Procedural Fairness and the Willingness to Accept Inequality*

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Abstract

The typical form of redistribution investigated in the lab resembles progressive redistribution in which money is transferred from rich to poor. There are, however, many governmental policies that involve regressive redistribution from poor to rich. Examples range from the TARP in the US to R&D subsidies for corporations and a variety of benefits given to firms in developing countries to encourage growth. Our question concerns the circumstances under which disadvantaged individuals would be willing to support this form of redistribution. We investigate this question by conducting experiments that measure willingness to support regressive transfers in which we vary the mechanism used to assign the roles. Mechanisms include random chance, meritocracy and two other mechanisms intended to appear arbitrary and unfair. Our findings generate doubt about commonly held views concerning how individuals view the fairness of certain procedures.

JEL Codes: C90, D61, D63, O15, O40

Key Words: inequality, efficiency, experiment

1 Introduction

The trade-offs between efficiency and inequality are basic concerns for economists and policy makers. Governments and policy makers often face the problem of having to allocate funds of a fixed size to different individuals and enterprises in the economy. In such an endeavor, allocation of the funds to businesses or wealthy individuals is often justified on the grounds that the eventual benefits of economic growth spurred by the allocation will “trickle down” to the rest of the society and it will lift up the level of income for everyone. This argument is in the background of policies such as the direct credit programs widely used in East Asian countries in the 1960s and 1970s and the recent TARP program used in the US.

Improvement in efficiency through such policies often comes at the expense of increased inequality. Everyone may get wealthier but the already wealthy beneficiaries of such policies may get richer at a faster rate, resulting in an increased level of inequality. A question that

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naturally arises is then why such growth-promoting policies that involve regressive transfers were readily adopted in some cases such as in East Asia whereas they often run into serious opposition in other cases such as the TARP program in the US? One obvious explanation is certainly that the central government’s in East Asia found it easier to implement such policies because the general public didn’t have much say in the absence of democracy whereas in the US the more open and democratic process allows for more vigorous debate. One of the key questions from a policy perspective is when would the public be willing to voluntarily accept growth-promoting policies at the expense of widening ex post inequality?

There are a few initial and obvious reasons why individuals might be against such growth promoting policies. Individuals may well not believe that the policies will have the pro-growth results that proponents of the policy argue. If the opponents of the policy accept the pro-growth properties they may still be against the policy due to a belief that the overall returns are not high enough to justify the widening of inequality. While these are perhaps the most obvious objections to such policies, these are not the issues in which we are interested. The situations in which we are interested are ones in which it is well known that the proposed policy will have the advertised effects and so in that sense it is the “right” policy. Our interest is in understanding what other societal or environmental factors may impact the degree to which individuals are willing to support the policy. Understanding this is important because under democracy, the support of the public 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.

The specific societal factors we intend to investigate concern the mechanisms through which individuals believe roles in society are determined. There is substantial evidence that has been accumulated showing that how individuals view inequality and redistribution is heavily influenced by the perceived source of inequality, see for example Fong (2001), Alesina, Glaeser, and Sacerdote (2001), Alesina and Ferrara (2005), Alesina and Ferrara (2005) and Esarey, Salmon, and Barrilleaux (2011). Much of this work has examined the degree to which people believe that ones income and earnings are based on luck versus individual effort with the focus being on how willing rich individuals are to support redistribution to help the less fortunate as these beliefs change. Our focus is in some respects the flip side of this question as we are looking at when less well off individuals are willing to support programs that redistribute their money to wealthier members of society. While one might suspect similar factors to affect the willingness of the poorer individuals to support these transfer programs, it is not entirely obvious from the previous literature how those effects will manifest.

We will investigate these issues using a controlled laboratory experiment. A laboratory experiment is necessary for looking at these issues due to the need to have careful control over the beliefs about the effects of the policy and the need to manipulate the mechanism which assigns people to their roles. Our approach will involve 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 that the disadvantaged person will be absolutely certain that
they will always receive more money in return than any amount they transfer. To examine the impact of different mechanisms for generating the class status, our treatments will involve varying the mechanism that is used to assign individuals to their roles to determine how these different procedures affect the willingness to accept inequality. The types of mechanisms we consider will include mechanisms involving pure luck and pure merit but we will also employ mechanisms designed that assign roles on grounds which are intended to appear illegitimate. We also will be providing our subjects various pieces of information about the individual with whom they are matched to determine how information about the other class might or might not help to counter any negative (or positive) effects driven by the assignment mechanism.

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.

As will be discussed later, the base motivation for why a disadvantaged person might not be willing to transfer their endowment to an advantaged person is that they might possess some form of inequality averse preferences, Bolton and Ockenfels (2000) and Fehr and Schmidt (1999). Charness and Rabin (2002) and Engelmann and Strobel (2004) both present evidence to suggest that inequality aversion may not be important in the presence of efficiency consequences of an allocation decision and so it is a priori not clear that disadvantaged individuals will do anything other than attempt to maximize efficiency. We will, however, find this not to be the case as our subjects are not willing to choose the outcomes that are the most efficient due, in all likelihood, to the degree of inequality involved. Fehr, Hoeff, and Kshetramade (2008) find perhaps even stronger results when their subjects in India demonstrate a willingness to exhibit spiteful preferences by reducing someone else’s earnings just to increase one’s own relative earnings. In their final sentences, Fehr et al. note that “it is a plausible hypothesis that the observed social preference patterns are at least partly shaped by this culture. Thus, an exciting question for future research is the extent to which different institutions and cultures produce preferences that are conducive or detrimental to economic development.” This is precisely the notion our study is intended to address.

Our study is also of course strongly related to a growing literature on procedural fairness including Bolton, Brandts, and Ockenfels (2005) and Hoffman, McCabe, Shachat, and Smith (1994). These papers demonstrate that different mechanisms 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. Cox, Friedman, and Gjerstad (2007) provides a theoretical framework capable of describing how different mechanisms may impact the observed level of social preferences as their model contains a term which depends
on the emotional state of the individual. This term affects the marginal rate of substitution between own and other payoffs. While this framework was developed with the intention of modeling reciprocity, the key insight that an individual’s generosity towards others can be dependent on their emotional state as affected by the mechanism through which they are interacting can also certainly be applied to this notion of procedural fairness.

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 Phase

The experiment consisted of two phases using a total of 20 subjects for each session. We will explain the second phase first as that is the main phase in which the subjects choose how to allocate their endowments. In this phase subjects are assigned roles as either (A)dvantaged or (D)isadvantaged 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 \( x \). It is again worth pointing out that the deterministic nature of the payoff functions should make it clear that this is not a trust game so 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.\(^1\)

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

The parameters for this payoff function were not chosen in an attempt to mimic any natural investment technology and the specific parameters were not 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 a highly inequality averse subject to be able to choose an \( x \) that would yield equality of final payoffs. Note, the use of the term “inequality” averse rather than “inequity” averse here. These concepts are related yet important to keep distinct. An inequality averse individual dislikes payoff differentials while an inequity averse individual dislikes payoffs that are perceived as unfair. This judgement regarding “unfairness” or “inequity” could be related to inequality but this study (and others before it) is (are) predicated on the idea that a judgement of inequity is based on more than just relative 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 and they were shown the intermediate consequences on their endowments. The impact of any choice of \( x \) on both intermediate endowments and on final earnings should have been quite clear to the subjects when 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 \( x^* = 5 \).

2.2 Assessment Phases

Prior to the redistribution phase of the experiment, the subjects proceed through four assessment phases. The first 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. The other assessment phases involve the subjects engaging in various forms of activity that will be used for multiple purposes in the redistribution phase. 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 stage was to use a standard version of a public goods game that would generate a reasonable degree of heterogeneity in terms of investment choices and based on the results

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 restored.
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. They 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 but are reported at the end of the experiment.

The final assessment phase involves the subjects indicating their preferences for artwork to sort them into groups, see Tajfel, Billig, Bundy, and Flament (1971) and Chen and Li (2009). This procedure consists of asking the subjects to state their preferences of 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.

**Figure 1:** Payoffs from any possible transfer choice.
2.3 Treatments and Information Revelation

The main use for the four different assessment tasks is to generate different ways of assigning types in the experiment. We have four treatments in this experiment. Each treatment differs with respect to which of these four assessment tasks is used to assign the D and A roles. In the Random treatment, subjects with even numbered id’s are assigned the A role and subjects with odd id’s are assigned the D role. Since this id was shown to them on their very first screen prior to them interacting in any way with the software, it should have been clear to them that this assignment to roles is completely random.

For the other three treatments we use the results from the other assessment tasks to generate type assignments. For our Public Goods (PG) treatment, the 10 individuals who contributed the least to the public good are assigned to the A role while the 10 who contributed the most are assigned to the D role. In our SAT treatment we order subjects from highest to lowest performers in terms of number of correct answers. The ten highest performers are assigned the A role while the 10 lowest performers are assigned the D role. For the Klee and Kandinsky (KK) treatment, the 10 individuals who indicated the highest relative preferences for Klee are assigned the A role and the other 10 the D role. In all three of these treatments, ties are certainly possible and any ties are broken randomly. In each case, subjects are not told specifics on their versus others contributions to the public good, score on the SAT questions or number of times they actually selected Klee paintings. They are only informed about to which relative group 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. Individuals in the high SAT group know their score on that section is in the top 10 for the session but they don’t necessarily know exactly how many questions they answered correctly.

After the assessment phases are finished, the instructions are begun for the redistribution phase. The assignment criterion used in that session was explained very carefully to make it clear how the assignment to type was made. Further, the language for the type 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 role assignment remains unchanged across these eight decisions. What does change is the information provided about a subject’s counterpart and the counterpart to which a subject is matched.

In all treatments, in round 1 of the redistribution phase the only information subjects know about each other is the information conveyed by the assignment criterion. This 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 they have an odd or even subject id, whether they are among the top or bottom 10 scorers on the SAT questions, whether they are among the top or bottom 10 in terms of contributions to the public good and whether they are identified as one of the 10 who relatively prefers Klee or Kandinsky. 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

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2 Instruction script with all relevant language available as an appendix.
the SAT score of your new counterpart, except in the treatment in which the SAT score determined the roles. 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, 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. In order to eliminate ordering effects from the data, the order in which different subjects sees information was not the same. Further, all subjects experienced each information condition 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 $38.70, 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 it’s 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.

**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 their position through personal merit that he feels more entitled to that position and therefore 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. An interesting 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.
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 share similar origin. 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. Both of these assignment schemes should be expected to generate a negative emotional state in the mind of the D subjects making them less willing to transfer money that would benefit the A subject even if it will benefit themselves. The potential effect of these treatments on the A subjects behavior is less clear and so we will not claim a formal hypothesis regarding it.

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 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. The idea is that 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 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 generated 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. It is important
to note though that our experiment was not designed primarily to address these issues and the design did little to make these social identities salient. Given the strength of some of the prior results it seems reasonable to examine our data 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. 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.

All of these hypotheses are easily derivable from standard models of inequity aversion under the assumption, as in Cox, Friedman, and Gjerstad (2007), that the underlying preference parameters 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 follow, we will of course only be able to identify both of those effects jointly.

### 4 Results

**4.1 Assignment Treatments**

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. 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. 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. In each of the other treatments, the amount they take
increases. The differences by treatment are consistent for the A subjects in the data from
all rounds but we find that in the D subject choices one of the directional eﬀects reverses.
This may be related to the interactions with the information revelation in that treatment
and so we can not attribute that to the assignment mechanism itself based on these simple
summary statistics.

The statistical tests of the specific hypotheses are contained in table 2 which presents
OLS regressions with robustly estimated standard errors to determine if the diﬀerences
in the summary statistics are signiﬁcant. 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. Our ﬁrst three results are
based on using the 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>
<thead>
<tr>
<th></th>
<th>Disadvantaged Round 1</th>
<th>Disadvantaged All Rounds</th>
<th>advantaged Round 1</th>
<th>advantaged All Rounds</th>
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<tr>
<td>Random</td>
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<td>3.35</td>
<td>3.73</td>
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<td>SAT</td>
<td>2.40</td>
<td>2.76</td>
<td>4.15</td>
<td>4.34</td>
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<td>2.27</td>
<td>3.85</td>
<td>4.22</td>
</tr>
<tr>
<td>PG</td>
<td>2.55</td>
<td>3.85</td>
<td>4.45</td>
<td>4.79</td>
</tr>
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</table>

Table 1: Average transfer amount.

<table>
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<th></th>
<th>Disadvantaged Round 1</th>
<th>Disadvantaged All Rounds</th>
<th>advantaged Round 1</th>
<th>advantaged All Rounds</th>
</tr>
</thead>
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<tr>
<td>SAT</td>
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<td>−0.388</td>
<td>0.800*</td>
<td>0.619***</td>
</tr>
<tr>
<td></td>
<td>(0.706)</td>
<td>(0.305)</td>
<td>(0.473)</td>
<td>(0.188)</td>
</tr>
<tr>
<td>KK</td>
<td>−1.600*</td>
<td>−0.881***</td>
<td>0.500</td>
<td>0.494***</td>
</tr>
<tr>
<td></td>
<td>(0.862)</td>
<td>(0.341)</td>
<td>(0.511)</td>
<td>(0.184)</td>
</tr>
<tr>
<td>PG</td>
<td>−1.300*</td>
<td>0.700***</td>
<td>1.100**</td>
<td>1.069***</td>
</tr>
<tr>
<td></td>
<td>(0.702)</td>
<td>(0.260)</td>
<td>(0.421)</td>
<td>(0.153)</td>
</tr>
<tr>
<td>Constant</td>
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<tr>
<td></td>
<td>(0.357)</td>
<td>(0.207)</td>
<td>(0.357)</td>
<td>(0.146)</td>
</tr>
</tbody>
</table>

Table 2: OLS regressions with robust standard errors. Dependent variable is amount
transferred.

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. 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.

**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 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. 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 KK effect is weak.*

### 4.2 Effects of Information

To examine how observing information about a counterpart affected decision making behavior we conduct regressions for D and A subjects separately to account for the fact that the two types of players might respond differently. Table 3 displays the results for D subjects and Table 4 for A subjects. We also conduct separate regressions to allow us to investigate the effect of each type of information revelation separately. The basic structure for each regression is an OLS regression with robust standard errors in which the dependent variable is the amount of the transfer. The data included in the SAT regressions consists of choices in the Random, KK and PG assignment treatments for the rounds in which the SAT score of the counterpart is shown and the rounds in which no information on the counterpart is shown. The other regressions are constructed similarly. By including only data for these rounds we obtain a clean comparison between choices in which the specific type of information is shown to the subjects and the rounds in which no information is shown to allow for clear tests of the specific hypotheses.

**Result 5** *Information Meritocracy Hypothesis Rejected. Disadvantaged subjects in the Low SAT group treat High SAT counterparts no differently from Low SAT counterparts. High SAT subjects are more generous towards High SAT counterparts, but treat Low SAT counterparts the same as if they observed no information about them.*
### Table 3: Effects of observing information on Disadvantaged players.

<table>
<thead>
<tr>
<th></th>
<th>SAT Info</th>
<th></th>
<th>KK Info</th>
<th></th>
<th>ID Info</th>
<th></th>
<th>PG Info</th>
<th></th>
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<tbody>
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<td></td>
<td>Low SAT</td>
<td>High SAT</td>
<td></td>
<td>High PG</td>
<td>Low PG</td>
<td></td>
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<td></td>
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<td>Info Shown</td>
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<td>1.257*</td>
<td>0.249</td>
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<td>-1.176*</td>
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<td>(0.434)</td>
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<td></td>
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<td>(0.846)</td>
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<tr>
<td>Constant</td>
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<td>2.709***</td>
<td>3.321***</td>
<td>3.143***</td>
<td>3.809***</td>
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<tr>
<td></td>
<td>(0.395)</td>
<td>(0.473)</td>
<td>(0.315)</td>
<td>(0.310)</td>
<td>(0.488)</td>
<td>(0.442)</td>
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</tr>
</tbody>
</table>

Robust Standard errors in parentheses, ***p < 0.01, **p < 0.05, *p < 0.1
Assignment treatment dummies included but suppressed.
Each regression excludes data from corresponding assignment treatment.

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 have therefore separated the regression into including only members of the Low SAT group and then another with only members of the High SAT group. Those in the Low group exhibit no differential treatment. Those in the High SAT group have a positive and significant coefficient of 1.257 for when they are shown information about their counterpart but the coefficient on the dummy for when that opponent is in the Low group is -1.540 and also significant. The total effect then of learning someone is in the Low group is therefore \(-0.283\) but it is not significantly different from zero \((p-value=0.625)\). This means that learning someone is in the Low Group leads to a response no different from the no information case. The way to interpret the base coefficient on whether the information is seen at all is therefore the effect of learning whether the counterpart is in the High SAT group. Since it is positive and significant, this leads to the interpretation that High SAT subjects treatment other members of that group more generously.

**Result 6**  Both Social Identity Hypotheses Rejected. Disadvantaged subjects treatment members of their own KK and ID groups no differently than members of the other group.

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 may have been more
### Table 4: Effects of observing information on Advantaged players.

<table>
<thead>
<tr>
<th>Info Shown</th>
<th>SAT Info</th>
<th>KK Info</th>
<th>ID Info</th>
<th>PG Info</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Low SAT</td>
<td>High SAT</td>
<td>High PG</td>
<td>Low PG</td>
</tr>
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<td></td>
<td>(0.243)</td>
<td>(0.331)</td>
<td>(0.210)</td>
<td>(0.318)</td>
</tr>
</tbody>
</table>

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

Assignment treatment dummies included but suppressed.

Each regression excludes data from corresponding assignment treatment.

willing to transfer their endowment. The two regressions examining this for the KK and ID information treatments shows that the effect of displaying this information is statistically insignificant as is the dummy variable indicating whether the counterpart is in the same group.

**Result 7** *Corruption Hypothesis not rejected. Disadvantaged subjects in the High PG group transfer less to counterparts in the Low PG group than in the High PG group.*

To investigate the effect of displaying the PG group membership of a counterpart we have split the sample again separating out those subjects who are in the High PG group from those in the Low group. For those in the High PG group we find that displaying information about the counterpart generates a negative and significant effect while the coefficient on the dummy variable for if the counterpart is in the High group is positive but insignificant. The way to interpret these coefficients is that the base dummy for if the information is shown represents how transfers change when the counterpart is found to be in the Low PG group and the effect is that transfers are lower compared to when the subject learns no information about their counterpart. The total effect from learning that the opponent is also in the High group involves summing the two coefficients generating a combined effect of -0.093 which is insignificant ($p$–value=0.887) indicating that High PG counterparts are treated no differently than if the subject knew nothing about them.

**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. Table 4 contains a set of regressions identical to those in Table 3 but now using only data from the choices by 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 for High SAT A subjects asking for a higher transfer from Low SAT counterparts but otherwise no effects are significant.

5 Conclusion

The focus of this study was on attempting to understand how different cultural elements in a society that lead to differences in status or position affect the willingness of individuals to support transfers modeled on certain policies that would be pro-growth yet lead to increased inequality. Our environment was carefully constructed so that the disadvantaged subjects knew with certainty that engaging in the regressive transfers were strictly in their own self-interest but they also knew that the transfer would help their already advantaged counterpart even more. The end result of any transfer then increased the earnings of both parties while simultaneously increasing the inequality in their earnings. Our findings demonstrate a number of interesting aspects of these decisions.

Perhaps the most surprising element given prior work demonstrating that inequality concerns are diminished when a transaction improves efficiency\(^3\) is that our subjects of both types are not willing to maximize their own welfare by transferring the entire endowment of the D subject to the A subject. 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 such as TARP or various subsidies to firms actually will generate long term benefits to all members of the society will not necessarily be enough to make them willing to support these policies

The relative effects we find regarding the impact of the treatments on the behavior of the A subjects is quite interesting and appears to be consistent with previous work. The effect found in the SAT treatment is of course similar to the entitlement effect found 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 could be seen as 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. In our current results, 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 inequality 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. 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 suggestive but not conclusive.

The behavior of the D subjects is perhaps more interesting as the behavior in 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 who were assigned to that position by random chance. The failure of the meritocracy result to be obtained might at first seem shocking, 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. That paper clearly suggests our advantaged subjects will be more willing to take advantage of their position, and they do, but the data in that paper provide little insight on how the disadvantaged subjects would respond to the meritocracy regime.

There are also several psychology studies which look at these issues of meritocracy and find that the effects of a meritocratic system are complex. McCoy and Major (2007) is one of the closest to our interest as the authors of that study examine how individuals in disadvantaged situations justify their role in response to meritocratic priming. They find that low status individuals may be more likely to accept their position as justified rather than the outcome of discrimination after receiving meritocratic priming but high status individuals tend to exhibit the opposite response and believe their outcome to be more likely to be the result of discrimination after receiving meritocratic priming. The latter response is similar to what we observe. These results clearly indicate that the response of individuals to a meritocratic setting is complex and we should not necessarily expect that individuals will be more likely to accept a disadvantaged position just because others performed better than them on the measure used to assign roles. Thus it is not clear that societies such as the US where there is a more general belief that wealth and position in society are determined on a meritocratic basis should be more likely to support policies of the sort considered here than societies such as in much of the developing world where there is a belief that roles in society might be determined more by cronyism and corruption than merit.

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 a disadvantaged
individual thought their counterpart had achieved their position through one of the negative assignment mechanisms, could positive information about their merit or type of person they are matched with, counter to some extent the negative treatment. Our findings are that this is not the case. If a disadvantaged person finds out that his counterpart really is more capable (i.e. had a higher SAT score), is generous in the PG phase or is a member of the same group, then in none of these cases does he treat that counterpart more generously. There were a few cases in which negative information did generate a negative response but the positive information never really generated the hypothesized positive response we had anticipated.

Taken as a whole, these results paint a very dismal picture for a policy maker who attempts to put together support from the disadvantaged members of society for an efficiency enhancing policy that involves some sort of regressive transfer. Our results demonstrate that support for such policies may not be strong even when members of a society fully believe in the efficiency enhancing properties and so in the face of uncertainty about those properties, support is likely to be lower still. Further, one of the most promising arguments for why such transfers should be acceptable, i.e. that those in the advantaged position earned their place in society, appears to fail to overcome resistance to such policies and in fact increases resistance.

References


Figure 2: Screenshot of interface with which subjects made their allocation decisions.