GG and YY proposals respectively (Result 5). Some care is required in the interpretation of these results. Although the subjects were incentivised to make proposals that would win and so they should respond to the voting propensities in Results 2 and 3, they may have made proposals for other reasons. For example, a Green candidate in GG elections might propose a less redistributive proposal than a Yellow candidate in YY elections because they value OGP and wish to express this feeling through their choice of proposal (i.e. they make the proposal expressively rather than instrumentally to win the election). Thus, the gap between GG and YY proposals may be driven by candidates responding to Result 3 but equally the result could be driven by these expressive OGP considerations. We have no way of disentangling these possibilities. Nevertheless, the fact that there is evidence of candidate responsiveness in the strong test provided by GY elections (i.e. Result 5) makes it unlikely that candidate responsiveness played no role in the difference between proposals in GG and YY elections.[11]

Third, these candidate proposal effects are reinforced by the voting ones to yield the conclusion that redistribution will, as a result of these specific influences of group identity in a representative democracy, be increasing in the representation of minority group candidates. This conclusion is, to the best of our knowledge, wholly new. It also appears to be robust because it is based on insights regarding voting and candidate behaviour that mostly hold across the various efficiency-equity trade-offs (see Results 2, 3, 4 and 5) and because these insights are largely supported through between subject comparisons with a baseline where there are no identities.

[11]Our baseline is no help on this occasion because the same problem of disentangling candidate motives arises when comparing the proposals that are made in the baseline where there are no identities with those, say, made by Greens in GG elections. Nevertheless, the between subject comparison with the baseline is broadly consistent with the within subject Result 4 because GG proposals are typically less redistributive and YY proposals are always more redistributive than in the baseline (see Table A9 in the Appendix. Further the other between subject comparisons that are afforded through the baseline are comforting: e.g in providing support for Result 1 and Result 3.

The conclusion is not only new, it is potentially important. For example, it throws light on the puzzle with which we began: i.e. why do we see differences in the extent of redistribution in countries that have similar levels of pre-tax and benefit inequality? The literature on this puzzle that is based either on survey or direct democracy experimental evidence suggests that heterogeneity may account for these differences because preferences for redistribution weaken in the presence of group identification. We qualify this suggestion in two respects. First, while Result 1 on OGP provides some support for this suggestion among the majority group, it strengthens the preference for redistribution among the minority group. Thus, the overall effect of heterogeneity operating via OGP on preferences for redistribution will depend on the relative magnitudes of these two conflicting effects: i.e. the relative size of the majority and minority groups. Second, there are additional mechanisms of group influence in a representative democracy that combine to make the political representation of the minority group another key determinant of redistribution outcomes in heterogeneous societies.

To put the identification of this new factor that comes from considering a representative rather than a direct democracy into perspective, consider how the magnitude of this new group factor compares in our experiment with the suggestion in the literature of how group identification affects redistribution outcomes in a direct democracy. For this purpose, it is useful (and interesting) to compare the respective group effects on redistribution outcomes in the two democracy cases with those that arise from the OECD and IMF conjectured change in the equity-efficiency trade-off.

In our experiment, the effect of the OECD/IMF change in the trade-off (operating via the social preferences for efficiency and inequality aversion) is captured in Decision 1a and it is quantitatively large: for example, in Decision 1a the proportion choosing the highest redistribution (option 3) increases by 34 percentage points in PEE as compared to

NEE. In a direct democracy with group identities, these ordinary social preference effects combine with the influence of OGP. The combined effect is revealed in Decision 1b. The influence of OGP in Decision 1b weakens the tendency to redistribute that comes from the ordinary social preferences in Decision 1a, as expected from the literature, but only slightly: the proportion choosing option 3 now increases by 30 percentage points as we move from NEE to PEE, as compared with the 34 percentage point change in the absence of group identities. So, in our experiment, the weakening influence of group identities on redistribution in a direct democracy is perceptible but relatively small when compared with the effect of the change in the equity efficiency trade-off.

In a representative democracy, matters are rather different. The influence of OGP still operates in voting, but in addition we have the influence of candidate identity. To pick up on the key new factor regarding the political representation of the minority group on voting, we look at how the voting for option 3 changes with the switch from a GG election to a YY. We use NoEE, as a neutral trade-off between the NEE-to-PEE shift that we are using for the comparison. The switch from GG to YY increases the vote for option 3 in those elections where it is offered (i.e. (1,3) and (2,3)) by 19 percentage points. Although this is a large change, the real quantitative significance of minority political representation on distribution outcomes in our experiment comes from the way it affects the likelihood of option 3 appearing on the ballot paper: i.e. the candidate proposal effect. This is because the majority typically vote for option 3 in our experiment if it appears on the ballot paper. So, if a candidate proposes option 3, it will typically be the majority choice and hence the actual outcome; whereas if no candidate proposes option 3, it cannot be the outcome. The proportion of candidate proposals with option 3 in YY elections compared with GG elections rises by 24 percentage points in NoEE. Thus, in practice, the switch from GG to YY increases the likelihood of option 3 being on the ballot by a large amount and hence likelihood that option 3 will be the outcome in a representative democracy in our experiment. To compare this effect with how the change in the efficiency-equity trade-off in a representative democracy similarly affects the likelihood of option 3 appearing on the ballot in a representative democracy and hence being the actual redistribution outcome, the biggest increase is in YY elections where it increases by 6 percentage points as we move from NEE to PEE.

Thus, while the influence of group identities on redistribution outcomes is relatively small compared with that of a variation in the trade-off in a direct democracy, the key combined effect of group identities in a representative democracy is relatively large compared with that of a change in the trade-off. This is why it is important to consider how group identities affect redistribution outcomes in representative democracies: their effects are not only more complicated, they appear in our experiment to be quantitatively more significant in a representative democracy than in a direct one.

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6 Appendix (Online publication only)

A. Additional Tables

Table A1: Ordered Logit Regressions - Voting Behaviour GG/YY Elections

	(1) Vote12	(2) Vote13	(3) Vote23	(4) All
GRich	-2.035***	-1.735***	-1.471***	-1.742***
	(6.96)	(0.278)	(0.332)	(0.259)
GPoor	-0.468	-0.094	-0.187	-0.205
	(1.24)	(0.382)	(0.342)	(0.327)
YY	0.154	0.384***	0.727***	0.450***
	(0.87)	(0.149)	(0.153)	(0.124)
NEE	-0.117	0.055	-0.205	-0.097
	(0.32)	(0.373)	(0.313)	(0.323)
PEE	0.250	0.210	0.138	0.206*
	(1.75)	(0.142)	(0.142)	(0.1117)
Dummy 1	-0.931***	-0.887***	-0.121	-0.568***
	(3.34)	(0.298)	(0.248)	(0.157)
Dummy 2	0.207	0.648***	0.708***	0.529***
	(0.71)	(0.241)	(0.199)	(0.118)
Vote12				0.202***
				(0.061)
Vote23				-0.389***
				(0.107)
Constant	1.604***	1.050**	0.604	1.124**
	(3.04)	(0.493)	(0.483)	(0.466)
N	1,260	1,260	1,260	3,780

Note: Standard errors in parentheses are clustered at the subject level. All regressions include period and group fixed effects. Dummy variable YY indicates an election is between two Yellow candidates. Significance Levels: ** p<0.1; ** p<0.05; *** p<0.01.*

Table A2: NoEE - Percentages of votes cast for the high level of redistribution in each election combination.

NoEE Percentage voting for high Redistribution			
	1 vs 2	1 vs 3	2 vs 3
Poor Baseline (PB)	0.83	0.83	0.76
Rich Baseline (RB)	0.47	0.47	0.37
Y in GG	0.93	0.85	0.78
Rich G in YY	0.45	0.45	0.45
Poor G in YY	0.90	0.86	0.82
G in YY	0.72	0.69	0.67
Sample of Baseline	0.67	0.68	0.58
p-values Mann-Whitney Tests			
PB vs Y in GG	0.0584	0.6826	0.7186
PB vs PG in YY	0.1525	0.5578	0.2583
RB vs RG in YY	1	1	0.4577
Sample Base vs G in YY	0.3801	0.8037	0.0953

Note: Sample Baseline is made up of all rich subjects in the baseline and a random sample of the poor in the baseline to allow for equal income distributions in the comparison between the Greens and the (sample) baseline. Mann-Whitney tests are two sided, null hypothesis states that the percentage of votes for the high level of redistribution is equal in the baseline and the treatment for each election contest.

Table A3: NEE - Percentages of votes cast for the high level of redistribution in each election combination.

NEE Percentage voting for high Redistribution			
	1 vs 2	1 vs 3	2 vs 3
Poor Baseline (PB)	0.79	0.74	0.61
Rich Baseline (RB)	0.47	0.37	0.17
Y in GG	0.85	0.80	0.63
Rich G in YY	0.52	0.50	0.42
Poor G in YY	0.86	0.84	0.80
G in YY	0.73	0.71	0.65
Sample of Baseline	0.67	0.59	0.39
p-values Mann-Whitney Tests			
PB vs Y in GG	0.3460	0.3680	0.7205
PB vs PG in YY	0.1524	0.0595	0.0019
RB vs RG in YY	0.7151	0.1970	0.0045
Sample Base vs G in YY	0.2591	0.0300	0.000

Note: Sample Baseline is made up of all rich subjects in the baseline and a random sample of the poor in the baseline to allow for equal income distributions in the comparison between the Greens and the (sample) baseline. Mann-Whitney tests are two sided, null hypothesis states that the percentage of votes for the high level of redistribution is equal in the baseline and the treatment for each election contest.

Table A4: PEE - Percentages of votes cast for the high level of redistribution in each election combination.

PEE Percentage voting for high Redistribution			
	1 vs 2	1 vs 3	2 vs 3
Poor Baseline (PB)	0.94	0.89	0.71
Rich Baseline (RB)	0.63	0.55	0.32
Y in GG	0.90	0.88	0.77
Rich G in YY	0.57	0.52	0.48
Poor G in YY	0.84	0.87	0.81
G in YY	0.73	0.73	0.68
Sample of Baseline	0.81	0.75	0.54
p-values Mann-Whitney Tests			
PB vs Y in GG	0.3104	0.8343	0.4335
PB vs PG in YY	0.0151	0.5341	0.0910
RB vs RG in YY	0.5764	0.8549	0.0931
Sample Base vs G in YY	0.0985	0.5992	0.0131

Note: Sample Baseline is made up of all rich subjects in the baseline and a random sample of the poor in the baseline to allow for equal income distributions in the comparison between the Greens and the (sample) baseline. Mann-Whitney tests are two sided, null hypothesis states that the percentage of votes for the high level of redistribution is equal in the baseline and the treatment for each election contest.

Table A5: Mann-Whitney Tests: Differences in the Preferences for Redistribution Treatment vs Baseline

	Distribution	Diff Baseline	Diff ID	p-value
A	NoEE	0.55	0.30	0.4051
	NEE	0.20	0.20	1
	PEE	0.65	0.50	0.5231
B	NoEE	1.78	1.53	0.0330
	NEE	1.36	1.03	0.1023
	PEE	0.86	0.90	0.8801
C	NoEE	0.22	0.15	0.7501
	NEE	0.64	0.45	0.4596
	PEE	0.26	0.10	0.3634

Note: Null hypothesis: Diff = abs(preference selection [1,2,3] - OGP) is equal when comparing the baseline and the treatment. In other words the null hypothesis states that the introduction of ID does not change the level of redistribution selected relative to OGP.
Panel A compares rich in baseline with rich greens in treatment after the introduction of ID
Panel B compares poor in baseline with poor greens in treatment after the introduction of ID
Panel C compares poor in baseline with poor yellows in treatment after the introduction of ID

Table A6: Mann-Whitney Tests: Differences in the Preferences for Redistribution by ID Groups .

Distribution	Green	Yellow	p-value
NoEE	0.06	-0.45	0.0099
NEE	0.26	-0.7	0.0004
PEE	0.04	-0.1	0.2387
Average	0.12	-0.42	0.0004

Note: Null hypothesis: The difference in the preferences for redistribution when subjects move from Stage 1 (before ID) to Stage 2 (with ID) is equal to zero for Greens and Yellows N=70.

Table A7: Mann-Whitney Tests: Differences in the Preferences for Redistribution by ID Groups **Poor Subjects** .

Distribution	Green	Yellow	*p*-value
NoEE	0.07	-0.45	0.0264
NEE	0.43	-0.7	0.0008
PEE	-0.03	-0.1	0.4605
Average	0.16	-0.42	0.0018

Note: Null hypothesis: The difference in the preferences for redistribution when subjects move from Stage 1 (before ID) to Stage 2 (with ID) is equal to zero for Greens and Yellows N=50.

Table A8: Wilcoxon signed-rank tests: Voting Behaviour Heterogeneous Elections

	NoEE		NEE		PEE	
GY	Green	Yellow	Green	Yellow	Green	Yellow
High-Low	70.2	80.6	70.2	78.3	74.2	88.3
Low-High	55.6	93.0	50.9	83.3	62.2	83.3
p-value	0.0526	0.0168	0.0107	0.5089	0.0050	0.1367

Note: Test are two sided. Null Hypothesis: The percentage of subjects voting for the high level of redistribution when proposed by a member of their own group is equal to the percentage of subjects voting for the high level of redistribution when it is proposed by a member of the other group. We use the average vote for each group, hence $n = 10$ for both Green and Yellow tests.

Table A9: Mann-Whitney tests: Candidate Proposals

	NoEE	NEE	PEE
Green-Green	2.2	2.2	2.3
Baseline	2.5	2.1	2.5
p-value	0.0077	0.8205	0.1298
Yellow-Yellow	2.6	2.6	2.7
Baseline	2.5	2.1	2.5
p-value	0.1966	0.0473	0.0473

Note: Null Hypothesis: Proposals in the homogeneous elections, GG and YY, are equal to the proposals in the Baseline, respectively. We take as the independent observation the group average per proposal per experimental session, hence $n = 20$ in each test.

B. Experiment Instructions

The instructions below are for the Group Identity Treatment. The instructions for the Baseline are identical with the exceptions that Stage 2 is omitted along with any reference to Green and Yellow groups. Additionally the Stage 3 (Stage 2 in the Baseline) elections consist of only 3 potential elections in the Baseline rather than the Social Identity Treatment total of 12 and candidates select only 1 policy to campaign on.

Thank you for taking part in this experiment.

In this experiment you can earn money in addition to the £2 show up fee. Please read the instructions carefully. During the experiment, we will refer to tokens instead of pounds. Your earnings will be calculated in tokens and paid to you in pounds in private at the end of the experiment. In this experiment: 25 tokens = £1.
If you have any questions at any point, please raise your hand and an experimenter will come to your desk. By clicking NEXT, you will proceed to the instructions.

The experiment consists of 3 independent Stages. You will be provided a set of instructions specific to that Stage before it begins. You will need to read the instructions very carefully for each Stage. In this experiment, you will be paid for each stage independently. The instructions for how you get paid in each Stage will appear before the Stage begins. By clicking the START button, you consent to participate in this experiment. Even if you decide to take part in the experiment, you are free to withdraw at any time. Withdrawing from this particular experiment will not affect your relationship with the laboratory.
By clicking the START button you will proceed to the experiment.

Stage 1

In this experiment you will interact with 6 other people. We will refer to you and the 6 others as your society. The people in your society will remain fixed throughout the experiment.

People in your society randomly receive an income of either 100 or 50 tokens. Two people are randomly assigned 100 tokens and five people are randomly assigned 50 tokens. In Stage 1, you are shown 3 screens. On each screen, you will be shown 2 redistribution options in addition to the current distribution. Each of these three distribution options consists of a pair of potential new income levels: one for those people who initially have 100 tokens and the other for those who initially have 50 tokens. You will also be shown the Total Wealth in the society under each option.

You will be asked to rank the options in your preferred order. Assign Rank 1 to the option you would most like to be implemented in the society, Rank 2 to your second preferred option and Rank 3 to your least preferred option.

At the end of the experiment, the computer will randomly select one screen for payment. Every subject will be paid according to that screen. To implement one distribution, one person in your society will be randomly picked by the computer and the ranking of that person will determine the payment for everyone. In particular, the computer will conduct a lottery as follows:

With 50% probability, every person in the society will be paid according to the distribution option that the randomly chosen subject chose as their most preferred option: Rank 1.

With 30% probability every person in the society will be paid according to the distribu-

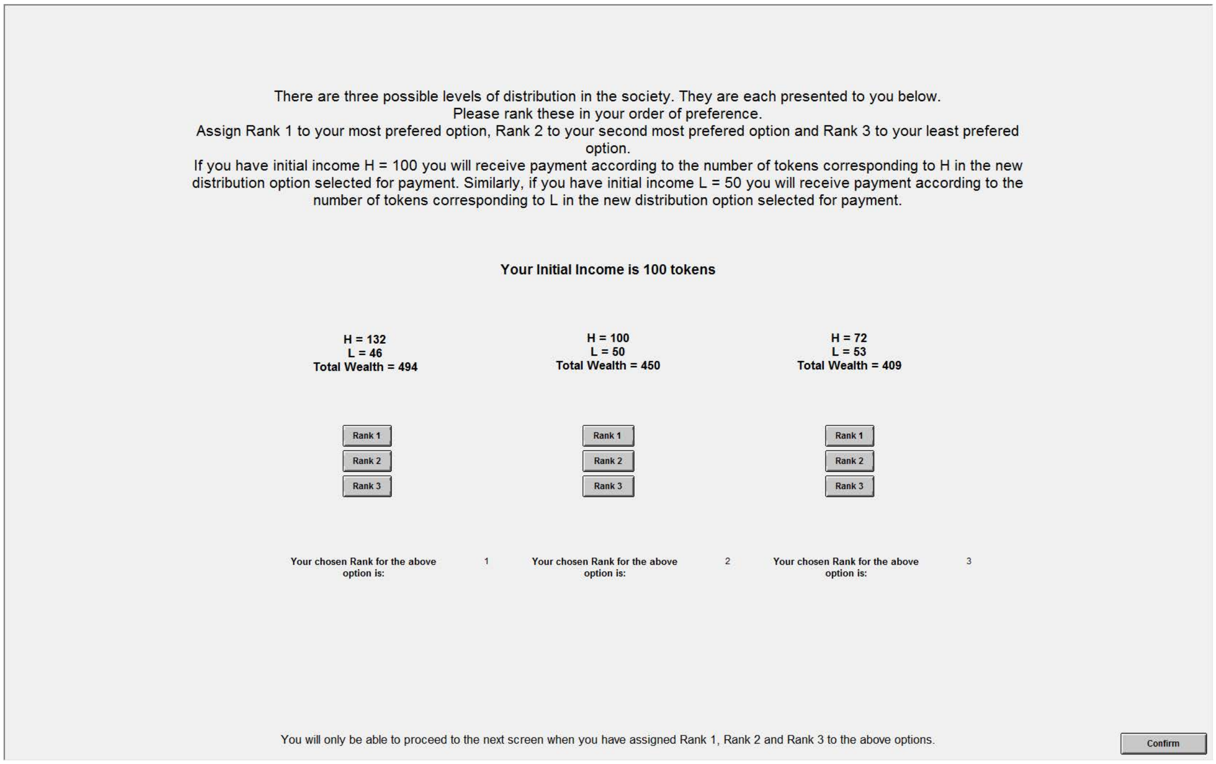

Figure A1: Stage 1 Redistribution Preferences

tion option that the randomly chosen subject chose as their second most preferred option: Rank 2

With 20% probability every person in the society will be paid according to the distribution option that the randomly chosen subject chose as their least preferred option Rank 3

Remember that in this experiment: 25 tokens = £1

If you have any questions, please raise your hand and an experimenter will come to your desk.

Income is Assigned and revealed to the subjects.

Stage 2

You keep the same income level (100 or 50) as you had in Stage 1.

Your income is 100/50.

In this stage, there are two groups. The Green group consists of 5 people and the Yellow group consists of 2 people. If you have an income of 100 tokens, you are allocated to the Green group. Those 5 people with an income of 50 tokens will be randomly allocated between the Green group (3 people) and the Yellow Group (2 people). So, Yellow group members always have an income of 50, but some Green group members have an income of 100 and some Green members have an income of 50.

Now, in Stage 2 you will be shown the same options as in the previous Stage and you are asked to rank them. In addition to the Total Wealth of the society, you are shown the Total Wealth that each redistribution option gives to each group: Green and Yellow.

You will receive payment according to the same mechanism in Stage 1. One screen will be randomly chosen and the chosen option from one random person in your society will be implemented according to the same lottery as before:

With 50% probability, every person in the society will be paid according to the distribution option that the randomly chosen subject chose as their most preferred option: Rank 1.

With 30% probability every person in the society will be paid according to the distribution option that the randomly chosen subject chose as their second most preferred option: Rank 2

With 20% probability every person in the society will be paid according to the distribution option that the randomly chosen subject chose as their least preferred option Rank 3

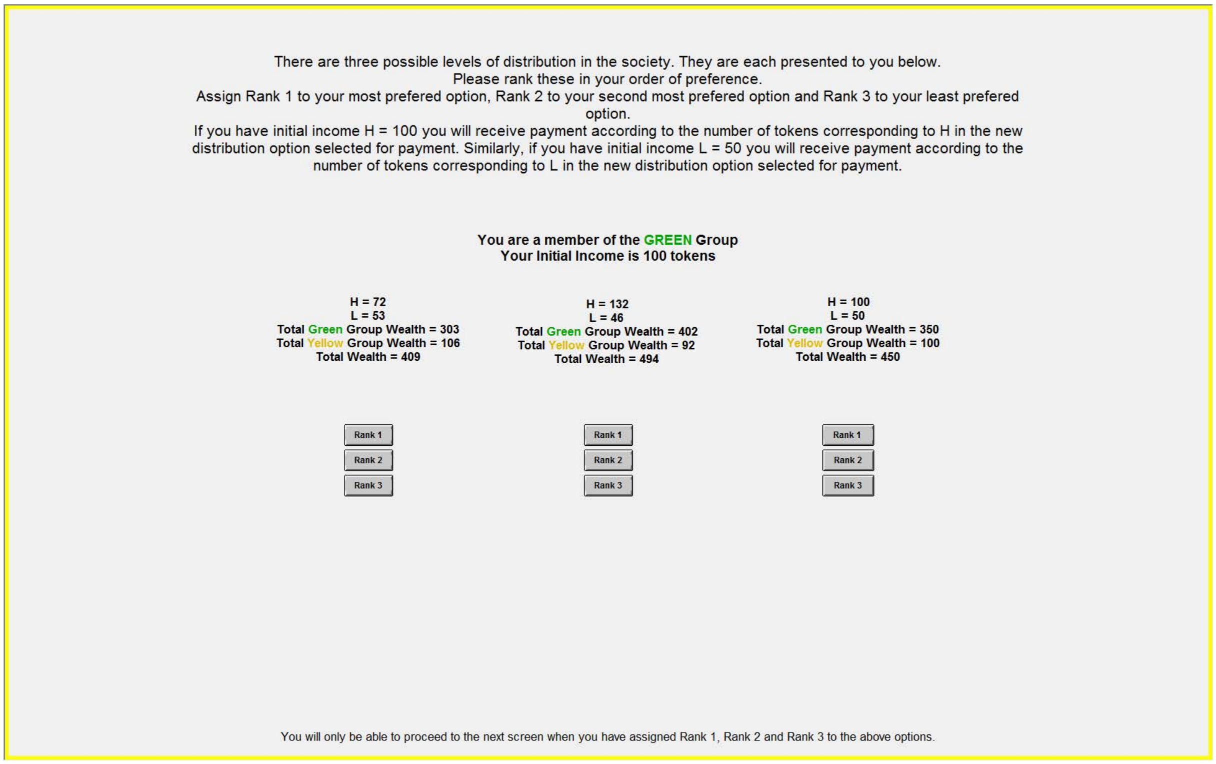

Figure A2: Stage 2 Redistribution Preferences: Group Identity

Remember that in this experiment: 25 tokens = £1

If you have any questions, please raise your hand and an experimenter will come to your desk.

Group ID is Assigned and revealed to the subjects.

Stage 3

In Stage 3, you keep the same income level as you had in Stage 1 and Stage 2 and the same group you have in Stage 2. Stage 3 consists of 9 elections.

Everyone in the society now participates in an election. Each person makes a decision as a potential candidate and as a voter.

First, as a potential candidate, you will be asked to select two distribution options: one is the option you would choose to campaign on if your opponent is a member of your own group: the Green/Yellow Group and the other is the option you would choose to campaign on if your opponent belongs to the other group: the Green/Yellow Group.

Second, after everybody has selected their two options as potential candidates, you will be asked to vote in a series of potential elections between different pairs of distribution options. There are 12 possible pairs of options.

Third, you will receive feedback on the outcomes of the 12 potential elections.

At the end of the experiment, one election will be selected at random for payment. In that election, two candidates will be selected randomly and the votes corresponding to the policies that those candidates selected will be counted. The distribution option that wins the majority vote in the selected election will be implemented.

In addition, if you are the selected candidate and you won the election, you will receive an additional payment of 70 tokens

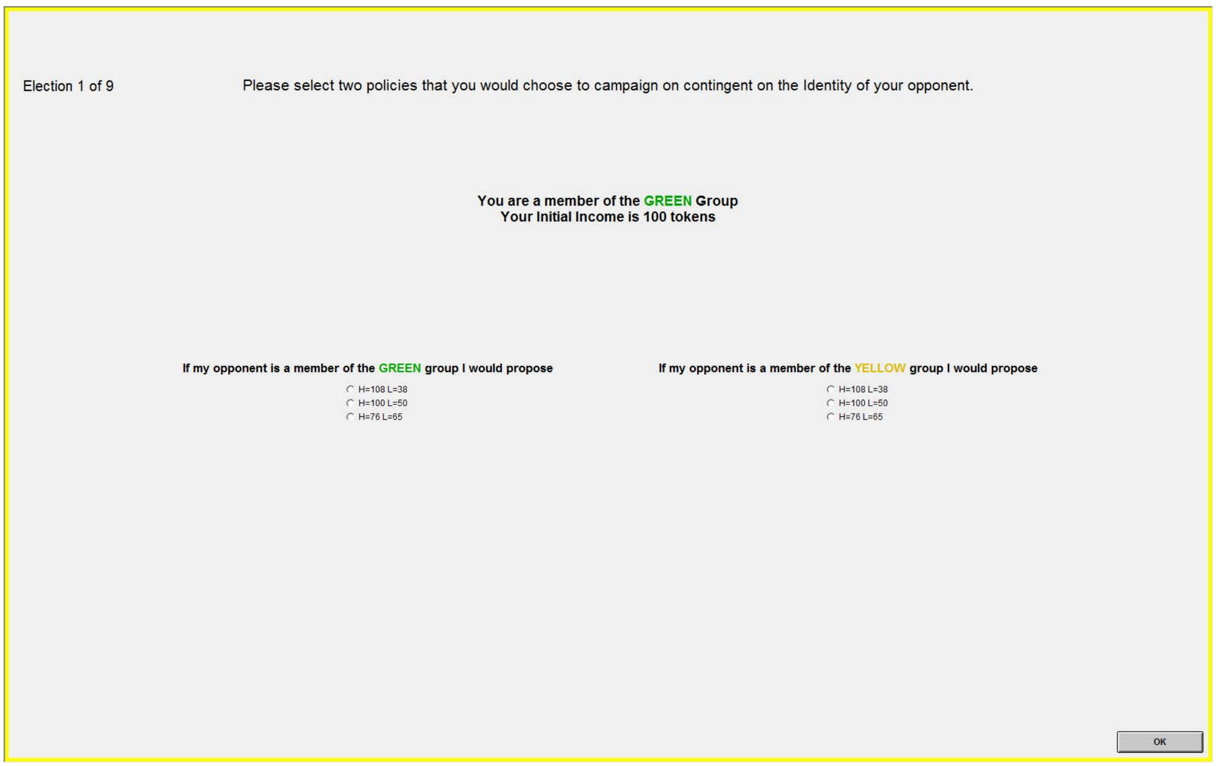

Figure A3: Candidate Policy Selection: Group Identity

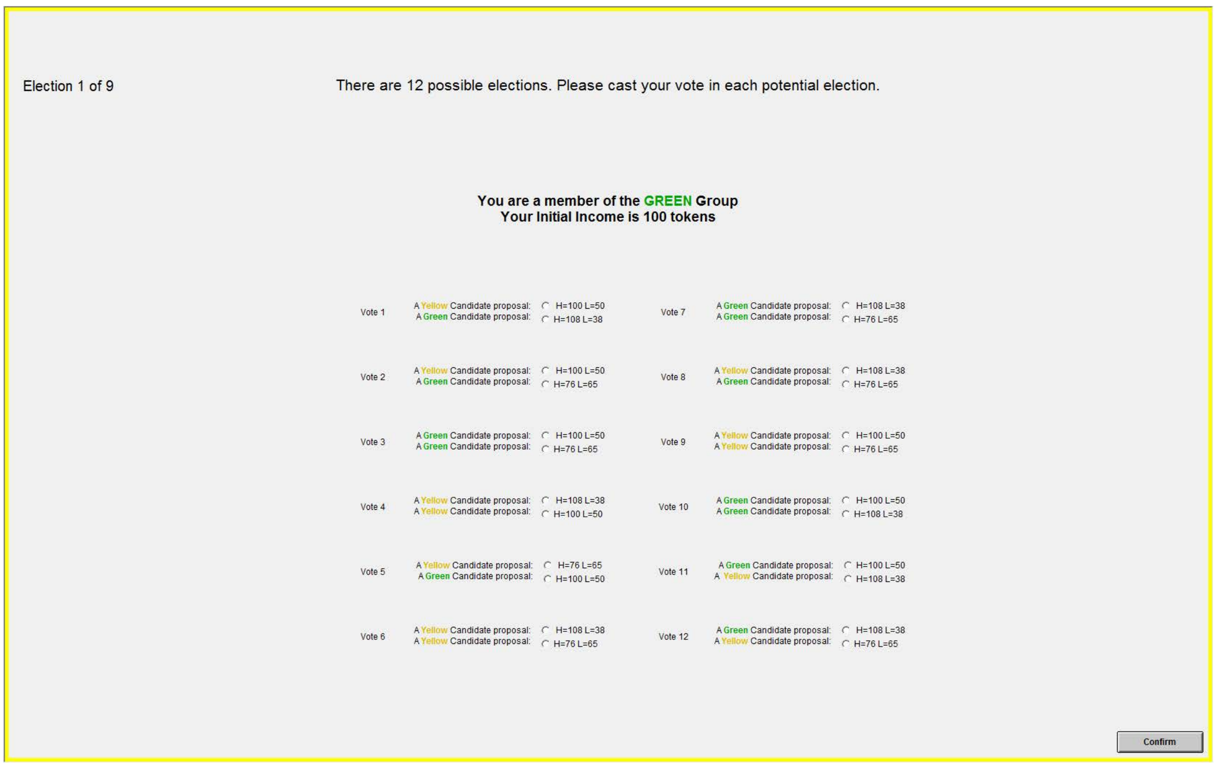

Figure A4: Pairwise Voting: Group Identity

Figure A5: Feedback: Group Identity