Experiments on Social Identity

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To my family.

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by

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Abstract

This thesis consists of three chapters examining the effects of social identity in an experimental framework.

The first chapter uses a novel subject pool, registered members of British political parties and subjects play the Ultimatum Game. Proposers and responders make offers or state their minimum acceptable offer (MAO) conditional on the political identity of the second player. Additionally we determine how behaviour in the ultimatum game is affected when proposers have the opportunity to earn additional income prior to a system of taxation and redistribution. We find that proposer's offers are significantly larger when paired with an in-group responder and responder's MAOs are significantly lower when paired with an in-group proposer. This is robust to the inclusion of earnings and redistribution.

The second chapter uses a laboratory experiment where we determine the effects of social identity on coordination and efficiency in a weakest-link game with endogenous linking. We find that endogenous linking alone increases efficiency through the exclusion of low effort players. Subjects are able to coordinate on a high effort level and maintain a highly connected network. However, the addition of social identity neither helps nor hinders this. As such we conclude that in problems of coordination with endogenous linking, social identity considerations are eclipsed by efficiency.

The final chapter considers social identity as a factor in determining the level of redistribution in society. We create two social identity groups where one group is numerically and economically dominant and then compare behaviour to a baseline treatment where social identity is absent. In both treatments we elicit subjects preferences for redistribution before they behave both as candidates and as voters in an electoral setting. Additionally we vary the equity-efficiency trade-off under which subjects make their decisions. We find that elections with only minority candidates result in the highest level of redistribution. Subjects are more likely, both to vote and to propose higher levels of redistribution under these candidates. However preferences for high redistribution are greatest when the equity-efficiency trade-off is positive, that is, when more equality is more efficient.

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Declaration

Chapter 2 is joint work with Ali al-Nowaihi and Sanjit Dhami. Authors are listed alphabetically. An early version of the chapter was presented at the Midwest Political Science Association 2016.

Chapter 3 is sole authored.

Chapter 4 is joint work with Shaun Hargreaves Heap, Konstantinos Matakos and Dimitrios Xefteris. Authors are listed alphabetically.

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Chapter 1

Introduction

The volume of experiments analysing the effects of social identity on behaviour and interactions between groups has increased exponentially in recent years. In this thesis we seek to incorporate social identity in new and novel ways into the experimental literature. Using a combination of laboratory and lab in the field experiments, we determine the effects of social identity on prosociality, efficiency and redistribution.

The central tenet of social identity theory is that people behave differently when interacting with members of their own group than they do when interacting with members of the out-group (Akerlof and Kranton, 2000). This insider/outsider distinction is often referred to as in-group bias. We use this well-established phenomenon to explore how the presence of social identity affects behaviour.

In Chapter 2 we utilise a novel subject pool, registered members of British political parties. Subjects play a lab in the field ultimatum game. The experiment consists of two treatments, the first is a standard ultimatum game, whilst the second is a modified version of the ultimatum game. In the second treatment proposers have the opportunity earn a higher endowment, thus creating an entitlement effect over proposer endowments. This earned income is then subject to taxation and a portion of that taxed income is redistributed to the responder. Under each treatment the subjects make six decisions, one when faced with a second player who's political identity is not revealed and the remaining five when faced with a second player whose political identity is known. Proposers are asked to state, independently, their offers in each of the six decisions, whilst from responders we ask for their Minimum Acceptable Offer (MAO). Thus, we are able to utilise the strategy method to maximise the number of data points.

We find a significant cost of belonging to a political party. Consistently responders whose political identity is not revealed receive higher offers from proposers than do any responders whose political identity is known. We additionally find that within the political identity framework, responders belonging to the Conservative Party or UKIP face a significant reduction in proposer offers in comparison to those made to left wing responders. Overall there is a preference for in-group members. This takes the form of higher offers on behalf of the proposers to in-group responders, whilst the responders state a lower MAO when matched with an in-group proposer. We are also able to show that like the proposers, responders state a lower MAO when the political identity of the proposer is absent, again suggesting a cost to belonging to a political party.

The results under the modified ultimatum game with earned income and fiscal redistribution reflect a similar pattern to those found in our standard ultimatum game. However, we see that as a proportion of the endowment, proposer's offers and responder's MAOs are significantly reduced. Strikingly the proportion that proposers reduce their offers by is almost identical to the percentage reduction in responders MAO. This suggests a shared understanding of the value of earning ones endowment and the effect that taxation and redistribution has on earned income, indicating that a social norm may be at play for both proposers and responders.

In Chapters 3 and 4 we conduct laboratory experiments. Chapter 3 takes the weakest-link game where we allow subjects to choose with whom they link. In addition to the presence of endogenous linking we induce social identity using a minimum group paradigm as in Chen and Li (2009). Subjects participate in one of three treatments. A baseline (BT) where subjects are linked with everyone and there is no social identity. A neighbourhood treatment (NT) where mutual consent is required for subjects to link and a final treatment where we incorporate social identity into the neighbourhood treatment (NT-SI). This chapter seeks to address the effects that social identity has on efficiency when subjects are able to choose with whom they link. The introduction of social identity in an endogenous linking framework could have ambiguous effects on efficiency. This is because endogenous linking allows for the possibility of excluding low effort players, whilst social identity may hinder trust amongst the group, reducing subject's overall preferences for linking. As such efficiency could deteriorate in the face of social identity.

Both BT and NT are direct replications Riedl et al. (2016), whilst the third treatment, as far as we are aware, is a new addition to the experimental literature. Consistent with previous findings, subjects in BT coordinate on low effort levels, thus subjects suffer inefficient outcomes. In NT subjects make simultaneous effort and linking decisions. Links can only be formed if both subjects mutually consent to link. Subjects are able to increase both the average and minimum effort levels exerted and by the final rounds most groups are able to coordinate on the highest effort levels, whilst also maintaining a highly connected network. Endogenous linking provides a mechanism through which it's possible to punish subjects exerting a low effort. Thus NT results in greater levels of coordination on a higher effort level, increasing efficiency compared to BT. The final treatment includes an initial stage where subjects make other-other allocations in the style of Chen and Li (2009) before following the procedures of NT. We find very strong and significant effects of group identity in the other-other allocations, however upon commencing the weakest-link game these effects disappear. Subject's decisions in NT-SI are indistinguishable from those in NT, with the exception of the decisions made in the first round of play. Initially, subjects in NT-SI have a preference for linking with members of their own social identity group. This social identity effect is however overcome by market considerations by the second round in the weakest-link game. Overall endogenous linking increases effort levels without significantly reducing the number of links a subject forms. These results are robust to the inclusion of social identity. This suggests that whilst social identity may be a salient feature in many games, for the weakest-link game financial/efficiency considerations very quickly take president over those of social identity.

The final chapter reports on an experiment which seeks to determine the effects of social identity on redistribution in an electoral setting. The experiment consists of a Baseline and a Social Identity Treatment. The Treatment uses a minimal group paradigm to randomly allocate subjects to a group identity. The social identity groups are constructed so that there exists a minority group that consists of only poor subjects and a numerically dominant group with both rich and poor subjects. Whilst the Baseline only has rich and poor subjects.

Subjects in the first stage take part in dictator style allocations. Subjects rank three possible redistribution options in their order of preference (with one option the status quo, (the exogenously determined income of the rich or poor), one option more redistributive and the third, less redistributive). This is repeated three times where the distributional choices either have the standard negative relationship between equity and efficiency, no relationship or a positive one where more equality creates more efficiency. The Treatment goes on to repeat this stage after the introduction of identity. The subjects then have the opportunity to behave both as candidates and as voters in a two candidate electoral setting. Subjects first must make redistributive proposals as candidates, before voting on each possible pairwise policy combination. In the Baseline subjects make one policy proposal, for subjects in the Treatment proposals are made conditional on the social identity of their opponent.

We find that in the Treatment poor members of the numerically dominant group have preferences for redistribution that are affected by the introduction of group identity. These subjects prefer levels of redistribution that are lower than the poor individuals in the Baseline, thus social identity works to weaken preferences for redistribution. As such we find that subjects care about their own group payoffs in addition to their individual payoffs. Voting behaviour is consistent with the desire to have an in-group member win the election in mixed group contests and with rewarding character in elections between candidates of the same group. Where we define character as a measure of sincerity of the candidates proposal. As a result, we find that redistribution is weakest in elections where both candidates belong to the dominant group and strongest in those elections where candidates come from the minority group. Thus, we conclude that the participation in elections of minority candidates critically affects the degree of redistribution observed in democracies.

Chapter 2

Prosociality, Political Identity and Earned Income

2.1 Introduction

Research on social identity theory is an active area of research within the social sciences. Evidence strongly suggests that people identify with social categories; social identity refers to ones social category (e.g. Protestant or Catholic, Democrat or Republican, African-American or Asian-American, black or white). Members of the same social category typically have shared norms of behaviour that they expect others in their social category to conform to. Such norms may be enforced by punishments or sanctions, or by the self-esteem individuals derive from conforming to them, or perhaps because they are hard-wired by evolution to do so (Tajfel, 1970; Tajfel and Turner, 1979, 1986; Gintis, 2009; Turner and Reynold, 2010). Different social contexts may trigger different identities a family identity, a regional identity, or a national identity (Turner et al., 1987).

The three main components of social identity theory may be summarised as follows (Dhami, 2016, Ch. 7). (i) Categorisation: People classify into the relevant categories. (ii) Identification: People identify with the norms and characteristics of their category. Members of the same category are termed as in-group members and members of other categories as out-group members. Identification typically involves favouring the in-group members over the out-group. (iii) Social comparisons: People compare their own group to other groups on some criteria.

In this paper we use a lab in the field ultimatum game to study the effects of political identity on social preferences. We also allow for the formation of entitlements by differentiating treatments in which income is earned and taxed, or not. There are very few papers on political identity due perhaps to the difficulty in getting access to registered party members.[1] We construct a novel data set in which the subjects are registered members of British political parties. We are interested in the prosociality of offers that proposers make to responders when the latter can be classified as in-group or out-group members based on their political affiliation. We allow for several identities, an anonymous identity and 5 possible political identities: Green, Conservative, Labour, Liberal Democrat and UKIP (UK Independence Party).

Our experimental design ensures that each of the components of social identity theory are present. Subjects classify themselves into their political identities by choosing to become members of political parties and by paying a membership fee (categorisation). Through their decisions made in the ultimatum game (as proposers and responders), they engage in identification and social comparison with subjects from different political identities.

We now consider the nature of our paper relative to the literature in more detail.

2.1.1 Minimal or Social Group Identity?

In many classic experiments on social identity, individuals are primed for a minimal group identity (MG) that bears little resemblance to outside-the-lab identities. Nevertheless, even when primed for trivial identities, say, red and blue groups, group members favour in-group members over out-group members, which is the main prediction of social identity theory (Billig and Tajfel, 1973; Tajfel and Turner, 1979, 1986; McDermott, 2009). Discriminatory behaviour arising from social identities can give rise to cooperation among in-group members but also socially harmful outcomes towards out-group members such as intolerance, discrimination, and prejudice. Typically students tend to form the basis of the subject pool for experiments using the MG design (Eckel and Grossman, 2005; Fowler and Kam, 2007; Chen and Li, 2009; Guala et al., 2013).

[1] Such access, at least in the UK, is tightly controlled by party offices who are under no obligation to publish the details of individual party members.

Natural group identities created by association with actual social groups (SG) have received lesser attention. However, this area is rapidly growing. Applications with the SG design include: field experiments with native social groups (Henrich et al., 2001); Swiss army trainees (Goette et al., 2006); ethnic groups (Habyarimana et al., 2007); effects of wartime violence on social cohesion (Gilligan et al., 2013); effects of internal sanctioning on cooperative behaviour (Grossman et al., 2012); ethnic factors in judicial decisions (Grossman et al., 2016); and the effects of exposure to religious messages on egalitarianism and activism (McClendon and Riedl, 2015).

Our interest in this paper is on natural social group identities (SG) that are formed by the self-selection of individuals into registered members of British political parties. Members pay a membership fee and receive party political literature. As such, political identity for these individuals is very salient. Furthermore, we prime this identity even further in our experiments by asking subjects to state the strength of their political identity and asking them to play an ultimatum game with fiscal redistribution, a policy area on which most political parties take an active stance. Hence, our work would appear to have strong ecological validity.

2.1.2 Political Identity and Laboratory Experiments

An understanding of the effects of political identity on prosociality may be critical to gain better insights into many important issues. These include the determinants of regional and national redistribution, progressivity of tax rates, decisions made in federations when the centre and a state may be occupied by different political parties, and partisan political decisions in legislatures.

Despite the explosion of field and lab experiments on social identity, surprisingly little attention has been given to political identity.[2] Fowler and Kam (2007) run dictator game experiments with students. They find that dictators offer more to receivers with similar ideological views. Thus, political identity is important and political party affiliation as a form of social identity influences the actions of players.

[2] We are not referring to here, the survey-based studies on partisan attitudes, particularly based on US data (Green, 2004; Iyengar et al., 2012; Mason, 2014; Iyengar and Westwood, 2015). Survey data is self-reported and may be subject to well-known cognitive biases, while experiments, if they are run in an incentive compatible manner, are not subject to this problem. However, we do not doubt the efficacy of the survey method for many kinds of hypothetical questions (e.g. death, suicides or prohibitively large negative losses).

Although the dictator game is widely used, its results lack robustness to the inclusion of strategic elements. Thus, it may not be a particularly good game to test alternative theories that require even a modicum of strategic interaction (Fehr and Schmidt, 2006; Dhami, 2016).[3] Hence, to gain a better understanding of the effects of political identity on social preferences, we use an ultimatum game in our experiment. The ultimatum game is possibly the most widely replicated experimental game; it has been played on all continents, with different levels of stakes, and among different social groups (Camerer, 2003; Dhami, 2016).[4] Using the ultimatum game when players are primed for their social identity, one can check to see not only if proposers make more favourable offers to in-group responders but also if responders are less likely to reject the offers of in-group proposers (Mendoza et al., 2014).

2.1.3 Earned versus Unearned Endowments

In typical lab experiments on social preferences, the endowments are provided by the experimenter. Dictator game experiments have shown that the introduction of earned income to dictators may reduce the extent of their pro-social offers (Cherry et al., 2002; Cappelen et al., 2007; Levitt and List, 2007). In Oxoby and Spraggon (2008) receivers in a dictator game earn the endowments, increasing the amounts transferred by the dictator. Thus, property rights may impact on experimentally observed social preferences. However, much less is known about the importance of property rights on prosociality arising through earned income in ultimatum games. Lee and Shahriar (2017) find that as the earned income component of the proposers income increases, the responders rejection rate falls.

Existing experiments do not, however, examine social preferences in ultimatum

[3]When dictators perceive that they are being watched (pictures of eyes in the room), they tend to make more generous offers (Haley and Fessler, 2005; Bateson et al., 2006). These results do not survive in games with even minimal strategic elements such as in the trust game (Fehr and Schneider, 2010). Players in their role as dictators sometimes prefer to exercise moral wiggle room and exit the experiment with a lower payoff than they could receive if they had played the game (Dana et al., 2006; Dana et al., 2007). However, it has proved hard to replicate results on moral wiggle room in games where some strategic interaction is involved (Van der Weele et al., 2014).

[4]The main results are as follows (Dhami, 2016, Section 5.2). The mean offer is 30 - 40 percent of the endowment and the median offer is 40 - 50 percent of the endowment. There are rarely any unfair offers (say, less than 10 percent of the endowment) or over-fair offers (say, over 50 percent of the endowment). Low offers are rejected and the main reason for the rejections is that the responders feel that the offers are unfair. These results continue to hold with reasonable increases in the stake size, although at very high stakes, responders are willing to receive lower offers.

games in the presence of earned income and redistributive income taxation; this is the setting closest to the real world. Furthermore, political identity plays a central role in issues of redistribution. For instance, in the US, the Democrat party is typically identified as the party of higher taxes and higher redistribution while the Republican party is identified as the party of small governments, i.e., lower taxes and lower redistribution (Dhami, 2003).

In our experiment we have two treatments. In the standard ultimatum game, Treatment 1, the endowments are provided by the experimenter. In the modified ultimatum game, Treatment 2, we allow proposers to earn their endowment, which is subject to an income-tax. A proportion of the income-tax revenues are redistributed to the responder to mimic societal redistribution. Treatment 2 enables us to examine the commonly expressed concern that the degree of prosociality observed in the standard ultimatum game may be misleading because it ignores earned income and income taxation for redistributive purposes.

2.1.4 Main Research Questions and Findings

The discussion above leads us to the following motivating questions.

1. How important is political identity when we consider an experimental game with an explicit strategic element, such as the ultimatum game?

2. What are the implications of political identity for prosociality when we replace student subjects with a real world subject pool whose political identity is demonstrably salient (i.e., registered fee-paying members of political parties)?

3. An important question in experiments on social preferences is the source of the endowments (earned or not? taxed or not?). These issues ultimately relate to how much realism we wish our experimental findings to reflect.

Our main findings are as follows.

1. Proposers make relatively higher offers to responders of the same political identity (in-group favouritism). When we differentiate between right and left wing political identities of the responder, those with a left wing/left leaning identity receive higher offers.

2. When responders state their minimum acceptable offer (MAO), they state a

lower MAO when the proposer shares their political affiliation, again showing in-group favouritism. When we compare differences in MAOs to proposers of different political parties using Wilcoxon signed-rank tests, responders state a higher MAO when they are faced with right wing proposers.

3. In Treatment 2, where proposers earn their taxable endowments, they make significantly lower offers relative to Treatment 1, where endowments are unearned and untaxed. The MAOs of the responders also decrease significantly in Treatment 2, relative to Treatment 1. There appears to be a shared understanding between proposers and responders, as is required in social norms, in the following sense. The reduction in the actual amounts offered by the proposers in Treatment 2 (relative to Treatment 1) is almost identical to the corresponding reduction in the MAO of the responders.

Section 2.2 outlines our experimental design and describes the subject pool comprising of registered British political party members. Section 2.3 gives the experimental results, sequentially, for proposers and responders. Section 2.4 concludes. The experimental instructions can be found in Appendix A.

2.2 Subject Pool and Experiment Design

2.2.1 Subject Pool

We use a novel subject pool, registered members of British political parties, who play the ultimatum game in the role of proposer or responder (but not both). Registered members of British political parties have made the conscious decision to join a political party and are likely to be some of the most politically engaged/aware members of society. Their political commitment is reflected in the costs associated with a political party membership that must be renewed each year. Additionally, party members may attend political meetings and normally receive literature on party positions and topical political debates from time to time. These individuals are likely to possess a strong political identity and engage in politically motivated activities, such as voting in elections. To the best of our knowledge, this is the first time that this subject pool has been studied in experiments of this kind.

We contacted five of the most widely supported national political parties in England for access to their registered members.[5] The five parties were the Green Party, the Labour Party, the Liberal Democrats, the Conservative Party, and the UK Independence Party (UKIP). This constitutes a richer spectrum of political parties relative to the few studies using US data (see the introduction). We were unable to garner sufficient observations from the UKIP supporters, possibly due to their relatively smaller number, hence, in this paper we focus mainly on the other four parties.[6]

British political party membership is generally set up so that only the local party office has access to the members contact information for their area. Emails were sent from a University of Leicester email account to the local party office. The initial email included a detailed outline of the research and what the experiment would entail; an email reminder was sent in most cases. The emails also briefly explained features of experiments within economics such as incentives and anonymity.[7] Given the UK Data Protection Laws, we requested that the parties contact their members themselves, through an email, containing the link to our experiment. Since the experiment distribution takes place through emails sent out by the political party offices themselves, this may have a priming effect on political identity, increasing the salience of already existing political identities. This lends even further credence to the ecological validity our results for the predictions of social identity theory.

Respondents from political parties completed an online questionnaire using the survey platform Qualtrics. Participation in the experiment was voluntary.[8] Due to the nature of online experiments, it was not possible to completely control either

[5]Only the local offices of parties in England were contacted. This was due to the salience of national identities in Wales, Scotland and Northern Ireland that results in large support bases for the nationalist parties in each country. Initially we had intended to collect our data only from the Leicestershire area but we were unable to garner a sufficient number of subjects. For this reason, we chose to expand our sampling area across England, focussing primarily on large cities.

[6]At the time of contacting UKIP they had already suffered serious electoral setbacks in the 2015 UK general election, which might have reflected in the lack of interest in participating in our study.

[7]It is impossible to publish any experiments in economics journals without an incentive compatible design. Interestingly, in contrast to this requirement, many of those contacted were put off by the payments that would be made from the outcome of the ultimatum game, often citing a willingness to help without monetary incentives instead.

[8]All respondents were required to give their consent for participation, without which they could not proceed any further. Those who were unwilling to give consent were thanked for their time and offered inclusion into a lottery to win £10 (this occurred only once in the experiment and the subject that declined consent did not select into the lottery).

the environment in which the experiment was conducted or the demographics of those who self-selected into the experiment.[9] However, this is unavoidable given UK data protection laws and the fact that the participation decision is voluntary. An advantage of using registered political party members is that it allows for a more demographically diverse, and politically primed, subject pool relative to a standard lab experiment with student subjects.

Whilst our experimental design does not randomly sample from the entire population of political party members in England, the demographics of our subject pool broadly reflect that of the party membership on aggregate. Data on political party make-up is hard to obtain because different parties classify membership differently and are under no legal obligations to report their membership numbers, let alone the demographic make-up of their members. However, using a House of Commons Briefing Paper: Membership of Political Parties (2015) we are able to make broad comparisons. Other than education (our subjects are slightly more educated) our sample is representative of the general membership of political parties.

Data collection was a slow and arduous process as we did not have direct access to the subjects. The only method of recruiting subjects was to continue to write to party offices who in turn made the decision to forward our request (or not) to their party members. The response from the different political parties was uneven; there were only 3 subject responses from UKIP, which we eliminate from our sample, and the number of subjects from the Conservative Party are the lowest among the remaining parties. A major problem in getting access to data arose from our use of incentivised experiments. Political party offices, not versed with experiments, were extremely reluctant to offer access to their members on account of the monetary payment for decisions to be made to their party members. Further studies of this subject pool are likely to encounter the same problems.

Our use of the strategy method to elicit the responses of both proposers and responders in an ultimatum game, significantly expands the data we gather. As part of the strategy method, subjects, say, in their role as responders (respectively, proposers) are asked to state their minimum acceptable offer (respectively, offer)

[9]For instance, online experiments can only be taken by those with internet accesses and, thus, may not be applicable to all sections of society although there is near-universal access to the internet in England.

when the other player is of any of 5 different political identities. Due to the smaller number of right wing parties (UKIP and Conservatives), our data is subject to the caveat that it over-represents left and left leaning parties (Green, Labour and Liberal Democrat).

Additional and unavoidable problems arose during the lengthy data collection process: the BREXIT referendum occurred, David Cameron resigned as Prime Minister, Nick Clegg resigned as leader of the Liberal Democrats, Ed Miliband resigned as leader of the Labour Party in conjunction with many other political occurrences. As most of these events are related to the BREXIT referendum we use Mann-Whitney tests to determine whether our responses change significantly after this event, they did not and for this reason we choose to pool the data. We also include time dummy variables in our regression analysis to control for the date at which subjects participated in the experiment. In conjunction, these results show that social identity and prosociality were not affected by the other political events that occurred during the data collection process.

2.2.2 The Experimental Design

The details of the experimental design can be found in Appendix A. Here we briefly outline the main features. Subjects begin by answering some demographic questions (age, gender, education). They then state their political affiliation, and the strength of their affiliation on a 5 point Likert Scale from very strong (1) to very weak (5). The ultimatum game is explained to the subjects and they must correctly answer two questions designed to test their understanding in order to proceed further in the experiment. Subjects who correctly answer the test questions are assigned either the role of the proposer or the responder for the rest of the experiment. Subjects sequentially play the following two treatments.

Treatment 1: Subjects play a standard ultimatum game augmented to include to the role of political identity. The proposer is given an endowment of £10. The proposer first played an ultimatum game against a responder whose political identity was anonymous (no political identity was given), this constitutes the first sub-treatment. The strategy method is then used to elicit the offers that proposers make to a responder with the following 5 possible political identities: Conservative, Green, Labour, Liberal Democrat and UKIP (second sub-treatment).

We elicit the minimum acceptable offer (MAO) that subjects in their roles as responders demand from proposers with an anonymous political identity (no political identity given). We then use the strategy method to elicit the responders MAO against the following possible political identities of the proposer: Conservative, Green, Labour, Liberal Democrat and UKIP. This describes the two sub-treatments for the responder.

The strategy method allows us to elicit the complete strategy of each player and leads to a substantial increase in the number of data points (Bardsley et al., 2010). All decisions by proposers and responders were made using a slider task (see Figures A.1 and A.2 in Appendix A). In order to eliminate potential order effects, we undertook two precautions. (i) The order of the two sub-treatments for the proposer and for the responder were randomised. (ii) In the strategy method for the proposer and the responder, the order of the party affiliations (Conservative, Green, Labour, Liberal Democrat and UKIP) of the other player was also randomised.

Treatment 2: Subjects play a modified ultimatum game, in which the only difference from Treatment 1 is that (i) proposers earn their endowments, which are subject to an income-tax, and (ii) a part of the tax revenues are redistributed to the responder. As noted earlier, this is designed to improve the ecological validity of our experiments to reflect a realistic real world earnings scenario in which prosociality and the effects of social identity could be examined.

Proposers were initially given an endowment of £10 and then given the chance to earn an extra £10 by correctly answering at least 4 out of 5 simple arithmetic questions (95% of our proposers got at least 4 correct answers). The purpose of this exercise was to create an entitlement effect on earned income. The difficulty of the questions has been shown to be inconsequential. Hoffman and Spitzer (1993) suggest that merely announcing entitlements is sufficient to induce property rights over the endowment.

Furthermore, we implement a fiscal redistribution system within the game. Proposers are subject to an income-tax at a rate of 30% on their endowment and half the tax revenues are redistributed to the responder. The remaining 50% of the

Table 2.1: Subjects by Political Identity

Party \Role	Proposers	Responders
Conservative	19 Participants	15 Participants
Green	32 Participants	28 Participants
Labour	52 Participants	51 Participants
Lib Dem	34 Participants	37 Participants
Total	137 Participants	131 Participants

tax revenues are taken out of the experiment (this portion can be thought of as non-redistributive government expenditures). The fiscal redistribution is common knowledge to the proposer and the responder, enabling them to take it into account when making their decisions.

In both treatments, subjects are informed at the start of the experiment that they will be randomly matched with a second player (a responder or a proposer, depending on their role) and one of the actual decisions will be selected at random and used to determine their payoffs.

Each subject played both treatments using the strategy method, hence, the number of data points for each player is 2x6 = 12 (2 is the number of treatments and 6 is the number of identities of the other player including 5 political parties and one anonymous identity). The survey was completed within 20 minutes for all respondents and the average payment was £4.59. The number of subjects corresponding to each political identity is described in Table 2.1.

We did not randomise between the two treatments (although we randomise between sub-treatments as explained earlier) for two reasons. (i) In Treatment 1 no tax is deducted while in Treatment 2 a 30% income tax is deducted. If we had played Treatment 2 first, then moving from Treatment 2 to 1, subjects might have been subject to a house money effect. (ii) Treatment 2 is significantly more complicated than Treatment 1 because it involves taxation and redistribution of income. As such, we are likely to get more accurate responses if subjects learn to play the simpler, Treatment 1, first.

Table 2.2: Descriptive Statistics: Proposer Offers

Proposer Offers	Anon	Green	Labour	Lib Dem	Con	UKIP
Treatment 1						
Mean	0.47	0.43	0.43	0.42	0.37	0.30
Median	0.50	0.50	0.50	0.50	0.49	0.30
Treatment 2						
Mean	0.39	0.37	0.37	0.38	0.31	0.26
Median	0.39	0.39	0.39	0.39	0.35	0.29

Table 2.3: Wilcoxon signed-rank tests: Proposer Offers

	Green	Labour	Lib Dem	Con	UKIP
Proposers					
Anon	-0.04***	-0.03**	-0.05***	-0.09***	-0.17***
Green	-	0	-0.01	-0.05***	-0.13***
Labour	-	-	-0.02	-0.06***	-0.13***
Lib Dem	-	-	-	-0.04***	-0.12***
Con	-	-	-	-	-0.08***
Proposers-Taxation					
Anon	-0.02	-0.03	-0.02	-0.08***	-0.14***
Green	-	0.07	0	-0.06***	-0.11***
Labour	-	-	0	-0.06***	-0.11***
Lib Dem	-	-	-	-0.07***	-0.12***
Con	-	-	-	-	-0.05***

Note: Wilcoxon signed-rank tests to test the pairwise differences of average proposer offers to responders of two different political identities, the column responder identity minus the row responder identity. Null Hypothesis: No difference in the offers made by proposers to a responder with a column identity and a responder with a row identity. All tests are two sided. Stars denote significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

2.3 Experiment Results

In this section, we present our results and demonstrate significant effects of political identity in determining proposer offers and the MAOs of responders.

2.3.1 Proposers

Table 2.2 gives the summary data for the offers made by proposers to each type of responder. The mean and median offers by proposers fall within the usual range observed in other ultimatum game experiments. Proposers offering over 90% of the endowment are clear outliers (less than 1.1% of total offers). All offers over 90% were to in-group members.

Table 2.3 uses Wilcoxon signed-rank tests to test for pairwise differences in the average proposer offers, as a percentage of their post-tax endowments, made to a responder with two different identities: the column identity minus the row identity. The post-tax endowment of a proposer who has an endowment of £20 is 20(1-0.3) = £14. Positive (respectively, negative) values, therefore, indicate a relatively higher offer to the column (respectively, row) responder. Thus, the very first number in the Table 2.3, in the north-west corner (-0.04) shows the difference in the average offer of the proposer to a responder of the Green Party relative to a responder with the anonymous identity (Anon), expressed as a percentage of the post-tax endowment of the proposer.

Consider the difference in offers from proposers to an anonymous responder, relative to a responder with any of the 5 political identities. We are able to reject the null hypothesis that these are equal for (1) all possible cases in Treatment 1 (see the top row of numbers in Table 2.3), and (2) in Treatment 2 when the column identity of the responder is a Conservative or UKIP member (see the last two numbers in the first row following Treatment 2 in Table 2.3). These differences are negative (and significant in 7 out of 10 cases) which shows that proposers offer less to a responder of any political identity relative to a responder with no political identity (Anon).

Result 2.1: On average, and not controlling for the political identity of proposers, relatively higher amounts are offered to a responder with an anonymous political identity relative to a responder with a political identity.

One possible explanation for Result 2.1 is that for subjects whose political identity is salient, other political parties may be viewed as competitors, as in the case of competition for votes in elections. Hence, a lower amount is offered to members of other political parties.

In Result 2.1, we only consider average offers across all proposers and do not control for the political identity of the proposer. Do proposers also make a smaller offer to a responder of their own affiliation relative to an Anon responder? When we consider the data on proposers disaggregated by political parties, Liberal Democrat proposers offer more to their in-group responders and the difference is statistically significant at the 5% level. The difference is also positive for proposers belonging to the Green Party, although it is significant only in Treatment 2 (at the 1% level). The differences in offers made to Anon and in-group responders are not statistically significant for proposers belonging to any other political party.

Let us omit the row for the Anon identity in Table 2.3 for the moment. Of the remaining data shown in Table 2.3, the numbers in the last two columns are statistically significant and negative, while none of the other numbers are significant. Thus, responders from either the Conservative Party or UKIP are made a lower offer. Conservative responders are made offers by proposers that are on average 6.5% lower than all other parties. Offers to Conservative responders are only higher relative to UKIP responders (8% higher in Treatment 1 and 5% higher in Treatment 2).

Result 2.2: Proposers offer less of their endowment to right wing responders compared to the offers made to left/left-leaning responders or anonymous responders.

A possible explanation for Result 2.2 is as follows. If proposers make relatively higher offers to in-group responders (see Result 2.3 below), then the smaller number of Conservative and UKIP proposers in our sample would have biased our results to reduce mean offers to responders from these two parties. Without additional data, we cannot be sure if Result 2.2 would be robust to a large sample. For this reason, we treat Result 2.2 as provisional. This would be an interesting question for future research to take up.

To allow for a closer examination of the effects of political identity in the ultimatum game, we run 6 OLS regressions that are reported in Table 2.4. We omit the anonymous identity here because we are interested in the in-group/out-group effects of political identity and the effects of fiscal redistribution on optimal offers by proposers. Each proposer makes 10 allocation decisions, one offer to each of the 5 political identities of the responder in each of the 2 different treatments, Treatment

1 and Treatment 2. We have 137 proposers in the sample, giving 1,370 observations on offers in total.

The dependent variable is the proposer offer to the responder as a percentage of their after tax endowment. Amongst our independent variables we include a dummy variable 'Own' that takes the value of 1 if the responder is of the same political identity as the proposer, and 0 otherwise. This variable allows us to explore the classic in-group/out-group effects in social identity theory. Additionally, recall that we have four categories of political identity (Conservative, Green, Labour and Liberal Democrat) after omitting UKIP. Using the category Conservative as our benchmark, we also use 3 dummy variables to control for political identity of the proposer. 'Green' equals 1 if the proposer is affiliated with the Green Party and 0 otherwise; 'Labour' equals 1 if affiliated with the Labour Party and 0 otherwise; and 'Lib Dem' equals 1 if Liberal Democrat and 0 otherwise. These variables allow us to examine the size of the offers by proposers of alternative political parties, relative to the benchmark of a Conservative proposer. The dummy variable 'Strength' gives the self-reported feelings of belonging to a political party, where 1 is the highest possible strength and 5 the lowest. This variable allows us to examine whether behaviour is influenced by how strongly one identifies with ones political identity. The dummy variable 'Entitlement' captures treatment effects. It takes a value 1 for Treatment 2 and value 0 for Treatment 1.

We also control for a range of demographic factors such as age, gender, and education alongside the date at which the respondent participated in the experiment. To control for the date the experiment was taken, we incorporate $T-1$ dummies, using the first day of the experiment as the benchmark. This results in a set of dummies where responses in day $t = 2$ are coded as 1 and 0 otherwise, another dummy records responses in day $t = 3$ as 1 and 0 otherwise. This continues until time $t = T$, the final day the experiment was ran. In effect we add time fixed effects to our regression analysis.

From the first row in Table 2.4 (see variable labelled 'Own'), proposers make significantly higher offers to responders who are of the same political identity (in-group members) as compared to responders with a different political identity (out-group members). These effects are robust to additional controls and are

Table 2.4: OLS Regressions: Proposer Offers

	(1)	(2)	(3)	(4)	(5)	(6)
Own	0.117***	0.117***	0.117***	0.117***	0.117***	0.114***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Green		0.129***	0.138***	0.138***	0.140***	0.141**
		(0.042)	(0.042)	(0.042)	(0.044)	(0.062)
Labour		0.094**	0.100***	0.100***	0.107***	0.121**
		(0.036)	(0.036)	(0.036)	(0.038)	(0.057)
Lib Dem		0.089**	0.094**	0.094**	0.095**	0.087
		(0.038)	(0.037)	(0.037)	(0.039)	(0.062)
Strength			-0.017	-0.017	-0.017	-0.017
			(0.014)	(0.014)	(0.013)	(0.016)
Entitlement				-0.053***	-0.053***	-0.054***
				(0.009)	(0.009)	(0.010)
Constant	0.339***	0.251***	0.277***	0.304***	0.323***	0.416***
	(0.013)	(0.034)	(0.042)	(0.043)	(0.060)	(0.088)
Demographics	No	No	No	No	Yes	Yes
Date	No	No	No	No	No	Yes
N	1,370	1,370	1,370	1,370	1,370	1,370
Subjects	137	137	137	137	137	137

Note: Dependent variable in each of the six reported regressions is the offer made by the proposer. Standard errors in parenthesis are clustered at the subject level. Demographic controls include age, gender and level of education. Significance levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

significant in all six regressions. On average, proposers transfer 11.65% more of their endowment to an in-group responder relative to an out-group responder. The inclusion of variables that control for the political identity of the proposer show that, compared to a Conservative proposer all other political affiliations offer a higher proportion of their endowment to the responder. The addition of the 'Strength' variable does not affect the in-group favouritism that proposers exhibit. This suggests that the degree of in-group favouritism is not affected by the strength of the proposers identification with their party.

Result 2.3: Proposers offer a higher proportion of their endowment to responders who share a common political identity, relative to a different political identity. This confirms the classic finding in social identity theory that in-group members

Table 2.5: Descriptive Statistics: Responder MAOs

Responder MAOs	Anon	Green	Labour	Lib Dem	Con	UKIP
Treatment 1						
Mean	0.41	0.41	0.39	0.41	0.46	0.49
Median	0.49	0.49	0.49	0.49	0.5	0.50
Treatment 2						
Mean	0.36	0.36	0.34	0.37	0.39	0.43
Median	0.36	0.36	0.36	0.36	0.38	0.39

are treated more favourably than out-group members.

One key element of our experimental design is that we are able to examine the effects of earned income and taxation on prosociality, through our dummy variable 'Entitlement'. This variable is negative and significant in all regressions where it is used. Thus, proposers reduce their offers to responders significantly when they earn their taxable endowments. Independent confirmation for this is found when we use a Wilcoxon signed-rank test to test the difference in offers between Treatments 1 and 2 for a proposer of each political party. When making an offer to a responder of the same party; average offers are significantly lower in Treatment 2 ($p < 0.000$ for each pairwise comparison). However, Treatment 2 (taxable earned endowment) does not reduce the effect of social identity in proposers offers in terms of in-group favouritism.

Result 2.4: The introduction of taxable earned income significantly reduces the average offers made by proposers.

2.3.2 Responders

In this section, we analyse the minimal acceptable offers (MAOs) of the responders and its correlates. Table 2.5 gives the summary data for the MAOs by responders as a percentage of the after-tax income of the proposers to make these figures comparable with Table 2.2.

In Treatment 1, the median MAO as a percentage of the proposers endowment

Table 2.6: Wilcoxon signed-rank tests: Responder MAOs

	Green	Labour	Lib Dem	Con	UKIP
Responders					
Anon	0	-0.01	0	0.06***	0.09***
Green	-	-0.02	0	0.05*	0.08***
Labour	-	-	-0.02	0.07***	0.10***
Lib Dem	-	-	-	0.05**	0.08***
Con	-	-	-	-	0.03***
Responders - Taxation					
Anon	0	-0.01	0.01	0.04**	0.07***
Green	-	-0.02	0.01	0.04*	0.07**
Labour	-	-	0.03	0.05**	0.09***
Lib Dem	-	-	-	0.02	0.06***
Con	-	-	-	-	0.04*

Note: Wilcoxon signed-rank tests to test the pairwise differences of average responder MAOs from proposers of two different political identities, the column proposer identity minus the row proposer identity, as a percentage of the proposers income. Null Hypothesis: No difference in the MAOs made by responder to a proposer with a column identity and a proposer with a row identity. All tests are two sided. Stars denote significance levels; * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

across all possible political identities of the proposer is almost 50%; thus responders demand an equal share of the proposers income. However, in Treatment 2, following the introduction of earned income and taxation, the median MAO as a fraction of the proposers after-tax income is significantly reduced. In contrast to the results for proposers, we have that for responders there is very little variation in MAOs when faced with proposers of different political identities; this result holds for both treatments. Our data is unable to distinguish whether the reduction in mean and median MAO from Treatment 1 to Treatment 2 is due to (i) earned income alone, (ii) fiscal redistribution alone, or (iii) the combination of (i) and (ii).

Table 2.6 uses Wilcoxon signed-rank tests to test for pairwise differences in the average responder MAOs, as a percentage of the endowments of the proposer, made to a proposer with the column identity minus the offer made to a proposer with the row identity. Positive (respectively, negative) values indicate a relatively higher MAO from the column (respectively, row) proposer. Thus, the very last number in the first row of Table 2.6 in the north-east corner (0.09) shows the difference in the average MAO of the responders to a proposer of UKIP relative to a proposer with the anonymous identity (Anon).

We only find significant pairwise differences in the MAOs of the responder when one the proposers has either a Conservative or UKIP identity, higher MAOs are required from such proposers. Thus, without controlling for the identity of the responder (Table 2.6 reports averages across responders of all possible political identities), we again see a bias against the right wing groups. As in the case of Result 2.2, this result may be driven by the smaller number of data points that we have for right wing political parties, and so its robustness needs to be tested by future research.

Result 2.5: The average MAOs of responders, when we do not take account of the political identity of the responders, are significantly increased when the proposer has a right wing political identity.

We now run OLS regressions on the MAO of responders which parallel our regression analysis for the proposers. The dependent variable is now the MAO of responders, expressed as a percentage of the proposers post-tax endowment and the party dummy variables now represent the political identity of the responder. All other explanatory variables are identical to those included in the proposer regressions and have been explained above.

As was the case for proposer offers, we find that 'Own' is statistically significant in all regressions. Responders consistently state a lower MAO when they share a political affiliation with the proposer. Unlike the proposers, only Liberal Democrat responders state a significantly different MAO to a Conservative responder, in this case MAOs are lower from Liberal Democrats than they are from the Conservatives. We again find that the treatment dummy, 'Entitlement', is negative and significant at the 1% level, which suggests that responders state lower MAOs (as a percentage of the proposers post-tax endowment) from proposers when the incomes of proposers are earned and taxed.

The findings on social identity for responders are summarised in the next result.

Table 2.7: OLS Regressions: Responder MAOs

	(1)	(2)	(3)	(4)	(5)	(6)
Own	-0.067***	-0.067***	-0.067***	-0.067***	-0.067***	-0.067***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Green		-0.058	-0.067	-0.067	-0.061	0.010
		(0.041)	(0.044)	(0.044)	(0.042)	(0.069)
Labour		-0.014	-0.023	-0.023	-0.014	0.081
		(0.035)	(0.037)	(0.037)	(0.037)	(0.074)
Lib Dem		-0.082**	-0.091**	-0.091**	-0.081*	0.019
		(0.038)	(0.041)	(0.041)	(0.041)	(0.061)
Strength			-0.018	-0.018	-0.015	-0.016
			(0.016)	(0.016)	(0.016)	(0.019)
Entitlement				-0.054***	-0.054***	-0.054***
				(0.011)	(0.011)	(0.011)
Constant	0.418***	0.460***	0.499***	0.526***	0.445***	0.325**
	(0.015)	(0.028)	(0.050)	(0.051)	(0.069)	(0.130)
Demographics	No	No	No	No	Yes	Yes
Date	No	No	No	No	No	Yes
N	1,310	1,310	1,310	1,310	1,310	1,310
Subjects	131	131	131	131	131	131

Note: Dependent variable is Responders MAO. Standard errors in parenthesis are clustered at the subject level. Demographics controls include age, gender and level of education. Significance Levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Result 2.6: The responders MAOs as a percentage of the proposers post-tax endowment are significantly lower when the proposer is an in-group member compared to when the proposer belongs to the out-group.

Result 2.6 shows that issues of social identity are significant for responders as well as proposers. However compared to proposers, MAOs are not as affected by the political identity of the decision maker. Strikingly, as one moves from Treatment 1 to Treatment 2, the amount that the responders reduce their MAO by is almost equal to the amount by which the proposers reduce their offers in all regressions, both expressed as a percentage of the proposers post-tax endowment. In conjunction, these results suggest that there might be a shared understanding (possibly a norm) of how much the responder is entitled to in the presence of the proposers entitlements to income.

2.4 Conclusion

In this paper, we conduct a lab in the field experiment using the ultimatum game with registered members of British political parties, to study the influence of social identity on prosociality. Furthermore, we distinguish between unearned-untaxed income and earned-taxed income in two different treatments in a novel experimental design.

We find symmetric effects for proposers and responders. Proposer offers are significantly reduced when responders belong to a different political identity (out-group members) relative to their own political identity (in-group members). This is the classic result in social identity theory. In parallel, responders when stating their minimum acceptable offers (MAOs) from the proposer consistently state a lower MAO when matched with a proposer of a shared identity. However for proposers we find that their offers are conditional on their political affiliation. Compared to Conservative proposers Green, Labour and Liberal Democrat members make significantly higher offers. For the responders we see only a difference for the Liberal Democrats, whose MAOs are significantly lower than those made by a Conservative responder.

The decisions of both proposers and responders are highly sensitive to treatment effects. In Treatment 1, the endowments are unearned and untaxed, while in Treatment 2, the endowments are earned and taxed. A part of the tax revenues in Treatment 2 are used to redistribute income to the responders. Proposer offers are reduced significantly as one moves from Treatment 1 to Treatment 2. Interestingly the MAOs stated by the responders also fall almost by the same amount on average as the fall in the offers by the proposers. This new finding suggests that there is a shared understanding of the appropriate MAO to ask for in the presence of earned and taxed income.

On average, when we do not control for the political identity of the proposer, lower offers are made by proposers to responders of right wing parties as compared to left wing parties. However, this result might partly or completely be driven by our smaller sample size of right wing parties and must be treated in a tentative manner; it needs to be checked for robustness in a larger sample size by future research. We find very little effect of demographic variables such as age, gender, and education on either the offers made by proposers or the MAOs stated by the responder. It is worth exploring why gender plays no role in our data when it plays a role in similar contexts elsewhere. Additionally it would be beneficial to add additional treatments that enable one to distinguish between the earned income effect and the effect of redistribution.

Chapter 3

Improving Efficiency in the Weakest-Link Game: Endogenous Linking or Social Identity

3.1 Introduction

Coordinating on an efficient outcome is a problem central to many contexts (Ochs, 1995), from improving efficiency levels in under performing organisations (Arrow, 1974; Bryant, 1983) to reducing the incidence of moral hazard in team production (Cooper and Ross, 1985). Coordination problems often arise with the presence of multiple Nash equilibria (Nash, 1950) which are possible to pareto rank. How to ensure coordination occurs efficiently has been studied extensively.

This chapter brings together two distinct literatures in an attempt to address the issue of coordination in the face of conflicting identities. Using endogenous linking and social identity to determine the effects on both efficiency and coordination, we are able to show that efficiency is unaffected by the inclusion of social identity. Players in weakest-link games typically struggle to achieve high levels of efficiency, choosing with whom one links can help overcome this phenomenon. However the addition of social identity could potentially hinder these effects. The presence of out-group members in the game has the potential to limit subjects preferences for linking, creating a bias against the out-group, thus reducing efficiency. To address this question we replicate the Baseline and Neighbourhood Treatments in Riedl et al. (2016) (henceforth, RRS) to ensure initially that endogenous linking does indeed increase efficiency relative to the weakest-link game with exogenous linking. Subsequently to determine the effects of social identity in a weakest-link setting with endogenous linking, we induce a minimal group identity in line with Chen and Li (2009) and confirm identity salience through the use of other-other allocations, before subjects participate in the weakest-link game.

Weakest-link games can be characterised by the pareto ranked set of Nash equilibria (Hirschleifer, 1983), typically coordination is not the problem, efficiency is. Experimental subjects are frequently observed to coordinate in the weakest-link game, but they do so on the lower effort levels (Van Huyck et al., 1990; Harrison and Hershleifer, 1989; Knez and Camerer, 1994; Weber et al., 2001). One possible solution put forward is the effect of group size. Di Girolamo and Drouvelis (2015) test the effects of both the gender make-up of the group and the group's size on the ability to coordinate efficiently. Whilst they find no difference in the ability to coordinate between single sex groups (male or female) and mixed groups they do show a significant effect of group size. Smaller groups find it easier to coordinate on more efficient outcomes. Similarly, Weber (2006) finds that this problem of group size can be overcome by beginning with a group of a smaller size and slowly adding entrants who are aware of the groups history of play. Whilst this certainly improves the probability of maintaining efficiency, unlike Di Girolamo and Drouvelis (2015), Weber (2006) finds that it does not do so with a reliable degree of certainty.

Alternatively, providing all subjects with more information has been shown to increase the levels of efficiency. Berninghaus and Ehrhart (2001) for example find that to increase exerted effort levels it is sufficient to provide subjects with the distribution of effort levels of the group. This yields the same result as providing subjects with individual specific effort levels. However, for the weakest-link game with exogenous linking one could argue that it is only the distribution of effort levels that is of importance, hence why they find that the two methods result in similar outcomes. Indeed individual specific effort levels are only of use when it is possible to exclude a particular subject. Whether subjects are provided with the distribution or individual specific effort levels, efforts are coordinated on a considerably higher effort level than merely providing subjects with the minimum effort exerted by the group (Van Huyck et al., 1990; Harrison and Hershleifer, 1989; Knez and Camerer, 2001). This mechanism stems from the fact that previously high effort individuals had been unable to determine if the low minimum effort was due to a single subject or if the low effort levels were ubiquitous. Information allows subjects to simply ride out the low effort levels when they are exhibited by only a small minority. A similar effect in Brandts and Cooper (2006) has been shown in the slightly different corporate turnaround game.

Whilst there have been numerous attempts to develop strategies for increasing efficiency and coordination in the weakest-link game, it is somewhat surprising to see little experimental research on endogenous linking in weakest-link games. One of the few papers to do so is RRS.[1] They find endogenous linking allows subjects to overcome the coordination failure of the standard weakest-link game. Subjects are able to coordinate effort on the highest pareto ranked equilibria and maintain full efficiency through maintaining the largest possible group size at the highest level of effort.

In addition to the weakest-link game this chapter draws on the social identity literature. In particular we follow the minimal group paradigm as laid out by Chen and Li (2009). Subjects are randomly allocated a colour identity and proceed to make other-other dictator allocations. Crucially the minimal group identity does not rely on outside influences and thus is entirely constructed as an endogenously formed identity within the laboratory setting. Thus allowing researchers to use this as a baseline for any further identity effects. Regardless of the somewhat arbitrary identities that this framework elicits, numerous studies have found that this is sufficient to affect subjects behaviour (Billig and Tajfel, 1973; Tajfel and Turner, 1979, 1986; McDermott, 2009). The addition of social identity can have important ef-

[1] Although there are few papers that look at the effects of endogenous linking in weakest-link games, there are a number of studies that examine the effects of endogenous linking in the public goods game (Cinyabuguma et al., 2005; Güth et al., 2007; Ahn et al., 2008; Ahn et al., 2009). Subjects are restricted by entry protocols, these enable significantly higher levels of coordination than restricted exit, free entry or free exit (Ahn et al., 2008). Ahn et al. (2009) show that restricted entry increases cooperation and allows group sizes to form which result in optimal payoffs. Similarly Charness et al. (2014) allow for endogenous group formation in the public goods game to examine the trade-offs between identity and monetary payoffs. They find that identity is eclipsed by financial considerations.

fects on subjects ability to cooperate (Brewer and Kramer, 1986; Brewer and Silver, 2000; Eckel and Grossman, 2005) and to increase the probability of equilibria selection (Chen and Chen, 2011). In two person minimum effort games Chen and Chen (2011) use social identity as a feature through which subjects are able to coordinate. Subjects are assigned an identity according to the minimal group paradigm, then are matched with another subject, either a member of their own group or a member of the other group and play a two person minimum effort game. They find that without any group enhancing procedures, social identity enables subjects to coordinate on a higher effort level when matched with an in-group subject relative to an out-group subject to some extent. Chen and Chen (2011), whilst utilising social identity to increase efficiency do not take into account how social identity may affect effort levels when a) the group is large and b) when the social identity make up in the minimum effort game is mixed with both in and out-group members, this is something we seek to achieve in this paper. Whilst numerous papers have shown that matching subjects with members of ones own group significantly increases cooperation, altruism and efficiency, the effects of interaction with mixed social identity groups or members of the other group are not as positive. Chen et al. (2014) find that priming for a fragmenting identity reduces cooperation in two person minimum effort games, whilst inducing a common identity such as school attendance, results in higher observed effort levels. However given that we are required to interact with individuals from outside our social identity groups on a daily basis, whether this be through team work in an ethnically diverse company or through contributions to the community, the key question is how do we ensure that coordination is achieved and is efficient in the face of conflicting identities?

The addition of social identity to the weakest-link game with endogenous linking, has as far as we are aware not been studied experimentally before. The joining of these two concepts leads to conflicting hypotheses on the effects on coordination and efficiency. On the one hand, endogenous linking allows for the exclusion of low effort players, which will enable high effort players to coordinate on a higher effort level than would be possible under exogenous linking. Social identity on the other hand has the potential to limit the benefit of endogenous linking through two routes; the number of links a subject has (neighbourhood size) and effort levels.

Firstly, in-group biases may lead to a preference for linking only with members of ones own social group, reducing the size of ones neighbourhood. Thus, limiting the potential for efficiency even when in-group members are able to coordinate on high effort levels. With only endogenous linking, exclusion of low effort players serves solely as a punishment mechanism, those excluded players can increase their effort in latter rounds to be included once again. Thus it is not unreasonable to assume that neighbourhood size may be large under endogenous linking. Secondly players may instead choose to connect to out-group members but simultaneously exert low levels of effort, as coordination relies on trust towards ones neighbours. Trust has been shown to be negatively affected in the presence of social identity (see Hargreaves Heap and Zizzo, 2009) and so we may find that effort levels are reduced in NT-SI. Thus the effects of combining endogenous linking with social identity are ambiguous.

This paper takes as its baseline the Baseline Treatment (BT) and Neighbourhood Treatment (NT) in RRS. We are able to replicate the findings that with endogenous linking subjects are able to coordinate on a higher pareto ranked Nash equilibria and are able to do so in large groups. To specifically address how one ensures that coordination is achieved and is efficient in the face of conflicting identities we add a third treatment, Neighbourhood Treatment with Social Identity (NT-SI). We incorporate social identity through minimal groups and then have subjects play the Neighbourhood Treatment. Subjects remain in the same social identity group for the duration of the experiment and there is always a 50-50 split between in and out-group members in the weakest-link game. We find that although the minimum group paradigm is successful in creating a social identity we do not find that social identity has any discernible effect on behaviour in the weakest-link game. This is with the exception of the first period of play, initially subjects prefer to link with members of their own group. However as in Charness et al. (2014) the affects of social identity are overshadowed by market considerations as quickly as the second period.

The paper is structured as follows, in the next section, Section 3.2 we describe the weakest-link game. Section 3.3 outlines the experimental design and the experiment procedures. Section 3.4 presents and discusses the results and Section 3.5 concludes.

3.2 Weakest-Link Game

The weakest-link game is a problem of coordination. Exerting high effort is both a costly and risky action, since efforts are exerted simultaneously and individually, the profitability of exerting a high effort level relies crucially on all others also exerting a high level of effort. Thus coordination is key. This is because payoffs in the weakest-link game are determined by the minimum effort exerted in the game. The standard payoff function is given by equation (3.1). The game is made up of N players, where $N = \{1, 2, 3..., n\}$. Each player is connected to all others. Players must simultaneously choose an effort level $e_i \in E$, where $E = \{1, 2, ..., S\}$, and $s = (e_i)_{i \in N}$ makes up the strategy profile.

$$\pi_i(s) = amin_{j \in N} e_j - be_i + c \tag{3.1}$$

The weakest-link game can be thought of as a joint production process, where the quality of the final product is determined by the lowest performing player. Payoffs then, are such that they depend on the effort exerted by ones self and the minimum effort exerted by the group as a whole. Integral to the payoff function in the weakest-link game are the parameters a and b. Where a is the marginal return from the lowest effort exerted by the set of players and b, the marginal cost of effort of player i. To ensure payoffs remain non negative the payoff function includes the parameter c such that $c>a>b>0$. The pure strategy equilibria is such that all players choose the same effort level e. The set of Nash equilibria can be pareto ranked such that $e = 1$ is the lowest ranked Nash equilibrium and $e = S$ the highest.

To incorporate the ability to choose with whom one links in the endogenous network formation treatments payoffs are structured as in equation (3.2). Note that the only alteration to equation (3.1) is the inclusion of the relative neighbourhood size ($\frac{n_i}{n-1}$). This structure ensures that it is always better to form links with other players than to remain isolated, since isolation induces a payoff of zero. Irrespective of neighbourhood size (when neighbours>0) the incentives coincide with those of the standard weakest-link game, such that it is always better to coordinate on an effort level, and these effort levels are pareto ranked as they are in equation (3.1). In addition whenever the player forms all possible links such that $n_i = n - 1$ then the payoff functions in equations (3.1) and (3.2) are equivalent for the player in question.

$$\pi_i(s) = \frac{n_i}{n-1}[\ a(min_{j \in N_i(s) \cup i}\{e_j\}) - be_i + c] \tag{3.2}$$

3.3 Experiment Design and Procedures

3.3.1 Experiment Design

This experiment utilises a between-subject design and consists of three treatments: Baseline Treatment (BT), Neighbourhood Treatment (NT) and Neighbourhood Treatment with Social Identity (NT-SI). We proceed by explaining each treatment below.

In BT subjects participate in a standard weakest-link game based on those of Van Huyck et al. (1990). Subjects are linked with 7 other subjects and are informed that these persons remain fixed for the duration of the experiment. All subjects are linked to one another and in each of the 30 rounds all must simultaneously choose an effort level $e_i \in E$, where $E = \{1, 2, ..., 7\}$. Payoffs in each round take the form of those in equation (3.1). As in RRS the parameters take the form of those in Van Huyck et al. (1990), that is a=20, b=10 and c=60. Final payments for subjects are the accumulation of earnings in each of the 30 rounds. At the beginning of each round subjects are automatically informed of the effort levels of all others with whom they interact and their own earnings in the previous round. In addition subjects are also able to access a complete history of play in each round, not just the effort levels and their own payoffs in the previous round. The screen depicting this information was laid out in a network structure, Figure 3.1 gives an example of the screen viewed by subjects in NT-SI.[2] Subjects did not know the identity of

[2]Here the network represents NT-SI. In BT all nodes are coloured black given the absence of

the players that correspond to each node and subjects always receive the label 'Me' whilst the remaining nodes are labelled 'A-G' clockwise. Subjects are aware that each node represents the same individual across rounds and that the position of each subject in the network is randomly determined by the computer.

The parameters in NT follow directly from BT with the exception that links are no longer exogenously formed. Simultaneously subjects choose both with whom they would like to interact and their effort level in the given round such that the neighbourhood size is given by $N = \{1, 2, 3..., n - 1\}$ (as subjects cannot link with themselves). Interaction between subjects relies on mutual consent between the two subjects; that is only if I propose to interact with you and you propose to interact with me do we actually interact, unilateral proposals to interact result in no interaction taking place. The payoff structure thus follows that of equation (3.2). As in BT the weakest-link game is repeated over 30 rounds and subjects have access to the complete history of their own and all others decisions across each round. The history of play is again framed using a network design, links between players that interact are shown as thick complete lines, whereas for unilateral proposals the link is represented by a thin incomplete line, falling short of the player to whom the unreciprocated proposal was directed. For example in Figure 3.1 'G' proposed to link with 'Me', but 'Me' did not propose to link with 'G'. Each subjects effort level in each round is also provided to the subjects.

The final treatment, NT-SI follows the same procedures as in NT. However, unlike BT and NT we first create the social identity groups. To do so, we introduce a stage prior to the weakest-link game. Following Chen and Li (2009), subjects are randomly allocated to one of two groups identities (Red or Blue). Subjects are assigned to their group identity based on no outside factors, instead assignment is randomly determined by the computer and subjects are informed as such. Following group assignment subjects participate in a series of other-other allocations, where in line with Chen and Li (2009) the endowments monotonically increase across rounds (with an initial endowment of 200 tokens and increasing in increments of 50 tokens).

social identity (in NT-SI the nodes reflect the social identity group to which the subject belongs). All links in BT are exogenously determined and so all subjects are connected with a line as players A and B are in the example screen. In NT all nodes are again black reflecting the absence of social identity, but the depiction of links between players follows that in NT-SI. For specific examples of both BT and NT screens see Appendix B.

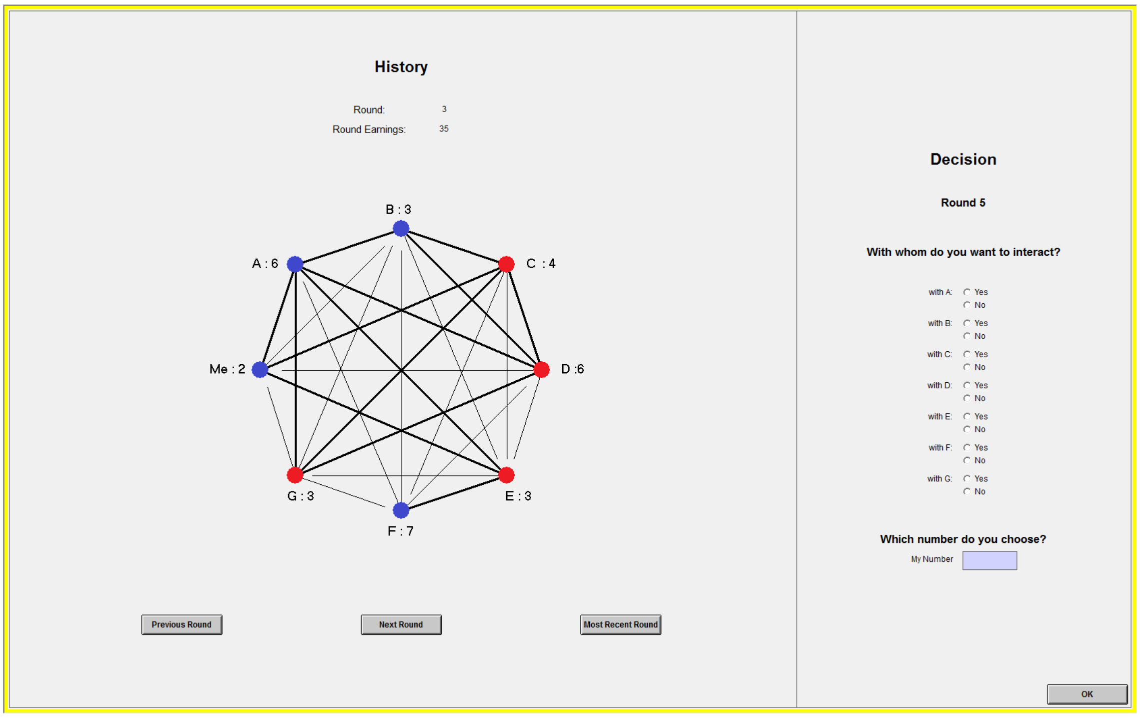


Figure 3.1: Neighbourhood Treatment with Social Identity

Other-other allocations as the name suggests, requires that the subjects allocate their endowment between two other players. The entirety of the endowment must be allocated and no portion can be allocated to oneself. Subjects make 3 decisions for each of the 5 endowments: 1) an In-In Group allocation 2) an Out-Out Group allocation and 3) an In-Out Group allocation. This procedure is a test of the effectiveness of the assignment to a group identity and of the salience of this identity for the experimental subjects. If group identity has no effects or the inducing of identity is ineffective, one would expect to see no differences in the allocations between in and out-group members. This stage is incentivised through the selection of a round at random where one of the three scenarios will be randomly selected and the decisions implemented (please see Appendix B for a complete set of experiment instructions).

Following the other-other allocations, subjects remain in their social identity groups to play the weakest-link game with the same design and protocols as in NT. Subjects are able to interact with 3 members of their own group and 4 members of the out-group. These 7 persons do not change for the duration of the experiment. Subjects use the same payoff structure and computer interface as those in NT, but here the group identity of the subjects will be identifiable from the colour of their

Table 3.1: Payoff Table

		$\min_{j \in N_i(s) \cup i}\{e_j\}$						
		7	6	5	4	3	2	1
	7	130	110	90	70	50	30	10
	6	-	120	100	80	60	40	20
	5	-	-	110	90	70	50	30
e_i	4	-	-	-	100	80	60	40
	3	-	-	-	-	90	70	50
	2	-	-	-	-	-	80	60
	1	-	-	-	-	-	-	70

node (red or blue rather than black as in the previous treatments). The experimental screen is depicted as in Figure 3.1. The positions of the persons on the screen are again randomly determined by the computer as they are in BT and NT. All other aspects in this treatment remain as in NT.

3.3.2 Experiment Procedures

The experiment took place at the University of Leicester Experimental Economics Lab (LExEcon) from January to March 2018. In total 224 subjects participated in the experiment - 64 subjects in the Baseline Treatment, 80 in the Neighbourhood Treatment and 80 in the Neighbourhood Treatment with Social Identity. Subjects interacted in groups of 8 which remained fixed for the duration of the experiment, this gives 10 independent observations for both NT and NT-SI and 8 independent observations for BT. For NT and NT-SI, all sessions were conducted with 16 subjects, consisting of 2 groups of 8. However, 6 of the 7 BT sessions we conducted consisted of only 8 subjects - thus only one session in BT had 16 subjects.[3] Given that subjects are aware with whom they were interacting we would argue that we should see increased levels of cooperation, through coordination on high effort levels in these sessions than would be typically found in the weakest-link game. However since we replicate the standard results whereby subjects quickly converge to the least efficient

[3]In sessions with only 8 subjects, the subjects were clearly aware with whom they were interacting. The experimental instructions were altered to reflect the fact that the groups of 8 were no longer randomly determined by the computer. Instead subjects were informed that they would interact with 7 other persons.

equilibria, we are happy that the change in set-up does not significantly alter our results in any meaningful way.

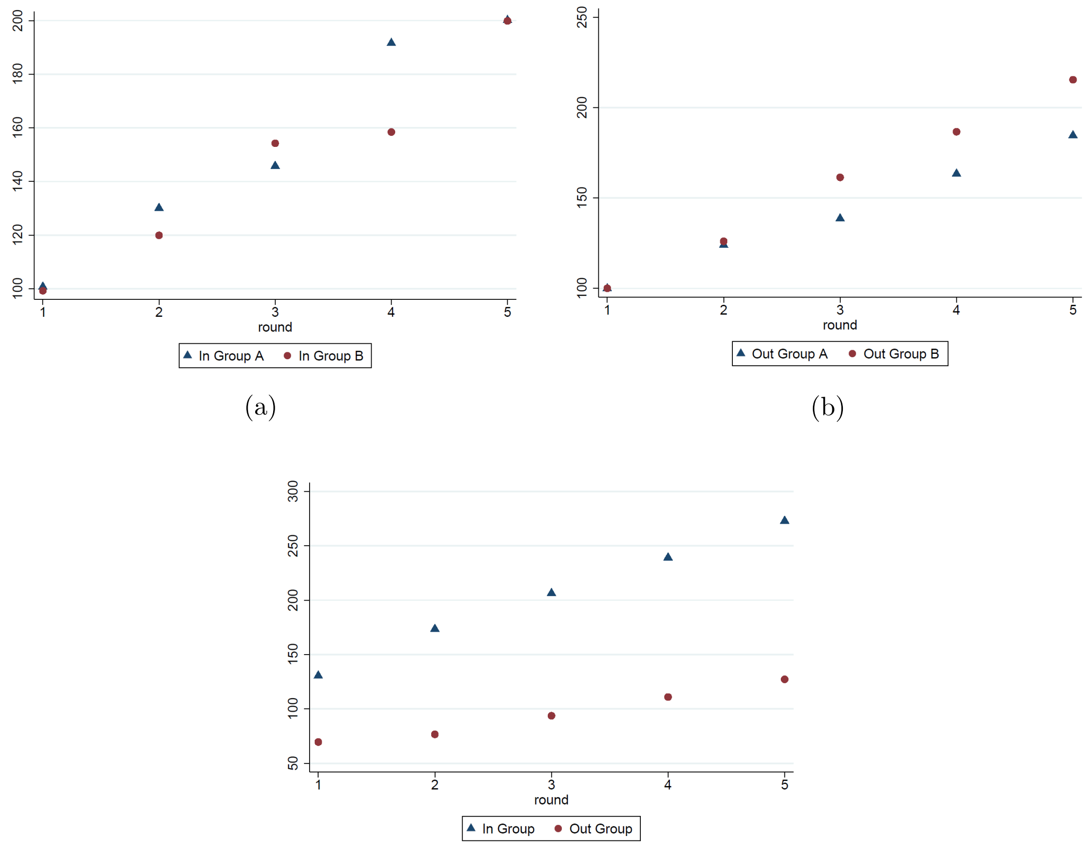
Upon entering the lab subjects were seated randomly at privately screened computer terminals to ensure privacy and to limit any opportunities for communication between subjects either verbally or visually. The experiment was fully computerised using the experiment software z-Tree (Fischbacher, 2007). The instructions appeared on the computer screen and were also read aloud to the room by the experimenter to ensure subjects had common knowledge. Table 3.1 shows the payoff table that results from equation (3.1), a copy of this was handed out to subjects for reference throughout the experiment - this was handed out during the second stage for NT-SI to avoid any confounding effects in the first stage decisions.[4] Following the instructions and prior to beginning the experiment subjects answered a series of questions to ensure the instructions were clear and understood. Those subjects who failed to answer any questions correctly were informed as such in private. All subjects had the opportunity to ask any clarification questions to the experimenter in private.

Sessions in BT lasted on average 43 minutes and the average payment including the £2 show up fee was £13.20. NT lasted on average 75 minutes and average payments were £16.35 again including the show up fee. Finally NT-SI lasted on average 87 minutes and average payments were £19.63 with the show up fee. Subjects were paid in cash in private at the end of the experiment.

3.4 Results

To begin the experiment, those subjects in NT-SI participated in Stage 1. Stage 1 consists of Chen and Li (2009) other-other allocations. Here we wish to test whether the random assignment of group identity is effective and that the group identity effect is of salience for the experimental subjects. All subjects participated in 3 types of other-other allocations (In-In Group, Out-Out Group and In-Out Group) for five levels of endowment. Figure 3.2 depicts the differences in allocations for

[4]Note that Table 3.1 shows that payoffs for those in BT. In NT and NT-SI subjects were also provided this table, however they were aware that the payoffs listed would be multiplied by their relative neighbourhood size.



(a) (b) (c)

Figure 3.2: Other-Other Allocations
(a) In-In Group Allocations, (b) Out-Out Group Allocations (c) In-Out Group Allocations

each allocation type. There are clear significant differences for the In-Out Group other-other allocations for all endowment levels (Wilcoxon signed-rank p <0.000). Whilst we do see some significance in the difference in allocations in In-In Group scenario for endowments 250 and 350 (p=0.0804 and p=0.0003 respectively), and similarly for Out-Out allocations for endowments 300 and 400 (p=0.0573 and p=0.0072 respectively), the remaining differences in allocations remain insignificant. We conclude that group identity holds significant relevance for the subjects and given the large and persistent differences in the endowments in In-Out Group allocations group identity is successful and a salient feature of the experiment.

Result 3.1: Other-other allocations confirm the salience of social identity. Subjects offer significantly more of their endowment to in-group members than to

Table 3.2: Descriptive Statistics: Average and Minimum Effort

	Average Effort		Minimum Effort		
	Mean	St.dev	Mean	St.dev	N
BT	2.62	0.83	1.48	0.67	8
NT	6.29	0.41	4.45	0.99	10
NT-SI	6.34	0.49	4.76	1.14	10

members of the out-group.

3.4.1 Effort Levels

Having established that the effects of group identity in NT-SI are present we begin the analysis of the weakest-link game by looking at the effort levels of subjects across the three treatments. Table 3.2 gives the average and minimum effort levels in each treatment. Whilst we find large differences in overall average effort levels, the average effort level observed in the first round of play does not vary significantly (4.98 in BT, 5.52 in NT and 5.46 in NT-SI). Conducting a Mann-Whitney (MW) test on first round individual choices of effort levels for NT and NT-SI, we cannot reject the hypothesis that the two effort levels are equal (p=0.9974 and n=160). Similarly we compare BT with NT and BT with NT-SI, we are again unable to reject the hypothesis of equality (p=0.1286 and p=0.1525 respectively, n=144). Although we do not see any significant differences in the first round of play, the average effort levels across rounds do differ significantly. This suggests that the dynamics of the game quickly diverge. MW tests confirm that there is a significant difference both between the average effort level in BT compared to NT and BT compared to NT-SI (p=0.0003 and p=0.0004 respectively n=18). However in comparisons between the average effort levels in NT and NT-SI we are unable to find any significant difference (p=0.6220, n=20).

Figure 3.3 shows the cumulative distribution of effort levels across rounds. Using Jockheere-Terpstra (JT) tests to analyse the frequency of effort levels in each treatment we are able to determine the evolution of effort levels across rounds. In

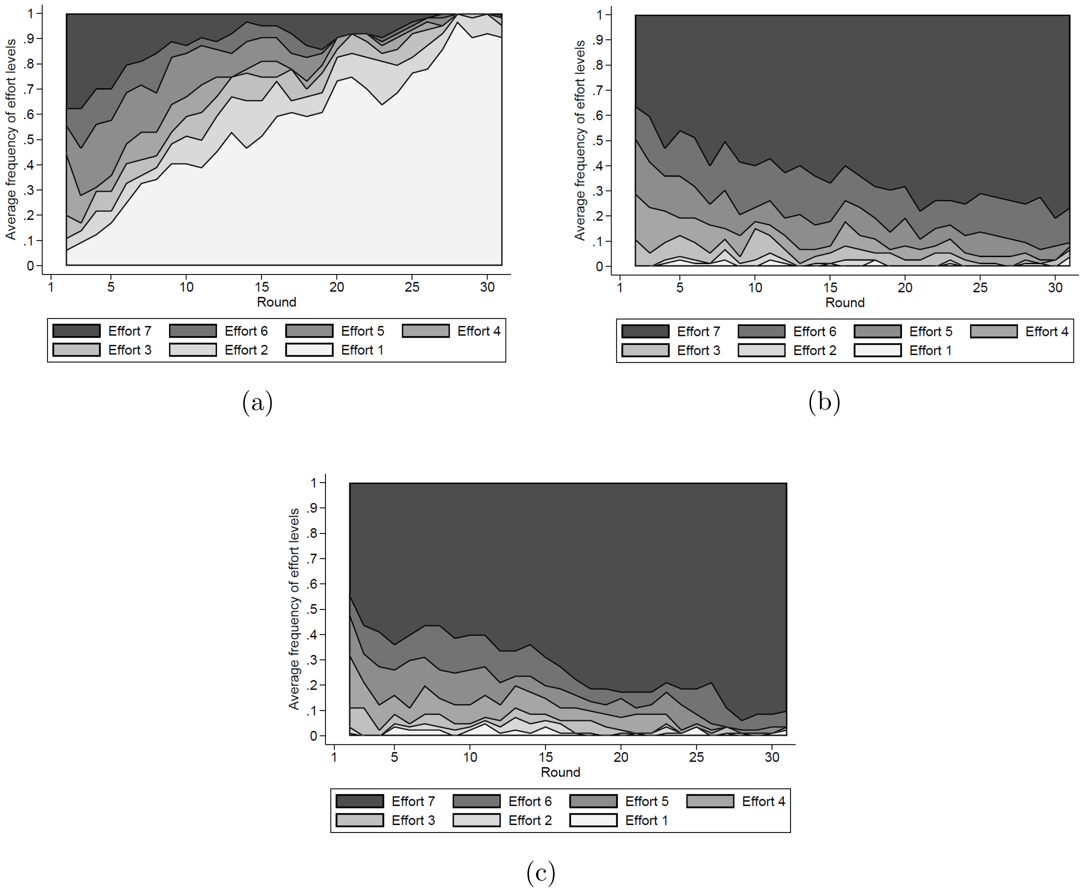


Figure 3.3: Cumulative Distribution of Efforts across rounds
(a) BT, (b) NT (c) NT-SI

the analysis that follows all tests are conducted at the group level thus, n=8, n=10 and n=10 in BT, NT and NT-SI respectively and all tests are two-sided, unless stated otherwise. In BT, 6.3% of effort levels in round 1 are that of effort 1 which increases over time to 90% in the final round. The frequency of the highest effort level reduces in frequency from 37.5% of subjects playing effort 7 in round 1 to 1.5% playing effort 7 in the final round. JT tests confirm that effort level 1 increases in frequency across rounds whilst the frequency of effort level 7 is reduced (p <0.0000 for both). With the exception of effort level 2 (who's frequency does not change) we see all effort levels greater than 1 reduce in frequency (p < 0.0034 for effort level 3 and p <0.000 for all remaining effort levels). Thus in BT, we are able to replicate the standard findings in the weakest-link game literature. Subjects coordinate on low effort levels.

Conducting a similar exercise for NT we find that 0% of effort levels in round

1 are that of effort 1, this low frequency of effort level 1 persists throughout the experiment. The highest level of effort increases in frequency from 38.6% in round 1 to 78.4% in the final round, reaching a consistent level of approximately 80% of the effort levels chosen by round 21. JT tests cannot reject whether or not effort level 1 increases or decreases across rounds, given that the fluctuations from zero are minimal this is to be expected (p=0.2755 (decreasing)). However at the other extreme, that of the highest possible effort level we are able to reject the hypothesis that effort level 7 remains constant across rounds. JT tests confirm that effort 7 is increasing across rounds (p <0.0000). Whilst we are unable to state that effort levels of 1 decrease in frequency across rounds, JT tests show that effort levels 2 through 6 are reducing in their frequency across rounds (p= 0.0559 for effort 2, p=0.0067 for effort level 6 and p <0.0000 for all other effort levels).

Finally considering NT-SI, we find similar patterns to those found in NT. We find very low frequencies of the lowest effort level, the frequency of which is 1.3% of effort levels in round 1 and fluctuates between 0% and 3.75% throughout the remaining rounds. Again, due to the low frequency of effort level 1 JT tests cannot reject that effort level 1 either increases or decreases across rounds (p=0.1095 (decreasing)). Conversely and again in line with the findings in NT, the highest level of effort increases in frequency from 43.8% in round 1 to 78.4%, again confirmed with a JT test (p <0.000). We also find that all remaining effort levels are decreasing in frequency as they do in NT (JT tests; p=0.0138 effort level 2, p <0.000 for effort levels 3 to 6).

Result 3.2: The frequency of low effort levels increases across rounds in BT, whilst we observe a decrease in the frequency of low effort levels in NT and NT-SI and an increase in high effort levels in these two treatments.

Comparing only first round effort levels across treatments, we find that minimum effort levels differ, even though average effort levels do not. Minimum efforts in BT (2) and NT (3.45) are statistically different using MW tests (p <0.0000 n=18), similarly comparing NT with NT-SI (2.9) we also find that we can reject the hypothesis that the minimum efforts are the same (p=0.0005, n=20), this is also the

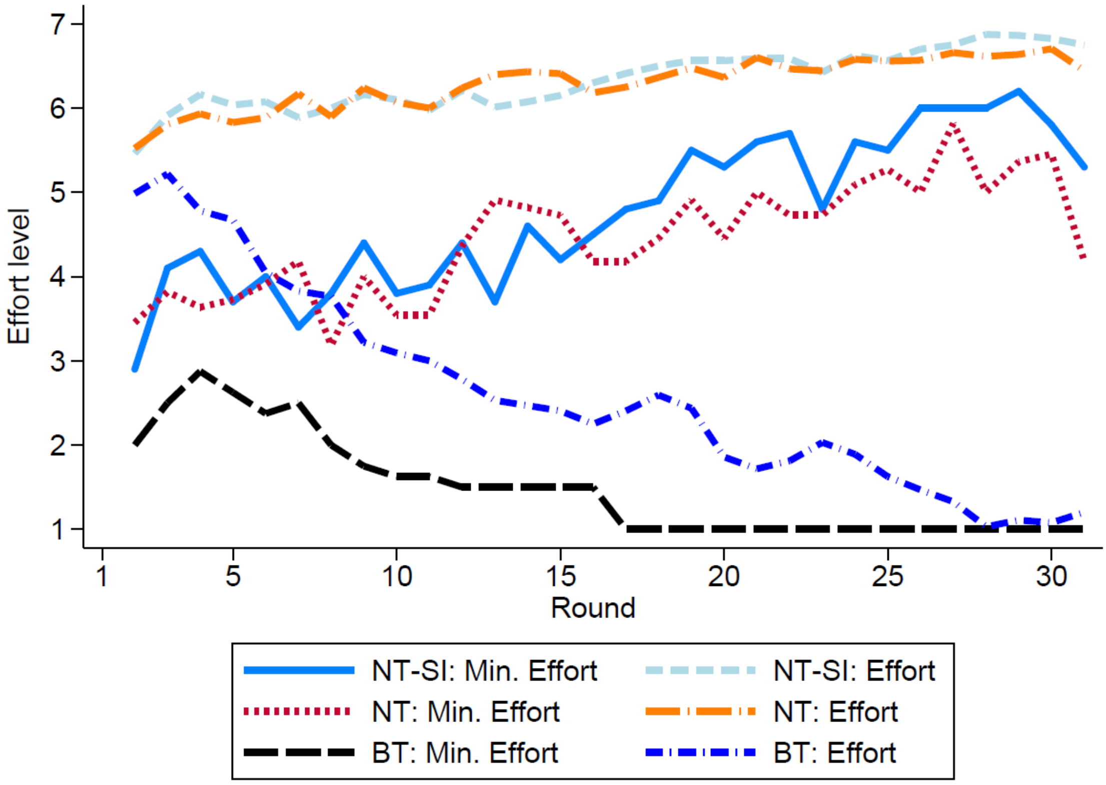
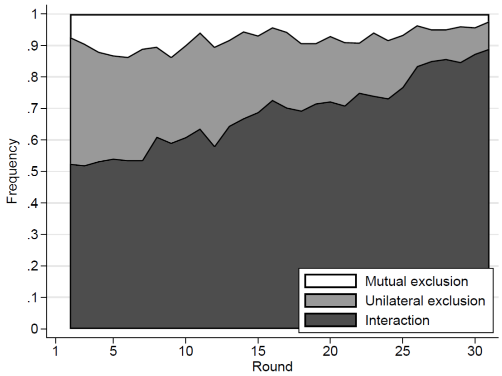


Figure 3.4: Mean and Minimum Effort Levels of Groups across rounds

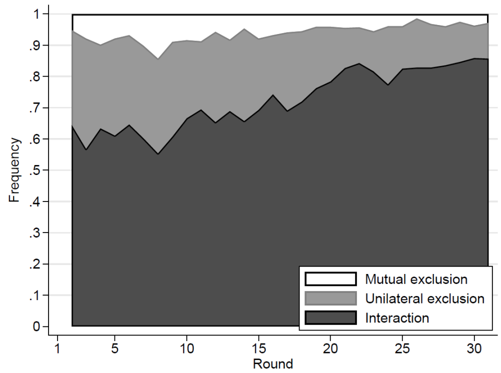
case for comparisons between BT and NT-SI (p <0.0000, n=18). This significant difference across minimum effort levels persists throughout the experiment when comparing the average minimum effort in BT to those in NT and NT-SI (p=0.0003 and p=0.0005 respectively). However across rounds we cannot reject that the hypothesis that minimum effort levels differ in comparisons between NT and NT-SI (p=0.3981). Figure 3.4 shows the average and minimum effort levels across rounds for the three treatments.

3.4.2 Interaction Frequencies

We have shown that effort levels differ significantly in BT compared to NT and NT-SI. In this section we examine interaction frequencies and exclusion as a possible explanation for the stark differences that emerge between the treatments. The ability to choose with whom one links, may be utilised as the mechanism through which high effort levels are established. Since it is better (in terms of payoffs) to link with more players than less ceteris paribus, those exerting a low effort can affectively be



(a) Exclusion - NT



(b) Exclusion - NT-SI

Figure 3.5: Exclusions across rounds

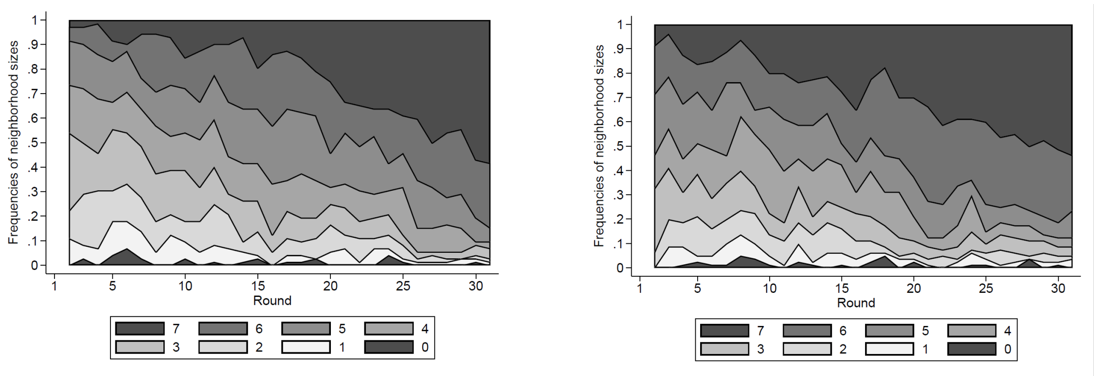
Table 3.3: Descriptive Statistics: Average, Minimum and Maximum Neighbourhood Size

	Mean	St.dev	Min	Max	N
NT	4.81	1.32	1.25	7	10
NT-SI	5.08	1.33	1	7	10

punished by the high effort types though exclusion. If this exclusion induces the low effort players to increase their effort levels, (to encourage others to link with them in future rounds), then endogenous linking provides a mechanism through which efficiency can be increased.

To establish if this is indeed the case we must first determine whether or not interactions are actually taking place and the frequency of those interactions. Recall that mutual consent is required for an interaction to take place. Hence we have three interaction types: interaction; where both parties signalled they wanted to interact, unilateral exclusion; where one party wished to interact but the other did not and mutual exclusion; neither party wanted to interact. Figures 3.5(a) and (b) depict the frequency of interaction and exclusion (mutual and unilateral) over time. We can clearly see that the frequency of interaction is increasing across rounds whilst unilateral exclusion and mutual exclusion are decreasing for both NT and NT-SI.

Figures 3.6(a) and (b) depict the frequency of neighbourhood sizes over rounds, where 7 is the maximal number of neighbours a subject can have (as a subject is not linked with themselves) whilst Table 3.3 gives descriptive statistics of neigh-



(a) Neighbourhood Size - NT

(b) Neighbourhood Size - NT-SI

Figure 3.6: Frequencies of interaction and neighbourhood sizes across rounds

bourhood size. In the first round 72.3% of possible interactions are proposed in NT, however only 52.3% of possible interactions actually take place with the average neighbourhood size of 4.5 persons. JT tests confirm that the number of interactions taking place increases over time, whilst the number of unilateral exclusions falls (p <0.000 for both). In contrast to NT, in NT-SI we find that of those links proposed a larger proportion of interactions take place in the first round, despite the fact that the number of proposed links is roughly similar at 79.7% (compared to 72.3% in NT). A total of 64.6% of possible links are formed. Across rounds on average 72.5% of possible links are established. However, unlike NT (where 68.3% of links are established across the 30 rounds on average), this does not reflect the dynamics at play. The introduction of social identity in Stage 1 opens up further channels for investigation. We are interested in whether the presence of group identity affects with whom a subject wants to link. Table 3.4 gives summary data on the links requested to members of a subjects in and out-groups. There are clear and significant differences in the first round of play. Subjects overwhelmingly propose more links to members of their own group than the other, and of those proposed considerably more are established. However, over time this effect disappears. Across all rounds we see no effects of social identity in choosing with whom to link. This suggests that financial concerns of subjects and by extension efficiency factors outweigh those of social identity. Similar to the neighbourhood formation dynamics found in NT we also find that the size of the neighbourhood is increasing across rounds in NT-SI. JT tests confirm that the frequency of interaction is increasing across rounds and the

Table 3.4: Links Requested and Established: Wilcoxon signed-rank test.

		Links Requested		Links Established	
		Mean	Std.dev	Mean	Std.dev
Round 1	In Group	88.8	0.32	78.3	0.41
	Out Group	73.1	0.44	54.4	0.50
	p-value	0.0051		0.0093	
Average	In Group	83.8	0.37	73.3	0.44
	Out Group	82.9	0.38	71.2	0.45
	p-value	0.1767		0.1973	

Percentages of links requested and established to members of In/Out Group and Wilcoxon signed-rank p-values.

incidence of unilateral exclusion is falling cross rounds (p <0.000 for both measures).

Result 3.3: Across rounds the number of interactions increases, with a simultaneous decrease for both unilateral and mutual exclusions in both NT and NT-SI.

Result 3.4: Social identity results in a preference for linking with ones own group in the initial round of the weakest-link game, however average preferences with regards to whom to link with do not differ by the social identity group of the potential neighbour. Efficiency takes precedent over group identity after round 1.

RRS in their NT Treatment are able to show that across rounds it is only neighbourhood sizes of 7 that are increasing, whilst our results are not as extreme we are able to show a similar directional effect. JT tests on the size of neighbourhoods across rounds confirm that neighbourhood sizes of only 6 and 7 are increasing across rounds (p <0.0001), whereas the incidence of all other sizes is decreasing in NT (p <0.0010) with the exception of isolation which does not change across the rounds. Similarly we find the same behavioural pattern in NT-SI, neighbourhood sizes of at least 6 are increasing across rounds (p <0.0001) and all others including isolation are decreasing (p <0.0430).

3.4.3 Exclusion Effects

Having established in the previous sections that both average effort levels and the frequency of interactions are increasing across rounds, we want to discover the mechanism behind this. Essentially we want to be sure that the excluded are excluded efficiently. Exclusion has the potential to be costly to both the excluder and the excluded. The excluder looses an additional link thus, ceteris paribus reduces their earnings, whilst at the same time they are able to increase the minimum effort in their group (if exclusion is based on excluding those who play the lower effort levels). For the excluded subject, the smaller neighbourhood size negatively affects their payoffs (providing that the excluding subject did not play the minimum in their neighbourhood). In this section we examine the costs and benefits of exclusion, both to the excluder and the excluded. To do so, for the excluder we calculate the alternative profit the excluder would have earned if they had not chosen to exclude and compare this to the actual payoff earned by the excluder. For the excluded subject we utilise a similar exercise where we calculate the alternative profit the excluded subject would have received had they not been excluded.

In NT, 29.8% of excluding subjects weakly benefit from excluding a subject in a given round. Strictly positive benefits amount to 11.7 tokens on average per beneficial exclusion. However the remaining exclusions are costly. On average these cost the excluder 12.9 tokens per exclusion. Thus overall exclusion is a costly action to the excluder in the short term. This could be beneficial to the group as a whole if the excluded subject also bears a cost and responds through increased effort levels in future rounds. For the excluded subject, exclusion is costly for 82.4% of exclusions, whist 17.6% see at least no reduction in their payoffs. Overall excluded subjects gain 4.7 tokens per beneficial exclusion whilst loosing 12.8 tokens per costly exclusion on average. Thus whilst exclusion is costly for the excluder, their is an overwhelming cost to the excluded.

We perform the same exercise for NT-SI, however we look at the costs to the excluder and excluded under two cases, one when the two subjects are of the same group identity and the other when they are not. For in-group exclusions, the excluder has some weakly positive benefit from exclusion in 27% of cases, for out-group exclusions those that suffer no cost from exclusion are 27.5% of cases. Both of

Table 3.5: Exclusion Rates and Response: NT

t-1	Effort of i relative to effort of j and efforts in j's neighbourhood					
	$e_i^+ : e_i \geq e_j$		$e_i^- : e_i < e_j$ but $e_i > min_{k \in N_j(e_k)}$		$e_i^{--} : e_i < e_j$ and $e_i = min_{k \in N_j(e_k)}$	
t	Exclusion rates (in %)					
	3.6 (322/9,061)		12.3 (75/600)		25.2 (337/1,335)	
t+1	i's response (in %)					
	$j \in I_i$	$j \notin I_i$	$j \in I_i$	$j \notin I_i$	$j \in I_i$	$j \notin I_i$
$e_i \uparrow$	9.6 (31)	6.8 (22)	41.3 (31)	17.3 (13)	43.6 (147)	21.1 (71)
$e_i =$	45 (145)	19.9 (64)	12 (9)	10.7 (8)	15.7 (53)	11 (37)
$e_i \downarrow$	13.7 (44)	5 (16)	12 (9)	6.7 (5)	5.9 (20)	2.7 (9)

which are remarkably similar, to each other and to the corresponding figure in NT. Of those excluding subjects that do suffer a cost, this amounts to 12.5 tokens for in-group exclusions and 13 tokens for out-group exclusions. Whilst the benefits to exclusion are 9.4 tokens and 11.2 tokens respectively. We therefore observe, as in NT that overall the costs of excluding subjects are higher than the benefits of exclusion. A similar story follows for the excluded subjects. In both in and out-group exclusions the costs to the excluded subject far outweigh the benefits. The benefit of being excluded is 11 tokens for in-group exclusions and 10.5 tokens for out-group exclusions. Whilst the costs of exclusion are 12.8 tokens for in-group exclusions and 13 tokens for out-group exclusions. Although the gains from weakly beneficial exclusions in NT-SI are larger than those in NT, the overall effect moves in the same direction.

Having established in both NT and NT-SI, that exclusion is costly to both the excluder and the excluded in the short term, we must check that the exclusion effect has the intended consequences of increasing future levels of efficiency. If this is not the case then there is little reason to exclude a subject at a cost to ones self.

Tables 3.5 and 3.6 examine the dyadic interactions of excluded subjects and their efficiency reactions in NT and NT-SI respectively. We pool the in and out-group pairings in the NT-SI treatment since we have shown there is little difference in the behaviour of subjects with regards to group membership. Subjects i and j interact in period $t-1$. In $t-1$ both subjects exert their effort level. Subject i can exert an effort level that is at least as high as subject j ($e_i^+ : e_i \geq e_j$), an effort level that is lower than subject $j's$ but is not so low that it is a minimum effort in $j's$ neighbourhood ($e_i^- : e_i < e_j$ but $e_i > min_{k \in N_j(e_k)}$) or finally subject i exerts an effort that is both lower than $j's$ and is a minimum in $j's$ neighbourhood ($e_i^{--} : e_i < e_j$ and $e_i = min_{k \in N_j(e_k)}$). These possibilities make up the top panel in Tables 3.5 and 3.6. In period t, subject j observes $i's$ effort level in $t-1$ and has the opportunity to exclude i from their neighbourhood. The central panel of Tables 3.5 and 3.6 give the exclusion rates of subject i in time t. Recall that exclusion in time t can only take place if the two subjects had an established link in $t-1$. For both NT and NT-SI a very low proportion of subjects are excluded when they play an effort level at least as high as subject j (3.6% and 2.9% respectively). However the exclusion rate jumps to approximately 25% for both NT and NT-SI when subject i plays the minimum effort in $j's$ neighbourhood. Thus exclusion occurs at a significantly higher frequency for those subjects playing an effort level lower than their neighbours, but an additional penalty is enforced for those who exert the neighbourhoods minimum effort.

We next want to examine whether subjects that have been excluded increase their effort levels in $t+1$. To recap, subjects i and j choose an effort level in time $t-1$, in time t, j observes $i's$ effort level and chooses whether or not to exclude i. At $t+1$ subject i observes whether or not they were excluded in time t. Thus the consequences of subject $i's$ actions in $t-1$ are only fully realised in $t+1$, any efficiency response should then arise in this period. The bottom panel addresses this. Here we show both that the excluded subjects subsequently exert higher effort levels and that they are not deterred from seeking to maintain their link with j ($j \in I_i$). In NT between 65.2 and 68.3% of excluded subjects request a link with j in $t+1$, whilst in NT-SI the figures increase to between 65.6 and 75.8%. Note however, that these are links requested by i they do not reflect the actual links established. It

Table 3.6: Exclusion Rates and Response: NT-SI

t-1	Effort of i relative to effort of j and efforts in j's neighbourhood					
	$e_i^+ : e_i \geq e_j$		$e_i^- : e_i < e_j$ but $e_i > min_{k \in N_j(e_k)}$		$e_i^{--} : e_i < e_j$ and $e_i = min_{k \in N_j(e_k)}$	
t	Exclusion rates (in %)					
	2.9 (289/10,055)		12.8 (61/476)		25.4 (297/1,169)	
t+1	i's response (in %)					
	$j \in I_i$	$j \notin I_i$	$j \in I_i$	$j \notin I_i$	$j \in I_i$	$j \notin I_i$
$e_i \uparrow$	7.6	5.2	34.4	26.2	54.5	16.2
	(22)	(15)	(21)	(16)	(162)	(48)
$e_i =$	56.7	19	23	8.2	15.2	6.1
	(164)	(55)	(14)	(5)	(45)	(18)
$e_i \downarrow$	5.2	6.2	8.2	-	6.1	2
	(15)	(18)	(5)	-	(18)	(6)

would seem then that exclusion does not act as a deterrent to the excluded subject with the majority still seeking to maintain the link.

Whilst the desire to maintain a link is crucial towards achieving maximum efficiency, this must come hand in hand with an increase in the effort levels from the excluded subjects. Without this increase the excluder has no motivation to reciprocate the request to link. Of those who were low effort providers in $t-1$ (e_i^- or e_i^{--}) between 58.6 and 64.7 subjects increase their effort levels in NT. Similarly in NT-SI the parallel numbers are 60.6 and 70.7%. In both treatments, excluding a subject has the desired effect of increasing the excluded subject's effort level without diminishing the desire of the excluded to link with the excluder.

Result 3.5: Subjects in both NT and NT-SI exclude those who exert the minimum effort in the neighbourhood more frequently than higher effort subjects. This, over time, increases the overall effort as excluded subjects respond through increased effort levels.

Whilst we have shown that exclusion is costly both to the excluder and the

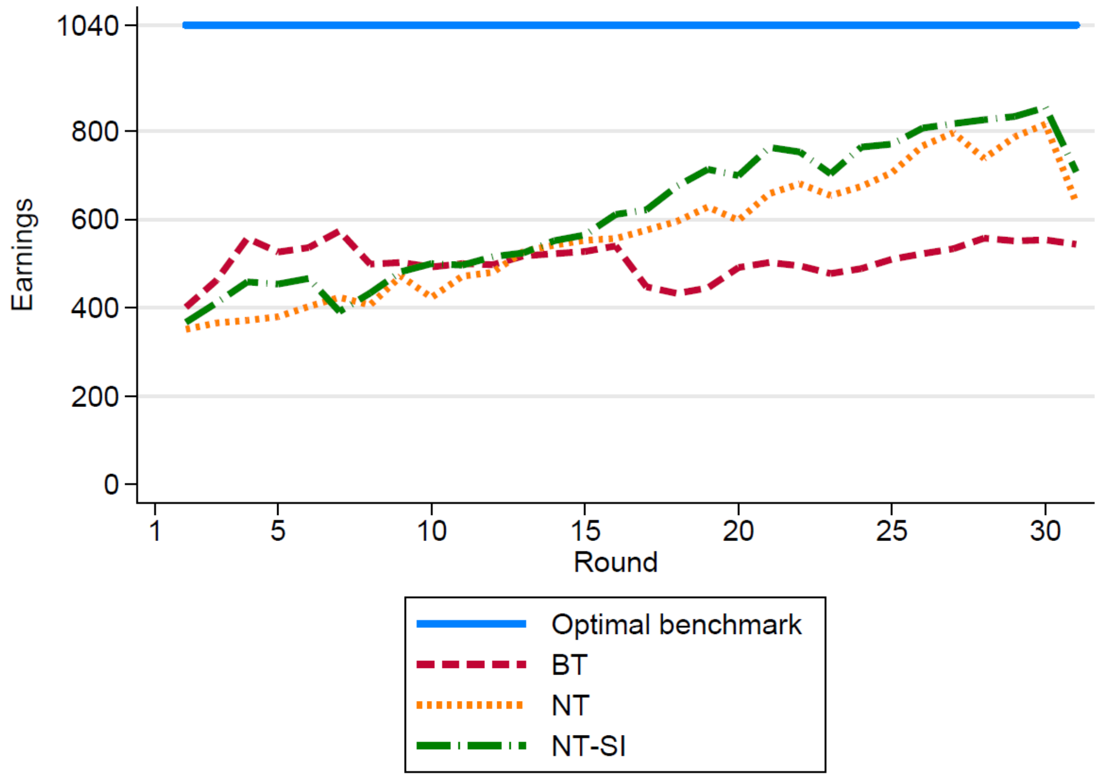


Figure 3.7: Welfare across rounds: BT, NT and NT-SI

excluded, this cost is outweighed by the future benefits that arise through increasing the average effort levels without reducing the long run neighbourhood size. This dynamic allows for the increases in effort levels exhibited in Figures 3.3(b) and (c), whilst the desire for maintaining links in the face of exclusion explains the dynamics in Figures 3.4(a) and (b).

3.4.4 Welfare

Central to problems of efficient coordination is the level of wealth or profit the group is able to produce. Under exogenous linking in the weakest-link game coordination failure can arise through two channels. Firstly subjects may not be able to coordinate on a high level of effort. This could in turn be due to two reasons, subjects who choose not to exert the high level of effort bring down the group earnings, thus making it impossible for others to have any incentive to contribute high effort levels themselves. Alternatively the low effort subject may persist in their poor behaviour regardless of the actions of other members of the group even when all others contribute a high effort level, again reducing the collective effort. Whilst this results in coordination failure with inefficient outcomes, inefficiencies can also arise through coordination. Under circumstances where subjects are able to coordinate but do so on a low effort we still see levels of inefficiency. Subjects even when they recognise this may not wish to deviate from the low but coordinated effort level for fear that they themselves will loose individual payoffs when others do not follow.

Endogenous linking adds a different element to the weakest-link game giving subjects a channel through which efficiency can be increased. The ability to exclude subjects allows those who exert low levels of effort to be unilaterally excluded. This exclusion mechanism should increase efficiency and so with exogenous linking welfare should increase. Figure 3.7 depicts the welfare levels across rounds for each treatment. The maximum profit possible in each round is given by 1040 tokens (8 players who individually can earn a maximum of 130 tokens - in the Neighbourhood Treatments this requires an individual to link with all seven other participants). Figure 3.7 clearly shows that with the Neighbourhood Treatments welfare increases over rounds - with some end game effects consistent with what has been depicted in end game effort levels in Figures 3.3(a), (b) and (c). JT tests confirm that welfare is significantly increasing across rounds for all treatments (p <0.000 for all treatments, BT: n=8, NT n=10 and NT-SI n=10.)

Result 3.6: Welfare increases across rounds. Subjects in all treatments make more efficient decisions over time.

Whilst there is a clear significant effect on welfare over time we are also able to show the under NT-SI subjects overall achieve a higher level of welfare than they do in BT (MW tests p=0.0209) whilst there are no differences in the welfare levels either between BT and NT or NT and NT-SI (p= 0.5340 and p=0.3258 respectively). Economically however we do see clear differences in the treatments: in BT groups earn on average 507 tokens, in NT 566 tokens and in NT-SI 617 tokens in each round (this does not include Stage 1 earnings for NT-SI). These figures suggest that there is a difference in the welfare effects by treatment, they are however not sufficiently large to make any further statistical inference from them.

Table 3.7: Random Effects Regressions: Group Level

	Effort	Min Effort	Welfare
NT	3.655***	3.774***	59.378
	(0.279)	(0.375)	(63.808)
NT-SI	3.716***	3.944***	110.905
	(0.279)	(0.375)	(63.808)
Constant	2.720***	0.498	506.958***
	(0.270)	(0.354)	(47.560)
N	840	840	840
Number of Groups	28	28	28
χ^2 Tests		p-values	
NT vs NT-SI	0.8184	0.6302	0.3917

Note: Dependent variables from left to right are: Average Group Effort, Minimum Group Effort and Total Group Earnings in a round. All include Period Fixed Effects. Standard errors in parenthesis are clustered at the group level. Significance Levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

3.4.5 Random Effects Models as Robustness Checks

In this final results section we utilise random effects models to provide robustness checks on our results. Following Reidl et al. (2016) random effects analysis we begin by performing regressions at the group level. Table 3.7 gives three regressions in this regard where the dependent variables are Effort, Minimum Effort and Welfare, where Welfare refers to the total profit earned by a group. Given that the analysis is at the group level these test serve only to determine the effects of the treatments on the 3 dependent variables. We show that there are clear effects on Effort levels in the NT and NT-SI treatments, with observed Effort considerably greater than those observed in BT for both average and minimum effort. We cannot however determine any difference with regards to effort levels between NT and NT-SI, this is consistent with previous results shown. Welfare according to these models is not affected by treatment, again largely consist with other results. We cannot include any further independent variables such as average neighbourhood effort or neighbourhood size, since these are individual specific in the neighbourhood treatments. In the following regressions we run random effects models where we cluster standard errors at the subject level to allow for this additional level of analysis.

Table 3.8 shows random effects models on effort levels. The dependent variable is the effort subject i chooses to exert in time t, whereas all our regressors rely on information that the individual already has in time t, hence they pertain to that which occurred in $t-1$. Column (1) offers a direct test on the differences between the effort levels in each treatment, we confirm, in line with findings stated above that effort is indeed higher in the endogenous linking treatments, however χ^2 tests find no difference in the overall effort levels between the neighbourhood treatments. Columns (2) and (3) incorporate additional controls on the effort levels of subject $i's$ neighbours (in BT recall that a subjects neighbours are all other seven persons the individual in matched with). Including controls on the average effort level of ones neighbours, 'Av-Effort', and the minimum effort in ones neighbourhood, 'Min-Effort', show that the effects of the treatments on effort remain although the size of the effects are reduced. Average and minimum efforts have the expected effect on an individuals effort, an increase in either of these aspects in $t-1$ increases ones own effort in t. The minimum effort nature of the payoff function means that there is an obvious correlation between the minimum (and average) efforts observed in the previous period and that exerted by subjects in the following period. Increased effort levels by ones own neighbours reduces the risk an individual takes exerting high effort in the following round, and so when efforts are high in $t-1$ this will induce high efforts in t.

Columns (4) through (6) include variables which are specific to the Neighbourhood Treatments. 'N-Size' refers to the neighbourhood size of individual i in $t-1$, with seven being maximal and efficient and zero isolation, 'Own' refers to the size of ones own social identity group the individual is linked with. In NT this variable takes the value of zero, whilst NT-SI it takes values 0 through 1, depicting the proportion of members of ones own group an individual is linked with. Finally 'Own*NSize' gives an alternative measure of in-group behaviour. The variable is defined as the number of in-group links in round $t-1$ divided by total number of links in round $t-1$. For example if individual i has no links with members of the other group but at least one link with a member of their own group then the measure would

Table 3.8: Random Effects Regressions: Subject Level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NT	4.711***	1.484***	1.184***	0.292	0.189	0.160	0.159
	(0.258)	(0.320)	(0.302)	(0.279)	(0.288)	(0.296)	(0.296)
NT-SI	4.246***	1.394***	1.227***	0.426	0.546	0.497	0.508
	(0.292)	(0.365)	(0.331)	(0.329)	(0.337)	(0.345)	(0.351)
Av-Effort		0.703***	0.519***	0.467***	0.466***	0.467***	0.467***
		(0.038)	(0.047)	(0.046)	(0.045)	(0.045)	(0.045)
MinEffort			0.236***	0.245***	0.245***	0.245***	0.245***
			(0.031)	(0.026)	(0.026)	(0.026)	(0.026)
N-Size				0.208***	0.227***	0.232***	0.232***
				(0.024)	(0.027)	(0.030)	(0.030)
Own					-0.307	-0.393	-0.396
					(0.190)	(0.219)	(0.222)
Own*NSize						0.189	0.194
						(0.338)	(0.342)
Other-Other							-0.000
							(0.001)
Constant	2.390***	0.874***	1.244***	1.472***	1.490***	1.485***	1.485***
	(0.187)	(0.190)	(0.193)	(0.191)	(0.191)	(0.191)	(0.191)
N	6,496	6,496	6,496	6,496	6,496	6,496	6,496
Subjects	224	224	224	224	224	224	224
χ^2 Tests				p-values			
NT vs NT-SI	0.1568	0.8035	0.8876	0.5625	0.2028	0.2220	0.2170

Note: Dependent variable is subjects Effort level in time t, regressors in time $t-1$. All regressions include Period and Group Fixed Effects. Standard errors in parenthesis are clustered at the subject level. Significance Levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

read one, on the other hand if the subject has established all possible links (7 links; 3 with members of ones own group and 4 links with members of the other group), the variable takes a lower value of 3/7. Thus this measure takes into consideration the comparative proportion of links to members of ones own group rather than the absolute number of links. We finally include a variable that takes into consideration the effects of the other-other allocations in NT-SI. We take the difference between the average offer to a member of ones own group minus the average offer to a member of the other group. Positive values of 'Other-Other' then indicate that social identity was salient for that individual, with the higher the value the greater the degree of salience. None of the three variables that control for social identity are significant. Our results for NT-SI are therefore completely unaffected by social identity and thus indistinguishable from those in NT.

The introduction of these controls, renders the treatment variables NT and NT-SI insignificant, given that we are now controlling for the neighbourhood aspect directly through the additional variables, this is unsurprising. We find only significance for neighbourhood size from these additional variables, and again are unable to find any differences in the effort levels between NT and NT-SI, nor does it seem that the proportion of own group members an individual has in their neighbourhood alters effort levels. This is consistent with results highlighted previously.

3.5 Conclusion

This paper address the intersection of coordination problems and social identity. Reidl et al. (2016) establish that endogenous linking allows subjects to coordinate on high levels of effort thus increasing efficiency. However the inclusion of social identity in the weakest-link game with endogenous linking has not been previously studied. We replicate the standard results in the weakest-link game in the Baseline Treatment. Subjects are linked exogenously and simultaneously choose an effort level. As is consistent with previous experimental evidence subjects struggle to coordinate on an efficient equilibria. The dominant effort level that subjects exert is the minimum available.

In the face of conflicting hypotheses with regards to the addition of social identity in the weakest-link game we find that the inclusion of social identity does not result in any significant differences between NT and NT-SI with regards to either effort or neighbourhood size. Social identity is shown to be a determining feature of linking preferences in the first round of play, consistent with the premise that in-group members have a preference for linking with one another rather than members of the out-group. However this effect disappears by the second round where efficiency takes president. The ability to choose with whom one links allows subjects to remove individuals from their neighbourhoods who do not exert a high effort level. We have shown that exclusion is an efficient mechanism through which the average and minimum efforts can be increased. Excluding subjects exclude low effort players at a greater frequency then high effort players. Those who are excluded do not respond by severing links with the excluder, instead the desire to link persists and is accompanied by an increase in the future effort levels of the excluded subject.

Thus the endogenous linking mechanism allows subjects to coordinate on higher effort levels than in BT. This is overwhelmingly the case for both NT and NT-SI. However whilst effort is consistently higher in the neighbourhood treatments, we do not see a significant difference in the welfare levels across treatments. One would expect that higher effort levels should result in higher welfare. This fails to occur due to the infrequency of the maximal neighbourhood size. Recall that the benchmark of 1040 tokens can only be reached when the network is fully connected and all effort levels are coordinated at the maximum. Since only a small number of our groups are able to sustain this maximal neighbourhood size we see a reduction in the welfare levels. Thus, whilst it is the case that Welfare is not significantly different across treatments, the welfare levels are achieved through different mechanisms. In BT subjects coordinate but fail to do so on a high effort level, whilst in NT and NT-SI subjects fail to form the efficient neighbourhood size, but do coordinate on a high effort level. With additional rounds of the weakest-link game in NT and NT-SI, this effect could be overcome. The frequency of the maximal neighbourhood size increases over time (disregarding end game effects), thus with sufficient number of rounds we should see Welfare levels further diverge.

Chapter 4

Group Identification and Redistribution in Democracies

4.1 Introduction

Inequality varies across democracies. The economic interests of the poor in voting for redistribution and the rich in opposing it, therefore, cannot be the only factor affecting how people vote. This chapter uses an experiment to examine one additional influence on how people vote that could help explain these differences: group identification. This rests on the premise that group identification can weaken the relation between an individuals economic self-interest and how he or she votes, particularly among the poor (Shayo, 2009; Klor and Shayo, 2010). Thus, in so far as group identifications are more or less strong across countries, they could explain voting differences. Indeed, there is evidence that preferences for redistribution are weaker in countries or areas with more heterogeneous populations, where one might expect group differences to be more salient (Alesina and Glaeser, 2004; Dahlberg et al. 2012).

Of course, there are many factors that might explain why people do not vote according to their economic self-interest. We focus on group identification with an experiment for several reasons.

First, within political science, group identification is attracting increasing interest as a source of political behaviour (see Kalin and Sambanis, 2018, for a survey). Voting is a key form of political behaviour and the possible influence of group identification has come to the fore in some recent electoral results. For example, the BREXIT vote in the UK, the Trump victory in the US and the Five Star Movement in Italy have all been associated, albeit sometimes controversially, with the rise of 'identity politics'.

Second, group affiliations might affect electoral outcomes through a variety of mechanisms or channels and it is important, not least for public policy purposes, to disentangle their respective contributions to distribution outcomes. The strength of the experimental method over other forms of empirical investigation is that it can, through the careful design of the experiment, isolate the influence of each mechanism. In particular, our experiment identifies four potential channels through which group identification might affect electoral outcomes. Three operate through the possible motives that may guide individual voting when there are groups: own group payoffs, own group victory and, when group identification is used to infer a candidates character, the presence of groups may activate a concern for candidate character in elections. The fourth mechanism arises from a possible bias in how candidates then select proposals to win elections when voters have these additional motives.

Third, we fill a gap in the experimental literature on group influences. 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 experiment to consider voting explicitly when there is scope for group identification.[1] In effect, they examine a kind of direct democracy as everyone votes on a redistributive tax rate and the median vote determines the outcome. A representative democracy where people vote for candidates who have made proposals is more usual. We consider this case both for this reason and because representative democracies open

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

up further mechanisms through which the presence of groups may influence electoral outcomes. In effect, Klor and Shayo (2010) examine one of the four mechanisms that we identify. We test for three additional channels of group influence that can occur with elections in representative democracies.

In our experiment, subjects always have either a high or low endowment (rich and poor) and the majority are poor. They make three distribution decisions where they can stay with the status quo or shift the distribution towards either one that is more equal or one that is more unequal. They make these decisions 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; and c) a positive trade-off whereby more equality boosts total income. This is another respect in which our experiment breaks new ground.

The first decision is a simple distribution decision. The individual decides on one of the three possible distributions (status quo, more equal and more unequal) and it is incentivised because one of these decisions will be played for real. The second decision is as a candidate in an election: subjects propose a distribution in an election. Subjects know that there is a chance that their proposal will be selected in an election and if their proposal wins that election, they receive a bonus. In the third decision, subjects vote on all the possible elections (formed by the various two candidate proposal combinations). This too is incentivised because the subjects know that an election outcome will be implemented to determine subject payments. This describes our Baseline. Subjects are only given economic interests. The Social Identity Treatment has the same set of decisions. The difference is that the subjects are also given an artificial group identification (Green or Yellow). The Green group is in a numerical majority and has a combination of rich and poor members. The Yellow group is in a numerical minority and only has poor members.

The comparison between the Baseline and the Treatment enables us to test for the influence of group identification in all three decisions. This, in turn, allows us to disentangle the influence of the four mechanisms and we can test whether the influence of each is sensitive to the nature of the trade-off between equity and efficiency. We use a stark case of non-economic group identification, where the minority group is also poorer than the majority group. We do so because this 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 use artificial non-economic groups rather than natural ones, in part because this enables better control and in part because the influence of non-economic groups is often clearer/stronger with artificial groups (see Lane, 2016).

We find, like Klor and Shayo (2010), that group identification works against redistribution through a preference for own group payoffs. In particular the Green poor are less likely to express a preference for redistribution than the poor in the Baseline where there are no group affiliations. However, this is much weaker when the trade-off between equity and efficiency is positive. This is important because it means that not only does a positive efficiency-equity relation remove efficiency as a consideration working against redistribution, it also weakens the influence of the own group payoff that otherwise works in the same direction. We also find evidence of own group victory and candidate character motives in voting, but the effect of them on redistribution is complicated. In particular, it depends on the nature of the political contest: whether it is between two Greens, two Yellows or a Green and a Yellow candidate. Two Green candidates discourage redistribution but two Yellow candidates encourage redistribution. Green-Yellow contests fall somewhere between the homogeneous elections. Thus, our important conclusion in this respect is that the new mechanisms we study reveal that political inequality (whether candidates come from the majority Greens or minority Yellows) critically affects how group identification impacts upon redistribution in representative democracies.

In the next section, we set out our hypotheses. Section 4.3 describes the experiment in detail. Section 4.4 gives the results and offers discussion. We then conclude in Section 4.5.

4.2 Background and Hypotheses

The basic idea that we wish to unpack formally and test in a representative democracy framework 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

(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 (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 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).

In our framework, 'divide and rule' may arise because the poor Greens do not value redistribution as much as the poor do in the absence of group affiliations. The poor Greens identify with fellow Greens and not just poor Yellows: their interests become divided. As a result there is less redistribution via the ballot box than would be the case in the absence of these group affiliations. This is the version of 'divide and rule' that we want to test. However, it is not just the overall effect on redistribution that concerns us. We also want to isolate and evaluate the specific mechanisms or channels through which the existence of such criss-crossing group identifications might affect electoral outcomes.

Towards this end, we isolate four mechanisms through which group identification might affect electoral outcomes. The first three arise directly from the way that the presence of groups can affect an individuals motives for voting. In particular, we assume that individuals may be motivated by a mixture of economic self-interest and social preferences for inequality aversion and efficiency in all circumstances. Additionally we assume that there are three additional motives that could come in to play when there are group affiliations.

1. Own group payoffs (OGP). That is, people may identify with their group through a concern with own group payoffs (Coate and Conlin, 2004). In effect, this is the mechanism in our framework that Klor and Shayo (2010) identify.

2. Own group electoral victory (OGV). People may identify with their group and so value someone from their own group winning the election (e.g. in contest games it is often suggested that people may overinvest in contests because they like winning and such overinvestment seems to be even larger in group contests, see

Abbink et al., 2010).

3. Character of candidates (CC). It has been argued that 'character' in the sense of sincerity can influence voting (see Kartik and McAfee (2007)) and it is possible that voters may be better able to infer the 'character' of a candidate when there are group affiliations.

The fourth mechanism is an indirect consequence of the operation of any or all of the three possible mechanisms above when the election is in a representative democracy.

4. Candidates want to win elections. Candidates who want to win elections know that the electorate are differently motivated in one or more of the above ways. Thus, when there are group affiliations candidates will adjust their electoral proposals accordingly. Observed policy proposals over redistribution may therefore differ in the presence of social identity.

To see how we specifically test for each mechanism, recall our framework: individuals are always either rich or poor and the majority are poor. With group identification, there are two groups: the minority Yellow group is poor and the majority Green group is a mixture of rich and poor subjects. The first decision problem is a distribution decision; individuals can either decide to maintain the status quo, redistribute in favour of the rich or redistribute in favour of the poor. There is no representative election contest.

With no group identifications, individuals will decide on the basis of their own payoffs and equity and efficiency levels. With group identification OGP becomes an additional and new consideration and it will in most circumstances work against redistribution from rich to the poor. This is because every pound taken from the rich in the majority group and given to the poor is shared in part by the poor from the minority group. Thus, so long as redistribution is neutral in its efficiency effects (or negative), this leakage of a part of each pound taken from the rich to the poor in the minority group must lower the dominant groups payoffs. This effect will be weakened, however, in so far as each pound taken from the rich increases in value significantly in the transfer to the poor so as to offset this leakage to non-members. That is, if there is a positive equity-efficiency trade-off. This is the sense in which, in most circumstances, redistribution to the poor will lower the dominant groups payoffs and so make those who value own group payoffs in the dominant group less inclined to redistribute than they would be if there were no group affiliations giving rise to such considerations. This own group payoff effect reinforces the self-interest of rich Greens in deciding against redistribution and the self-interest of the Yellows in favouring such redistribution. The key people are the poor Greens who find their self-interest in redistribution is now checked by this own group payoff consideration. Hypothesis 1 (H1) follows and addresses the first OGP mechanism.

Hypothesis 1: The introduction of Green/Yellow identification in the negative efficiency-equity trade-off (NEE) and in the no trade-off distribution (NoEE) weakens the preference for redistribution in the distribution decision among the Green poor relative to the poor when there are no group identifications.

In the electoral setting subjects vote on all possible pairs of distribution proposals and, when there are group affiliations, each proposal also has group identification. Notice, that OGV, the second mechanism, only occurs as a possible motivation in Green-Yellow electoral contests. Notice also that for any given pair of proposals the effect on OGP is the same, independently of the group identity of the proposers, so the first mechanism provides no reason for preferring a particular proposal more when made by a fellow group member in these Green-Yellow contests. Hypothesis 2 (H2) follows and tests for the influence of the second mechanism, OGV.

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

In the experiment, candidates are simply known by the proposal they make when there are no groups. As a result there is no basis on which voters can assess the sincerity or 'character' (in the Kartik and McAfee (2007) sense) of the candidates in the experiment: that is, how close the candidate proposal is to the candidates own preferences. When candidates are group affiliated, there is, however, the information of the group affiliation and this may be used to infer 'character'.[2]

In particular, Yellows are always poor and, on average, Greens are richer than Yellows. In so far as the rich typically prefer less redistribution to the poor (which they do), then on 'representativeness' grounds voters may infer that in Green-Green contests, the candidate proposing the least redistribution is liable to have more character than the one proposing more redistribution. This is because being less redistributive is what distinguishes Greens from Yellows. In contrast, in Yellow-Yellow contests, the Yellow proposing the most redistribution exhibits the most character because Yellows are known to be more redistributive in general. Again this is because what distinguishes a Yellow from a Green is that they are more redistributive. On this account, character will not push in one way or the other in Green-Yellow contests. If Yellow is more redistributive and Green is less, then both are representative and both have character, and so it fails to distinguish between candidates. Conversely, if the Yellow candidate proposes less redistribution than the Green, then both are acting out of character and again character ceases to be something that distinguishes the candidate proposals. The latter observation is important for the connection between H2 and the second mechanism. Hypothesis 3 (H3) follows to address the third mechanism. In Green-Green and Yellow-Yellow contests the choice of who to vote for will not affect the group affiliation of the winner and so OGV cannot have an influence over which candidate to vote for. Further, with the same pair of proposals, the effect on OGP will be the same whatever group is making the proposals and hence H3 tests for CC.

Hypothesis 3: When deciding between any given pair of proposals, voters

[2] Voters in our experiment do not know whether candidates are rich or poor. This is an extreme representation because some voters will know more about the candidates than others and those who are better informed will likely know broadly the incomes/wealth of the candidates. Nevertheless, we built this into the experiment to mark what is a clear difference in practice with group identification. When there is group affiliation, voters know the candidates group affiliations. This is because, in contrast to income/wealth levels, voters will know, for example, whether a candidate is male or female, black or white, etc, without the need to acquire information.

prefer less redistribution in Green-Green contests than when the same proposals are made in Yellow-Yellow contests.

We now turn to the fourth mechanism of proposal selection when candidates want to win the election and there are the above possible ways that group identification influences voting. We consider same group election contests first. Hypothesis 4 (H4) follows from the logic of H1 and H3 in same group contests. This is because H1 encourages less redistribution in Green-Green contests and H3 has the same effect as less redistribution signals 'character'. The effect on Yellow-Yellow contests is likely to be the reverse. H1 weakens the interests that poor Greens have in voting for redistribution, but assuming economic self-interest is still valued more highly than own group benefits, then Yellows will do better to increase redistribution to attract the poor Greens by appealing to their economic self-interest. Or to put this slightly differently, compared with when there are no groups, H1 puts a break on the effect that redistribution has on attracting poor Greens. As a result more redistribution has to be proposed to get the poor Greens to vote for the relatively higher level of redistribution than would be the case if there were no groups. In addition from H3, the influence of character will push in the same direction because a more redistributive a candidate is more 'representative' of a Yellow.

Hypothesis 4: Candidate proposals are less redistributive in Green-Green contests than when there are no groups. Candidate proposals in Yellow-Yellow contests are more redistributive compared with when there are no groups.

We now consider candidate proposals in Green-Yellow contests, H5 is related to Matakos and Xefteris (2017). In effect, OGV in these contests solidifies voting on group lines (see H2). The importance of this is that, as above, it affects the trade-off that a candidate faces when deciding on the level of redistribution to propose in a Green-Yellow election as compared with a Green-Green election. In a Green-Green election, for example, a Green candidate who offers more redistribution has to trade off how this undermines both his or her CC and the Green group's OGP for the gain that comes by appealing more to the economic self-interest of the poor. In Green-Yellow elections, the same Green does not need to worry about the adverse CC effect and with OGV solidifying the voting on group lines, the effect of OGP undermining same group vote will be weaker. The change in the balance of incentives encourages greater redistribution. By analogous reasoning, the Yellow in a Green-Yellow contest need not fear, when they reduce redistribution to appeal to the economic self-interest of the rich, the loss of support that would occur in a Yellow-Yellow contest from CC nor the loss of support from falling OGP undermining group voting because OGV solidifies the group solidarity. Yellow moves towards less redistribution.

Hypothesis 5: In Green-Yellow contests, Greens propose more redistribution compared to their proposals in Green-Green contests and Yellows propose less redistribution compared to their proposals in Yellow-Yellow contests.

4.3 Experiment Design and Procedures

We begin with a description of what subjects are asked to do that is common to the Baseline and the Social Identity Treatment. For a complete set of experiment instructions, please see Appendix C.

Subjects always make the same three types of decisions: 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 candidates distribution proposals (Decision 3). They are always assigned to a population grouping of 7 and randomly endowed with either a high endowment of 100 tokens (2 people) or a low endowment of 50 tokens (5 people). We refer to the former as 'rich' and the latter as 'poor' in what follows but these titles were not used in the experiment itself. These endowments remain fixed throughout the session, as do the assignments to population groupings and subjects know this, are aware that their endowment is randomly determined by the computer, is not based on any other factors and that the endowments for the population follow this aggregate distribution.

Table 4.1: Description of Option Sets under each Trade-off

Option Set	Rich	Poor	Total	RMax	PMax	GMax	YMax	Eq/Ef
NoEE								
1	120	42	450	x		x		
2	100	50	450					
3	80	58	450		x		x	Eq
NEE								
1	132	46	494	x		x		Ef
2	100	50	450					
3	72	53	409		x		x	Eq
PEE								
1	108	38	406	x				minEf/Eq
2	100	50	450			x		
3	76	65	477		x		x	maxEf/Eq

Note: No Efficiency-Equity Trade-off (NoEE), Negative Efficiency-Equity Trade-off (NEE), Positive Efficiency-Equity Trade-off (PEE).

The Decision 1 dictator decision always has three options {1, 2, 3} for outcomes in their population grouping. Option 2 is always the status quo (i.e. the 5 poor subjects get 50 tokens and the 2 rich subjects get 100 tokens). Option 1 is always less redistributive (more unequal) relative to the status quo, in the sense that the relative position of the rich improves and the poor worsens compared with option 2. Option 3 is always more redistributive (more equal) relative to the status quo in the sense that the relative position of the poor improves and the rich worsens. We therefore always refer to votes or proposals being more or less redistributive in this sense and the degree of redistribution is captured by the size of the option number.

The subject's decision is to rank the options. There are 3 rounds, one for each of 3 different option sets. The Option Sets always have option 2 as the status quo, but the effect of more or less redistribution of moving between option 3 and 1 on total wealth varies. That is, they vary according to the presumed trade-off between equity and efficiency in the option set. In the NEE option set, there is a negative trade-off. In PEE, there is a positive trade-off and in NoEE there is no trade-off. The order of these options sets is randomised. Table 4.1 gives the precise option outcomes {1,2,3} for the rich and poor subjects in each option set.

We incentivised 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 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% probability the randomly chosen subjects least preferred option is implemented for the population 7. This ensures subjects truthfully rank 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.

In the next part of the experiment, the election, subjects are asked to make decisions both as candidates making distribution proposals and as voters in elections involving pairs of candidate proposals. There are nine elections: three for each option set (NoEE, NEE, PEE). Each option set-election is repeated three times to allow for learning to take place regarding what proposals win elections. As in the dictator decision the order in which the different option sets appear is randomised, this time across population groupings rather than across subjects.

For each election, the proposal decision involves subjects choosing a proposal from the option set {1,2,3} for a two candidate election. The voting decision is then made over all the possible pairings from the option set: i.e. {1,2}, {1,3} and {2,3}. So we are, in effect using the strategy method to elicit voting behaviour over all possible election contests. These decisions are incentivised in the following way. Subjects are told as candidates that two people from their population grouping will be selected. The proposals of these two candidates form the electoral contest. For the electoral contest that is formed the votes will be counted and the outcome of the election will be implemented and the candidate that wins this election gains a bonus of 70 tokens. 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. This describes the set-up in our Baseline. The Social Identity Treatment is in these respects the same but is distinguished from the Baseline after the dictator decision by allocating the 7 population members into either a Green group (with 2 rich and 3 poor) or a Yellow Group (2 poor). The subjects know this. In this way, they know that the majority-minority split is of the same magnitude for endowments as for social groups (2 are rich, 5 are poor and 2 are Yellow and 5 are Green). The effect of group identification in the Social Identity Treatment is the following thereafter.

1. Subjects repeat the dictator decision in the knowledge of their identity and how any redistribution affects group earnings.

2. When subjects make their proposal decision they know their group identity and they know the group identity of their opponent in the two candidate contest. As a result, each subject now makes two proposals for each option set: one for a contest with an in-group candidate and one for a contest with a candidate from the out-group.

3. When subjects make their voting decision, they know the group identity of each candidate proposal. As a result, subjects make more voting decisions than in the Baseline. For example, in the Baseline a subject faces a choice {1,2}, whereas in the Social Identity Treatment of the same, they have four versions: GG, YY, GY and YG for the mapping on to {1,2}. That is, option 1 can be proposed by a G or a Y and option 2 can be proposed by a G or Y, yielding four contests with the same options.

The comparison in the dictator decision between the Baseline and the Social Identity Treatment reveals the influence of OGP. When voting between a given pair of options, for example, {1,2}, the OGP is the same whether {1} is proposed by G or Y. However, the OGV when say G proposes {1} and Y proposes {2} will not be the same as when Y proposes {1} and G proposes {2} because when G proposes {1}, Greens have an extra reason to vote for it as it increases the chances of OGV. Likewise Yellows have an extra reason to vote for {2} in these circumstances as contrasted with when {2} is proposed by a Green. If there is no satisfaction from an own candidate winning, then there will be no difference in voting whoever proposes

$\{1\}$. Again when comparing GG with YY for the same set of proposals, OGP will be the same, but when the proposals come from GG, the least redistributive will attract CC as compared with when the proposals come from YY and most redistributive attracts CC. If CC does not motivate, there will be no difference between the GG and YY version of the same pair of proposals.

The experiment was conducted in the University of Leicester Experimental Economics Lab (LExEcon) in October 2017. All participants were undergraduate and masters students at the University of Leicester. 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. Any questions 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 140 subjects participated in the experiment - 70 subjects in both the Baseline and the Social Identity Treatment. Sessions consisted of 14 subjects. We therefore have 10 independent observations at the population grouping level in each treatment. Average payments in the Baseline were £8.98 including the £2 show up fee and sessions lasted on average 33 minutes. In the Social Identity Treatment, the average payment was £12.19 again including the show up fee and sessions lasted on average 64 minutes.

4.4 Results

We begin by analysing the effects of group identity on the elicited preferences of subjects in Stages 1 and 2 - the dictator style decisions. Recall that the experimental design elicits subject's preferences over redistribution for each of the equity-efficiency trade-offs both before the introduction of identity, when subjects are only aware

Table 4.2: Wilcoxon signed-rank tests: Preferences

ID/Income	Distribution	Preference Before	Preference After	p-value
Green Rich	NoEE	1.3	1.35	0.5637
	NEE	1.2	1.2	1
	PEE	1.85	1.7	0.2568
Green Poor	NoEE	2.6	2.53	0.4837
	NEE	2.47	2.03	0.0478
	PEE	2.87	2.9	0.9720
Yellow	NoEE	2.4	2.85	0.0147
	NEE	1.85	2.55	0.0067
	PEE	2.87	2.9	0.4142

Note: Tests are two sided. Null hypothesis: The average preferences before and after the introduction of group identity are equal. Tests are conducted at the subject level: Green Rich n=20, Green Poor n=30, Yellow n=20.

of their incomes and after when both income and identity are known. Table 4.2 gives the average level of redistribution preferred by the subjects by income and identity group. We conduct Wilcoxon signed-rank tests to test whether subject's preferred level of redistribution changes with the presence of identity. Rich subjects are unaffected by the introduction of identity, under no distribution can we say that rich members alter their preferences. This whilst on the surface may suggest that the induction of identity has little salience with the rich members, the options selected as the most preferred before the introduction of identity are close to that which would maximise group earnings. If salience of identity manifests through the desire to maximise group earnings (OGP) we are unlikely to see any fluctuations in preferences for the rich. Hence their preferences are consistent with maximising OGP and their economic self-interest. With regards to the Green poor we are able to discern significance for the NEE trade-off and although we lack significance in NoEE, the directional effect suggests that subjects alter their preferences in favour of those which maximise OGP. However the largest effects of the addition of identity emerge in the Yellow group. Recall that this group is always made up of poor individuals. The addition of identity shifts preferences closer to the level of redistribution that maximises OGP in NoEE and NEE. With regards to PEE we see a slight increase in the average preference again towards that which maximises OGP. However given that high redistribution is already prefered prior to the introduction of identity we

Table 4.3: Mann-Whitney Tests: Green Poor and Baseline Poor Preferences

Distribution	Green Poor	Baseline Poor	p-value
NoEE	2.53	2.73	0.0254
NEE	2.03	2.36	0.1023
PEE	2.9	2.74	0.2176
Average	2.49	2.63	0.1046

Note: Tests are two sided. Null hypothesis: The average preference of the poor subjects in the Baseline and the poor Green Subjects are equal. n=80 for all tests; $n = 30$ Poor Greens and $n = 50$ Poor Baseline subjects.

are unable to state any significance in the change in preferences with there being little room for manoeuvre. For Yellows, like the Greens, both the rich and the poor seek to maximise not only their economic self-interest but also their OGP once identity is introduced.

Under our experimental set up, it is the Green poor individuals that are designed to be conflicted between their economic interests and their social identity. To test H1 we compare Green poor subjects preferences with the poor subject's preferences in the Baseline. Table 4.3 gives the average level of redistribution preferred by the Green poor subjects and the Baseline poor. We conduct Mann-Whitney (MW) tests on the choices of the Green poor in Stage 2 compared to the Baseline poor in Stage 1. We find that on average Green poor subjects prefer a lower level of redistribution compared to their poor counterparts in the Baseline. Whilst on aggregate we are unable to draw any significance on the average preference differing between the poor in the Baseline and Treatment, we can state significance under the NoEE distribution. The reduction in the average preference of Green poor members is consistent with these subjects having a preference closer to that which maximises OGP. Additionally although we lack significance, the directional affects under NEE also point towards a preference for a lower level of average redistribution for the Green poor, which is consistent with the OGP motive. There is no difference when there is a positive trade-off; again this is consistent with the expected weaker influence of own group payoffs in these circumstances.

Whilst the basic non-parametric tests are illuminating in terms of the aggregate effects identity has on preferences, we also run individual Ordered Logit regressions

Table 4.4: Ordered Logit Regressions: Preferences

	(1)	(2)	(3)
GreenPoor	-1.040**	-0.900**	-1.580***
	(0.473)	(0.462)	(0.596)
NEE		-1.189***	-1.425***
		(0.231)	(0.341)
PEE		0.559*	-0.224*
		(0.322)	(0.322)
GP*NEE			0.417
			(0.478)
GP*PEE			1.959**
			(0.765)
N	240	240	240
Subjects	80	80	80

Note: Dependent variable is the level of redistribution preferred by the subject and ranges from 1-3 where the lowest level of redistribution is categorised as 1, and the highest as 3. Standard errors in parentheses are clustered at the individual level. All include period fixed effects. Significance Levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

on the preferences of the Green and Baseline poor subjects as shown in Table 4.4. Here the dependent variable is the subject's preferred level of redistribution where the lowest level of redistribution is categorised as 1, and the highest as 3. We cluster the standard errors at the individual level as there is no feedback between decisions, hence each subject constitutes an independent observation. We include a dummy variable that takes the value one for the Green subjects and zero otherwise, whilst in column (2) we control for the distribution, where NoEE is the omitted category. Finally we include an interaction term of the previous variables. These models confirm that which was found in Table 4.3. Green poor subjects have lower preferences for redistribution than their Baseline poor counterparts (column (1)). Controlling for the distribution in which the individual makes a decision shows that compared to NoEE, NEE results in a weaker preferences for redistribution whilst PEE produces stronger preferences for redistribution. Further controlling for the Greens behaviour in each trade-off shows that consistent with previous findings shown above, the Greens in PEE exhibit a greater preference for redistribution than they do in NoEE. Thus whilst there is a clear OGP effect, this is dependent on the equity-efficiency trade-off. When OGP are positively affected by increased redistribution this option becomes significantly more favourable.

Result 4.1: Green poor subjects reveal a preference for lower levels of redistribution compared to the Baseline poor, in NoEE and NEE trade-offs.

This first result is consistent with what has been found in Klor and Shayo (2010), identity causes the poor to have a lower preference for redistribution than their income would suggest giving credence to the 'divide and rule' effect. Thus, having established that OGP is a motivating factor over preferences for redistribution, we next turn to the second mechanism - that of Own Group Victory. To determine the effects of OGV we consider the voting decisions (Decision 3). Table 4.5 considers the voting behaviours in mixed group elections (GY contests). We compare the proportion of subjects who vote for the high level of redistribution when proposed by a Green candidate with the proportion of subjects who vote for that same high redistribution policy when it is proposed by a Yellow candidate. In other words, if H2 holds true, in Table 4.5 we should see a larger percentage of high redistribution votes in the first row than the second for Green group members, whereas for Yellow members the reverse should hold. We test the difference using Wilcoxon signed-rank tests. Green members are always statistically more likely to vote for a high level of redistribution when it is proposed by a member of their own group. Thus for Green members we can confirm our second hypothesis, OGV is indeed a motivating factor in voting behaviour. For Yellow subjects the directional effect also confirms H2, however we can only claim significance for voting behaviours in the NoEE distribution.

Further confirmation of H2 can be found in Table 4.6. Using the Logit model, we conduct individual level regressions on votes cast in mixed group elections. The dependent variable takes a value of 1 if the subject votes for the high level of redistribution in a given pairwise vote, and 0 otherwise. We include in the independent variables two dummy variables that control for the subjects income and social identity. 'GRich' takes the value 1 if the subject is both rich and Green and 0 otherwise. 'GPoor' similarly takes the value 1 if the subject is a Green and poor and 0 oth-

Table 4.5: 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	0.0525	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 4.6: Logistic Regressions: Voting Behaviour in Heterogeneous Elections

	(1)	(2)	(3)	(4)
GRich	-1.547***	-1.554***	-1.579***	-1.592***
	(0.219)	(0.220)	(0.224)	(0.225)
GPoor	-0.320	-0.321	-0.326	-0.329
	(0.262)	(0.262)	(0.266)	(0.269)
NEE		-0.145	-0.147	-0.147
		(0.152)	(0.154)	(0.155)
PEE		0.215**	0.218**	0.220**
		(0.103)	(0.104)	(0.106)
Vote12			0.153***	0.151***
			(0.053)	(0.052)
Vote23			-0.483***	-0.489***
			(0.062)	(0.063)
OwnCand				0.396***
				(0.102)
Constant	1.422***	1.401***	1.533***	1.349***
	(0.251)	(0.250)	(0.277)	(0.292)
N	3,780	3,780	3,780	3,780
Groups	10	10	10	10

Note: Logistic Regression on GY votes. The dependent variable takes the value of 1 if the a vote was cast for the high level of redistribution in an election and zero otherwise. Standard errors in parentheses are clustered at the group level. All include period fixed effects. Significance Levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

erwise. Hence all our analysis is then compared to Yellow voting behaviour. As we have shown subjects behave considerably differently in PEE compared to NoEE and NEE, we control for the distribution in which the vote is cast through further dummy variables. We also include dummy variables for the vote type, 'Vote12' takes a value 1 if the vote in question was between the lowest level of redistribution and the status quo and 'Vote23' between the status quo and the highest level of redistribution. Finally, we include 'OwnCand', this variable takes a value of 1 if the candidate proposing the high level of redistribution is a member of the voting subjects social identity group and 0 otherwise.

In line with standard models of preferences, we find that the rich subjects are significantly less likely to vote for redistribution than their poor Yellow counterparts. Interestingly this directional effect extends also to the Green poor. Green poor subjects, ceteris paribus are less likely to vote for high redistribution than Yellow poor members, although we fail to find any significance for this effect. Controlling for the vote type we find what amounts to a status quo bias. Recall that the initial endowments are represented by option 2. The coefficient on 'Vote12' is always positive and significant, subjects are more likely to vote for high redistribution when that option is the status quo. 'Vote23' on the other hand is negative and significant, thus subjects are less likely to vote for high redistribution when the alternative is the status quo. Controlling for the identity of the candidate offering the high level of redistribution links directly to H2. We find that subjects are significantly more likely to vote for high redistribution if it is proposed by a member of their own group, as shown by the positive and significant coefficient on 'OwnCand'. This confirms the findings in Table 4.5.

Result 4.2: Subjects are more likely to vote for a proposal made by a fellow group member in mixed identity elections (GY) than when the same proposal is made by a member of the other group, thus we confirm that subjects have a strong preference for own group victory.

We conduct a similar exercise for homogeneous election contests. Table 4.7 contrasts the proportion of votes for the high level of redistribution in a Green-

Table 4.7: Wilcoxon signed-rank tests: Voting Behaviour Homogeneous Elections

	NoEE		NEE		PEE	
	Green	Yellow	Green	Yellow	Green	Yellow
GG	60.2	85.6	59.8	76.1	65.3	85.0
YY	69.6	90.0	69.3	84.4	71.3	91.1
p-value	0.0737	0.0801	0.0825	0.0858	0.2588	0.3666

Note: Tests are two sided. Null Hypothesis: 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 both Green and Yellow tests.

Green contest with the proportion of votes for the high level of redistribution in Yellow-Yellow contest. We conduct the analysis again using Wilcoxon signed-rank tests. Consistent with H3 we find that subjects are more inclined to vote for a high level of redistribution in Yellow-Yellow contests (although this is only weakly significant). This is consistent with subjects desiring candidates to exhibit a high level of sincerity or character. The expectation of subjects is that on average Yellow candidates who are proposing a high level of redistribution are more sincere in their proposals, since the Yellow candidate themselves would be more inclined to vote for a high level of redistribution. Hence their proposals are sincere, high levels of redistribution should be voted for more frequently in YY contests. This is indeed what we find.

We again conduct individual level regressions on voting behaviour. Table 4.8 follows the same regression models as those in Table 4.6, the difference being that now we conduct the regressions only on homogeneous contests and so we omit 'OwnCand' as it is no longer relevant. In place of this, we include a dummy variable, 'YY', that takes the value 1 if the election is Yellow-Yellow and zero if the election is Green-Green. We see a similar pattern with regards to 'GRich' and 'GPoor' as we do in Table 4.6. Rich subjects are significantly less likely to vote for a high level of redistribution than the Yellow subjects. Again we see a similar directional effect for Green poor subjects but we are unable to determine this with any level of significance. We also are able to replicate the status quo bias previously found. The positive and significant coefficient on 'YY' confirms the findings in Table 4.7.

Table 4.8: Logistic Regression: Voting Behaviour in Homogeneous Elections

	(1)	(2)	(3)	(4)
GRich	-1.716***	-1.721***	-1.742***	-1.760***
	(0.244)	(0.245)	(0.248)	(0.249)
GPoor	-0.186	-0.186	-0.188	-0.189
	(0.277)	(0.278)	(0.281)	(0.283)
NEE		-0.110	-0.111	-0.112
		(0.154)	(0.155)	(0.157)
PEE		0.183	0.185	0.186
		(0.095)	(0.096)	(0.097)
Vote12			0.167**	0.168**
			(0.069)	(0.070)
Vote23			-0.401***	-0.405***
			(0.103)	(0.104)
YY				0.441***
				(0.139)
Constant	1.598***	1.574***	1.671***	1.467***
	(0.223)	(0.245)	(0.293)	(0.281)
N	3,780	3,780	3,780	3,780
Groups	10	10	10	10

Note: Logistic Regression on GG/YY votes. Standard errors in parentheses are clustered at the group level. All include period fixed effects. Significance Levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Subjects are more likely to vote for a high level of redistribution in Yellow-Yellow elections than in Green-Green elections. The effects of CC are dependent on the candidates in the election, in Green-Green CC further reduces preferences for redistribution whilst in Yellow-Yellow it increases preferences for redistribution.

Result 4.3: Elections between minority candidates result in an increase in votes for high redistribution compared to those elections between majority candidates.

We finally turn to the fourth mechanism, candidates desiring to win the election. H4 concerns Green-Green and Yellow-Yellow elections in comparison to the Baseline. Table 4.9 shows the average proposals in each election contest. We find that in GG contests the level of redistribution selected as a policy proposal is lower than in the

Table 4.9: 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	0.0207	0.6844	0.0902
Yellow-Yellow	2.6	2.6	2.7
Baseline	2.5	2.1	2.5
p-value	0.4813	0.0174	0.1391

Note: Tests are two-sided. Null Hypothesis: Proposals in the homogeneous elections (GG/YY) are equal to proposals in the Baseline for each identity group. We take as the independent observation the group average per session, hence $n = 20$ in each test.

Table 4.10: Ordered Logit Models: Candidate Proposals

	(1)	(2)	(3)
GG	-0.425	-0.204	-0.244
	(0.351)	(0.484)	(0.493)
YY	0.670	0.196	0.168
	(0.544)	(0.616)	(0.621)
Rich		-1.546***	-1.571***
		(0.369)	(0.377)
Green-Rich		-0.220	-0.232
		(0.511)	(0.521)
NEE			-0.531***
			(0.140)
PEE			0.140
			(0.113)
N	1,260	1,260	1,260
Groups	20	20	20

Note: Ordered Logit Models Candidate Proposals. Standard Errors in parenthesis are clustered at the group level. All include period fixed effects. Significance Levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Baseline for both the NoEE and PEE distribution. Comparing YY elections to the Baseline we find that the average level of redistribution proposed is higher in YY, but this is only significant in NEE.

Following the same analysis strategy as our previous results we also run individual Ordered Logit regressions, shown in Table 4.10. The dependent variable again runs from 1 to 3, with 1 being the least redistributive option and 3 the most.

Table 4.11: Wilcoxon signed-rank tests: Candidate Proposals

	NoEE	NEE	PEE
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: Tests are two-sided. Null Hypothesis: Proposals in the homogeneous elections (GG/YY) are equal to proposals in GY for each identity group. We take as the independent observation the group average per session, hence $n = 10$ in each test.

We include dummy variables GG and YY that equal 1 when candidates are from the same group identity, as such all results are compared to candidate proposals in the Baseline. Additionally we control for the income of the candidate and the distribution of the election. We see that the rich candidates propose significantly less redistribution and, as would be expected in response to efficiency considerations in voting, the proposals are less redistributive when there is a negative efficiency-equity trade-off and they are more redistributive when there is a positive efficiency-equity trade-off. However although the GG coefficient is negative and the YY one is positive, as H4 predicts, neither is statistically significant. Thus we have weak evidence that group identity affects policy proposals of candidates in homogeneous elections. Whilst this is not strongly evidenced there is no significant evidence to the contrary.

Result 4.4: On average candidates in Green-Green elections propose lower levels of redistribution than when there are no group affiliations, whilst candidates in Yellow-Yellow elections propose higher levels of redistribution than when there are no groups.

To test the final hypothesis we conduct Wilcoxon signed-rank tests on the average proposals in heterogeneous and homogeneous group identity elections. Table 4.11 details the average proposals by group identity in homogeneous and heterogeneous

Table 4.12: Ordered Logit Models: Candidate Proposals, Treatment only

	(1)	(2)	(3)
GG	-0.583***	-0.875***	-0.888***
	(0.204)	(0.256)	(0.255)
YY	0.536	1.026**	1.037**
	(0.487)	(0.499)	(0.500)
Rich		-1.154***	-1.167***
		(0.376)	(0.377)
Green		1.231***	1.248***
		(0.355)	(0.352)
NEE			-0.211**
			(0.105)
PEE			0.245***
			(0.076)
N	1,260	1,260	1,260
Groups	10	10	10

Note: Ordered Logit Models Candidate Proposals. Standard Errors in parenthesis are clustered at the group level. All include period fixed effects. Significance Levels: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

elections. We find that across all distributions, Greens propose a lower level of redistribution in homogeneous contests than in mixed elections, whilst the reverse is the case for Yellows.

To confirm this we run Ordered Logit regressions on the proposals of candidates (shown in Table 4.12), as in Table 4.10 the dependent variable again runs from 1 to 3, with 1 being the least redistributive option and 3 the most. We include the same independent dummy variables as in Table 4.10, but with the addition of the dummy variable Green, which is equal to one when the subject is a Green member and zero otherwise (we also omit the Green-Rich variable as now the only high income group is the rich Green subjects). We confirm our findings in Table 4.11 that proposals are lower in Green-Green elections than in heterogeneous elections for Green subjects, evidenced through the negative sign on 'GG'. We also show that proposals are higher in Yellow-Yellow elections than in heterogeneous elections for Yellow candidates, evidenced through the positive coefficient on 'YY'.

Result 4.5: Green subjects propose a higher level of redistribution in mixed elections than they do in homogeneous ones, whilst Yellow subjects increase their level of proposed redistribution in homogeneous elections compared to the proposals in mixed elections.

This result shows that the inclusion of minority group candidates into the election results in higher redistribution proposals. However this effect is not limited to homogeneous elections between minority candidates. The positive and significant 'Green' coefficient in Table 4.12 shows that Greens propose a higher level of redistribution in heterogeneous elections than do Yellow candidates. This effect is likely driven by the poor Greens (note the large negative coefficient on 'Rich'). This could be due to the motivating factor of OGV. Poor Green candidates are aware that voters have a preference for a member of their own group winning an election, thus proposing a higher level of redistribution will not significantly affect their chances of victory. This enables poor Greens to propose a level of redistribution which more closely matches their economic interests.

4.5 Conclusion

In this paper we use an experiment to address the question of how group identification affects redistribution in democracies. We often see large variations in the level of redistribution in economies with similar pre-tax levels of equality, one explanation for this is group identity. The variations in the level of group affiliation in democracies could have a significant impact on the level of observed redistribution. Poor members of a social identity group, which on average is financially better of than the other identity group, shift their preferences for redistribution to reflect the average preference of their social identity group rather than their income group. In this experiment we design the society to reflect this. Those conflicted between their income and social identity, do indeed reduce their preferences for redistribution. Thus, whilst their own income is worse off, that of the group improves. This however comes with a caveat, it is dependent on the trade-off between equity and efficiency. This is consistent with what has been found previously (Charness and Rabin, 2002). When there is no trade-off or if the trade-off is negative, we see this effect, positive trade-offs however result in an increased preference for redistribution compared to the previous two. This has important implications, if it can be managed so that more equality creates more efficiency there is a higher chance of greater levels of redistribution as this trade-off weakens the OGP preferences we see in other trade-offs.

Whilst there are clear affects on the preferences for redistribution induced through OGP, we also identify two further mechanisms through which voters preferences for redistribution are affected. These are, Candidate Character and OGV. Candidate Character works in the same direction as OGP in Green-Green elections, voters infer that the candidate offering the lowest level of redistribution is more sincere and thus vote for them. In Yellow-Yellow elections the reverse is true, thus here CC works in the opposite direction to OGP. The second mechanism OGV reinforces the electorate to vote along group lines. That is if the candidate offering the higher level of redistribution is a member of ones own group then the propensity to vote for the higher level of redistribution increases.

We also show that whilst the group affiliation of the poor affects the level of redistribution in an economy, this is not the only factor. Candidates too play a role. We find that when both candidates come from the majority group the average proposals for redistribution are lower than they would be in a mixed candidate election. The converse is the case when both candidates are from the minority group, then proposals are for higher levels of redistribution. In order to increase the level of redistribution in a society, the identity make up of candidates is of great significance. To have the greatest chance of higher redistribution, candidates should be fielded from the minority group. This results in higher proposals for redistribution not only under elections with two minority candidates but also under mixed identity elections. This suggests that measures of group identity are insufficient in predicting the level of redistribution, the identity make up of the candidates must also be considered.

Chapter 5

Conclusion

This thesis examines the effects of social identity on behaviour and interactions. We examine how political identity affects prosociality in the ultimatum game, how group identities can effect coordination and effciency in the weakest-link game and how the presence of group identities affect the observed level of redistribution in democracies.

We find that there is an inherent bias against those who poses a political identity in the ultimatum game. This bias takes the form of lower offers from proposers when the political identity of the responder is revealed, whereas the responders state a higher minimum acceptable offer for those proposers whose political identity is revealed. We additionally find that proposers offer higher proportions of their endowment to in-group responders and that responders also exhibit in-group favouritism through lower MAOs. The introduction of earned income and redistribution significantly lowers both proposer offers and responder MAOs. In-group favouritism is robust to these additions.

In the Chapter 3 we found that there is little effect of social identity in the weakest-link game with endogenous link formation. We find that compared to exogenously linked players, endogenous linking allows for increased levels of coordination on high effort levels whilst maintaining a highly connected network. The inclusion of social identity into the endogenous setting does not significantly alter the results. Initially we find subjects have a preference for linking with members of their own group but this very quickly disappears as financial considerations take president.

In the final chapter we considered the effects of social identity on redistribution.

We find that there is not a straightforward relation between the presence of social identity and the observed level of redistribution. Experimental subjects are shown to have a preference for having a member of their own group win an election, to prefer levels of redistribution that are beneficial for their identity group and to alter their policy proposals as candidates depending on the group identity of their opponent. However, these effects are dependent on the trade-off between equity and efficiency. Positive trade-offs significantly weaken the effects of own group payoffs, and thus makes higher levels of redistribution more probable than under a negative or no trade-off. The presence of minority group candidates increases the level of redistribution, both proposed by candidates and voted upon by the electorate. Thus minority representation is crucial to increase observed levels of redistribution.

This thesis has shown that social identity can affect experimental subjects behaviour. This can result in biases against out-group members and to increased levels of observed redistribution. However, social identity is not always a determining factor in the decision making process. We have shown that when the financial and efficiency considerations are sufficient social identity fails to motivate. Thus, social identity whilst determining behaviour under many circumstances is not always sufficient to induce a behavioural change.

Appendix A

Experiment Instructions

Comments for the reader (and not our experimental subjects) are enclosed by **. Subjects initially completed a consent form that highlighted several points such as voluntary participation, anonymity of data, and the use of the data for research purposes only.

All Participants who gave consent are presented with the following demographic questions.

Age

☐ 18-24

☐ 25-34

☐ 35-49

☐ 50-64

☐ 65+

Gender

☐ Male

☐ Female

Political Affiliation

☐ Conservative

☐ Green

☐ Labour

☐ Liberal Democrat

☐ UKIP

How Strong is your support for the political party you affiliate with?

☐ Very Strong ☐ Strong ☐ Somewhat ☐ Weak ☐Very Weak

The Ultimatum Game is played between two people; the PROPOSER and the RESPONDER. The PROPOSER is given £10 to divide between themselves and the RESPONDER. The PROPOSER'S offer is put to the RESPONDER. If the RESPONDER accepts the offer from the PROPOSER then they both receive this split. If the RESPONDER rejects the PROPOSERS offer then they both receive £0. The final amounts that the PROPOSER and the RESPONDER receive is called the outcome.

Example 1: Sally and James are playing the Ultimatum Game. Sally is the PROPOSER, James is the RESPONDER. The PROPOSER is given £10. Sally proposes a split of £7 for herself and £3 for James, the RESPONDER. If the RESPONDER rejects this offer, how much will they both receive?

☐ Sally £7, James £3

☐ Sally £3, James £7

☐ Both receive £0

Example 2:

This time Sally; the PROPOSER offers James; the RESPONDER £5. The RESPONDER accepts this offer. How much do they both receive?

☐ Both receive £5

☐ Both receive £0

☐ Sally £0, James £5

You will now have the opportunity to play the Ultimatum Game in four different scenarios. One of these games will be selected at random and you shall receive the monetary outcomes from it based on the choices you make. The game that is randomly selected will be paired with another randomly selected participant in the study who is playing the opposite role to you. If you are a PROPOSER your match will be a RESPONDER. If you are a RESPONDER your match will be a PROPOSER. Payment details will be given at the end of the survey.

Subjects are randomly assigned as Proposer or Responder and remain in that role for the duration of the Experiment. We first give the instructions for Treatment 1, followed by the instructions for Treatment 2. Instructions follow for subjects in the role of Proposers.

You are a PROPOSER You face an anonymous individual, the RESPONDER and are asked to split £10 between yourself and the RESPONDER. You do not know anything about the person you are playing with. Please indicate how much you are willing to offer to the RESPONDER.

Slider Task here. For a screenshot when the responder has several possible political identities, please see Figure A.1.

Here, you will play the Ultimatum Game five times. You face five individuals, the RESPONDERS, one at a time. You are asked to split £10 between yourself and each of the RESPONDERS, making your decision one at a time. You do not know anything about the person you are playing with apart from their political affiliation. The political affiliation of each RESPONDER is indicated on the left. Please indicate how much you are willing to offer to each of the RESPONDERS.

Slider Task. See Figure A.1 for a screenshot.

Instructions follow for subjects in the role of Responders.

You are a RESPONDER. You face an anonymous individual, the PROPOSER. The PROPOSER is asked to split £10 between themselves and you, the RESPONDER. You do not know anything about the person you are playing with. Please

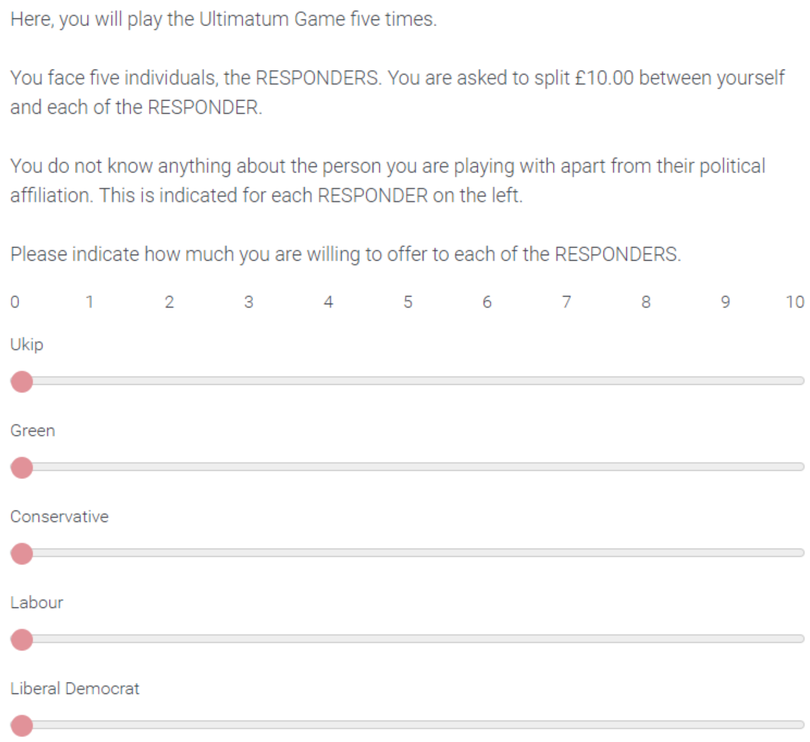


Figure A.1: Slider Task Proposers

indicate the amount below which you will reject the PROPOSERS offer.

Slider Task. For a screenshot when the proposer has several possible political identities, please see Figure A.2.

Here, you will play the Ultimatum Game five times. You face five individuals, the PROPOSERS, one at a time. Each PROPOSER is asked to split £10 between themselves and you, the RESPONDER. You do not know anything about the person you are playing with apart from their political affiliation. The political affiliation is indicated for each PROPOSER on the left. Please indicate the amount below which you will reject each PROPOSERS offer.

Slider Task. See Figure A.2 for a screenshot.

**This concludes the experimental instructions for Treatment 1. Below are

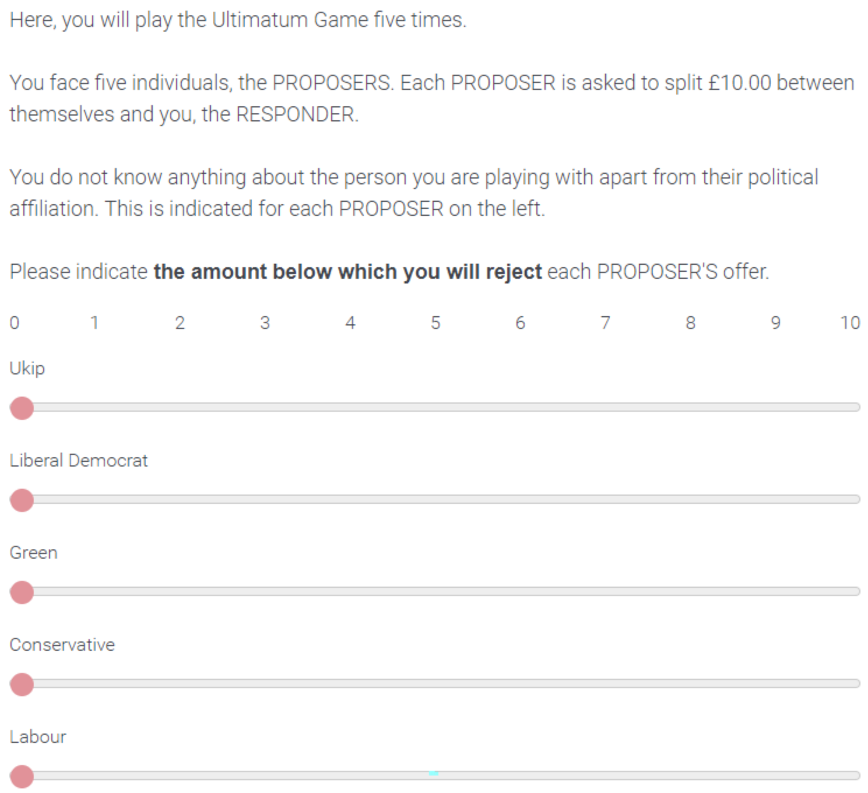


Figure A.2: Slider Task Responders

the experimental instructions for Treatment 2 in which proposers could earn their endowments and these endowments are taxed and partly redistributed. Proposers are shown the following screens.**

You, the PROPOSER have the opportunity to earn some extra money, over and above your £10, to play the upcoming Ultimatum Game. You must answer 5 questions. If you answer 4 or more correctly you play the Ultimatum Game with £20. If you answer less than 4 correctly you will play the Ultimatum Game with £10.

The five questions follow.

45 + 21 + 9 = ?

43 + 18 + 21 = ?

57 + 9 + 20 = ?

24 + 53 + (2x4) = ?

(17+18)/2 = ?

Depending on the number of questions answered correctly subjects are shown one of the two statements: "You have earned £20 to play the Ultimatum Game." "You have earned £10 to play the Ultimatum Game." First we give the instructions for proposers who play the ultimatum game with £20.

You face an anonymous individual, the RESPONDER and are asked to split £20 of your earned income between yourself and the RESPONDER. HOWEVER, your income is subject to a tax rate of 30%. You are left with an after-tax income of £14. 50% of your tax payment is redistributed and is given to the RESPONDER. The RESPONDER will receive £3. You are now asked to split your after-tax income with the RESPONDER. You do not know anything about the person you are playing with. Please indicate how much you will offer to the RESPONDER.

The remaining instructions for the proposer are as in Treatment 1, so we omit them. Now we give the instructions for proposers who play the ultimatum game with £10. The only difference from the case where the proposer has £20 is given in the following instructions.

You face an anonymous individual, the RESPONDER and are asked to split £10 of your earned income between yourself and the RESPONDER. HOWEVER, you are subject to a tax rate of 30%. You are left with an after-tax income of £7, 50% of your tax payment is redistributed and goes to the RESPONDER. The RESPONDER will receive £1.50.

The remaining instructions are as for a Proposer with an income of £20, hence, are omitted here. This is followed by instructions for Responders. These instructions are identical to those described in Treatment 1, so these are omitted. Responders were fully aware of the taxation and redistribution of the Proposers income in Treatment 2.

Thank you for taking the time to answer the decision part of the survey. Please could you now take a few minutes to complete some follow up questions.

What is your Marital Status?

☐ Single

☐ Married or Domestic Partnership

☐ Divorced

What is your Occupation?

What is the highest level of schooling you have completed?

☐ Higher Degree (e.g. MSc or PhD)

☐ Degree (including foundation degrees and PGCE)

☐ A-level, Vocational level 3 and equivalent

☐ GCSE/O-level, Vocational level 2 and equivalent

☐ Other Qualifications

☐ No Qualifications

To try to ensure we have surveyed a representative population of the area please leave your postcode:

Thank you for your time. Payments will be made via PayPal, all that is required is your email address. Please provide this below. Alternatively, if you wish to receive your payments via an alternative method, e.g. postal cheque please leave these details. All payments made will be the outcome of the randomly selected round of the Ultimatum Game. If payments for your outcome are delayed, they will be subject to an interest rate paid for the delay in line with the Bank of England base rate. This will be added to your payment.

Appendix B

Experiment Instructions

Where specific instructions only appear for 1 or 2 treatments this is denoted by [BT/NT/NT-SI Specific Instructions**] for each of the treatments the instructions are relevant for; Baseline Treatment (BT), Neighbourhood Treatment (NT) or Neighbourhood Treatment with Social Identity (NT-SI).**

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: 150 tokens = £1.
If you have any questions at any point, please raise your hand and an experimenter will come to your desk.

[**NT-SI** The experiment consists of two Stages, you will be paid for each stage independently. 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. By clicking the START button you will proceed to the experiment.]

[**NT-SI Stage 1** In this stage the computer will assign you randomly to either the Red group or the Blue group. You will remain in this group for the duration of the experiment. You will be asked to allocate a number of tokens between two other persons under three scenarios:

1. If both are from your own group,
2. If both are from the other group, or
3. If one is from your group, and one is from the other group.

You have the same number of tokens for each scenario and you must allocate all tokens between the two persons in each scenario. Allocations must be whole numbers e.g. 1, 2, 3 and so forth. Remember that you cannot allocate any tokens to yourself.

You will face five screens. In each of these screens you face the three scenarios outlined above. The number of tokens varies from screen to screen. In each screen you receive 50 more tokens than you did in the previous screen.]

[**NT-SI** Your decisions will be used to determine other persons payoffs. Similarly, your payoff will be determined by other persons decisions. The decisions you make do not affect your own payoffs.

Each person is automatically given an ID number by the computer. The computer will generate a random sequence of these ID numbers. The first number in the sequence will be the ID number of the person who allocates to the second and third IDs. The second ID drawn will allocate to the third and fourth IDs and so on. The last ID will allocate to the first and second IDs. Therefore, your payoff will be the sum of tokens allocated to you by the two persons preceding you.

For example, the computer generates the following sequence of the ID numbers, 9, 4, 1, 5, 12, . , 2, and 3. Then person 9 will allocate tokens to person 4 and 1. Person 4 will allocate tokens to persons 1 and 5 and so on. Person 3 will allocate to persons 9 and 4. Therefore, person 1s payoff will be the sum of the tokens allocated to them by person 9 and person 4.

Remember in this experiment: 150 tokens = £1

If you have any questions, please raise your hand and an experimenter will come to your desk, if you do not have any questions please click the Start button to proceed to Stage 1.]

[**NT-SI Stage 2**]

[**NT-SI** You keep the same Group you were assigned in Stage 1. You are a member of the Red/Blue Group.]

[**BT, NT** You are able to interact with seven randomly selected persons.]

[**NT-SI** You are able to interact with seven persons. These persons have been randomly selected by the computer but, it will always be the case that three persons will belong to your group, the Red/Blue Group, the remaining four persons will belong to the other group, the Red/Blue Group.]

These seven persons will not change for the duration of the experiment.

Each person is labelled by a letter: A, B, C, D, E, F or G. The same letter always refers to the same person. You will receive the label ‘Me’. The positions of the persons on the screen are randomly determined by the computer.

[**NT-SI Stage 2**] [**BT, NT** The Experiment] consists of 30 rounds. In each round you can earn tokens. Your final earnings [**NT-SI** in this stage] are the sum of your earnings in each of the 30 rounds.

In each round, you - and the other seven persons - [**NT, NT-SI** will have to make two decisions. These decisions will influence your payoffs. You make one decision called ‘With Whom Would You Like to Interact?’ and another decision called ‘Which Number Do You Choose?’] [**BT** will have to make a decision. This decision will influence your payoffs. The decision you make is called ‘Which Number Do You Choose?’] Your decisions and the decisions of the other seven persons with whom you can interact will influence your payoffs (as well as the payoffs of the other seven persons with whom you are able to interact). [**NT, NT-SI** These decisions are] [**BT** This decision is] explained in detail on the following screen.

[**NT, NT-SI** Decision: With Whom Would You Like to Interact?

You have to decide with whom you would like to interact. You can propose to interact with any of the other seven people and you can make as many proposals as you want. (You can also decide not to make any proposals.) Your interaction proposals together with the proposals of the other persons with whom you can interact determine with whom you actually interact (in the respective round).

You will not interact with another person if neither of you proposed to interact with each other.

You will not interact with another person if only you or only the other person proposed to interact.

You will interact with a person if you and the other person proposed to interact with each other. Both persons proposing to interact is the only way for an interaction to take place. We will refer to those persons with whom you interact as your neighbours.]

Decision: Which Number Do You Choose?

In each round, you and each of the 7 persons you can interact with has to choose a single number from 1 to 7; i.e. either 1, 2, 3, 4, 5, 6, or 7. [**BT** We will refer to those persons with whom you interact as your neighbours.]

Your payoffs in each round depend on:

1. Your own choice of Number

2. The smallest Number chosen by your neighbours and yourself

[**NT, NT-SI** 3. The Number of neighbours you have]

Here is the Payoff Table: (See Table 3.1)

You will also find a hard copy of the payoff table on your computer desk. This can be used for reference for the duration of the experiment.

Your payoff is determined by the cell in the row of 'Your Chosen Number' and the column of the 'Smallest Number Chosen by Your Neighbours and Yourself'.

In the table there are cells with '-' . This indicates that such a combination of 'Your Chosen Number' and the 'Smallest Number Chosen by Your Neighbours and Yourself' is not possible. For example, if 'Your Chosen Number' is 4, the 'Smallest Number chosen by Your Neighbours and Yourself' cannot be 7, 6, or 5.

Your earned tokens in a round will be the payoff as given in the table [**NT, NT-SI** multiplied by your number of neighbours divided by 7.

For each person who is not your neighbour you earn 0 tokens. For example, if you have no neighbours in a round, then you earn 0 tokens in that round.]

Examples:

[**NT, NT-SI** Suppose you have four neighbours. You chose 3 and the smallest Number chosen by your neighbours and yourself was 3, you earn 4/7 * 90 = 51 3/7

tokens.
Suppose you have three neighbours. You chose 5 and the smallest Number chosen by your neighbours and yourself was 3, you earn 3/7 * 70 = 30 tokens.
Suppose you have four neighbours. You chose 5 and the smallest Number chosen by your neighbours and yourself was 4, you earn 4/7 * 90 = 51 3/7 tokens.
Suppose you have three neighbours. You chose 7 and the smallest Number chosen by your neighbours and yourself was 4, you earn 3/7 * 70 = 30 tokens.]

[**BT** Suppose you chose 3 and the smallest Number chosen by your neighbours and yourself was 3, you earn 90 tokens.
Suppose you chose 5 and the smallest Number chosen by your neighbours and yourself was 3, you earn 70 tokens.
Suppose you chose 5 and the smallest Number chosen by your neighbours and yourself was 4, you earn 90 tokens.
Suppose you chose 7 and the smallest Number chosen by your neighbours and yourself was 4, you earn 70 tokens.]
The above examples are used to illustrate how the payoffs work in each round, they do not intend to suggest how you should make your decisions.

Information about Computer Interface
You now get information about the computer interface that you will use for the remaining duration of the experiment. As with the examples in the previous screen, the information on the following example screen is for illustrative purposes only and does not intend to suggest how you should make your decisions.

History: This window holds information about past rounds. At the beginning of a new round you will automatically receive information in this window about decisions made in the previous round. (In the example, this is round 2 (see upper part of the screen).) In the window there are 8 circles, named 'Me, A, B, C, D, E, F and G'. 'Me' always refers to you. The letters refer to the other seven persons with whom you can interact. [**NT-SI** The colour of each circle represents the group to which that person belongs; Red or Blue.]

[**NT, NT-SI** A thick complete line between two persons indicates that they both proposed to interact with each other, that is they were neighbours and, hence, did actually interact with each other. (See, e.g., the line between ‘Me’ and ‘F’).
A thin incomplete line between two persons indicates that only one of them proposed to interact. That is, they were not neighbours and, hence, did not interact with each other. Such a line starts on the side of the person that proposed to interact, and stops just before the circle of the person that did not want to interact. (See, e.g. the line between ‘D’ and ‘F’ on the example screen: ‘F’ proposed to interact with ‘D’, but ‘D’ did not propose to interact with ‘F’.)
No line between two persons indicates that neither of them proposed to interact. That is, they were not neighbours and hence, did not interact with each other.]
Next to the letters you see Numbers between 1 and 7. These Numbers indicate the chosen Numbers of those seven persons with whom you can interact. The Number next to letter A shows the chosen Number of person ‘A’. The Number next to letter B shows the chosen Number of person ‘B’ and so forth. (For example in the screen, persons ‘A’ and ‘G’ have chosen Number 5, while the persons ‘Me’ and ‘E’ have chosen Number 7.)
At the bottom of this window you find three buttons: ‘Previous Round’, ‘Next Round’ and ‘Most Recent Round’. You can use these buttons to look at the decisions in all previous rounds. The button ‘Most Recent Round’ brings you back to the last round played.
Your earnings (in tokens) in the corresponding round can be found at the top of the screen.

Decision: In this window you make your decisions.
[**NT, NT-SI** 1. With whom would you like to interact? Below this question you see the seven letters which refer to the seven other persons with whom you can interact. You can propose to interact with another person by clicking the button ‘yes’. If you do not want to interact with a person or if you want to remove a proposal to interact, you activate the button ‘no’.
2.] Which Number do you choose? In the small window next to ‘My Number’ you type in the Number you want to choose.

When you are satisfied with your decisions you confirm your decisions by clicking on the button ‘Ok’ .

At the beginning of each new round the [**NT, NT-SI** interaction proposals and the] number you have chosen in the previous round will appear on the left of the screen in the History window.

After each round you will receive information about all the decisions made by yourself and all other seven persons with whom you can interact that is, [**NT, NT-SI** all interaction proposals and] each persons chosen Number. All other seven persons on your screen will also receive information about all your decisions.

This is the end of the instructions. You will now have to answer a few questions to make sure that the instructions were clear. If you have any questions at any point please raise your hand and an experimenter will come to your desk.

Remember in this experiment: 150 tokens = £1

Control Questions

** Subjects are shown an example screen from which the following questions are based.**

- [**NT, NT-SI** With how many other persons are you interacting with in this round (excluding yourself)?]
- Are the 7 persons with whom you can interact always the same in all rounds?
- What is the smallest Number that is played by any of the 8 persons?
- Who played this smallest Number?
- How many points would ‘Me’ earn in this round?
- How many points would ‘E’ earn in this round?
- [**NT, NT-SI** With whom did you interact in the round? In other words, who were your neighbours?
- Who proposed to interact with you in the previous round?

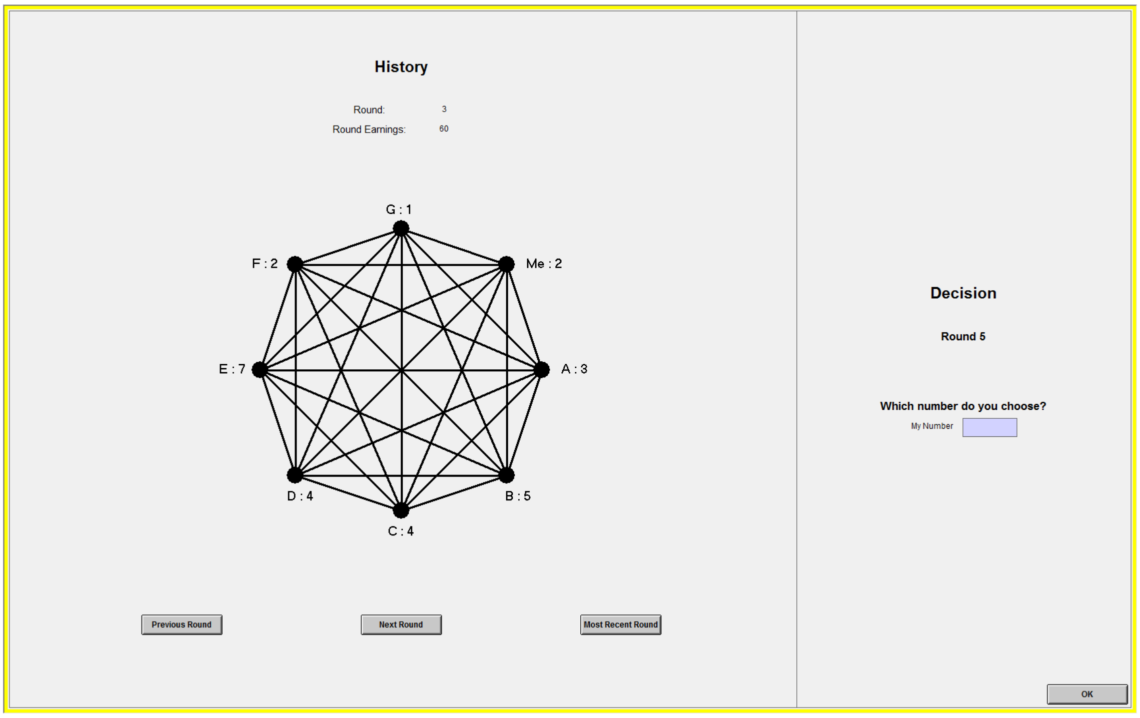


Figure B.1: Baseline Treatment

- With whom did person 'E' interact in the previous round? In other words, who were the neighbours of person 'E'?]
- [**NT-SI** Which group does person 'A' belong to?]

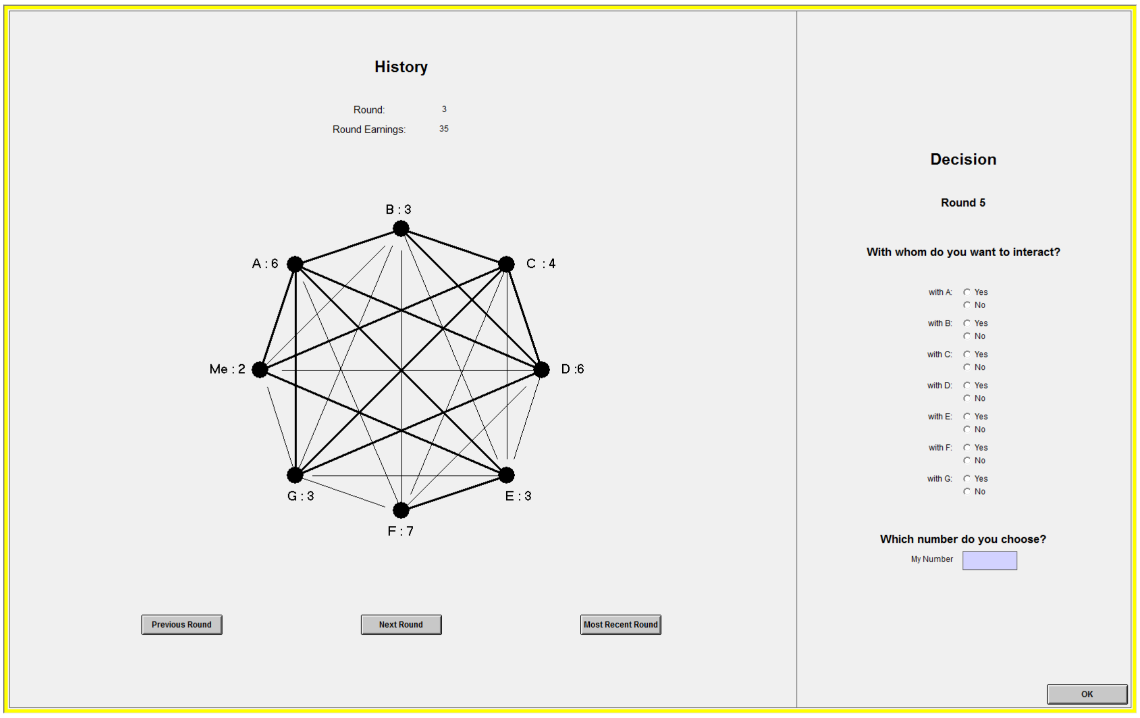


Figure B.2: Neighbourhood Treatment

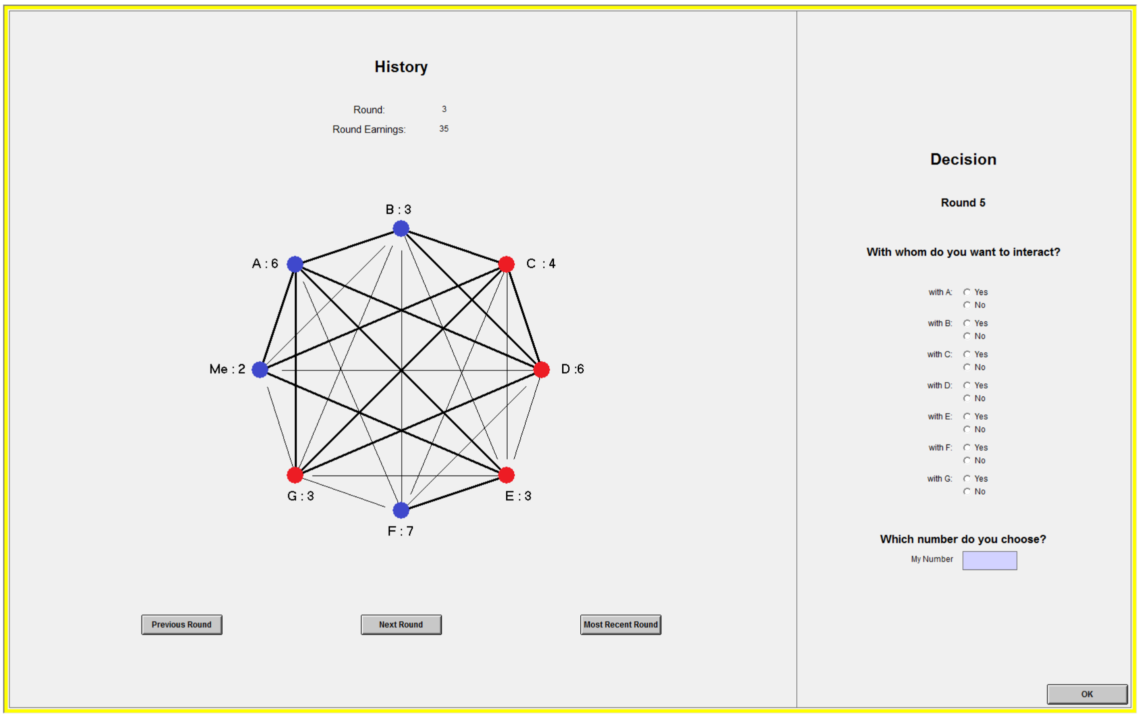


Figure B.3: Neighbourhood Treatment with Social Identity

Appendix C

Experiment Instructions

The instructions below are for the Social 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 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

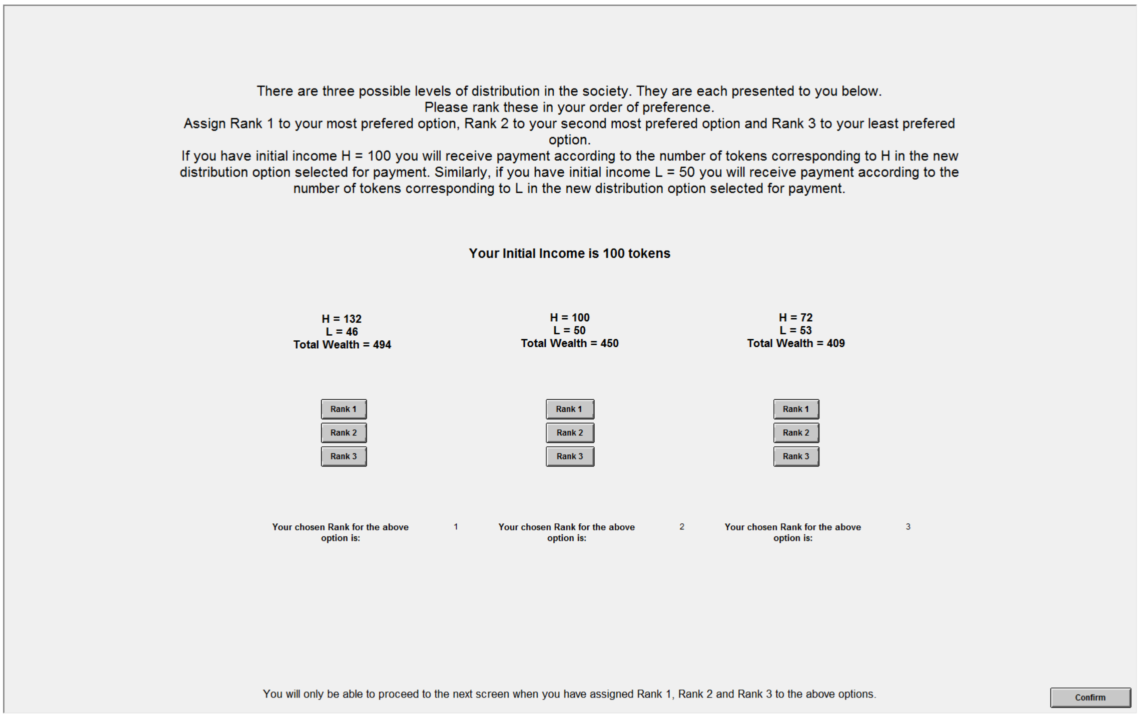


Figure C.1: Stage 1 Redistribution Preferences

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

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.

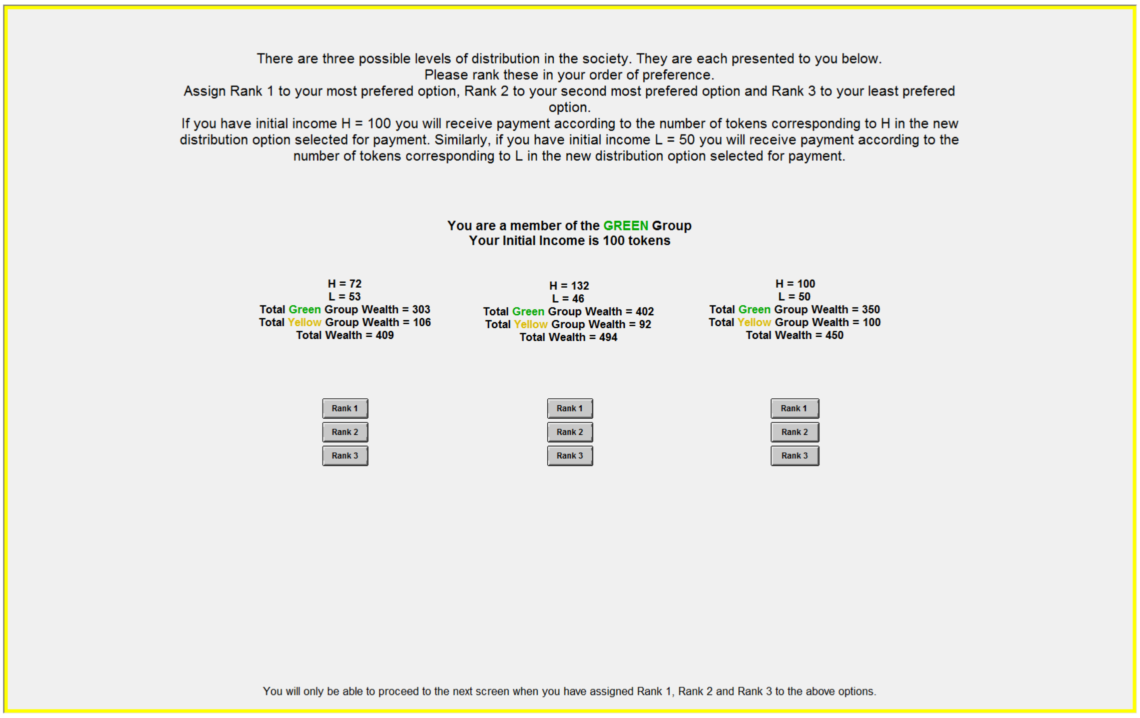


Figure C.2: Stage 2 Redistribution Preferences: Social Identity

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

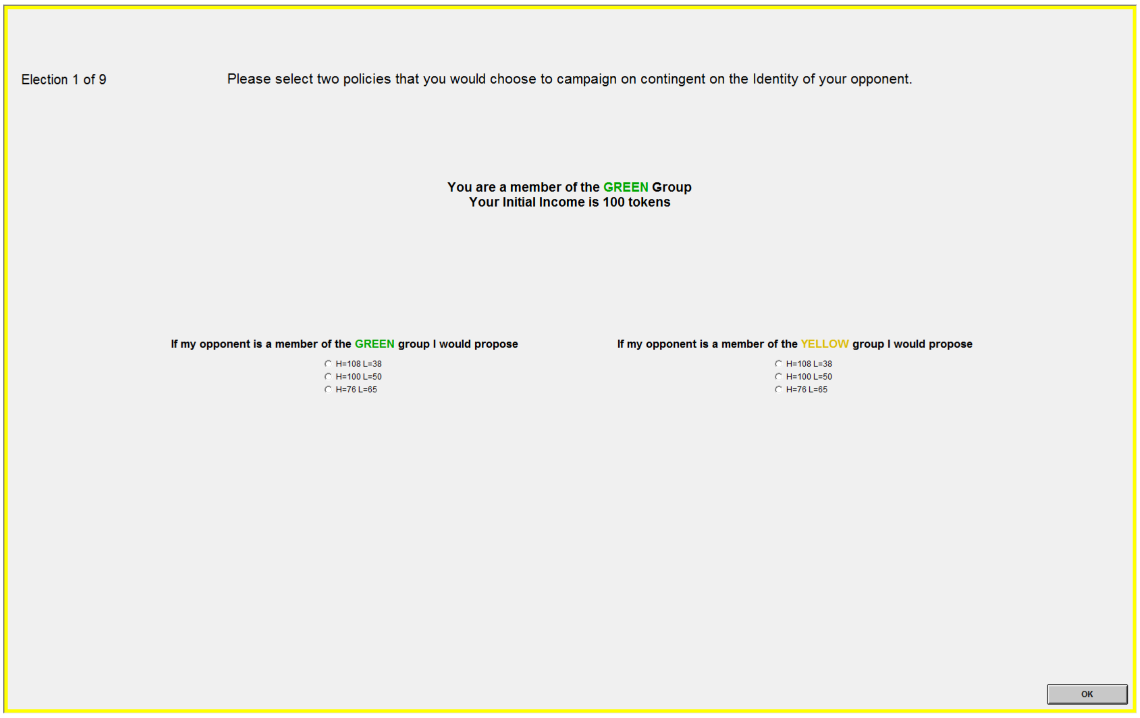


Figure C.3: Candidate Policy Selection: Social Identity

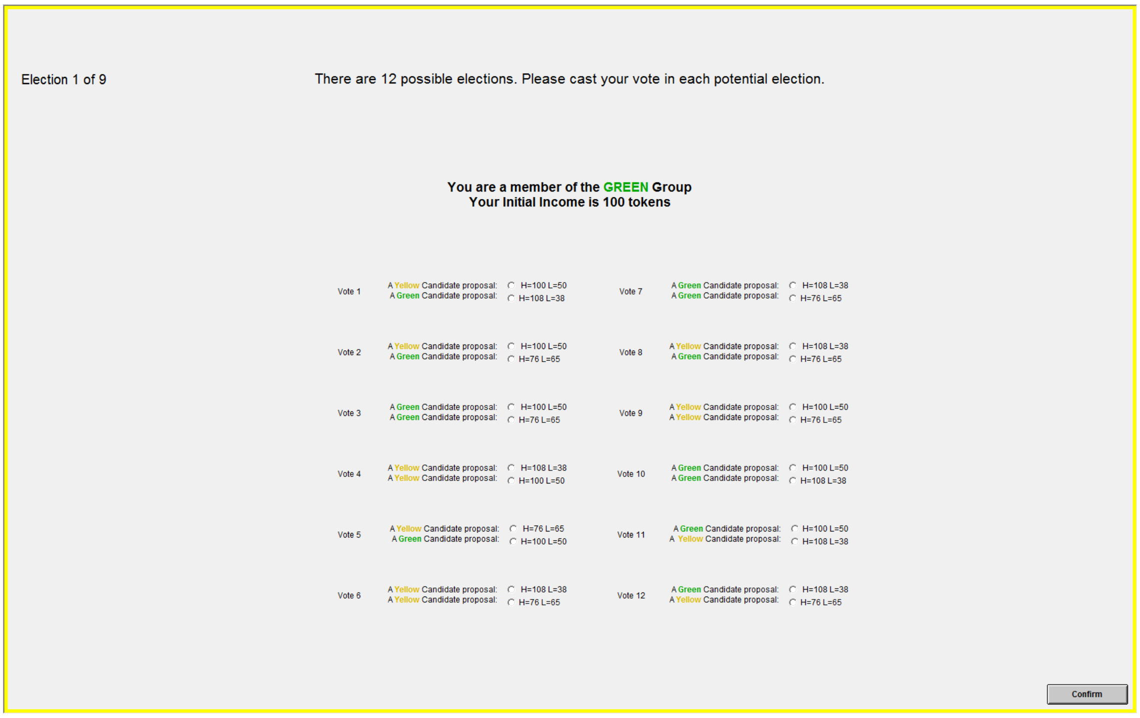


Figure C.4: Pairwise Voting: Social Identity

Figure C.5: Feedback: Social Identity

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