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Group Status, Minorities and Trust

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Abstract
We present the results of an experiment that attempts to measure the impact of majority and minority groups, and high status and low status groups, on well-being, cooperation and social capital. In the experiment, group membership is induced artificially, subjects interact with insiders and outsiders in trust games and periodically enter markets where they can trade group membership. We find that trust falls with groups because of discrimination against outsiders. Against this, however, there is evidence that low group status and minority subjects are less satisfied, and that low status subjects trust less other low status subjects.

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groups, trust games, social capital, status effects, relative size effects.

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1. Introduction

In recent years economists have paid an increasing attention to the importance of groups in understanding and explaining social capital, cooperation and conflict. For example, there is some evidence from experimental economics that membership in a group matters for individuals in the sense that it can affect their behavior in prisoner's dilemma and battle of the sexes games (Gary Charness et al., 2007; Lorenz Goette et al., 2006), in the public goods game (e.g., Jonathan H. W. Tan and Friedel Bolle, 2007), in bargaining settings (Shaun P. Hargreaves-Heap and Yanis Varoufakis, 2002; Daniel J. Zizzo, 2003), two person sequential games (Yan Chen and Sherry Xin Li, 2009), trust games (Shaun P. Hargreaves Heap and Daniel J. Zizzo, 2009) and in coordination games (Roy Chen and Yan Chen, 2010). Intergroup bias forms naturally already with children (e.g., Muzafer Sherif, 1967; James Sean Files et al., 2010) and group identification has been hypothesized to be a source of individual well-being (e.g., George A. Akerlof and Rachel E. Kranton, 2000); indeed, there is some neurobiological evidence that being a member of a group produces an endorphin rush (see Robin Dunbar, 2006).

The starting point of this paper is that groups come in different relative sizes and statuses. When there is discussion of ethnic conflict, for example, this is often phrased along the lines of majorities and minorities.[1] Similarly, perceptions of different social status are often perceived to accompany members of different groups:[2] for example, within an organization, members of a given team or of an 'old boys and/or girls network' being given status that other teams or those who are not in the elite network within the same organization do not have; or, more generally, Ivy League graduates vs. graduates from a local community college; or members of different castes in the traditional Hindu societal structure (BBC,

[1] For example, Hutu majority and Tutsi minority in Burundi and Rwanda; Sinhalese majority and Tamil minority in Sri Lanka; the French majority and Roma minority in France; the national ethnic majority and the Jewish minority at various times of European history, such as 16th century Spain or Nazi Germany; the black majority and white minority in South Africa at the time of apartheid.

[2] Psychological theories that have tried to explain hierarchical group relationships are social identity theory (Henri Tajfel and John Turner, 2001), where lower status groups contribute less to individual social identity therefore leading to differential attitudes; more directly, social dominance theory (Jim Sidanius and Felicia Pratto, 1999), which posits that low status group exhibit less ingroup bias insofar as they see the status differential as legitimate; and system justification theory (John T. Jost and Mahzarin R. Banaji, 1994), in which low status subjects are seen as internalizing beliefs that serve to maintain the status quo, even when detrimental to themselves.

2007). In some cases majority and minority status coincide with high and low status respectively: for example, domestic nationals versus perceived foreigners that are deemed of low status (e.g., Kimberly Costello and Gordon Hodson, 2010). Recent policy measures in both western Europe and United States have come under scrutiny for the extent to which they may hide, or institutionalize, intergroup discrimination.[3] In Japan, the segregation between the ordinary citizens who belonged to four recognized ranks (samurai warriors; farmers; artisans and trade people) and the minority outsiders were institutionalized in the sixteenth century. The discrimination against the outcast group still exists nowadays (Yoshio Sugimoto, 2003). However, a connection between social status and majority/minority status is not always present: for example, while the Finnish minority in Sweden is seen as low status, the Swedish community in Finland is seen as high status (Karmela Liebkind et al., 2008).

A key step in trying to understand the implications of groups for economies and organizations is then to look at how changing relative group size and/or changing group status affects preferences and behavior. The novelty of this paper is in making a first step towards understanding the impact of relative group size and status effects on the perceived incentivized valuation of groups and on corresponding incentivized behavior. We do so by running an experiment in the context of trust games, which is a natural stylized modeling setup for the operation of social capital. For example, in an organizational context, and as noted by Noel D. Johnson and Alexandra Mislin (2008), trust is recognized to raise efficiency by lower monitoring costs and turnover and by increasing uncompensated positive behavior from employees. Exchanges between group members are usually thought to incur smaller transaction costs (i.e., waste fewer resources) than would otherwise be the case (see, e.g., Francis Fukuyama, 1995, Economic Journal, 2002). These are normally seen as working in the direction of what Shaun P. Hargreaves Heap and Daniel J. Zizzo (2009; HHZ in what follows) label *positive discrimination*: i.e. greater trust and trustworthiness than what would

[3] Three recent examples are the new controversial anti-immigration policy in Arizona, which is being legally challenged by the Obama administration (CBS News, 2010); the recent waves of anti-Roma raids in France (Ethel Brooks, 2010); and the curious way in which the U.K. the increase in high fee paying international student numbers is seen as a problem requiring a tightening in immigration policy (Helene Mulholland, 2010), at a time in which heavy public funding cuts should imply a greater rather than lower need for the income that these students bring.

be expected if there were no partitions of agents into groups.[4] Cooperation, as expressed in trust and trustworthiness, could however also work in the direction of what HHZ label *negative discrimination*: i.e. lower trust and trustworthiness than what would be expected if there were no partitions of agents into groups. HHZ found evidence of negative discrimination in their trust game experiment, which suggests that the relationship between groups and social capital may be more complex than it is usually posited, at least in a trust game setup.[5]

In what is a first step towards analyzing relative group size and status effects, our focus is on a simple, incentivized, behavioral experiment that implements our experimental treatment manipulations in a minimal way. We stress the word 'incentivized' because, although there is work in experimental psychology looking at relative group size and status effects, the focus is typically (a) on attitudes rather than behavior (e.g., Naomi Ellemers et al., 1992; John T. Jost and Diana Burgess, 2000; Markus Lücken and Bernd Simon, 2005; Alberto Voci, 2006); (b) when behavior is involved, deception is systematically used in invoking the connection between responses and behavior (e.g., Itesh Sachdev and Richard Y. Bourhis, 1985, 1991); (c) deception is more generally and systematically employed in treatment manipulations and elsewhere in the experimental design (e.g., all the studies listed under parts a and b). This is not to say that this psychological research is not useful: it clearly is, and we shall relate and compare our findings with those from the psychological research in the discussion section; but it also shows that it can only be used as a starting point. For example, Edward L. Glaeser et al. (2000) found that the reported answers to survey questions on trust were often not associated with how subjects actually trusted one another in an

[4] One can always label the set of all subjects as a group, and so in a sense one could rephrase this as stating that the introduction of *further* sub-groups would lead to *further* trust and trustworthiness within the sub-group relative to the baseline in which no sub-group would exist. The same would be true in an experimental setting, i.e. all subjects in an experimental group could identify themselves as a single group in addition to any further group manipulation; but this does not prevent us from identifying experimentally the effects of having a further group manipulation, and this is what HHZ already do. As a result, we find it simpler to rely just on the standard terminology of talking of either partitioning subjects into groups or not having groups; and we follow others (such as Yan Chen and Sherry Xin Li, 2009; Roy Chen and Yan Chen, 2010 and HHZ) in doing so.

[5] Table 2 of Yan Chen and Sherry Xin Li (2009) shows evidence of positive discrimination but, arguably, stronger evidence of negative discrimination in their sequential bargaining games. The findings on negative discrimination stand in contrast with the emphasis by some on positive discrimination (e.g., Marilynn B. Brewer, 1999).

experimental trust game; and there is no behavioral research referring specifically to majority/minority and social status relationships in trust settings.[6]

Experiments with natural groups are less likely to employ deception (e.g., Christopher L. Abelson and Lauren M. Howanski's, 2002, attitudinal study), but, once natural groups as opposed to artificial groups are introduced, the question arises of disentangling what we might label as the 'pure' effect of group membership and intergroup relations from what may be, and often plausibly are, expectations and stereotyping effects at play (e.g., Jasmina Arifovic et al., 2010; Karmela Liebkind, Anna Henning-Lindblom and Erling Solheim, 2008). This is what makes employing artificial groups desirable in a first step to identify relative size and status effects. This approach has a further advantage. Once the 'pure' effects of group membership have been identified, they can be used in the future to disentangle the two types of influence in natural groups. In other words, our results form a potential baseline for future studies that attempt to identify the particular contribution that comes from an actual group's constitutive norms.

Our experimental design benchmark is HHZ. To identify relative group size effects, all we do, relative to the random color group assignment treatment of HHZ, is to vary the group size from the two HHZ groups of 6 subjects each to having one group of 8 subjects and one group of 4 subjects.[7] We control for absolute group size effects by also having control treatments in which we either pair two groups of 4 subjects each or we pair two groups of 8 subjects each.[8] To identify status effects, all we do is to change the labeling frame we adopt in experimental sessions: rather than talking of a Blue group and a Red group (as we do in the other treatments, finding symmetrical results as HHZ do), we phrase the instructions in terms of a Blue group and in terms of subjects who are "not belonging to any group" or "outsiders to the group". This is a minimal status group manipulation: that of being in a first group and

[6] Alberto Voci (2006) considers trust attitudes towards insiders and outsiders in a questionnaire study involving northern and southern Italians. He finds that trust attitudes mediate questionnaire evaluations of ingroup and outgroup members.

[7] We rely on random matching to prevent the problem of differential frequencies of interactions with insiders or outsiders, leading to potential differential repeated game effects. More details on this are in section 3.

[8] Larger majorities and smaller minorities are of course of interest for future research, but they are not suitable for a first study since, because of the random matching mechanism needed as per the previous footnote, it would reduce considerably the amount of data from insider – outsider interaction, and the likelihood to have the power to detect intergroup discrimination in the lab.

that of being in a second group which is defined purely negatively by *not* being a member of the first group. Such a minimal status group manipulation arguably has the advantage of improving the interpretability of the results relative to stronger manipulations based on entitlements inducing quizzes and/or a public ceremony at the end of the experiment (e.g., Sheryl Ball et al., 2001). Deservingness perceptions – as induced by quizzes - is an alternative obvious source of differential behavior (e.g., Elizabeth Hoffman and Matthew L. Spitzer, 1985, James Konow, 2000), and public ceremonies arguably increase the likelihood of experimenter demand effects driving the results.[9] The subtlety of our cue helps reduce the likelihood of experimenter demand effects; we use a variant of a Charles A. Holt and Susan K. Laury (2002) task as an extra control for whether such effects drive our results, and we find that they do not.[10] It also has external validity insofar as it mirrors the way in which sometimes low status is associated to being perceived as being outsiders to the high status groups, as the *buraku* are relative to the four recognized ranks of traditional Japanese society (Sugimoto, 2002).

To give a preview of our other findings, intergroup (negative) discrimination is robust across majorities and minorities, though there is a distinct sense that majority subjects like being in the majority and, in general, subjects who value more their own group membership are also subjects who discriminate more. Minority subjects appeared to discriminate less and comparatively disliked being in the minority; while low status subjects comparatively disliked being low status and discriminated less by pulling other low status subjects down.

The rest of this paper is organized as follows. Sections 2 and 3 present the experimental design and results, respectively. Section 4 discusses the results, while section 5 concludes.

2. Experimental Design

A. *Outline and Stage 1*

[9] Sheryl Ball et al. (2001) claim that experimenter demand effects do not drive their results; for the opposite view, see Daniel J. Zizzo (2010).

[10] See Daniel J. Zizzo (2010) for a discussion of experimenter demand effects. Section 4 contains a further discussion of our status manipulation.

The experiment was conducted in January and February 2010 at our university.[11] Apart from the experimental instructions and a control questionnaire, the experiment was fully computerized. The experiment was programmed and conducted with the software z-Tree (Urs Fischbacher, 2007). Almost all subjects were university students, from a wide variety of subject backgrounds. A total of 312 subjects participated in the 26 sessions. Subjects were randomly seated in the laboratory. Computer terminals were partitioned to avoid communication by facial or verbal means. Subjects read the experimental instructions and answered a control questionnaire, to check understanding of the instructions, before proceeding with the tasks. They were advised individually if any answers in the questionnaires were incorrect. The experimental instructions had a neutral frame (e.g., did not refer to 'trust', 'trusters' or 'trustees') except for the context manipulations specified below. The experiment used 'experimental points' as currency, each worth 4 UK pence (0.04 pounds).

There were five experimental treatments, each with two groups per session: two groups frame of 8 subjects (88); two groups frame of 4 subjects (44); two groups frame, one with a majority of 8 subjects and the other with a minority of 4 subjects (84); one group frame, with the minority group of 4 being 'the group' (S8); one group frame, with the majority group of 4 being 'the group' (S4). As baselines for the data analysis, we also rely on the no groups treatment (B) and the two groups of 6 subjects each with random matching also reported in HHZ (66).[12]

At the start of the experiment subjects were randomly assigned to one of two groups,[13] to be made of either 4 or 8 subjects depending on the treatment, and this was common knowledge. In two groups frame treatments, the groups were labeled the Red group and the

[11] The experimental instructions are provided at http://www.uea.ac.uk/~ec601/MinExpEAppendix.pdf.

[12] B had 5 sessions and 66 had 4 sessions, implying a total of an additional 9 x 12 = 108 subjects for the data analysis. 66 is labeled as C treatment in HHZ; we have changed the labelling to make it more congruent with that of our experimental treatments. Using two treatments from HHZ as baselines is especially useful as the subject samples were comparable, as (a) they were run in the same university and so with the same kind of subject pool and (b) the same experimenter (namely, the first author of this paper) materially conducted both our experimental sessions and those reported in HHZ.

[13] A similar minimal group manipulation has been used for example by Shaun P. Hargreaves-Heap and Yanis Varoufakis (2002).

Blue group.[14] In the one group frame treatments, there is a Blue group and there are four or eight subjects (depending on the treatment) who in the instructions are referred to as "not belonging to any group" or "outsiders to the group".

Our key experimental treatments are those that look at majorities and minorities: 84, S4 and S8. 84 considers the effect of having a majority and a minority framed as providing two group identities of equal status except for the potential implications of being a majority group or a minority group. S4 and S8 consider the effect of differential status by having a single group labeled as such, as discussed in the introduction and further in section 4. In S4 the high status framed group is the minority; in S8 it is the majority and so the potential effects of being in the majority and being the framed group combine.

Any difference in behavior from being in the majority or in the minority may however not be due to any majority-minority effect (whether or not combined with a 1 group frame effect) but rather to the absolute size of the group (4 or 8 subjects). In 44 and 88 there are no majorities and minorities, but by having groups of 4 (in 44) and of 8 (in 88) we are able to control for absolute group size effects. 44 and 88 do however change the absolute number of people participating to each experimental session (8 and 16, respectively, relative to the 12 of the other treatments), and to control for this as well we can employ the experimental data from 66 in HHZ. Finally, there is a question about what would happen if there were no groups, and the B treatment from HHZ provides such a useful baseline.[15]

Each session was divided into four stages plus four additional behavioral tasks, and the overall experimental sequence and set of treatments is summarized in Table 1.

(*Insert Table 1 about here*.)

B. *Stage 1*

Stage 1 had three rounds and was common to all treatments. Each round was a standard Joyce Berg et al. (1995) basic trust game. The truster (the 'First Mover') received 24 experimental points and had to decide how many points (if any) to give to the other person and how many (if any) to keep. All the points given were multiplied by a *conversion rate*

[14] In treatment 84, three sessions were run with the Red group as the majority group and three with the Blue group as the majority group.

[15] HHZ also test the effect of not having markets on trust game play, and find it has no significant effect.

equal to 3 before they were received by the trustee (the 'Second Mover'). The trustee then decided how much (if any) to keep and how much (if any) to return to the truster. Subjects were matched randomly and anonymously each round.[16] The only information they received was about their round co-player's decision and about their own round earnings; in treatments with groups, they had no information about the color group of co-players. The key purpose of stage 1 was to provide subjects practice and experience with trust games.

C. *Stages 2 Through 4*

Trust games. In stages 2, 3 and 4 subjects played six trust games as in stage 1, but with the following differences. Each round trusters were allocated 48 points rather than the 24 of stage 1. In each round they were randomly matched with another participant in the room, and informed whether the co-player belonged to the blue group or to the red group or to no group, though they were not told their identity. They were assigned at least once the role of trusters and at least once that of trustees. They were provided, on a round-by-round basis, with a table containing information on average giving rates and average return rates by members of each group (or "not belonging to any group") with respect to insiders and outsiders. In addition, they received a summary table with average giving and return rates for each stage from the second onwards by members of each group with respect to insiders and outsiders.

Market for groups phase. Before stages 2, 3 and 4 of the trust games were played, there was a market for groups as in HHZ. Subjects were given an endowment of 48 points. In the two groups frame treatments, they were first asked to state whether, if they could choose and both options were free, they would rather stay in their group or switch to the other. If he or she stated they would rather switch, then they became a potential buyer for the membership of the other group and they were asked how much they were willing to pay to swap places with a member of the other group. They could state any value between 0 and 48 points, the value of her endowment. Using this method we measured the WTP of agents, with a common upper limit of 48 points chosen to avoid bankruptcy problems or the dependence of the WTP range on previously earned money. Similarly, if the subject stated they would rather stay, they became a potential seller of group membership and he or she was asked to state how

[16] They were asked to make decisions within 1 minute and a half, and a small clock on the computer display informed them of how much time they had. In practice, however, they could take more, though they rarely did.

much they would need to be paid by a member of the other group in order to swap places, again with a an upper limit of 48 points. Subjects were also given the option to state that they were not willing to switch group at any price within the allowed range (0 to 48 points). Using this method we obtained information on the WTA of agents.

The one group frame treatments were identical in structure but the instructions were framed in terms of being in or out of the Blue group. For all treatments, the market then operated as a Walrasian clearinghouse, where the price was set so that the number of sellers was equal to the number of buyers of membership of the other group. Whenever there was a range of possible market-clearing prices, the lowest market-clearing price was chosen. As in HHZ, the mechanism only operated by swapping players between groups, so that each group remained with the same number of subjects throughout the experiment.[17]

D. *End of Experiment Tasks*

At the end of the experiment we had four behavioral tasks presented in randomized order and aimed to try to measure risk aversion, loss aversion, ambiguity aversion and sensitivity to experimenter demand.[18] They corresponded to (a) a standard Holt and Laury (2002) questionnaire in the domain of gains; (b) an equivalent task in the domain of losses; (c) an ambiguity aversion task; and (d) a sensitivity to experimenter demand task. The tasks details are provided in the experimental instructions. The number of times subjects choose the safer option can be taken as a measure of risk attitude in task a. Task b consisted in a set of choices between risky options as in a, but framed in terms of losses rather than in gains; we combine task a choices of the safer option with task b choices of the riskier option to get a proxy for degree of loss aversion.[19] Task c followed the lead of Engle-Warnick and Laszlo (2006) and offered a choice between an increasingly ambiguous lottery and the same lottery disambiguated but at a price in terms of lower expected value. The number of times subjects went for the unambiguous measure can be used as a measure of ambiguous aversion.

Task d presented an option between two lottery choices, one increasingly dominated by the other; the dominated option was characterized by a smiley face and a sentence stating that

[17] Subjects were told that they should make their market decisions within four minutes.
[18] Due to a computer failure, we were not able to collect this data for one session of treatment 88.
[19] A loss aversion subject would be risk loving in the domain of losses while being risk averse in the domain of gains.

"it would be nice if some of you were to choose" such an option. The nudge provided towards choosing the dominated lotteries was significant by the standard of what we know about experimenter demand characteristics (see Zizzo, 2010), with the smiley face providing a social cue to interpret the sentence being provided.[20] As a result, we measure the degree of sensitivity to experimenter demand as the number of dominated options choices being made.

E. *Payments*

Each session lasted around 75 minutes. The average earnings were 14.39 UK pounds per subject (approximately 22-23 US dollars). Payment was based on the earnings from each of the markets; plus those from a randomly chosen round from each of the four stages; plus those from one of the ten choices for each of the end of experiment tasks, with any relevant lottery being played out by the computer. Subjects were privately paid and left the laboratory one at a time in an order designed to minimize the likelihood of seeing each other.

F. *Relationship to Baseline Treatments from HHZ*

The 1G66 treatment was identical in stages 1 through 4 structure to that of our experimental treatments under a two groups frame, with the key difference being that there were two groups of six subjects.[21] The B treatment had 12 subjects per session as the 66, 84, S8 and S4 treatments. There was no assignment to groups. Stage 1 was identical to stage 1 of the other treatments. Stages 2, 3 and 4 had no market for groups. In its place, at the beginning of each of stages 2, 3 and 4, there was a two minutes waiting period, at the start of which subjects were paid an additional 48 points. This was meant to mirror the other treatments, both by providing the same money amounts and by creating a temporal wedge between trust game tasks. Stage 2, 3 and 4 trust games were as in the other treatments, to mirror the information provided in stages 2, 3 and 4 of the other treatments, the computer screen displayed information on average giving rate and average return rate, with a summary table

[20] Note that we could not say that "it would be nice if *all* of you were to choose" the dominated option, since this sentence would in fact have been deceptive given our experimental goals (the usefulness of the measure is in having a distribution of subjects based on the measure). We further discuss the validity of this measure in section 4.

[21] A second small difference concerned the matching rule. HHZ's matching rule ensure that in every stage subjects were precisely matched the same number of times with insiders and outsiders, although this implied that the likelihood of being rematched with insiders and outsiders was slightly different. We could not retain this constraint with the uneven number of subjects in our key treatments and so we opted instead for pure random matching in our experiment, with any other subject in the room being equally likely to be matched with any given subject, no matter the group.

on average giving and return rates from stage 2 onwards being provided at the end of each stage. None of the HHZ treatments tried to control for risk aversion, loss aversion, ambiguity aversion and experimenter demand sensitivity by the means of end of experiment tasks.

3. Experimental Results

A. *Giving and Return Rates: Bivariate Tests*

We define the *giving rate* as the fraction of the endowment given by trusters to trustees, and the *return rate* as the fraction of the amount received by trusters which is returned by the trustees to the trusters (where the amount received by trustees is 3 times what was given in all treatments except SI, where it is either 2 or 4 times what was given).

(*Insert Table 2 about here*.)

Table 2 shows the average giving and return rates in each experimental treatment plus the HHZ baselines. If we focus on our experimental treatments and on stages 2-4 where group information is known, we find that in 21 sessions out of 26 giving rates were lower in stages 2-4 relative to stage 1 (Wilcoxon $P < 0.001$),[22] while in 25 sessions out of 26 return rates were lower in stages 2-4 relative to stage 1 (Wilcoxon $P < 0.001$).[23] This replicates what was found in HHZ.

There is overall evidence of ingroup favoritism across our experimental treatments: giving rates and return rates are lower for other subjects with the same group status than for those with a different group status (Mann Whitney $P = 0.031$ in both cases). We can determine whether this discrimination is positive or negative by comparing stages 2-4 giving and return rates towards insiders and outsiders against the stages 2-4 baseline of the B treatment.

There is no evidence from Table 2 that trusters give more to ingroup members in the group treatments, including all new experimental treatments, than in the baseline with no groups. As a result, there is no preliminary aggregate evidence of positive discrimination, i.e. that cooperation is higher with insiders than it would be if there were no groups. The

[22] Throughout this paper, all reported tests are two tailed except where otherwise specified.

[23] Here and elsewhere, statistical significance is estimated by treating session averages as the unit of observation, in order to control for possible non-independence of choices within each session. In all the 66 treatment sessions stage 2-4 giving rates and return rates are lower than in stage 1.

evidence seems to suggest that, if anything, same group status giving rate is lower than in the B baseline (Mann Whitney P = 0.027); the robustness of this result will need to be verified in the regression analysis. Same group status return rates are either the same or lower than return rates in B (Mann Whitney P = 0.090).[24]

There is preliminary evidence from Table 2 suggesting that there is aggregate negative discrimination, although there seems to be variation in the extent to which this operates across treatments. Giving and return rates to different group status subjects are both lower than B treatment giving and return rates (Mann Whitney P = 0.001 and 0.011, respectively).[25] One problem with interpreting discrimination in return rates is that subjects may simply return proportionally less because they have been given less. This might occur for a number of psychological motives which have been documented in other experiments, such as inequality aversion (Ernst Fehr and Klaus M. Schmidt, 1999), reciprocity (Armin Falk and Urs Fischbacher, 2001) or trust responsiveness (Gerardo Guerra and Daniel John Zizzo, 2004). We can control for this in regression analysis on return rates, which we shall present shortly.

Pure group size effects can be investigated by considering treatments 44, 66 and 88, where the group size effect is not combined with those of majority-minority relationship or one group framing. The one statistically significant result we find is that 88 subjects return more to subjects of the other group than 44 and 66 (Mann Whitney P = 0.039), and this is reflected in lower discrimination in return rates (Mann Whitney P = 0.039).

Figure 1 displays giving and return rates highlighting the role of majorities, minorities and one group framing.

(*Insert Figure 1 about here*.)

In Mann Whitney tests, we find no evidence of different giving and return rates relative to treatments with no group size inequality in Mann Whitney tests. Both majorities (Wilcoxon P = 0.007) and minorities (Wilcoxon P = 0.049) discriminate against different

[24] These results are not dependent on our inclusion of 66 from HHZ in the sample. If we exclude the 66 treatment, the respective Mann Whitney P values are P = 0.004 and P = 0.068 with respect to same group status giving and return rates, respectively.

[25] Again, this result is robust to excluding 66 from the sample, with regards to both other group giving and return rates (Mann Whitney P = 0.001 and P = 0.016, respectively).

group status subjects with respect to giving rates, while there is not sufficiently strong session level evidence of discrimination in return rates on the part of majorities (Wilcoxon P = 0.379) or minorities (Wilcoxon P = 0.301) to achieve statistical significance. Obviously the regression analysis will be useful to verify the robustness of this result.

What Figure 1 clearly shows, in relation to one frame treatment sessions, is that Not Blue subjects give less on average; this appears driven by lower giving to other Not Blue subjects (Wilcoxon P = 0.021). While Blue trusters gave 40.6% of their endowment to other Blue trusters, Not Blue trusters only gave 27.1% of their endowment (Wilcoxon P = 0.039).

B. *Giving and Return Rates: Regression Analysis*

The above analysis is preliminary both because it conservatively operates purely at the session level and because bivariate tests obviously miss out on the effects of covariates, such as behavioral reciprocity in the case of return rates. Table 3 employs regression analysis on mean Stage 2-4 giving and return rate by each subject to verify treatment effects, and the role of possible covariates, in a fine-grained way.

(*Insert Table 3 about here*.)

In the regressions, error clustering is used to take into account the possible non-independence of observations by different subjects in the same session.[26] We have treatment dummies for Group (=1 in all treatments except B), GruopOf8 and GroupOf4 (=1 if subject belongs to group of 8 subjects or 4 subjects, respectively), Majority (=1 if subject belongs to strict majority), Minority (=1 if subject belongs to strict minority) and NotBlue (=1 if subject is in the a low status framed subject). The return rates regressions have Giving Rate received as the Second Mover as an independent variable: that is, the mean stages 2-4 giving rate the subject has received when playing as a trustee. This allows us to control for the positive relationship which we might expect between giving rate and return rate. Both giving and return rate regressions control for Stage 1 giving and return rates. Finally, there are a number of dummy variables that capture individual-specific heterogeneity: stage 1 giving and return rate, age, and dummies for gender (= 1 for women), economics or management educational

[26] We have also run random effects regressions getting the same broad picture in terms of mean giving rates, whereas estimates of regressions on mean return rates collapses to OLS estimates due to zero variance being explained by the session level random coefficients; this leaves OLS with error clustering as the better estimation option.

background (= 1 if applicable) and nationality (UK = 1 for UK subjects and China = 1 for Chinese subjects).

Models 1 and 3 in Table 3 show that, although negatively signed, the introduction of groups *per se* does not decrease giving and return rates to subjects with the same group status; of course, it also does not increase them. Models 2 and 4 show that, when controlling for a number of covariates including (in relation to return rates) behavioral reciprocity, the introduction of groups decreases giving rates by around 20% and return rates by around 9%.

RESULT 1. There is no evidence of positive discrimination. There is evidence for negative discrimination both in giving rates and in return rates.

However, low status subjects give 11% less to other low status subjects (P = 0.037); return rates to other low status subjects also appear lower but miss statistical significance in a two tailed test (P = 0.107); while there is no statistically significant evidence that low status subjects decrease their giving or return rates towards high status subjects.

RESULT 2. Low status subjects trust less other low status subjects

Result 2 agrees with our findings from the behavioral tests in section B. The one other treatment variable that is statistically significant in Table 3 is Minority in relation to the return rate to different group status subjects: minority subjects give more to majority subjects, and this is statistically significant (P = 0.037).

RESULT 3. Minority subjects return more to majority subjects.

Results 2 and 3 offer potential qualifications to Result 1, to the extent that they operate in the direction of reducing discrimination, either in giving rates (Not Blue, low status subjects) or in return rates (minorities). We shall investigate this more in the next section, which will present regressions on *the extent* of discrimination.

No other treatment variable is statistically significant in Table 3. Among the other covariates, there is evidence of behavioral reciprocity in Models 1, 2 and 3, though less so in Model 4 (P = 0.064 one tailed). There is also various (though not universal) evidence of dependence on giving and/or return rates in stage 1, which may be due to a combination of individual characteristics and learning dynamics.[27] No individual specific dummy variable is statistically significant at P < 0.05 or better, although there is marginal evidence (P < 0.1) for UK subjects to give more (Models 1 and 2) and for Chinese students to return less to different group status subjects (Model 4).

C. *Perceived Values and Discrimination*

We now focus on the treatments with groups. This is (a) to try to get an understanding about whether subjects value groups equally depending on whether they are majority or minority groups, or whether they are framed as not belonging to any group at all; and (b) to get a better understanding of discrimination using perceived value information and, in the subset of experimental treatments that we ran (and so excluding 66 from HHZ), information from the end of experiment tasks.

Perceived value measurement. Following HHZ, we measure perceived value subjects placed on own group membership by the extent to which subjects place a value on own group membership in excess of its material value. The markets at the start of stages 2, 3 and 4 in the C, SG, SF and SI treatments (and stages 3 and 4 in SM) provided an incentive-compatible mechanism for the revelation of individual preferences for staying in one's own group (the willingness to accept, WTA, value) or for switching groups (the willingness to pay, WTP, value). As there were repeated markets, subjects also had the opportunity to gain experience both about the nature of the social commodity being traded (i.e. membership of a given group) and about the market mechanism itself.[28]

WTA(own) is the positive price a subject needed to be paid to be willing to switch and WTP(own) is equivalent to a *negative* price on own group membership, and so they reveal

[27] The two may of course interact: for example, subjects may acquire information about the social preference distribution in the population by the stage 1 play experience (e.g., David Levine, 1998).

[28] HHZ discuss whether this is a genuine psychological benefit rather than the artifact of well known mechanisms, like reference dependence. They conclude that, at least partially, it corresponds to genuine perceived value.

the value that an individual places on membership of his or her group.[29] Figure 2 provides information on WTA and WTP values observed in the experiments.

(*Insert Figure 2 about here*.)

We use HHZ's simple perceived value measure M as equal for each subject and market to WTA(own) or – WTP(own), whichever is the case.[30] Unlike in most of HHZ's treatments, all of our experimental treatments, and 66 from HHZ, are such that group membership does not imply, either directly or in terms of frequency of interaction, a financial advantage in being inside rather than outside a group.[31]

One problem with the average M measure of the mean psychological benefit from own group membership is that subjects could state that they were not willing to lose their membership at any allowed price between 0 and 48 points. 9.5% of the choices were of this 'definite stay' kind (between 5.6 and 13.4% in different treatments). As in HHZ, we opted for two routes to deal with this problem. M contains all observations, but conservatively introduces a valuation of 49 for these definite stays:[32] since the lower bound for valuations was – 48 (the budget), if anything, for $x > 49$, this introduces a downward bias. Mb simply omits 'definite stay' observations and also introduces a downward bias in average M1 estimates. Either way, in the light of possible downward biases, our average M1 estimates

[29] A price of zero was also a possibility, allowed by the experimental program in relation both to WTA and WTP valuations. Therefore, technically we always had a *non-negative* price (WTA(own)) or a *non-positive* price (WTP(own)), with one further qualification to be mentioned shortly.

[30] In HHZ, this is the most effective measure in predicting discrimination. Note that in each market subjects *either* choose a WTA *or* choose a WTP; they do not do both. Hence, a strategy by which a subject places both a WTA and a WTP is not feasible. In addition, a strategy of putting a a high WTA just in case there is an opportunity of making money, rather than because of a desire to sticking with one's own group, is not incentive compatible: if I do not care about which group to be in, even if I am offered a single unit I should rationally oblige and accept, and therefore I should place a WTA = 1.

[31] However, we also carried robustness checks in our analysis by computing the equivalent of the M2 and M3 measures in HHZ: M2 is equal to M1 minus the expectation of the material gain from a switch when these expectations are formed adaptively (i.e., the expected relative material/gain is the same as that in the last stage for the markets at the start of stages 3 and 4, and to zero for the market at the start of stage 2 as no past information on relative group trustworthiness is then available); and M3 is equal to M1 minus the expectation of the material gain from a switch when these expectations are formed rationally (i.e., the expected relative material gain/loss is the same as actually occurs).

[32] We chose the closest integer value to 48 in keeping with the experimental procedure, where for simplicity subjects could only provide integer valuations: therefore, 49 is the lowest value in keeping with this constraint. In terms of upward bias of perceived value estimates, the 'worst case scenario' for this modeling choice would be if all six agents had a true value of 48.001 and preferred not to round their valuation to 48; even in this stress case scenario, the implied upward bias would only be 0.5. In practice, none of our key results would change if we were to choose a value, say, of 48.001 for 'definite stay' cases.

should be interpreted as conservative estimates of the revealed perceived values of own group membership.

Results on perceived values. Table 4 presents M and Mb mean values by treatment, while Figure 3 illustrates mean M values graphically.[33]

(*Insert Table 4 and Figure 3 about here*.)

The perceived value of groups is positive in all the sessions and whatever the measure used (Wicoxon $P < 0.001$); in the market with the greatest experience, namely the stage 4 market, it is 21 on average for M and 17 for Mb. We are not able to detect statistically significantly differences between aggregate majority and minority mean values. In contrast, in the differential status treatments, Not Blue subjects perceive value of not Blue membership as being markedly lower than that of Blue subjects (Wicoxon $P < 0.007$ in relation to the M measure and $P < 0.037$ in relation to the Mb measure): for example, in treatment S8 Blue's mean perceived value is 28 points, whereas not Blue's mean perceived value is just 13 points, i.e. just around half as much.[34]

To investigate the determinants of psychological value further, we present some regression analysis on M (regressions 5 and 7) and Mb (regressions 6 and 8) in Table 5, again controlling for possible session level specific effects by using error clustering.[35]

(*Insert Table 5 about here*.)

The independent variables are all those from Table 3 that can be defined in the subsample. Regressions 7 and 8 restrict the samples to those sessions for which end of experiment tasks have been measured and therefore, while losing statistical power, are able to include proxies for Risk Aversion, Loss Aversion, Ambiguity Aversion and Conformism. Risk aversion is proxied by the number of risk averse choices in the domain of gains; Loss Aversion by the sum of risk averse choices in the domain of gains and risk loving choices in

[33] The same aggregate value result if HHZ M2 and M3 values are used instead.

[34] The contrast remains if M2 and M3 are used, and so does not appear to be explained by different group membership profitability expectations. Table 4 also shows that the gap in perceived value persists throughout the experiment. One implication of high perceived values is *social inertia*, in the sense that subjects tend to stay in their group rather than switch: only 11.6% of choices result in a group switching deal (between 8.9% and 14.8% depending on the treatment).

[35] Random effects regressions on psychological value collapse to OLS estimates due to zero variance being explained by the session level random coefficients; this again leaves OLS with error clustering as the better estimation option.

the domain of losses; Ambiguity Aversion and Conformism respectively by the number of ambiguity averse and socially conformist choices.

Majority and NotBlue are significant at $P < 0.05$ or better across all four regressions: subjects are willing to pay 7-9 points more on average for being in a majority group ($P < 0.01$) and they are willing to pay less for being low status members by 13-16 points ($P < 0.001$).

RESULT 4. When covariates are controlled for, subjects appear to like being in majorities.

RESULT 5. In the differential status treatments, subjects dislike being in the low status group.

Both results are plausible and will be discussed in section 4. Chinese subjects appear to value group membership less, but the effect only holds in regressions 7 and 8.

Explaining discrimination. Let DGivingRate (DReturnRate) be equal to mean giving (return) rate by a subject to same group status subjects minus mean giving (return) rate by the same subject to different group status subjects. To probe more deeply in explaining discrimination, we ran regressions, controlling for session specific effects using error clustering, on DGivingRate (regressions 9-14) and DReturnRate (regressions 15-20).[36] The results of these regressions are given in Table 6.

(Insert Table 6 about here.)

Regression 9 contains the same independent variables as the regressions in Table 5. Regression 10 adds the M variable and regression 11 adds interaction terms between M and GroupOf8, Majority, Minority and NotBlue. Regressions 12-14 are the corresponding regressions adding the end of experiment task variables (and working on the subset of sessions for which the end of experiment task data is available). Regressions 15-20 are the

[36] Random effects regressions on DAvgGivingRate give similar qualitative results, while those on DAvgReturnRate once again collapse to OLS estimates due to zero variance being explained by the session level random coefficients

corresponding regressions to Regressions 9-14 but with DReturnRate as the dependent variable and DGiving Rate as Second Mover as an added independent variable (defined as the giving rate from insiders minus the giving rate from outsiders which the subject has experienced as a trustee).[37]

Regressions 9 and 12 show that NotBlue subjects discriminate less by about 8%. This agrees with the earlier finding, summarized in Result 2, that being labeled as an outsider to the one group depresses giving rates to other outsiders. We also just determined that NotBlue is a strong negative predictor of perceived value (Result 5 and Table 5). That being the case, it is not surprising that NotBlue becomes statistically insignificant as perceived value measures are introduced: its effect appears to operate through the impact it has on perceived value, which, in turn and as in HHZ, predicts less discrimination in giving. The natural interpretation of this is that, because low status subjects dislike being low status, they discriminate less in giving.

RESULT 6. Subjects who value their group more give comparatively *more* to insiders relatively to outsiders.

RESULT 7. In the differential status treatment, as they place a lower perceived value on their own group status, low status subjects discriminate less in giving rates between insiders and outsiders.

No other variable is statistically significant in the DGivingRate regressions. The picture from the DReturnRate regressions is more complicated. Subjects who place more value on their own group discriminate *less* rather than more (at least $P < 0.05$ in regressions 16-17, 19-20). This finding is in contrast to HHZ and will be discussed in section 4 together with Result 6. We also find that M × NotBlue is negative and statistically significant ($P < 0.05$ in regressions 19 and 20): subjects who are more content with being low status subjects,

[37] We report only M variable regressions to have more statistical power. However, results are similar if the Mb variable is used instead as the dependent variable. We have also tried to replace M with M2 or M3. As a predictor of discrimination, M2 generally works as effectively as M, while M3 tends to be less effective in predicting discrimination: these results replicate HHZ (p. 413, footnote 29).

therefore placing higher M value on their own group, are less likely to discriminate in return rates.

RESULT 8. Subjects who value their group more return comparatively *less* to insiders relatively to outsiders.

RESULT 9. In the differential status treatment, low status subjects discriminate less in return rates between insiders and outsiders.

We also find that, when controlling for the M interaction terms, there is evidence that minority subjects discriminate less ($P < 0.05$ in regressions 17 and 20); this may be seen as an implication of Result 3 above that minority subjects return more to majority subjects.

RESULT 10. When covariates are controlled for, minority subjects discriminate less in return rates against majority subjects.

British students appear to discriminate more in regressions 15-17 ($P < 0.05$), but this effect disappears when controlling for end of experiment task measures as in regression 18-20. Similarly, the standard prediction of behavioral reciprocity (in this case being the victim of more discrimination in giving rates implying greater discrimination in return rates) receives some mild support in regressions 15-17 ($P < 0.05$ if one tailed test) but this is no longer the case in regressions 18-20. There is some evidence from regressions 18-20 that risk averse subjects appear to discriminate less ($P < 0.05$ or 0.06), loss averse subjects appear to discriminate more ($P < 0.05$ or 0.06) and ambiguity averse also appear to discriminate more ($P < 0.05$ or 0.05); our measure of social conformism however is uncorrelated with behavior.

RESULT 11. To the extent that they are proxied by our measure of social conformism, experimenter demand effects do not explain perceived value or discrimination in either giving or return rates.

4. Discussion

Do relative group size and social status affect the basic finding of intergroup discrimination found in HHZ, Yan Chen and Sherry Xin Li (2009), Gary Charness et al. (2007), and elsewhere? The answer is a possible yes for relative group size effects, and a very likely yes for social status effects. In the psychological research, majorities and minorities can both display ingroup bias (Geoffrey J. Leonardelli and Marilynn B. Brewer, 2001), though with more bias typically expressed by minority groups (e.g., Brian Mullen et al., 1992); we do find the reverse potential asymmetry, insofar as minority subjects discriminate less in return rates against majority subjects when covariates are controlled for (Result 10); and this appears to be connected to greater giving to majority members (Result 3). Three obvious qualifiers to our finding apply. First, our group identity manipulation is minimal, and, while this is a good first step to allow interpretability, it is possible that stronger group manipulations may be required to obtain larger or different asymmetries in behavioral results; we know that in other settings the strength of group manipulation does affect behavioral findings (e.g., Gary Charness et al. 2007; Roy Chen and Yan Chen, 2010). Second, we cannot rule out that different findings might follow from more unbalanced group manipulations. As noted in an earlier footnote, random matching was required to avoid different frequencies of play with insiders and outsiders depending on group membership, and, as a by-product of random matching, we required sufficiently large minority groups to ensure that subjects played with both insiders and outsiders and we maximized statistical power. However, more unbalanced groups is an obvious direction for future research. Third, we do find that, once covariates are controlled for, subjects like being in majority groups (Result 4); this agrees with psychological research that shows lower satisfaction from minority members relative to majority members (Markus Lücken and Bernd Simon, 2005).

We find a greater behavioral impact from our minimal social status manipulation, and this is the more striking precisely because it is a minimal manipulation. Low status subjects gave 11% less to other low status subjects (Result 2), they discriminated less in giving rates (Result 7) and discriminated less also in return rates (Result 9). It appears that, while still

placing a positive value on own group membership, this value is considerably less than that for high status subjects. In turn, as we find (as in HHZ) that greater own group valuation increases discrimination in giving rates, the reduced difference in valuations between groups underlies reduced discrimination, although this works not by bringing the high status subjects but rather by bringing the other low status subjects down.

The point that, as shown by Tables 6 and 4 respectively, ingroup favoritism and positive perceived value are still present on average with low status subjects is a significant one in the following sense. One potential criticism of our minimal status manipulation is that what we are really inducing is the perception of not being in any group. That a positive valuation is still placed on average on own group membership, and that overall discrimination is still present (at least when covariates are taken into account), is useful information because it implies that Not Blue subjects still perceive themselves as a group (if a less desirable, low status group) as opposed to perceive themselves as belonging to no group.[38] There are also real world examples, such as the traditional Japanese caste system discussed in the introduction, where being classified as outsiders to the high status group or groups is the defining feature of how the low status group is identified.

It is also interesting to compare these results with those from the psychological research, in relation to which the stylized finding is that, while not universally so, low status groups tend to display outgroup favoritism as opposed to the usual ingroup bias (e.g., Steve Hinkle and Rupert Brown, 1990; John T. Jost and Diana Burgess, 2000; Miles Hewstone et al., 2002). It is difficult to say how much of this stark result is due to a combination of having attitudinal studies, non incentivized and/or replete with deception; it is clear though that the psychological research is right in suggesting that low status group members value less own group membership and treat outgroup members comparatively less unkindly than high status subjects would. In addition, our study replicates the psychological finding that low status subjects get lower satisfaction from group membership than on high status subjects (e.g., Naomi Ellemers et al., 1988, 1992; Itesh Sachdev and Richard Y. Bourhis, 1987); so do

[38] For example, in all 10 status differential sessions Not Blue perceived values were equal to 0 (sign test P = 0.005 in relation to both M and Mb).

minority subjects, with minorities generally being perceived more negatively than majorities (Viviane Seyranian et al., 2008).

One overall interpretation of these results is that low status subjects appear to have low morale and this negatively affects the extent to which subjects are willing to trust and reciprocate; and, since there is no symmetrical effect on the part of high status subjects, managers and policy makers need to be aware of the potential unintended consequences for trust and trustworthiness of creating status based group partitions in an organization. The further warning for policy makers and managers is the obvious one: namely, as our global finding is one of negative discrimination as in HHZ, this also should operate as a cautionary tale against necessarily equating groups with a beneficial increase in social capital, although of course this is likely not to be true in other settings to the same extent and further research is needed.

One finding which is inconsistent with HHZ is that, in return rate regressions, higher perceived own group value is associated with less discrimination. If our result were replicated, one possible interpretation of this would be along the lines that more stringent expectations of trust are associated with members of one's own group, leading to lower return rates for each given level of trust. Put it differently, insiders would feel more let down by any given level of trust from insiders 'like them' (Michael Bacharach et al., 2007; Pierpaolo Battigalli and Martin Dufwenberg, 2007), and as a result they would feel less incline to fulfill trust as a result. Obviously, however, future research needs to determine the extent to which ours is a genuine result, given the inconsistency of our result with HHZ's, let alone whether our interpretation has some merit.

We did not find that our Charles A. Holt and Susan K. Laury (2002) related measures of risk aversion, loss aversion, ambiguity aversion and experimenter demand effects explained behavior. In relation specifically to our measure of experimenter demand effects (Result 11), an alternative interpretation is that it captures subjects being nice towards the experimenter as opposed to a form of experimenter demand effect. The problem with this argument is that being nice towards the experimenter could precisely be interpreted as a form of experimenter demand effect; while the argument that the manipulation simply captures

subjects being nice, i.e. altruistic, does not explain why it does not correlate with greater average trust or trustworthiness. Furthermore, the fact that face based stimuli are sufficient to induce compliant behavior (e.g., Melissa Bateson et al., 2006; Terence Burnham and Brian Hare, 2007; Kevin J. Haley and Daniel M. T. Fessler, 2005) makes clear that such visual aids are effective in providing social meaning towards the choice of the dominated option.[39] The fact that our results are robust to our experimenter demand effect instrument is at least suggestive evidence that such effects are not a problem in our experiment, or at least they are less of a problem than in most other experiments given the subtlety of the social statue cue involved.[40]

There is a potential parallelism between our findings on minority subjects and our findings on low status subjects; in both cases, there is evidence of comparative unhappiness with one's own position and of muted discrimination towards outsiders, though the latter operates differently for the two cases. It is too early to say whether, in a sense and contrarily to the psychological research that emphasizes the differences between the two types of treatment manipulations (e.g., Miles Hewstone et al., 2002), subjects perceived being in a minority as being of low status. If replicated, however, as an approximate simplification for a number (though by no means all) of real world cases, it would obviously be a useful finding for economists, managers and policy makers.

5. Conclusions

We have presented an experiment which employed trust games to look at the impact on well-being, cooperation and social capital from having comparative majority/minority groups and differential status groups. We generally replicated the basic finding of negative intergroup discrimination that was found in HHZ. We found that minority subjects appeared to discriminate less and comparatively dislike being in the minority; and that, and to a greater

[39] In work done for another experiment (Piers Fleming and Daniel J. Zizzo, 2010), this behavioral measure has been found to be significantly related to Joachim Stöber (2001) standard 17 items psychological questionnaire measure of sensitivity to social desirability pressures (Spearman $\rho = 0.226$, $P = 0.017$, $n = 112$). This further validates our interpretation of choices of the dominated option as sensitivity to experimenter demand characteristics.

[40] Further, if more indirect and less cogent, evidence against this interpretation of the results is provided by the fact that it cannot explain why we find that perceived values are a significant predictor of behavior, or arguably why we observe systematically negative as opposed to positive discrimination.

degree, low status subjects comparatively disliked being low status and discriminated less by pulling other low status subjects down. There is at least a partial parallelism between the effects of our majority/minority manipulation and of our group status manipulation, which might suggest that, in many real world cases, minority subjects do perceive themselves as having low status (although of course sometimes the reverse is true). It appears that the negative implications for well being, cooperation and social capital from group partitions may be differentially distributed across different sets of people, and potentially worse in relation to status manipulations, when we allow for differential group sizes and for differential group status, even when introduced in a minimal way. Obviously, further research is needed to verify the robustness of our results.

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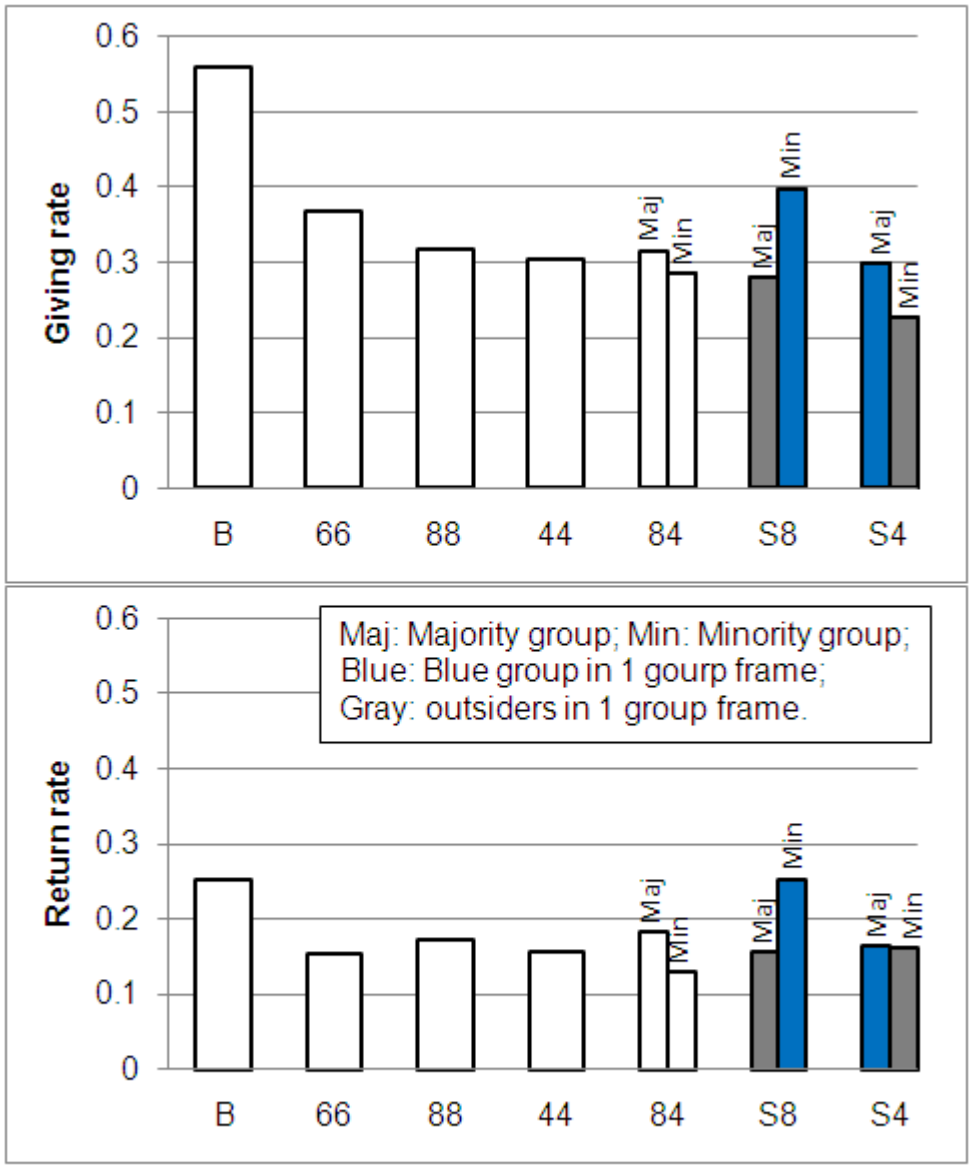
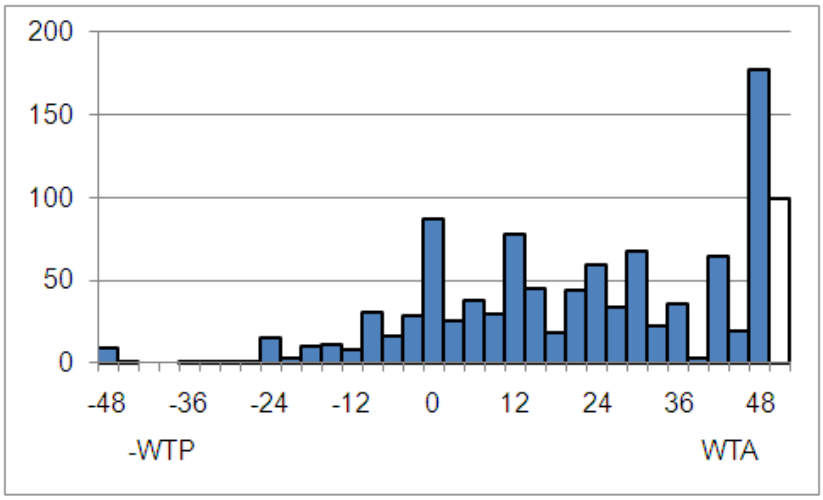


Figure 1. Giving and Return Rates in Stage 2–4



Notes: The histograms are built from individual choices; n = 210 for -WTP observations (mean = 9.986, standard deviation (s.d.) = 11.759) and n = 810 (mean = 30.095, s.d. = 16.132) for WTA observations. The white column stands for choices of not being willing to accept any price up to 48 to switch group.

Figure 2. Histogram of Willingness to Pay (WTP) and Willingness to Accept (WTA) in Relation to the Other Group

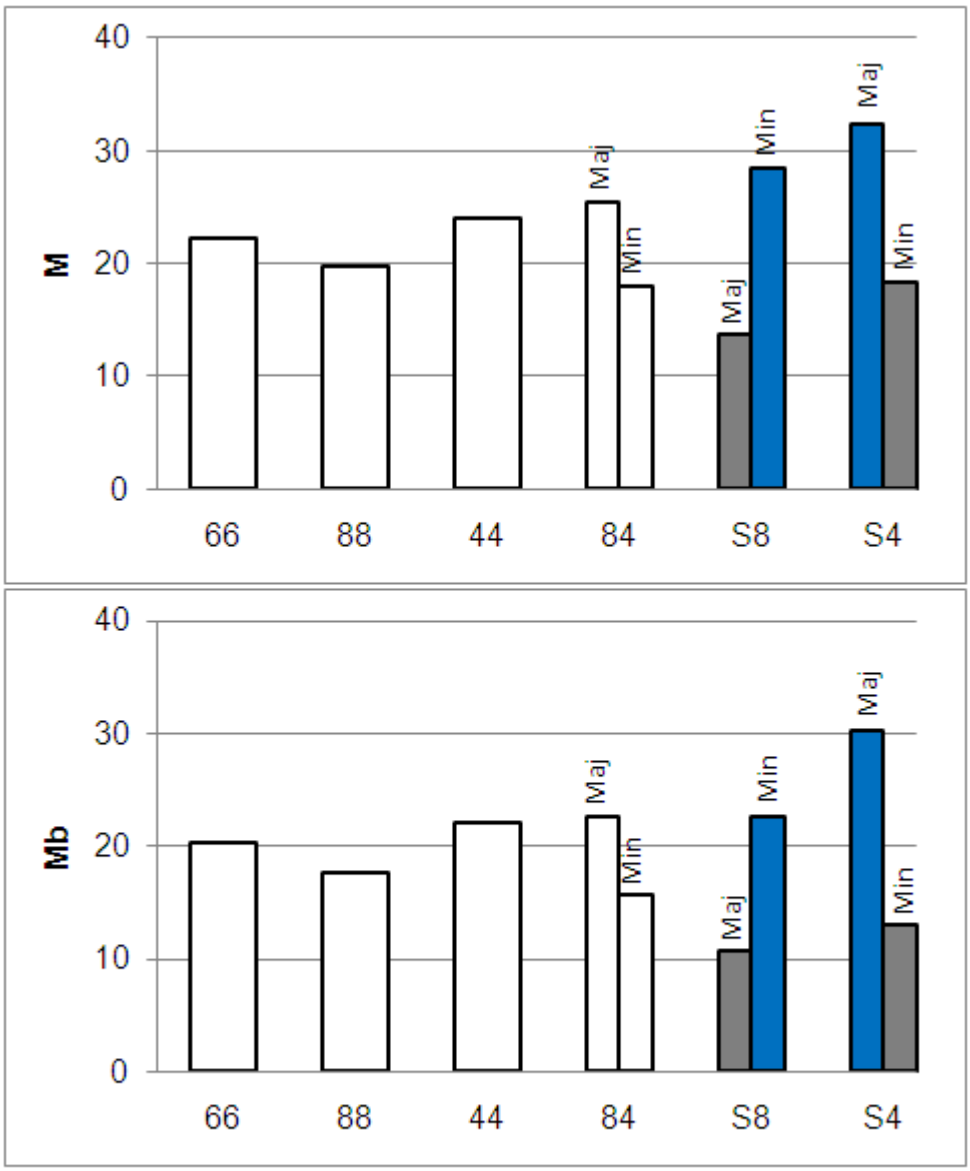


Figure 3. Mean Psychological values

Table 1 — Experimental Sequence

Experimental sequence	Task	Number of rounds
Stage 1	Trust games	3
Stage 2	Market 1 or Waiting Period 1, trust games	6
Stage 3	Market 2 or Waiting Period 2, trust games	6
Stage 4	Market 3 or Waiting Period 3, trust games	6

Notes: At the start of each of stages 2, 3, and 4, the experiment had a waiting period in the B treatment, and markets for group(s) in the 66, 88, 44, 84, S8 and S4 treatments (technically, there were two markets, one to pay for membership of each of the two groups).

Table 2 — Giving and Return Rates

	B	66	88	44	84	S8	S4
Stage 1 giving rate	0.562	0.457	0.433	0.350	0.367	0.477	0.413
Stage 2–4 giving rate	0.558	0.369	0.316	0.303	0.305	0.318	0.275
to own group members		0.449	0.348	0.344	0.333	0.334	0.319
to other group members		0.288	0.289	0.249	0.265	0.296	0.213
Stage 1 return rate	0.318	0.219	0.270	0.304	0.226	0.270	0.238
Stage 2–4 return rate	0.252	0.153	0.173	0.157	0.166	0.189	0.163
to own group members		0.200	0.185	0.187	0.177	0.204	0.170
to other group members		0.106	0.161	0.110	0.148	0.168	0.151

Table 3 — Regressions on Mean Giving and Return Rate

Regressions on stage 2–4 mean giving rate	Regression 1			Regression 2		
	β	*t*	*p*	*β*	*t*	*p*
	To own group			*To other group*		
Stage 1 giving rate	0.424	7.930	0.000	0.293	5.510	0.000
Stage 1 return rate	0.143	1.920	0.064	0.242	3.320	0.002
Group	-0.059	-0.720	0.477	-0.214	-3.160	0.003
GroupOf8	-0.061	-0.630	0.536	0.012	0.200	0.841
GroupOf4	-0.040	-0.430	0.669	-0.017	-0.290	0.771
Majority	-0.010	-0.130	0.899	-0.045	-1.150	0.258
Minority	0.020	0.290	0.774	0.061	1.270	0.211
NotBlue	-0.117	-2.170	0.037	-0.048	-0.980	0.335
Gender	-0.040	-1.340	0.190	-0.004	-0.120	0.904
EcMgt	-0.012	-0.400	0.695	-0.021	-0.890	0.380
UK	0.062	2.000	0.053	0.039	1.770	0.086
China	0.015	0.300	0.764	-0.023	-0.550	0.588
Age	0.000	-0.140	0.890	0.002	1.090	0.284
Constant	0.272	3.320	0.002	0.246	3.290	0.002
Regressions on stage 2–4 mean return rate	Regression 3			Regression 4		
	β	*t*	*p*	*β*	*t*	*p*
	To own group			*To other group*		
Trust rate as 2nd mover	0.131	2.380	0.023	0.089	1.560	0.128
Stage 1 giving rate	0.060	1.820	0.078	0.015	0.510	0.615
Stage 1 return rate	0.186	2.670	0.012	0.230	5.620	0.000
Group	-0.009	-0.280	0.783	-0.095	-2.600	0.014
GroupOf8	-0.003	-0.120	0.906	0.036	1.510	0.141
GroupOf4	-0.012	-0.450	0.657	-0.014	-0.580	0.568
Majority	-0.006	-0.300	0.765	-0.005	-0.200	0.845
Minority	0.013	0.350	0.731	0.069	2.170	0.037
NotBlue	-0.026	-1.350	0.186	-0.031	-1.320	0.197
Gender	-0.029	-1.600	0.120	-0.019	-0.860	0.396
EcMgt	-0.031	-1.630	0.112	0.014	0.530	0.599
UK	-0.012	-0.620	0.542	-0.019	-1.020	0.316
China	-0.015	-0.590	0.561	-0.057	-1.930	0.062
Age	0.000	0.240	0.808	0.001	0.430	0.671
Constant	0.100	1.790	0.083	0.126	1.950	0.059

Notes: $n = 328$, $R^2 = 0.324$ for Regression 1, $n = 327$, $R^2 = 0.366$ for Regression 2, $n = 324$, $R^2 = 0.175$ for Regression 3 and $n = 318$, $R^2 = 0.234$ for Regression 4. We employ error clustering to control for session level effects.

Table 4 — Mean Psychological Values

Treatment	Stage	M			Mb		
			Majority	Minority		Majority	Minority
66	2	21.04			19.83		
	3	24.65			23.02		
	4	21.06			17.81		
	Total	22.25			20.25		
88	2	21.11			20.03		
	3	17.91			15.84		
	4	20.09			16.88		
	Total	19.70			17.61		
44	2	24.95			24.33		
	3	24.40			22.41		
	4	22.45			19.50		
	Total	23.93			22.14		
84	2	21.94	24.31	17.21	21.56	24.31	15.83
	3	24.71	27.63	18.88	21.67	24.57	16.14
	4	22.03	24.15	17.79	17.16	18.41	14.95
	Total	22.89	25.36	17.96	20.23	22.61	15.64
			(NotBlue)	(Blue)		(NotBlue)	(Blue)
S8	2	18.72	11.40	33.35	15.96	10.44	29.44
	3	20.90	18.63	25.45	15.94	15.25	17.60
	4	16.08	10.95	26.35	11.02	6.72	20.69
	Total	18.57	13.66	28.38	14.33	10.79	22.68
			(Blue)	(NotBlue)		(Blue)	(NotBlue)
S4	2	31.23	34.70	24.30	28.89	33.11	19.94
	3	26.73	32.40	15.40	22.80	28.88	11.67
	4	25.27	30.18	15.45	22.13	28.65	7.06
	Total	27.74	32.43	18.38	24.63	30.24	12.98

Table 5 — Regressions on Psychological Value

	Regression 5 mean M			Regression 6 mean Mb		
	β	*t*	*p*	*β*	*t*	*p*
GroupOf8	-2.233	-1.150	0.258	-3.152	-1.290	0.207
Majority	7.781	4.110	0.000	8.808	3.500	0.002
Minority	2.817	0.770	0.447	-0.237	-0.060	0.954
NotBlue	-13.884	-4.410	0.000	-16.308	-4.510	0.000
Gender	3.223	1.710	0.098	3.138	1.580	0.125
EcMgt	-3.988	-1.620	0.115	-3.410	-1.270	0.215
UK	1.249	0.630	0.535	2.260	1.160	0.254
China	-3.925	-1.410	0.168	-5.011	-1.520	0.138
Age	0.323	1.820	0.079	0.161	0.860	0.395
Constant	13.842	2.420	0.022	15.828	2.850	0.008
	Regression 7 mean M			Regression 8 mean Mb		
	β	*t*	*p*	*β*	*t*	*p*
GroupOf8	-3.291	-1.630	0.116	-4.392	-2.180	0.039
Majority	7.499	3.520	0.002	8.856	3.240	0.004
Minority	1.680	0.500	0.624	-1.264	-0.340	0.733
NotBlue	-13.487	-4.560	0.000	-15.970	-4.470	0.000
Gender	3.846	1.760	0.091	3.969	1.790	0.087
EcMgt	-4.318	-1.570	0.128	-4.102	-1.380	0.180
UK	1.383	0.570	0.575	2.010	0.790	0.438
China	-5.939	-2.310	0.030	-7.258	-2.140	0.043
Age	0.233	0.970	0.342	0.106	0.430	0.669
Risk Aversion	-1.368	-0.710	0.485	-0.484	-0.260	0.793
Loss Aversion	1.381	0.620	0.543	0.464	0.220	0.829
Ambiguity Aversion	2.110	0.900	0.376	0.805	0.360	0.719
Conformism	0.170	0.340	0.740	0.094	0.160	0.873
Constant	21.490	2.730	0.012	20.405	2.830	0.009

Notes: $n = 358$, $R^2 = 0.113$ for Regression 5, $n = 353$, $R^2 = 0.126$ for Regression 6, $n = 294$, $R^2 = 0.159$ for Regression 7 and $n = 289$, $R^2 = 0.159$ for Regression 8. We employ error clustering to control for session level effects.

Table 6 — Regressions on DGiving and DReturn Rate

Regressions on	Regression 9			Regression 10		
DGiving Rate	*β*	*t*	*p*	*β*	*t*	*p*
GroupOf8	-0.066	-1.220	0.231	-0.060	-1.130	0.267
Majority	0.053	1.030	0.311	0.033	0.670	0.509
Minority	-0.038	-0.960	0.347	-0.046	-1.210	0.237
NotBlue	-0.077	-2.560	0.016	-0.042	-1.330	0.195
M				0.003	3.580	0.001
Gender	-0.040	-1.370	0.182	-0.049	-1.670	0.107
EcMgt	0.028	0.930	0.361	0.038	1.280	0.212
UK	0.028	0.870	0.392	0.025	0.800	0.429
China	0.035	0.910	0.368	0.045	1.090	0.284
Age	-0.002	-1.170	0.253	-0.003	-1.540	0.135
Constant	0.192	2.560	0.016	0.156	2.070	0.047
	Regression 11			Regression 12		
GroupOf8	-0.081	-1.100	0.281	-0.028	-0.470	0.645
Majority	0.034	0.430	0.673	0.044	0.760	0.454
Minority	-0.056	-1.070	0.293	-0.005	-0.110	0.912
NotBlue	-0.015	-0.260	0.793	-0.079	-2.410	0.024
M	0.002	2.530	0.017			
M × GroupOf8	0.001	0.470	0.639			
M × Majority	0.000	-0.120	0.903			
M × Minority	0.000	0.330	0.746			
M × NotBlue	-0.001	-0.730	0.474			
Gender	-0.048	-1.620	0.116	-0.033	-0.940	0.356
EcMgt	0.039	1.310	0.200	0.004	0.120	0.904
UK	0.027	0.930	0.360	0.025	0.670	0.509
China	0.045	1.080	0.289	0.041	0.920	0.366
Age	-0.003	-1.430	0.164	-0.002	-0.630	0.532
Risk Aversion				-0.001	-0.040	0.970
Loss Aversion				0.005	0.110	0.910
Ambiguity Aversion				0.007	0.160	0.875
Conformism				-0.003	-0.770	0.450
Constant	0.158	2.110	0.044	0.155	1.560	0.131

Regressions on	Regression 13			Regression 14		
DGiving Rate	*β*	*t*	*p*	*β*	*t*	*p*
GroupOf8	-0.019	-0.310	0.757	-0.037	-0.460	0.651
Majority	0.022	0.400	0.694	0.056	0.650	0.523
Minority	-0.010	-0.250	0.808	0.009	0.150	0.879
NotBlue	-0.040	-1.180	0.249	-0.021	-0.360	0.720
M	0.003	3.660	0.001	0.003	2.650	0.014
M × GroupOf8				0.001	0.470	0.642
M × Majority				-0.002	-0.640	0.526
M × Minority				-0.001	-0.370	0.718
M × NotBlue				-0.001	-0.640	0.531
Gender	-0.044	-1.250	0.222	0.004	0.130	0.898
EcMgt	0.016	0.540	0.592	-0.001	-0.030	0.973
UK	0.021	0.590	0.563	0.001	0.030	0.974
China	0.058	1.200	0.241	-0.004	-0.940	0.358
Age	-0.002	-0.850	0.405	-0.044	-1.210	0.239
Risk Aversion	0.003	0.080	0.941	0.020	0.650	0.520
Loss Aversion	0.001	0.020	0.987	0.026	0.750	0.461
Ambiguity Aversion	0.000	0.010	0.990	-0.055	-1.120	0.275
Conformism	-0.004	-0.830	0.416	-0.002	-0.670	0.510
Constant	0.092	0.970	0.340	0.066	0.730	0.472
Regression on	Regression 15			Regression 16		
DReturn Rate	*β*	*t*	*p*	*β*	*t*	*p*
DGiving rate as 2nd mover	0.085	1.890	0.068	0.086	1.830	0.077
GroupOf8	-0.030	-0.820	0.418	-0.037	-1.020	0.314
Majority	-0.031	-0.840	0.405	-0.011	-0.290	0.772
Minority	-0.046	-1.210	0.238	-0.040	-1.150	0.261
NotBlue	-0.006	-0.130	0.897	-0.040	-0.890	0.383
M				-0.002	-3.240	0.003
Gender	0.004	0.120	0.904	0.011	0.380	0.710
EcMgt	-0.010	-0.260	0.794	-0.019	-0.500	0.618
UK	0.064	2.750	0.010	0.068	2.830	0.008
China	0.046	1.090	0.283	0.037	0.900	0.378
Age	0.002	0.990	0.329	0.003	1.390	0.174
Constant	0.061	0.830	0.416	0.094	1.290	0.206

Regressions on	Regression 17			Regression 18		
DReturnRate	*β*	*t*	*p*	*β*	*t*	*p*
DGiving rate as 2nd mover	0.080	1.800	0.083	0.069	1.340	0.193
GroupOf8	-0.034	-0.430	0.669	-0.004	-0.090	0.930
Majority	-0.073	-0.800	0.431	-0.034	-0.860	0.399
Minority	-0.126	-2.090	0.046	-0.023	-0.450	0.655
NotBlue	0.082	1.060	0.297	-0.012	-0.250	0.805
M	-0.003	-2.250	0.032			
M × GroupOf8	0.000	-0.050	0.959			
M × Majority	0.002	0.740	0.462			
M × Minority	0.004	1.440	0.162			
M × NotBlue	-0.006	-2.310	0.028			
Gender	0.017	0.620	0.540	-0.020	-0.560	0.579
EcMgt	-0.026	-0.680	0.504	-0.029	-0.660	0.517
UK	0.067	2.620	0.014	0.041	1.470	0.154
China	0.031	0.710	0.486	0.085	1.840	0.078
Age	0.003	1.400	0.172	0.001	0.340	0.734
Risk Aversion				-0.056	-2.110	0.046
Loss Aversion				0.059	2.050	0.052
Ambiguity Aversion				0.054	1.820	0.081
Conformism				-0.004	-0.650	0.523
Constant	0.100	1.250	0.222	0.083	0.910	0.370
	Regression 19			Regression 20		
DGiving rate as 2nd mover	0.065	1.200	0.241	0.061	1.190	0.244
GroupOf8	-0.014	-0.280	0.780	-0.082	-0.790	0.436
Majority	-0.013	-0.310	0.761	-0.097	-0.910	0.372
Minority	-0.018	-0.390	0.702	-0.184	-2.440	0.023
NotBlue	-0.049	-1.020	0.317	0.079	0.970	0.341
M	-0.003	-3.080	0.005	-0.006	-3.940	0.001
M × GroupOf8				0.003	0.730	0.474
M × Majority				0.003	0.840	0.409
M × Minority				0.007	2.470	0.021
M × NotBlue				-0.006	-2.100	0.047
Gender	-0.011	-0.310	0.761	-0.056	-2.030	0.053
EcMgt	-0.041	-0.900	0.378	0.059	2.000	0.057
UK	0.045	1.560	0.131	-0.057	-1.850	0.076
China	0.070	1.500	0.147	-0.003	-0.380	0.709
Age	0.001	0.650	0.524	-0.002	-0.060	0.955
Risk Aversion	-0.059	-2.360	0.027	-0.048	-1.090	0.285
Loss Aversion	0.062	2.280	0.032	0.041	1.320	0.201
Ambiguity Aversion	0.060	2.090	0.048	-0.063	-1.180	0.251
Conformism	-0.004	-0.530	0.599	0.001	0.440	0.667
Constant	0.140	1.670	0.107	0.233	2.610	0.015

Notes: n = 358, R^2 = 0.037 for Regression 9, n = 358, R^2 = 0.069 for Regression 10, n = 358, R^2 = 0.070 for Regression 11 and n = 294, R^2 = 0.026 for Regression 12; n = 294, R^2 = 0.066 for Regression 13, n = 294, R^2 = 0.070 for Regression 14, n = 355, R^2 = 0.034 for Regression 15 and n = 355, R^2 = 0.060 for Regression 16; n = 355, R^2 = 0.080 for Regression 17, n = 292, R^2 = 0.039 for Regression 18, n = 292, R^2 = 0.066 for Regression 19 and n = 292, R^2 = 0.094 for Regression 20. We employ error clustering to control for session level effects.