Procedural Fairness and the Tolerance for Income Inequality*

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Abstract

The determinants of support for redistribution from the rich to the poor in a society have been the subject of much empirical investigation across a range of literatures. There has, however, been little in the way of investigating the determinants of tolerance for inequality by the poor. This paper presents an experiment investigating what cultural and institutional factors underlying a society might render its members more or less tolerant of inequality in favor of greater efficiency. The specific institutional factors we address concern the fairness in the procedures or mechanisms through which individuals believe initial positions or roles in society are determined. Subjects’ initial positions (rich vs. poor) are determined based on various criteria (random, meritocratic, arbitrary, and rewarding uncooperative behavior) and individuals’ willingness to approve Pareto improvement when the improvement is mainly in favor of the already rich is measured. Our findings show that individuals’ willingness to accept higher but more inequitable outcomes depends on the source of the initial inequality and random assignment leads to the most tolerance for disadvantageous inequality, generating doubt about commonly held views concerning meritocracy. Moreover, holding the procedures constant, subjects reveal greater tolerance for inequality when self and the opponent share common group identity.

Key Words: inequality, efficiency, experiment, procedural fairness, social identity

1 Introduction

That the utility of individuals depends on relative as well as absolute earnings (Duesenberry, 1949, Easterlin, 1974, Frank, 1985) has been well established in the economics literature.

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and investigation to understand the precise nature of individual behavior with respect to inequality is one of the most lively areas of research recently (see e.g. Bolton and Ockenfels (2000), Charness and Rabin (2002) and Fehr and Schmidt (1999)).

The issue of relativity and inequality becomes particularly salient when it comes to the adoption of growth and development policies. While economic growth is a fundamental goal embraced by most nations, many policies aimed at promoting growth have features that are inequality increasing, and often lead to segments of the population being unwilling to support them. Consider a policy that might improve growth / enhance efficiency yet does so at the expense of the poorer segment of society. In such a case, that segment would obviously not be in favor of the policy. Suppose instead that there is a set of policies that are guaranteed to raise income for everyone but to raise the income of the already wealthy by a disproportionate amount leading to an increase in inequality. Would the poorer segment who receives lesser benefits be opposed to the policy due to concerns over the increase in inequality? If so, what should the policy maker do? How much consideration, if at all, should be given to the public’s concern about their relative earnings? And what factors might be accountable for the public opposition to Pareto improving yet inequality expanding policy changes?

This paper aims to advance our understanding of individuals’ tolerance for income inequality in the context of economic growth. Specifically, we investigate what cultural and institutional factors underlying a society might render its members more or less tolerant of inequality when the increased inequality comes along with greater efficiency. Under democracy, public consensus and support is critical to the successful implementation of policies. Even if policy makers have the “right” set of interventions ready, their implementation and success is not guaranteed unless the public supports their adoption. This question is made more interesting by the observation that the support for policies of this nature appears to vary from country to country. For example, directed credit programs that provide loans to priority sectors on preferential terms were readily adopted and became a leading tool of development policy in East Asia in the 1960s and 1970s whereas programs such as the recent Troubled Asset Relief Program (TARP) in the US run into serious opposition. One could certainly point to the difference in the levels of democratization between the East
Asian countries and the US but it is worth examining the question at a deeper level to
determine if there are other reasons why different societies might be more or less willing to
embrace economic policies of this nature.

The specific institutional factors we address concern the fairness in the procedures or
mechanisms through which individuals believe initial positions or roles in society are deter-
mined. If a proposed policy is expected to advance some people more than others, do those
who will benefit more deserve their position? Our study is strongly related to a growing
literature on procedural fairness including Hoffman, McCabe, Shachat, and Smith (1994)
and Bolton, Brandts, and Ockenfels (2005). These studies demonstrate that different mech-
anisms that are used to determine the roles in an experiment can have strong impacts on
how fair individuals perceive the situation to be and that this can have a strong impact on
behavior. There is substantial evidence that has been accumulated showing that how indi-
viduals view inequality and redistribution is heavily influenced by the perceived source of
inequality, see for example Piketty (1995), Ravallion and Lokshin (2000), Alesina, Glaeser,
and Sacerdote (2001), Fong (2001), Alesina and Angeletos (2005), Alesina and La Ferrara
(2005), Benabou and Tirole (2006), Durante and Putterman (2009) and Esarey, Salmon,
and Barrilleaux (2011). Much of this work has examined the degree to which people believe
that one’s realized income and earnings are based on luck versus individual e¤ort with the
focus being on how willing rich individuals are to support _ex post_ redistribution to help the
poor as these beliefs change. Our focus is in some respects the flip side of this question as we
are looking at when the poor/middle class are willing to support _ex ante_ will raise their own income but that of the already wealthy direct beneficiaries of the
programs at a faster rate, a key feature present in many of the pro-growth policies.\footnote{Examples include industrial policies and directed credit programs, a leading tool of development policy for East Asian countries in the 1960s and 1970s (see e.g. Stiglitz and Uy, 1996, Vittas and Cho, 1996) as well as the trade liberalization that many developing countries underwent in the 1980s and 1990s (Goldberg and Pavcnik (2007)).} While
one might suspect similar factors to affect the willingness of the disadvantaged individuals
to support policies of this nature, it is not entirely obvious from the previous literature how those effects will manifest.

Our design also allows us to investigate conditional on the procedures or mechanisms that
determined the initial positions, whether and how the relative characteristics of individuals
in the pair might affect their tolerance for the inequality. For instance, individuals may have
differential tolerance for the advancement of others depending on how closely they identify
with the other member in the pair. There is a long literature showing that individuals
exhibit in-group favoritism even when the group identity is based on a very minimal and
virtually meaningless distinction between groups, such as the color of their shirts (Tajfel,
Billig, Bundy, and Flament (1971)). We examine if and to what extent some additional
information on the relative characteristics of the individuals in a pair can mitigate or amplify
the effect of procedures or mechanisms in use.

We investigate these issues using a controlled laboratory experiment, in which we vary
the mechanism which assigns people to their roles (advantaged vs. disadvantaged) as well as
manipulate the information displayed regarding the characteristics of the players in a pair.
Our approach involves having subjects in the role of disadvantaged citizens choosing how
much of their endowment to pass to an advantaged individual knowing that when they do
so the money transferred and the endowment of the advantaged person will jointly generate
an investment return which will then be shared between the two individuals according to
a fixed and known rule. An important feature of this investment technology is such that
the disadvantaged person will always receive more money in return than she transfers to
the advantaged individual, making full transfer a transparently money maximizing choice.
This investment and transfer technology are common knowledge which means subjects know
for each level of possible transfer the specific amount they will receive in return with no
uncertainty.2 We consider a baseline mechanism for assigning roles involving pure luck, a
mechanism based on performance on a quasi-SAT test (a standard measure of meritocracy
in the literature), one involving preferences for Kandinsky vs. Klee paintings (which is in-
tended to appear arbitrary), and a mechanism that rewards uncooperative behavior (which
is intended to mimic the situation in which advantage in society is achieved through un-

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2 When dealing with issues of inequality and redistributing endowments, there is of course a vast literature
involving experimental examinations of related issues. Our base game is a variant of a dictator game,
Forsythe, Horowitz, Savin, and Sefton (1994), as subjects are making allocation decisions rather than offers
that could be rejected. The design is also similar to, though very importantly NOT identical to a trust
game, Berg, Dickhaut, and McCabe (1995), as the subject is choosing an amount to pass which will then be
multiplied by some factor to generate a total return. The key difference is that in our design the advantaged
person will not be choosing how much to return while in the trust game the person receiving the passed
funds decides how much to send back. In order to ensure common beliefs about the level of return, the
money derived from the investment will be split in a known manner.
ethical behavior). We also check whether providing some additional information about the subject and her counterpart (e.g. opponent belongs to the same/opposite painter group as self), which are orthogonal to the role assignment mechanisms, might mitigate or exacerbate any effects driven by the role assignment mechanism.

Our main findings can be summarized as follows. First, even when the initial positions or roles are randomly assigned, disadvantaged individuals do not choose the transfer amount which will maximize their own income (as well as the social efficiency), which confirms the presence of inequity averse preferences as documented many times in prior literature dating back to Bolton and Ockenfels (2000), and Fehr and Schmidt (1999). The surprising aspect to our finding though is that we find inequity averse even in the presence of efficiency concerns which was not found in Engelmann and Strobel (2004) and Charness and Rabin (2002). Second, compared to the baseline mechanism (random assignment), the other three mechanisms (“meritocracy”, “arbitrary rule”, and “corruption”) lead the disadvantaged individuals to make choices that are associated with lower efficiency (and lowered inequality). Third, conditional on the mechanisms used for role assignments, disadvantaged individuals become more tolerant of inequality when they learn that their opponent (the advantaged) is of the same identity type as self, which echoes the findings in the social identity literature (see e.g. Tajfel, Billig, Bundy, and Flament (1971) and Chen and Li (2009)).

Hirschman and Rothschild (1973), in their famous work on the changing tolerance of inequality in the course of economic development emphasized the need to carefully consider a society’s institutional environment and tolerance for inequality in deciding whether to pursue the tasks of growth and equity sequentially or simultaneously. This paper is an attempt at such an endeavor. Our findings show that individuals’ tolerance for (disadvantageous) inequality does depend on the perceived fairness in the procedures that led to the inequality. Moreover, our finding on the role of information on the relative characteristics of players suggests a potential avenue for policy makers to overcome the effect of pre-existing institutions and induce support and cooperation among citizens for socially desirable policies.

In the next section we present the design of our experiment. Section 3 will present the hypotheses we intend to test and section 4 will present the results. Finally, section 5 will
provide some concluding thoughts.

2 Experiment Design

2.1 Redistribution Stage

The experiment consisted of two stages using a total of 20 subjects for each session. We will explain the second stage first as that is the main stage in which the subjects choose how to allocate their endowments. In this stage subjects are assigned roles as either advantaged (A) or disadvantaged (D) individuals and this assignment never changes. The A subjects are endowed with $15 while the D subjects are endowed with only $5. Subjects are paired together such that there is one A and one D subject in each pair and are then allowed to choose some amount \( x \in [-5, 5] \) to transfer from the endowment of the D player to the A player. Positive values of \( x \) involve the D subject transferring some portion of his $5 endowment to the A subject while negative values of \( x \) would involve the A subject transferring some of her endowment to the D subject. Both A and D subjects make this decision and then each choice has a 50% chance of being the transfer that is used. This allows us to give both subjects the incentive to truthfully reveal their preferences regarding the transfer. Subjects made a total of eight choices with random repairing each time under different conditions as will be explained below. One of the eight rounds was randomly chosen to generate the final payments.

After the transfer is made, that amount and the endowment of the A subject is used to generate a pot of money that will be shared by both subjects according to a fixed and known sharing rule. The general form for the payoffs for both subjects is given by equations (1) and (2). \( W_D \) and \( W_A \) refer to the endowments of the two subjects with \( W_D = 5 \) and \( W_A = 15 \). The amount transferred, \( x \), is multiplied by a factor of \( \beta = 4.75 \) while A’s endowment is multiplied by \( \gamma = .9 \) and these amounts are then added to yield the total amount of money to be divided among the two subjects. The D subject receives 30% of the amount while the A subject receives 70%. For both subjects, it is trivial to see that they have a payoff maximizing choice of \( x^* = 5 \). For both subjects, their dollar valued payoff is strictly increasing in \( x \). The deterministic nature of the payoff functions should make it
clear that this is not a trust game. The fact that the D subject’s payoff is strictly increasing in \( x \) does not rely on any action of the A subject and therefore there is no requirement that the D subject trust the A subject.\(^3\)

\[
\pi_D = W_D - x + \alpha(\beta x + \gamma W_A) \quad (1)
\]
\[
\pi_A = W_A + x + (1 - \alpha)(\beta x + \gamma W_A) \quad (2)
\]

The parameters for this payoff function were not chosen in an attempt to mimic any natural investment technology and the actual equations were neither shown to nor explained to the subjects. The parameters were chosen in order to generate the properties thought necessary for the investigation of the key questions. For our purposes, it is important that the payoff functions generate substantial inequality that is increasing in \( x \) in a salient and easily identifiable manner. Further, it should be possible for an individual who strongly objects to the existence of inequality in a particular set of circumstances be able to choose an \( x \) that would yield equality of final payoffs.

In order to make the subjects understand the impact of different choices of \( x \) on final payoffs, they were shown on their screen a figure similar to Figure 1 that allows them to clearly see the relative payoff consequences for any choice of \( x \). They chose \( x \) by clicking along a slider bar and the figure would update with every click showing them where along that graph they were. Further they were also shown the calculated numerical values of the final earnings for both subjects as well as the intermediate consequences on their endowments. This way of presenting the payoff structure to the subjects was chosen so that the impact of any choice of \( x \) on both intermediate endowments and on final earnings was absolutely clear to the subjects while they were making their decisions. In particular, there should have been no uncertainty on part of the subjects in regards to the fact that payoffs are increasing for both subjects in \( x \) and that the payoff maximizing choice for either individual or the pair is \( x = 5 \).

\(^3\) Of course in many field analogs of this situation, the factory owner who receives the government subsidy does get to choose how much to pay his workers, how much of his profits to reinvest and so forth meaning he does get to choose how much of the surplus to share with the disadvantaged individuals. The degree to which the disadvantaged trust the advantaged individual is no doubt important to these situations. Our goal though is to determine how preferences are formed absent this complication as it will help to better understand how preferences might be formed when the trust component is included.
2.2 Assessment Stage

Prior to the redistribution stage of the experiment, the subjects proceed through four assessment phases. Subjects went through all assessment phases, in the same order, in all treatments. The first phase is trivial and consists only of the experiment software informing them that they have been randomly assigned a subject ID between 1 and 20. It is important for how this ID is used in the experiment that it be clear to the subjects that this ID has been randomly assigned and has no relation to anything they have done. In order to ensure that perception, they are informed of their ID on the first screen they see for the experiment prior to any interaction the subject has had with the computer.

The other assessment phases involve the subjects engaging in various forms of activity that will be used for multiple purposes in the redistribution stage. At the time they engage in each assessment task, the subjects are not aware of how the information will later be used. The second of the four tasks is a standard linear public goods game. Our desire for this phase was to use a standard public goods game that would generate a reasonable degree of heterogeneity in terms of generosity. Based on the results in Isaac, Walker, and Thomas (1984) it seemed that a design involving groups of 4 and an MPCR of around .3 would achieve these goals. For this phase, subjects are randomly assigned into groups of
4 and given 10 tokens to allocate between an individual and a group account. Each token retained in the individual account earns that individual $0.20 while each token placed in the group account yields $0.07 to each group member or $0.28 to the group in total. This phase consists of a single decision and results from this phase are not reported until the end of the experiment.

The third assessment phase consists of the subjects being asked to provide answers for verbal and mathematics questions taken from a sample SAT exam. Each section is administered separately. Subjects are given eight minutes to complete as many of the verbal questions as they can and twelve minutes to complete as many of the math questions as they can. Performance is incentivized with the subjects earning $0.05 for each correct answer but losing $0.01 for each wrong answer to retain standard SAT scoring rules. Subjects are able to skip forward or backward through the questions as they choose. Results are again not reported at the end of the phase or stage but are withheld and only reported at the end of the experiment.

The final assessment phase involves the subjects indicating their preferences for artwork, see Tajfel, Billig, Bundy, and Flament (1971) and Chen and Li (2009). This procedure consists of asking the subjects to state their preferences regarding five pairs of paintings with each pair consisting of one painting by Paul Klee and another by Wassily Kandinsky. This preference elicitation is obviously unincentivized and subjects are not told for what purpose these choices will be used. The oddness of this task actually makes this task very well suited for the main purpose for which we wish to use it. Again, results of this task are withheld until the end of the experiment.

2.3 Mechanisms to Assign Roles and Information Revelation

The main use for the four different assessment phases is to generate different ways of assigning the D and A roles in the experiment. We have four assignment treatments in this experiment. Each treatment differs with respect to which of these four assessment phases is used to assign the D and A roles. This leads to our having four main treatments:

- **Random:** Subjects with odd numbered ID’s are assigned the A role and subjects with even ID’s are assigned the D role.
- **SAT**: Subjects are ordered from highest to lowest performers in terms of number of correct answers on the SAT questions. The ten highest performers are assigned the A role while the 10 lowest performers are assigned the D role.

- **Klee and Kandinsky (KK)**: The 10 individuals who indicated the highest relative preferences for Klee are assigned the A role and the other 10 the D role.

- **Public Goods (PG)**: The 10 individuals who contributed the least to the public goods (i.e. the least generous) are assigned to the A role while the 10 who contributed the most (i.e. the most generous) are assigned to the D role.

In the SAT, KK, and PG treatments, ties are certainly possible and any ties are broken randomly. In each case, subjects are not told specifics on their versus others score on the SAT questions, number of times they actually selected Klee paintings, or contributions to the public good. They are only informed about to which relative group (e.g. even ID, high SAT, Klee, or least contribution to PG) they were assigned. This is done in an attempt to make the information content of the assignment mechanism as consistent as possible across individuals and sessions. For example, individuals in the high SAT group know their score on that section is in the top 10 for the session but they don’t know their ranking among that 10 or exactly how many questions they answered correctly. They also do not know how many more questions they answered than someone in the bottom 10.

After the assessment phases are finished, the instructions are begun for the redistribution stage. The assignment criterion used in that session was explained very carefully to make it clear how the assignment to D vs. A roles was made. Further, the language for the role assignment was repeated at the top of each decision screen to attempt to make this information as salient as possible. As explained earlier, subjects make eight redistribution choices with one chosen at random to generate earnings. The mechanism to assign roles and the role assignment remains unchanged across these eight decisions, i.e. a D subject remains D throughout. However, the counterpart to which a subject is matched changes from round to round.

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4Instruction script is available in the appendix.
One might be concerned that the earnings from the assessment stage might interfere with subjects’ later decisions in the redistribution stage. However, as stated above, earnings from the assessment stages were not revealed until the end of experiment. Moreover, the earnings between the A and D subjects in the redistribution stage were on average separated by $30 ($13.54 for D subjects and $43.21 for A subjects) while average earnings in the SAT phase were less than $1 (even in the SAT treatment the difference between A and D subjects was only $0.37) and were around $2.25 in the PG phase (average difference was $1.01 between A and D subjects in PG treatment). Even were these earnings known, they should have little impact on distributional preferences during the redistribution stage.

In order to understand the effects of the relative characteristics of individuals in a pair, we also varied information a subject learns about their counterpart over the eight decisions. In all treatments, in round 1 of the redistribution stage the only information subjects know about each other is the information conveyed by the assignment criterion. The experiment was specifically designed this way to give us one round in which we can clearly identify a treatment effect on choices that is not confounded by any other information revelation. In the subsequent seven rounds, information is revealed to both parties about their counterpart through an information matrix on their screen. The possible information consists of whether their counterpart has an odd or even subject ID, whether they are among the top or bottom 10 scorers on the SAT questions, whether they are identified as one of the 10 who relatively prefers Klee or Kandinsky, and whether they are among the top or bottom 10 in terms of contributions to the public goods. Since subjects are matched with new counterparts in each round, knowing that your last counterpart was among the top 10 SAT scorers gives you no information on the SAT score of your new counterpart. While the first round always gives no additional information, the information revealed in the subsequent rounds is randomly determined according to a few principles. First, to eliminate ordering effects from the data, the order in which different subjects see information was not the same. Each pair receives the same information about each other so if the A subject sees the KK information on their counterpart, so does the D subject. Different pairs, however, would see different information about counterparts in a given round. Second, the information randomization was also set up so that all subjects experienced each information condition exactly once in the first four
rounds and once again in the last four.

We have conducted eight sessions of this experiment with 2 sessions of each treatment for a total of 160 subjects. The overall average earnings to the subjects was $41.43 ($26.50 for D subjects and $56.35 for A subjects), which includes a $10 participation fee, for sessions that lasted 1-1.5 hours. The experiments were conducted using z-Tree, Fischbacher (2007), and subjects were recruited using a system based on ORSEE, Greiner (2004).

3 Hypotheses

Each of these treatments and information conditions were designed to test specific hypotheses about how each would affect behavior. We briefly summarize each hypothesis as well as its source as a way of presenting a road map for how the data will be analyzed. The first set of hypotheses concerns how the assignment treatments will affect behavior and then a second set specifies our expectations on how the different information conditions might affect behavior.

3.1 How Do Assignment Mechanisms Affect Subjects Behavior?

Assignment Hypothesis 1 Meritocracy Hypothesis: Both Advantaged and Disadvantaged subjects should be more willing to transfer D’s endowment in the SAT assignment treatment than in the Random assignment treatment.

This hypothesis is derived from previous work such as Hoffman, McCabe, Shachat, and Smith (1994). That study showed that offers decline in both Ultimatum and Dictator Games when the first mover has earned the right to be in that advantaged position through their performance on a current events quiz relative to when the roles are randomly assigned. These past results have been interpreted to indicate that when an individual has earned his position through personal merit that he feels more entitled to that position and therefore he is more willing to exploit it, see for example Jakiela, Miguel, and te Velde (2010). Consequently we expect that a similar effect will be observed in our data.\footnote{One might find it controversial to use a score on SAT questions as a proxy for merit as some might claim that genetics or luck in terms of where one was educated also plays a role in these scores. Likely a reader will find the claim that SAT scores confer merit more controversial after seeing our results. We acknowledge...}
note about the results in Hoffman, McCabe, Shachat, and Smith (1994), is that their data actually provides no insight on the behavior of second movers in Ultimatum Games and therefore the implications for our D subjects is unclear. The reason is that the overall number of rejections found in their data is very low (2 out of 24 rejected offers in random assignment and 0 out of 24 in the merit assignment) and so making strong claims about how the second movers’ view of offers changed based on the treatment is not possible from that data. Consequently, the data from this prior paper strongly suggests that our Meritocracy Hypothesis will hold for our A subjects but it provides a only very weak indication of how the D subjects might respond.

The next two assignment treatment hypotheses are derivable from standard models of inequity aversion augmented by a structure such as in Cox, Friedman, and Gjerstad (2007) to allow for the underlying preference parameters to depend on the contextual circumstances. In such a framework, the fundamental supposition behind each hypothesis concerns whether and how the different environmental conditions affect the social preferences an individual might possess. These hypotheses suppose a particular directional effect of an underlying preference shift and that the shift will be large enough to alter behavior. In the tests that will follow, we will of course only be able to identify both of those effects jointly.

**Assignment Hypothesis 2** *Arbitrariness Hypothesis: Disadvantaged subjects will be less willing to transfer their endowment in the KK assignment treatment than in the Random assignment treatment.*

**Assignment Hypothesis 3** *Corruption Hypothesis: Disadvantaged subjects will be less willing to transfer their endowment in the PG assignment treatment than in the Random assignment treatment.*

Both hypotheses 2 and 3 are constructed for the D subjects only and they are based upon the supposition that the two assignment mechanisms will generate a negative emotional state.
for the D subjects. The KK assignment scheme was designed to appear arbitrary to the subjects as there seems little justification for why one’s preferences for art should determine your status ranking in a society. The PG assignment scheme was designed intentionally to appear as an illegitimate way to assign status as the most selfish subjects were assigned the advantaged position. Under both schemes the most reasonable emotional response from the D subjects, if one occurs, is a negative one of disappointment or even anger. According to the Cox, Friedman, and Gjerstad (2007) model, such a negative emotional state should make the D subjects less willing to take the welfare of the A subject into account and consequently one would expect them to be more averse to the inequality and less willing to transfer their endowment to the A subject. We will make no formal hypotheses regarding the behavior of the A subjects in these treatments because any claims regarding the expected emotional response on their part due to these assignment treatments is less clear. One could make arguments in favor of both positive and negative emotional responses for the A subjects while for the D subjects the only sensible possibilities seem to be no response or a negative response.

3.2 Can Additional Info on Self and Counterpart Change Subjects Behavior?

Information Hypothesis 1  Meritocracy Hypothesis: Learning that a counterpart is High (Low) SAT when a subject is Low (High) SAT should make Disadvantaged subjects More (Less) willing to transfer their endowment compared to the no information condition.

The genesis for this hypothesis on how information about a counterpart might affect behavior is similar to the Meritocracy Hypothesis regarding the assignment treatments. If indeed being a High SAT person is seen to entitle an individual to the Advantaged status, then it seems reasonable that learning someone is in the High SAT group could have a similar effect even if that was not the basis for their assignment. While a person may have been assigned to a position without earning it, perhaps knowing that they actually did deserve that position anyway makes others more willing to treat them generously. The opposite is perhaps also possible and so those found to have a Low SAT score in the advantaged position may be treated less generously.
**Information Hypothesis 2** Social Identity Hypothesis A: Disadvantaged subjects will be more willing to transfer to those in own painting preference group compared to those in the other preference group.

**Information Hypothesis 3** Social Identity Hypothesis B: Disadvantaged subjects will be more willing to transfer to those in own ID group compared to those in other ID group.

These two social identity hypotheses are derived from the prior related literature, see Chen and Li (2009) and Tajfel, Billig, Bundy, and Flament (1971), which find results that even assigning individuals to groups through a randomization procedure can generate an in-group bias. One could interpret the assessment phases that assign subjects to the Odd and Even groups as mimicking these random assignment to group procedures and so it is possible that doing so generates such a bias. The painting preference assessment phase is taken directly from these past studies and could also have generated some sort of in-group bias. While our design did little to make these social identities salient, the strength of some of the prior results suggests that it should still be interesting to determine if these social identity effects exist.

**Information Hypothesis 4** Corruption Hypothesis: Disadvantaged subjects in the High PG group will be less willing to transfer to a counterpart in Low PG group than a counterpart in the High PG group.

The final hypothesis concerns how individuals might respond to learning that their counterpart is a member of the Low PG group or is one of the individuals who contributed the least in the PG phase. Learning that the advantaged person with whom you are matched is a non-generous type could generate a negatively reciprocal response as this too might put an individual in a negative emotional state causing a shift in social preferences creating a similar effect as described above. While a D player does not know that the person showed lack of generosity to him, he does know that the A player dealt selfishly towards three individuals and that certainly has the possibility of generating the same effect as if the A player showed a lack of generosity to the D player. If so, then we should see High PG D players treat High and Low PG A players asymmetrically. A similar effect could exist for
D subjects who are in the Low PG group but it seems less likely that these subjects would punish another for doing what they themselves did.

4 Results

4.1 Effects of Role Assignment Mechanisms

We will first investigate the hypotheses regarding the effects of the assignment treatments. An initial look at the results in this regard can be seen in Table 1 which contains the relevant summary statistics of the average amount of $x$ chosen by A and D subjects by treatment. The table shows the average for round 1 separately from the averages overall. Breaking out round 1 by itself is important because this is the key round for investigating the pure effect of the assignment mechanisms that is not convoluted with the effect of observing additional information about the counterpart. The decisions in all subsequent rounds can be affected by the information revelation mechanism and so a pure assignment treatment effect is more difficult to extract out of these rounds.

The summary statistics make the directional effect of all of the treatments quite clear. If we examine the round 1 data we see that for the D subjects, the amount they are willing to transfer is on average $3.85$ of their $5$ endowment in the Random treatment where types are assigned based on whether the subject ID is odd or even. For any of the other assignment treatments, they are willing to transfer over $1$ less on average which is a sizable effect. By examining Figure 2 which shows the histograms of transfer choices by D subjects in round 1 we can observe how the distribution shifts across treatments. The modal choice is always 5 and when comparing the distribution in the Random treatment to any of the others there is a leftward shift from the distribution in the Random treatment. Of particular note is the

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<td>4.79</td>
</tr>
</tbody>
</table>
Figure 2: Histogram of transfers by D subjects. Data includes only round 1.

Figure 3: Histogram of transfers by D subjects. Data includes only round 1.
increase in the number of negative transfers in the treatments other than Random.

For the A subjects it is interesting to note that in the Random treatment, they only take an average of $3.35 from the D subject’s endowment which is less than what the D subjects themselves are willing to transfer though the difference is not statistically significant ($p$-value from $t$-test is 0.329, and from a Wilcoxon rank sum test 0.260). In each of the other treatments, the amount they take increases compared to the Random treatment baseline. Figure 3 displays the histograms for the A subjects from round 1 data and they show a clear shift to the right when comparing the Random treatment to any of the other treatments.

If we examine the data from all rounds, the differences by treatment are consistent with the round 1 data for the A subjects but we find that in the D subject data, one of the directional effects reverses. As we will show in the next section this difference between round 1 and the later rounds is due to interactions with the information revelation in that treatment and so this reversal should not be interpreted as due to the assignment treatment.

The statistical basis for the tests of the hypotheses are contained in Table 2 which presents OLS regressions to determine if the differences in the levels of transfers are significant. The regressions include dummy variables for the SAT, KK and PG treatments leaving the Random treatment as the baseline. Thus the significance of the coefficients can be interpreted as tests regarding whether the behavior in the relevant treatment is different from the behavior in the Random treatment used as the baseline. We have conducted the regressions using only round 1 data as our primary examination of the treatment effect but we have included the regressions based on the data from all rounds for completeness. We will return to that data later to examine how the information presentation affects the decision making. Our first three results are based on using the round 1 results in this table to test the three hypotheses related to the assignment treatments.

**Result 1** *Meritocracy hypothesis rejected for D but not for A. D subjects transfer less of their endowment in the SAT assignment treatment than in the Random assignment treatment. A subjects transfer more.*

For the D subjects, there is a substantial and significant difference between the amount
Table 2: Examination of the effects of assignment treatments on transfer levels.

<table>
<thead>
<tr>
<th></th>
<th>Disadvantaged subjects</th>
<th>Advantaged subjects</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Round 1</td>
<td>All Rounds</td>
</tr>
<tr>
<td>SAT</td>
<td>-1.450**</td>
<td>-0.388</td>
</tr>
<tr>
<td></td>
<td>(0.706)</td>
<td>(0.305)</td>
</tr>
<tr>
<td>KK</td>
<td>-1.600*</td>
<td>-0.881***</td>
</tr>
<tr>
<td></td>
<td>(0.862)</td>
<td>(0.341)</td>
</tr>
<tr>
<td>PG</td>
<td>-1.300*</td>
<td>0.700***</td>
</tr>
<tr>
<td></td>
<td>(0.792)</td>
<td>(0.260)</td>
</tr>
<tr>
<td>Constant</td>
<td>3.850***</td>
<td>3.150***</td>
</tr>
<tr>
<td></td>
<td>(0.357)</td>
<td>(0.207)</td>
</tr>
<tr>
<td>Obs</td>
<td>80</td>
<td>640</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.055</td>
<td>0.042</td>
</tr>
</tbody>
</table>

Robust Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

they transfer to their A counterpart in round 1 of the SAT treatment relative to round 1 of the Random treatment in that subjects in the SAT treatment transfer less than those in the Random treatment. The A subjects in the SAT treatment, however, request a substantially larger transfer than they do in the Random treatment and this too is significant. This indicates that the A subjects may well feel more entitled to their position based on having earned it. Interestingly the D subjects do not see the A subjects as being more entitled to the advantaged position. This difference in how the subjects view the entitlement is fascinating and a topic we will return to later for additional discussion.6

Result 2 Arbitrariness hypothesis is not rejected. Disadvantaged subjects transfer less of their endowment in the KK assignment treatment than in the Random treatment.

Result 3 Corruption hypothesis is not rejected. Disadvantaged subjects transfer less of their endowment in the PG assignment treatment than in the Random treatment.

The support for both of these results is clear as in the round 1 regressions, the coefficients on both treatment dummy variables are negative and significant. While there were no a

---

6Based on the possibility that the treatment effects we find could be due to selecting individuals by specific characteristics rather than how they respond to the treatment we conducted additional regressions to see if assignment characteristics correlate with transfer choices. We find that for the D subjects, no characteristics have a significant effect though the KK assignment characteristic has a marginally significant effect for the A subjects. We suppress the results to conserve space but have provided the analysis in a separate Appendix.
*priori* hypotheses on the treatment effects for the A subjects for these two treatments, it is still interesting to examine their behavior to determine if there are treatment effects. What we see in Table 2 is that for the PG treatment, there is a positive and significant effect on the transfer they request. This effect is the same found previously for the SAT treatment in that A subjects increase their request relative to the baseline Random treatment. The KK treatment had a positive effect but it was not strong enough to gain significance in the round 1 data. This leads to our fourth result.

**Result 4** *Advantaged subjects make use of any assignment rule to justify requesting more of the Disadvantaged subject’s endowment, though the effect is insignificant in the KK treatment.*

In Table 2, we see that the strength and occasionally the sign of the shift D subject’s responses to the assignment mechanisms changes when we examine the results from the regressions using all rounds of data. This suggests that revealing the additional information about the subject and her counterpart may be having significant effects on the behavior of the subjects, even though the additional information had nothing to do with the initial role assignments. We present the effects of information revelation in detail the next subsection.

### 4.2 Effects of Observing Additional Info on Self and the Opponent

We have so far focused on identifying the effect of the assignment mechanism. In this subsection, we examine the effect of observing additional information on self and opponent. To examine precisely how observing information on the relative characteristics of players in a pair, which was orthogonal to the criteria for the role assignment, might affect decision making behavior, we present results from regressions for D and A subjects in Table 3. The first four columns of Table 3 display the relevant regressions for D subjects and the last four for A subjects. Each regression is conducted with the data for the relevant assignment treatment excluded from consideration since we cannot examine the impact of revealing a particular characteristic to the players using the data for the treatment in which that characteristic was the basis for the assignment. The basic structure for each regression is a panel regression with individual fixed effects and with standard errors clustered on each
subject in which the dependent variable is the amount of the transfer. Since each subject receives only one assignment treatment throughout, the effect of the assignment treatment is subsumed in the individual fixed effects. The independent variables include the relevant combinations in which information can be displayed. For the ID and KK related information the two important categories are simply whether the A and D subjects observe that they are of the same or different type. For the SAT and PG info, it might well matter whether the subjects learn that they are in a higher or lower group than their counterpart and then if they are in the same group. We have constructed dummy variables for each of these cases. The excluded case in all regressions is the no information revealed baseline.

**Result 5** Information Meritocracy Hypothesis Rejected. Disadvantaged subjects in the Low (High) SAT group do not demonstrate favorable (unfavorable) treatment to counterparts in the High (Low) SAT group.

The hypothesis concerning how subjects would respond to information about the SAT score of their counterpart involved those in the High and Low SAT groups responding differently to counterparts from the opposite group. We find the neither relative effect turns out to be significant. We do, however, find a non-hypothesized effect in that learning that you are of the same type as your counterpart seems to make the subjects more willing to transfer to the other. So instead of Low SAT individuals being willing to accept the claim to legitimacy of the High SAT A subjects, they instead transfer more to fellow Low SAT counterparts.

**Result 6** Both Social Identity hypotheses fail to be rejected. D subjects increase their transfer level relative to the no information case when they learn that their counterpart is in the same ID or KK group as themselves.

Both of the social identity hypotheses were predicated on the notion that two of our assessment phases could have possibly generated some feeling of social identity or group membership among our subjects. If they had, then when subjects found out that they were matched with another member of their same group then they would have been more willing to transfer their endowment. The two regressions examining this for the KK and
<table>
<thead>
<tr>
<th></th>
<th>Disadvantaged subjects</th>
<th>Advantaged subjects</th>
</tr>
</thead>
<tbody>
<tr>
<td>Other is same ID Group as Self</td>
<td>0.519**</td>
<td>0.050</td>
</tr>
<tr>
<td></td>
<td>(0.256)</td>
<td>(0.086)</td>
</tr>
<tr>
<td>Other is different ID Group than Self</td>
<td>0.318</td>
<td>0.121</td>
</tr>
<tr>
<td></td>
<td>(0.391)</td>
<td>(0.180)</td>
</tr>
<tr>
<td>Self scored lower on SAT than Other</td>
<td>0.032</td>
<td>0.117</td>
</tr>
<tr>
<td></td>
<td>(0.726)</td>
<td>(0.145)</td>
</tr>
<tr>
<td>Self and Other scored the same on SAT</td>
<td>0.587**</td>
<td>0.072</td>
</tr>
<tr>
<td></td>
<td>(0.251)</td>
<td>(0.122)</td>
</tr>
<tr>
<td>Self scored higher on SAT than Other</td>
<td>-0.216</td>
<td>0.099</td>
</tr>
<tr>
<td></td>
<td>(0.630)</td>
<td>(0.179)</td>
</tr>
<tr>
<td>Other is same KK Group as Self</td>
<td></td>
<td>0.708**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.266)</td>
</tr>
<tr>
<td>Other. is different KK Group than Self</td>
<td>-0.092</td>
<td>-0.005</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.297)</td>
</tr>
<tr>
<td>Self contributed less to PG than Other</td>
<td>-0.312</td>
<td>0.421*</td>
</tr>
<tr>
<td></td>
<td>(0.248)</td>
<td>(0.217)</td>
</tr>
<tr>
<td>Self and Other contributed equally to PG</td>
<td>-0.025</td>
<td>0.212</td>
</tr>
<tr>
<td></td>
<td>(0.385)</td>
<td>(0.155)</td>
</tr>
<tr>
<td>Self contributed more to PG than Other</td>
<td>-0.869</td>
<td>0.144</td>
</tr>
<tr>
<td></td>
<td>(0.734)</td>
<td>(0.211)</td>
</tr>
<tr>
<td>Constant</td>
<td>2.683***</td>
<td>0.419***</td>
</tr>
<tr>
<td></td>
<td>(0.133)</td>
<td>(0.098)</td>
</tr>
<tr>
<td>Observations</td>
<td>240</td>
<td>240</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.017</td>
<td>0.004</td>
</tr>
<tr>
<td>Number of pid</td>
<td>60</td>
<td>60</td>
</tr>
</tbody>
</table>

Robust standard errors clustered by subject are in parentheses, *** p<0.01, ** p<0.05, * p<0.1
ID information treatments shows that we do indeed get a positive and significant effect of subjects finding out that their counterpart is in their same group among the D subjects.

**Result 7** *Corruption hypothesis is rejected. D subjects show no responsiveness to any information regarding the PG group of their counterpart.*

This result is clearly supported by the regression in which the PG assignment data is excluded. We find that none of the information regarding a D subjects counterpart’s PG status has any significant effect on contributions.

**Result 8** *Learning information about a counterpart has generally little effect on the transfer requests of A subjects.*

Our hypotheses concerning how subjects might respond to the information about their counterpart were not formulated with respect to the A subjects, but we can again examine how they responded to the information to determine if there were any interesting or systematic responses. The last four columns in Table 3 contains results for A subjects. Generally we find that for the A subjects, learning the characteristics of their counterpart has little effect on the transfer they request. There is a statistically significant effect indicating that A subjects who contributed less in the PG than their counterpart request slightly higher transfers than otherwise but all other coefficients are insignificant. One possible explanation for the lack of the information on a counterpart having any effect is that any positive effects may have been masked due to there not being substantial room to increase over the already high transfer requests occurring in many treatments.\(^7\)

5 Discussion

This paper presents an experiment exploring how different institutional elements in a society that lead to differences in status or position affect the willingness of individuals to support transfers that would improve everyone’s income but that of the already wealthy direct

\(^7\)We have also conducted regressions in subsamples to see whether the information effects might vary depending on the underlying assignment treatments. The analysis is contained in a separate Appendix.
beneficiaries of the programs disproportionately. An important aspect of the design is that transfers increase the absolute payoffs of both players but increase inequality between them and that this aspect of the payoffs is presented in a way that is easy for subjects to see and understand. Our findings demonstrate a number of interesting aspects of these decisions.

One perhaps surprising element given prior work demonstrating that inequality concerns are diminished when a transaction improves efficiency (e.g. Charness and Rabin (2002) and Engelmann and Strobel (2004)) is that our subjects of both types are not willing to maximize their own welfare or total efficiency by transferring the optimal amount. This is again despite the fact that this full transfer maximizes earnings for both parties. This finding indicates that simply demonstrating to a citizenry that policy proposals that promise to generate long term benefits to all members of the society yet may increase inequality will not necessarily be enough to make them willing to support these policies. While we do not have the data to support this, we suspect that the reason for the difference between our results and the prior literature on this stems from the low returns to the D subjects in our design. Were more of the surplus shared with the D subjects it seems likely that their self-interest would come to dominate their concern for inequity as found in Charness and Rabin (2002) and Engelmann and Strobel (2004). We therefore do not see our results as being in conflict with prior work. Our results do indicate that the distributional preferences can be active even in the presence of efficiency enhancement and whether these distributional concerns are relevant for any particular policy proposal would depend on the magnitude of the relative returns for both groups.

The relative effects we find regarding the impact of the treatments on the behavior of the A subjects appears to be consistent with previous work. The effect in the SAT treatment is similar to the entitlement effect for first movers in ultimatum games found in Hoffman, McCabe, Shachat, and Smith (1994) when the first movers “earned” the position. Our A subjects seem to believe that their having earned their position entitles them to more earnings. This effect also emerges in the PG treatment and maybe to a lesser extent the KK treatment. The indication could be that the A subjects will seize on any justification for being in the advantaged position and use it to justify asking for higher earnings. They do this in spite of appearing relatively inequality averse in the Random assignment treatment.
This is consistent with results in Dana, Weber, and Kuang (2007) in which it is found that individuals like to not appear as unfair, rather than necessarily dislike actually being unfair. This is also consistent with the model involving a combination of cognitive dissonance and self-deception presented in Konow (2000) and Konow (1996) which is used to explain how individuals can come to self-deceive themselves into beliefs of entitlement. Translating these explanations into our context, it could be that in the Random assignment treatment, the A subjects can identify no reason why they should earn so much more than the D subjects and therefore exhibit the inequity aversion that accounts for the low transfer requests. In the other treatments where the A subjects are given some sort of justification, they use it as a way of rationalizing the more selfish behavior either based on the self-deception argument in Konow (1996) or through the ability to justify acting according to their rational self-interest as not being unfair as in Dana, Weber, and Kuang (2007). Of course it must be pointed out that the A subjects being more selfish is actually in the material self-interest of the D subjects as well. This makes strong claims about the motivation of the A subjects difficult to support. Consequently we see this interpretation as plausible but not conclusive.

The behavior of the D subjects is perhaps more interesting as their behavior in the SAT treatment is substantially different than anticipated. In the SAT treatment where the A and D subjects earned their positions by performance on a small scale SAT exam, the D subjects were less willing to transfer their endowment to the A subjects than were the subjects in the Random treatment. The failure of the meritocracy result to be obtained might at first seem surprising, but on a deeper examination it is not inconsistent with prior results. Hoffman, McCabe, Shachat, and Smith (1994), for example, is often seen as providing support for this meritocracy notion but as previously discussed, the implications of those data for the disadvantaged individuals are not clear.

While the conventional wisdom regarding a meritocracy is that it will be accepted as a fair way of determining roles in a society, there is certainly other evidence indicating that this is not so clear. McCoy and Major (2007) for example find that some individuals when given meritocratic priming before receiving negative news are more likely to see the negative news as a result of unfairness and discrimination than they would without the priming. Those authors argue that this is due to how individuals will attempt to justify the
outcome to themselves. In that study the individuals receiving the meritocratic priming were more averse to admitting to themselves that the negative outcome could have been deserved and so chose to view the judgment as being illegitimate. This is of course the opposite of the result one expects in this situation as the meritocratic priming is intended to make the decision appear to be more likely to be based on valid principles but as their study shows, people do not always react to a meritocracy as the conventional wisdom would expect. A similar effect would manifest in our experiments if subjects placed in the D role were more likely to judge that as being unfair under a meritocratic assignment scheme, i.e. the SAT treatment, than under a treatment with no meritocratic overtones, i.e. the Random treatment. As that is exactly what we find, the results of these two studies appear consistent.

One way of explaining this reaction to meritocracy is that learning that you are the loser in the meritocratic system can generate a variety of negative emotions including jealousy or envy regarding the success of others as well as shame regarding your poor performance on the assessment exam. Any of these negative emotions could trigger the same sort of negative emotional state responsible for the decrease in other regarding preferences in the KK and PG treatments and thus the same behavioral response.

One might alternatively argue that the failure of the meritocracy result is due to the subjects not viewing answering SAT questions as a proper way of determining a meritocratic ranking. It is certainly the case that it is not universally agreed upon that academic accomplishments should determine one’s role in life as famously indicated in a commencement address at Yale by President George W. Bush: “To those of you who received honors, awards and distinctions, I say well done. And to the C students, I say you too may one day be President of the United States.” This sort of sentiment could certainly have led students who fared poorly on the SAT exam to believing themselves as equally deserving of the advantaged position as those who did well. The problem is that it is impossible to distinguish between the D subjects not being willing to accept this particular meritocratic mechanism from their not being willing to accept the idea of meritocratic sorting. We specifically chose to use an SAT-like instrument because college students all know and have recent experience with SAT scores being used to determine important aspects of their place in society in the
form of college admission and scholarships. They have therefore received substantial societal priming to accept its legitimacy. If one believes that the failure of the meritocracy is due to the mechanism and not the principle, then this too is an interesting finding given that the mechanism we used is strongly related to one of the most common meritocratic mechanisms used for determining education and employment opportunities. Further, it opens up a potential search to determine if there exists a meritocratic mechanism that would be accepted as valid by those who come out as the losers. We see that as beyond the scope of this paper. Also, based on our evidence and previous literature it seems more plausible that the likelihood of an individual being willing to accept the outcome of a meritocratic mechanism as valid is increasing in the advantageousness of the position they are assigned through the mechanism. This is the interpretation suggested by our results and supported to some extent by those in McCoy and Major (2007), but that interpretation can certainly not be deemed conclusive as our data does not disprove the existence of a mechanism that might yield a different result.

Our other assignment treatments which involved an arbitrary assignment mechanism and a mechanism designed to appear illegitimate and perhaps even corrupt did generate the expected behavioral response. Both of these methods for assigning roles made our disadvantaged subjects less willing to transfer their endowment than when those roles were assigned randomly. The clear implication here is that to the extent that disadvantaged members of a society view the advantaged members of that society as having achieved their position through arbitrary or corrupt means, they will be less likely to support efficiency-enhancing regressive transfers.

Our experiments were also designed to help determine if learning various pieces of information about your counterparts would counteract some of the negative impacts from the assignment mechanism. In particular we were interested in determining if positive/negative information about the merit or type of person an individual is matched with could counteract any treatment effects. Our findings were not entirely as hypothesized but are still informative and useful. We find that learning your counterpart is in the same group as yourself generally makes an individual more willing to transfer to them. This result is consistent with previous work on social identity indicating that people will deal more charitably
towards individuals in their own group even when those groups have been formed randomly or through other minimal procedures (see Tajfel, Billig, Bundy, and Flament (1971) and Chen and Li (2009)). The fact that this result holds even in this environment where the saliency of social identity is somewhat marginalized is quite surprising.

The results from this experiment suggest that members of societies such as the US that view their society as more meritocratic in terms of who becomes rich versus poor may not necessarily be more supportive of pro-growth policies than societies such as those in Europe that are generally seen to have the perception that luck or randomness plays a larger role. Further, societies that are more homogenous in their social identity may also find policies like this more palatable. The strength of the social identity results further suggest that a way of promoting support of these policies would be by highlighting common identity or social ties between the advantaged and disadvantaged classes. Unlike the mechanisms which are more difficult to change, individuals’ social identity is malleable to policies. Tanzania’s conscious effort to forge a common national identity across ethnic groups (Miguel (2006)) is an example illustrating how emphasis on common social identity can mitigate the negative impact of a pre-existing institution, namely ethnic stratification, on the economic performance of countries (Easterly and Levine (1997), Alesina, Baqir, and Easterly (1999), and Alesina and La Ferrara (2005)).

References


Appendix A

A.1 Potential Selection Issues

One might be concerned that the assignment results could simply be due to selection effects since the assignment mechanisms could be seen as assigning different types of individuals to the roles and those types may simply make different choices. That is, the reason we find that transfers are lower among disadvantaged subjects in the SAT treatment than in the Random treatment could be because subjects with low SAT scores might generally transfer less than those with higher SAT scores even in the absence of any interaction with assignment rules. Were that true then by separating subjects by SAT score we would be getting transfer differences based on the selection criteria rather than due to a response to the treatment. As a way to demonstrate that this is not a feature in our data we examine whether transfers vary according to the characteristics of individuals used as the assignment criteria. Table A presents results from a series of regressions of the transfer choice of subjects.
in round 1 on one of the characteristic variables at a time. In each of the regressions, data from the assignment treatment that uses the characteristic under investigation are excluded. For example, when we examine the characteristic variable related to SAT performance, we exclude data from sessions for which the assignment treatment itself was based on SAT since in that case we would be picking up the response to the assignment treatment. So in each column of the table, the heading refers to the assignment treatment for which data is not considered. Generally we find that the size of the transfer is unrelated to the characteristics used to assign subjects to roles. The only marginally significant effect is that we find that among A subjects, individuals who relatively prefer paintings by Klee happen to transfer less than the subjects who prefer Kandinsky. This finding helps explain why the only treatment effect we fail to find significant in the previous analysis was for the A subjects in the KK assignment treatment. Recall that the A subjects in that treatment were the ones who relatively prefer Klee. As all of the other results indicate a lack of a significant effect based on the characteristics, there is no evidence that our significant treatment effects are due to selection issues.

A.2 Information Use by Treatment

In the main text we presented the results of the information treatments in the pooled sample. In order to see whether the information effects might vary depending on the underlying assignment treatments, we have examined the three information conditions that turned out to be significant in explaining subjects’ behavior (i.e. self and opponent belong to the same ID, SAT, or KK groups) in subsamples divided by the underlying assignment treatments. The results are presented in Table B. The column heading indicates the underlying assignment mechanism employed for that sample. The positive information effects obtained in the pooled sample are still present across the subsamples for the D subjects although the magnitude and statistical significance vary. The effects of common SAT and KK groups are amplified when the underlying assignment was Random. Also, in the case of KK assignment treatment, knowing that the counterpart is of the same ID

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8The variables used in the regressions reported are dummy variables for being in the group that would be assigned to the A role. We have also conducted these regressions with the raw scores on the various measures and the significance of the coefficients remains unchanged.
Table A: Regressions to identify any differences in transfer behavior according to subject characteristics.

<table>
<thead>
<tr>
<th></th>
<th>Disadvantaged subjects</th>
<th>Advantaged subjects</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>ID</td>
<td>SAT</td>
</tr>
<tr>
<td>Even ID</td>
<td>-0.200</td>
<td></td>
</tr>
<tr>
<td>High SAT</td>
<td>0.250</td>
<td></td>
</tr>
<tr>
<td>Klee</td>
<td>0.612</td>
<td></td>
</tr>
<tr>
<td>Low PG</td>
<td>-0.973</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>2.500***</td>
<td>2.750***</td>
</tr>
<tr>
<td></td>
<td>(0.533)</td>
<td>(0.533)</td>
</tr>
<tr>
<td>Obs</td>
<td>60</td>
<td>60</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.001</td>
<td>0.002</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Only Round 1 data used. Each column has data from one treatment excluded identified by column heading.
group as self doesn’t seem to increase transfer, which is quite intuitive – based on the more salient task of KK, a subject already determined that the opponent is in the out-group. Therefore, finding out that the opponent is of the same ID group is unlikely to render the subject view the opponent as an in-group member. In the case of the A subjects, mixed or no information effects are found, consistent with the finding in the pooled sample.
<table>
<thead>
<tr>
<th>ID</th>
<th>SAT</th>
<th>KK</th>
<th>PG</th>
</tr>
</thead>
<tbody>
<tr>
<td>Self and Other scored the same on SAT</td>
<td>0.816***</td>
<td>0.509</td>
<td>0.432</td>
</tr>
<tr>
<td>Other is same Painter Group as Self</td>
<td>0.905*</td>
<td>0.616</td>
<td>0.519</td>
</tr>
<tr>
<td>Other is same ID Group as Self</td>
<td>0.578*</td>
<td>0.051</td>
<td>0.642**</td>
</tr>
<tr>
<td>Constant</td>
<td>2.966***</td>
<td>2.628***</td>
<td>2.192***</td>
</tr>
</tbody>
</table>

Robust standard errors clustered by subject are in parentheses, *** p < 0.01, ** p < 0.05, * p < 0.1