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Matthew Harvey
University of Washington-Tacoma

David Nickerson
Temple University

Abigail Wozniak
Federal Reserve Bank of Minneapolis and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

When Fairness Matters: Cross-Race Responses to Intentionally Fair Treatment*

Do White and Black Americans differ in their response to fair versus unfair treatment, and do these reactions depend on whether treatment is intentional? We study an ultimatum game in which we non-deceptively vary three dimensions: racial identities of participants, offer inequality, and whether the offer was made intentionally or assigned by lottery. Unequal offers are more likely to be rejected in all conditions, but participants differed in how intentionality behind an offer affected their response. White respondents did not differentiate between intentional and randomly assigned offer inequality. In contrast, among Black respondents, intentionality increased acceptance of fair offers.

JEL Classification: D63, J71, C90

Keywords: race identities, fairness, ultimatum game, inequality aversion, intentionality

Corresponding author:
Abigail Wozniak
Federal Reserve Bank of Minneapolis
90 Hennepin Avenue
Minneapolis, MN 55401
USA

E-mail: abigailwozniak@gmail.com

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I. Introduction

Many economic interactions involve the potential for unequal treatment. For example, pay and task assignment at work, legal decisions, and customer-supplier exchanges all involve the potential for similarly situated individuals to be treated differently. Rules are sometimes used to curtail the ability of participants to act unequally. Pay progression may be determined by a schedule or quantitative metrics, legal decisions may need to follow guidelines, and establishments may be required to provide equal service to all paying customers. Participants in these interactions face decisions about how to respond to the treatment they receive. In doing so, they are likely to take into account the behavior itself as well as rules governing the interaction.

Unequal treatment may be a particularly salient part of daily life for Black Americans. Black Americans are far more likely than White Americans to perceive racial inequality in a wide range of topics (Horowitz, Menasce, and Brown 2019). This raises the question, do Black and White participants respond to unequal treatment in the same way? Attentiveness to inequality may make Black Americans more alert to the intentionality behind unequal treatment or the identities of participants in an interaction. Amidst concern about systemic inequities that disproportionately affect Black Americans, daily interactions are often highlighted as a place where inequitable treatment persists (Sue et al. 2007; Skinner-Dorkenoo et al. 2021).[1] Yet little is known about whether responses to unequal treatment differ between Black and White Americans, or about whether the context of the treatment might affect these responses.

If Black and White Americans respond differently to unequal treatment or the intentionality behind it, such responses could generate group differences in outcomes. Consider how this might operate in the labor market. If the potential for unequal treatment on the job or the intentionality (discretion) behind it is more relevant for one group than another, this adds complexity to the job search for workers seeking a particular environment.[2] Workers have to seek out jobs that are agreeable to them on these additional dimensions, in addition to seeking higher wages or other improvements. Moreover, job characteristics like the propensity for unequal treatment and manager discretion may be hard to observe in the application process. These added search challenges would function as a

[1] See Lilienfeld (2020) and Williams (2020) for discussions of psychology research on microaggressions.
[2] Rotemberg (2006) surveys field evidence on altruism and cooperation in workplaces. Loewenstein, Thompson, and Bazerman (1989) and Rabin (1993) note that workplace interactions provide many opportunities for fairness and equality to impact behavior.

barrier to make job mobility more difficult (Kline, Rose, and Walters 2022; review in Charles and Guryan 2011). Searching for non-discriminatory employers is costly, and therefore workers cannot quickly punish discriminators by choosing a new firm. Without understanding responses to unequal treatment at a more fundamental level, we may not have a complete understanding of what drives observed differences in the labor market or other settings where participants face unequal treatment.

In this paper, we contribute to this discussion by examining micro-level interactions that involve unequal treatment between American participants identifying as Black or White. We build on a large literature showing that aversion to unequal treatment is widespread and a smaller literature examining intentionally unequal treatment. We follow previous researchers and use an ultimatum game to study these questions in a large, representative sample of Black and White Americans. Our ultimatum game is one-shot and provides no further context for the source of funds to be divided, so we interpret equal treatment as fairness. Our design allows us to study the roles of intentionality and participant race identity in responses to unequal treatment, and our paper is the first to generalize standard inequality aversion tests to a representative population of Black respondents. Specifically, we report the results of a lab-in-the-field investigation into responses to unequal treatment among pairs of participants with various pairings of race identities. We also vary the conditions under which unequal offers arise. Offers may be assigned by the experimenters or made intentionally. This allows us to investigate whether the response to unequal treatment is affected by the conditions under which it happens.

We non-deceptively solicited ultimatum game offers to divide $10, $20, or $50 from proposers. We then invited a large sample of Black and White respondents to play an ultimatum game by phone. These 532 respondents were drawn from registered voter rolls in Georgia and stratified on race and neighborhood income to ensure representation and balance across these dimensions. Respondents were randomly paired with a proposer's offer, and the race of the proposer was indicated to respondents using versions of the proposers' names that were masked to preserve confidentiality while retaining racial distinctiveness. Respondents heard a script informing them either that the proposer chose the division or that the division was assigned by a computer; the statement in the script was true of the division in question. This step allowed us to vary intentionality. All respondents played the game once for real stakes. Proposers each made three offers, and one randomly chosen offer was played for real stakes.

As is consistent with prior literature, we find that more unequal offers are more likely to be rejected in our sample.[3] However, participants differed in how the intentionality behind an offer affected their response to splits. Among White respondents, more equal divisions were more likely to be accepted, but their acceptance was unaffected by whether the offer was made intentionally or exogenously imposed. Among Black respondents, intentionality increased the sensitivity to equal versus unequal treatment. We find that this greater sensitivity is driven by an increase in acceptance of relatively equal offers among Black participants when those offers are made intentionally. The effect is substantial: an intentional offer raises acceptance of equal offers by about 25 percentage points. We also explore whether responses in the game differ with other individual or pair-wise characteristics, including age, party affiliation, and gender. We find that race identity is the primary respondent characteristic along which game responses differ substantively. The focus of our experiment is to measure responses to unequal treatment under different conditions of participant intentionality and identity.We speculate that two mechanisms may be at work. Black respondents may be more attentive to details regarding unequal treatment as a result of experiencing such treatment more often. Alternatively, intentionally equal treatment may be a signal about future treatment, and if this signal is more valuable to Black respondents, it may influence their acceptance choices.

Our paper makes two contributions. First, it adds to our understanding of micro-level interactions between people with different race identities where unequal treatment is possible.[4] Previous experimental studies of the ways race affects an interaction have focused on quantifying Whites' (or a general population's) reactions to Black versus White subjects in a remote interaction. This approach includes studies of lender responses to photos of Black versus White loan applicants in an online direct lending platform (Pope and Sydnor 2011), buyer responses to Black versus White sellers on eBay (Doleac and Stein 2013), donor responses to photos of Black versus White flood victims (Fong and Luttmer 2009), and, of course, Black- versus White-sounding names on resumes (Bertrand and Mullainathan 2004; Kline, Rose, and Walters 2022). In our study, Black participants and White participants have equal agency, and our inclusive sample construction enables us to measure responses of Black participants with equal statistical power. Although the context is different, this makes our study

[3] See Bolton and Ockenfels (2000) and Cooper and Kagel (2015) for an overview.

[4] Our experiment focuses on two race identities. The voter registration files from which self-reported race is drawn allowed respondents to report exactly one of six race/ethnic identities: White, Black non-Hispanic, Asian, Hispanic, Native American, or other.

similar to the experimental literature on persistent gender gaps in pay and promotion (Niederle and Vesterlund, 2007; Buser, Niederle, and Oosterbeek, 2014).

Our second contribution is methodological. We develop an experimental design in which Black respondents can be invited to participate in a way that is much more representative of the population of Black Americans than has been possible in previous studies. The design meets the standards for non-deception in economics and more closely approximates direct interaction between Black and White participants than the studies listed above do. Underrepresentation in research spaces has likely contributed to the fact that economists have less evidence on how different race identities between agents in an interaction affect outcomes (relative to the amount of evidence on how different gender identities affect them). In particular, lab experimental evidence involving large numbers of Black participants is rare. This is in part because common subject pools for experiments lack appropriate representation of Black Americans. Black Americans are underrepresented on the college campuses where significant experimental programs in the social sciences are found (Carnevale and Strohl 2013). Similarly, Black Americans constitute a lower percentage of participants in common pools of online experimental subjects such as mTurk (Walters, Christakis, and Wright 2018) and Lucid (Coppock and McCellan 2019). High quality subject panels that are representative of the population, like the Understanding America Study or NORC's AmeriSpeak panel, recruit participants in proportion to their presence in the overall US population; this approach generally leaves insufficient numbers of Black respondents to deeply investigate cross-race differences in experimental or survey choices.[5] Griffin, Nickerson, and Wozniak (2012) employed a similar sample to explore inequality aversion across races. The experiment in this paper extends their design to explore the role of intentionality in ultimatum game responses for Black and White participants. Our approach also solves an important experimental design challenge in their paper, which is how to non-deceptively match large numbers of proposers and responders outside a university lab setting.

II. Experimental Design and Subject Recruitment

Participants were recruited separately for two roles: proposers and respondents. Respondent behavior is the focus of our study, but proposers were recruited first to generate a non-deceptive interaction. We invited 27 students from a university outside Chicago to participate in an on-campus experiment. The invitation was made broadly, but students were invited into the study on the basis of having a

[5] For example, see "Technical Overview of the AmeriSpeak Panel" (NORC at the University of Chicago 2022).

racially distinctive name. The rules of the ultimatum game were explained to each proposer, and they were asked how they would divide $10, $20, and $50 between themselves and a respondent. Proposers were informed that one of their proposed splits would be randomly selected and played later with a respondent subject for real stakes, but they otherwise had no information about potential responders. Self-identified race of the proposers was also recorded. Within race categories, proposer first and last names were then recombined to create racially distinctive names for the proposer that preserved the proposer's anonymity. Two of these reconstituted names were selected to be presented to respondents in the experiment because they were racially distinctive: Peter O'Sullivan and Ray'von Brown. Offer, stakes, and proposer race combinations were then selected from the set of real proposals and used repeatedly. This achieved non-deception, since every responder heard a proposal that was made by an actual proposer. Every proposer had one of their offers selected and played for real stakes, consistent with our experimental instructions.[6] Appendix Table 1d shows the distribution of offer-stakes combinations that were administered to respondents, along with acceptance rates. Each offer-stakes combination appears at least 40 times in the data.

Respondents were then recruited by phone to play the game. We used random samples of the Georgia voter rolls maintained by Survey Sampling, Inc. (SSI). The SSI file contained name, phone number, address, self-identified race from the state voter file, age, party affiliation, estimated household income, and other demographic variables.[7] To better isolate the unique contribution of race in reactions to inequality, we randomly selected Black voters and then randomly sampled an equal number of White voters within each income category.[8] Our resulting potential subject pool constituted a representative sample of Black registered voters in the state with a phone number on file and a randomly selected pool of White respondents who matched on income.

Upon answering the phone, respondent subjects were invited to take part in a three-question research study. If they agreed, the rules of the ultimatum game were explained to them. They then heard a script in which the following four game parameters were randomly populated: participant race identities as connoted by the proposer's name, an intentional or assigned context of offer making, offer amount, and stakes of the game. We selected the offers presented to respondents from actual offers made, and we delivered these repeatedly to responders. Intentionality of an offer was conveyed by

[6] Proposers were paid for the outcome of the live round of their offer after the conclusion of our survey of respondents.
[7] These were sometimes probabilistically assigned by the survey firm when not observed for an individual.
[8] Voters for whom SSI listed an income less than $10,000 per year, the lowest income category, were dropped because this category is very heterogeneous with regard to socio-economic status.

informing respondents that they had been selected at random to play a game with a proposer who had made the proposed division they would hear. Non-intentionality was conveyed by informing respondents that they had been selected at random to play a game with a proposer and that the division they would hear had been assigned by researchers to the pair. Because proposals were made by real proposers and selected proposals were randomly assigned to respondent-proposer pairs, both statements are true. The intentionality treatment is therefore determined by how the offer is presented to respondents. Proposals were made with the understanding that respondents would be randomly contacted by researchers to play the game over the phone. Proposers had no knowledge of responders. Full script texts and other details of the experimental design are available in Appendix 1.

Among the numbers dialed by the survey firm, 3.5% of voters answered the phone and completed the experiment; the final pool consisted of 532 respondents. Appendix Table 3a presents summary statistics on the respondents. The sample is balanced across all game variables (stakes, offer, share, etc.) and also appears balanced across demographic covariates except age, sex, marital status, and political affiliation. Given the differences in age, political affiliation, marital status, and sex, we control for these variables in further analysis. We also conduct subsample analysis that examines whether outcomes differ across groups defined by these dimensions.

Respondents were considered "treated" in the race dimension if the proposer's masked name implied the proposer was Black. Appendix Table 1b tests whether respondents correctly perceived their proposer's race from the proposer's masked name. We find that responders correctly identify the implied race of their proposer in over 60% of cases, and we therefore believe the intent to treat on the basis of the proposer's masked name is strong.[9] This rate seems to be generally unaffected by individual demographics, with the exception of race, with Black responders less likely to correctly identify the implied race of their assigned proposer. Black proposer names were also less likely to be correctly identified, although this did not differ across respondent race.

III. Acceptance Decision Framework

[9] In Appendix Table 3e, we confirm that our main results are not sensitive to using perceived versus assigned race of the proposer. A portion of our sample declined to report perceived race, which reduces sample size and increases standard errors, but estimates using this method are broadly similar.

To study responses to unequal treatment by different partners and under different conditions, we model acceptance of unequal offers in the ultimatum game. Our empirical model is an application of the standard approach to estimating choice models using a logit specification after assuming errors follow an extreme value distribution. Specifically, we assume a respondent's utility from accepting an offer in our experiment takes the form

(1) $v\big(a(g)\big) = o_g - \gamma_1 r_g - \gamma_2 I_g,$

where $a(g) \in \{0,1\}$, r_g is relative inequality, defined as $r_g = (0.5 - \frac{o_g}{s_g})$ where s_g is the stakes and o_g is the offer,[10] and I_g is an indicator for the intentional division condition. By construction, r_g has a range of -0.5 to 0.5, with negative values indicating "hyperfairness," in which proposers allocate less to themselves than to their respondent partner. A value of 0.5 is completely unfair, with proposers allocating all of the stakes to themselves. In our experiments, r_g ranged from near zero (fair) to 0.3. The utility function above can be estimated directly from acceptance choices using the following logit equation, where $y_i = \{0,1\}$ is an indicator for acceptance, under the assumption that ε_i follows a Type I extreme value distribution:

(2) $y_i = \beta_1 o_g + \beta_2 r_g + \beta_3 I_g + \varepsilon_i.$

In our main estimating equations, developed in the next section, we allow coefficients to vary with the race identities of participants.

Before moving to that, we compare our framework with others that incorporate variation in intentional behavior. In economics, humans choosing freely are framed as behaving intentionally (Cooper and Kagel 2015). There are two broad approaches to creating a treatment without intentionality in experimental economics. The first is to partner participants with a computer that has been programmed to play the same game that humans play in the intentional treatment arm, instead of partnering them with a human. The assumption is that intention is absent from the computer's behavior but not the human's. The second approach is to assign a partner's choice using a rule. A common rule is to randomly select from the possible choices humans might otherwise make intentionally,

[10] Griffin, Nickerson, and Wozniak (2012) show that an offer's absolute inequality is orthogonal to acceptance once relative inequality is included. We repeat their specification selection analysis for our sample in Appendix 2 and verify that the same pattern holds.

sometimes drawing from distributions of observed human choices. We take the second approach in our design.

Distaste for unequal treatment is a common phenomenon in studies that employ the ultimatum game (Cooper and Kagel 2015). The literature is less settled on individuals' response to intentionality. Blount (1995) is the earliest example of an experiment with varying intentions. Blount uses the ultimatum game and varies whether proposed offers were assigned or intentional, but the study's sample sizes were small, and the game was not played for real stakes. Peterburs et. al. (2017) and Radke, Guroglu, and Brunijin (2012) explore the effects of intentionality and context on ultimatum game acceptance using computer-generated offers, but their samples were also small (34 and 50, respectively). The experiment in Falk, Fehr, and Fishbacher (2008; 112 subjects) shows responders reacting to intentionality. The most marked difference between our experiment and previous studies is the makeup of the sample. Previous studies drew participants from populations of university students and make no mention of race at all. Both our black and white samples have a larger number of subjects in each cell in the intention treatment than the total number of subjects in even the largest of the prior studies listed above (Black: 138 intentional and 134 random; White: 128 intentional and 132 random). The makeup of our sample also facilitates the interaction between races, with 132 white-white pairs, 141 black-black pairs, 132 black subjects with a white proposer, and 128 white subjects with a black proposer. Furthermore, we purposefully oversampled the unfair offers presented to subjects (without deception) to ensure that half of the offers were fair splits (i.e., 50/50) and half were unfair (i.e, $2 of $10; $5 of $20; and $10 of $50). The distribution across all possible cells is in Appendix 1a. Thus, our experiment provides unique insight into how race interacts with intentionality.

IV. Results

We begin by tabulating acceptance decisions for respondents in each of the main treatment cells of the experiment. These are reported in Table 1, along with subsample sizes for each of the treatment cells. A number of patterns in the table align with expectations. We expect nearly 100% acceptance of even splits. Table 1 shows that acceptance of even splits is quite high, though not 100%. Relatedly, we would expect that even splits are accepted more often than uneven splits, and Table 1 shows this is the case for every otherwise identical treatment cell. Finally, we observe less acceptance of uneven splits when these are intentional as opposed to non-intentional (with one exception). Together, these facts give us confidence that respondents understood the game and played it in a way that largely aligns with previous experiments.

We have fewer priors to inform us about two other patterns in the table: differences in acceptance rates for even splits made intentionally versus non-intentionally, and cross-race differences in acceptance rates. Should intentionality matter when an even division is on the table? And should the combination of proposers' and responders' races matter for behavior in any ultimatum game setting? Our experiment answers these questions, and Table 1 provides a preview. The top panel shows that the largest gaps in acceptance rates occur between intentional and non-intentional uneven offers made by a proposer from the responder's own race group. White responders are 16 percentage points less likely to accept an uneven offer from a white proposer if it is made intentionally; for Black responders the number is 18 percentage points. The bottom panel shows that white responders are more likely to accept an even split if it is made non-intentionally, but Black responders are more likely to accept an even split made intentionally. The raw acceptance rates in both panels suggest that intentionality may shape acceptance, and this may interact with the races of participants.

To understand the separate roles of intentionality, division fairness, and participant race, we turn to a linear probability representation of the acceptance decision. We estimate Equation 2 using OLS.[11] The coefficients on r_g and I_g indicate whether relative inequality or intentionality, respectively, shape acceptance decisions. We then expand this specification to allow the effect of intentionality to differ across our various unequal treatment contexts. Specifically, we interact the indicator for intentional division with offer amount and relative inequality as follows:

$$(3)\quad y_i = \beta_1 o_g + \beta_2 r_g + \beta_3 I_g + \beta_4 I_g * o_g + \beta_5 I_g * r_g + \gamma' X_{i,g} + \varepsilon_i.$$

This allows us to assess whether the likelihood of acceptance varies when more or less equal offers are made intentionally. The term X_g is a vector of controls for respondent and game characteristics included in our preferred specifications for robustness to any residual non-randomness in assignment of the game conditions.[12]

Column 1 of Table 2 reports the results from estimating Equation 2. Subsequent columns show results from estimating the expanded model in Equation 3 with different sets of control variables. Specifically, columns 2 through 4 show results from estimating Equation 3 with no controls, with type controls, and with the expanded set of demographic controls that subsume the type controls, respectively. To

[11] We prefer to use the linear probability model for ease of interpretation, particularly of interactions in later specifications (Ai and Norton 2003). Estimates from logit estimation of Equation 3 are available in Appendix Table 3b.
[12] We estimated a version of Equation 3 that controls for the various offer levels using fixed effects. Our conclusions are unchanged under this alternate specification, reported in Appendix Table 3f.

investigate cross-race differences in the role of inequality and intentionality in acceptance, we then estimate the acceptance model separately on Black and White subsamples and report the results in columns 5 and 6. Our experimental design allows us to use randomization inference to assess significance.[13] Throughout our main tables, we report conventional robust standard errors in parentheses and p-values from our randomization inference with 10,000 simulations of the specifications in brackets. The differences are minimal.

Results in columns 1 through 4 are very similar to one another, so we discuss them together. Respondents' willingness to accept an ultimatum game offer is affected by inequality aversion, consistent with the large ultimatum game literature. The coefficient on relative inequality is negative and statistically significant at the 1% level across columns 1 through 4. (Note that we discuss statistical significance based on the robust standard errors reported in parentheses in the tables.) Its magnitude remains the same across columns as well. A 0.1 increase in relative inequality is associated with about a 10 percentage point decrease in the probability that a respondent will accept an offer. This effect is sizeable given that the average acceptance rate is about 72%. The role of intentionality in the full sample is less clear. The intentionality dummy and its interactions are imprecisely estimated, though the sign and magnitude are consistent across columns 2–4.

In columns 5 and 6, we estimate Equation 3 separately in subsamples split by respondent race. The effect of relative inequality on acceptance is large, negative, and statistically significant in both subsamples, and similar in magnitude to estimates from the pooled sample. The results also show that intentionality has no statistically significant effect for White respondents, either directly or indirectly. Among Black respondents, however, intentionality has a pronounced effect on acceptance. This works both through a main effect of intentionality and through changing responsiveness to relative inequality. Column 6 shows a 24 percentage point increase in the probability of acceptance for intentional offers among Black respondents, but this is offset quickly at higher levels of relative inequality. The probability that a Black respondent accepts an intentionally unequal offer falls by about 8 percentage points for every 0.1 increase in relative inequality.

[13] Because the assignment to treatment conditions occurs via a known process, it is possible to calculate exact p-values with no modeling assumptions using randomization inference (Abadie, 2002; Ding, Feller, and Miratrix, 2016). We elected not to implement adjustments for multiple hypothesis testing. The outcome of interest and the main variables of interest are unchanged across the specifications presented in the main exhibits. In our view, such adjustments are not valuable in this setting.

We then turn to the question of whether the racial identity of proposers or respondents affects acceptance choices and whether the role of intentionality differs across interactions defined by the four possible pairs of proposer and respondent race combinations. This approach allows parameters governing acceptance to differ across the dyads. The results are reported in Table 3. The first two columns show that among White respondents, the determinants of acceptance are statistically similar to results in Table 2, regardless of the race of the proposer, although standard errors increase in the smaller subsamples. White respondents in both pair subsamples are insensitive to intentionality.

Among Black respondents, point estimates of the main effect of relative inequality and offer are also similar regardless of whether this subsample is partitioned by proposer race. By contrast, the effect of intentionality on acceptance differs by proposer race, with intentionality playing a large and significant role in acceptance when Black individuals are responding to White proposers. In Table 3, intentionality increases the likelihood of acceptance by 44 percentage points in such interactions, before its interaction with relative inequality is added. When Black individuals are responding to Black proposers, the impact of intentionality is statistically insignificant, small, and similar to that among White respondents. However, the standard errors do not allow us to reject that the responses to intentionality are the same for Black respondents in both pair types, despite large differences in the point estimates. We therefore view this result as suggestive of a distinct role for intentionality in interactions where Black individuals are responding to offers from White individuals, but it does not definitively point to one.

We further explore the role of intentionality in acceptance choices in the four pair-wise subsamples by calculating and plotting the net impacts implied by the Table 3 estimates. Results are shown in Figure 1. A dot indicates the net difference in acceptance rates across intentional and non-intentional offers within the subsample at the relative inequality level indicated.[14] Blue (circles) and orange (triangle) points show the difference in acceptance rates of an intentional versus a non-intentional offer of $9.50 in interactions with White respondents, at various levels of relative inequality. Gray (diamond) and yellow (square) points show the analogous differences in interactions where the respondent is Black.

Figure 1 shows a negative slope within the two sets of points for Black responders. This means that among Black respondents, intentional equal offers are more likely to be accepted than intentional

[14] Offer is held constant at its sample mean of 9.5 across the calculations of the 16 differences.

unequal offers. However, this general pattern differs across dyads with Black responders. When Black responders face White proposers, intentionality increases acceptance of equal offers but has no effect on acceptance of unequal offers. This difference is statistically significant: the gray square markers in the upper left of the Figure 1 are significantly different from zero. When Black responders face Black proposers, this is reversed: intentionality reduces acceptance of unequal offers but has no effect on equal offers. However, the differences are not statistically significant in this case. Among White respondents, intentional offers are no more likely to be accepted that non-intentional offers within either dyad type, as none of the blue or orange dots are significantly different from zero. This is true at all levels of relative inequality.

We have demonstrated that the intentional nature of an offer is important to Black respondents but irrelevant for White respondents. Among Black respondents, intentionality is particularly salient for relatively equal offers. These are more likely to be accepted when made intentionally. In the final step of our main analysis, we ask whether these differences are unique to comparing the choices of Black and White respondents. We consider four other exclusive and exhaustive subgroups in our sample: gender, age (older versus younger), political affiliation, and income (above versus below sample median). We estimate Equation 3 with a full set of interactions between the game parameters ($\beta_1$through β_4 in Equation 3) and an identifier for each group and report the results in Table 4.

The first column of Table 4 reports results for the Black-White sample division. Each cell reports the coefficient on the interaction between an indicator for a Black respondent and the game quantity indicated in the row. The results show that there is no statistical difference in the impact of offer amount, relative inequality, or the offer-intentional interaction in Black and White acceptance choices. On the other hand, an intentional offer is more likely to be accepted by Black respondents (RI p-value of 0.13). Black respondents are also weakly less likely to accept an intentional and unequal offer (RI p-value of 0.16). These differences are consistent with the estimates from separate Black and White subsamples in Table 2. The remaining columns repeat the analysis, replacing interactions with a Black respondent with an indicator for the subgroup noted first in the column heading. The second column shows that women do not respond differently than men to relative inequality, the offer amount, intentionality, or any of their interactions. The same is true for older (over 70) versus younger, Democrat versus non-Democrat, and higher versus lower earning household status respondents. The two Black-White differences identified have the lowest p-values in the table, although women are

weakly more responsive to relative inequality than men are.[15] We conclude that the role of intentionality is materially different for Black responders than for White ones in our ultimatum game, and this difference is unique to this subgroup comparison.

V. Robustness and Alternative Specifications

We assessed the robustness of our findings to a key sample restriction and to a number of alternative specifications. First, we exploit a follow up question asked after respondents made their experimental decision in order to identify potentially inattentive respondents. If subpopulations differ in their tendency to inattentiveness, then inattention, rather than true differences in preferences, might drive different estimates across subgroups. We assume inattentiveness is approximated by inconsistent acceptance choices, and we define inconsistency as accepting an offer that is lower than a respondent's stated reservation offer, elicited after their acceptance decision. Appendix Table 3a shows that Black respondents are somewhat more likely to provide inconsistent responses. To gauge the impact of this difference on our results, we remove inconsistent respondents from our sample and re-estimate the regressions in tables 2–4. Results (available upon request) are consistent with our main estimates and indicate that our findings are not driven by inconsistent respondents.

We also examined results from two alternative specifications. We explored the effect of the proposer's race on general acceptance behavior in order to understand whether Black proposers were more or less likely to have offers accepted overall. We re-estimated equation 3 with a dummy variable indicating that the proposer had a name commonly identified as Black. Results are reported in Appendix Table 3c. We find that Black proposers are associated with about an 8 percentage point increase in offer acceptance. This is true in the full sample and in separate Black and White subsamples. Other parameter estimates are little changed by this modification.

VI. Conclusion

Our experiment contributes to understanding how race identities and intentionality shape responses to unequal treatment. We studied a one-shot interaction in the ultimatum game between two participants where we experimentally and non-deceptively varied the participant race identity, offer fairness

[15] The test of overall model equality across the subgroups reported at the bottom of Table 4 rejects the idea that women and men follow the same acceptance model. This is driven by higher overall acceptance rates for women.

(inequality), and intentionality behind the offer. We find that unequal offers are less likely to be accepted, consistent with an extensive previous literature. Increasing the inequality in an ultimatum game offer (or unfairness) reduces acceptance among our sample of Black and White respondents similarly. We then find that acceptance depends on whether an offer is made intentionally, but only among Black respondents. White respondents are no more likely to accept an intentional offer than they are to accept an assigned offer. By contrast, among Black respondents, the effect of intentionality on acceptance is substantial. An intentional offer raises acceptance of nearly equal offers by nearly 25 percentage points in total.

Regarding our final question of whether the combination of participants' races in our ultimatum game affects responses, our findings suggest a different role for intentionality when Black responders face White proposers as opposed to Black proposers. Point estimates suggest that acceptance of relatively fair offers increases more when Black respondents face White proposers, while rejection of unfair offers rises when Black respondents face Black proposers.

What mechanisms might explain this difference? We find two explanations plausible. First, frequent unequal treatment may mean that Black respondents assess the nature of such treatment more quickly. If White respondents experience unequal treatment less often, they may respond to the first feature of the offer (an unequal split) but not the second (intentionality). A second possibility is that Black participants value intentionally equal treatment more because it signals future treatment. In a market where discrimination is present, Black workers may be concerned that they will not be treated fairly, perhaps by receiving less credit for their work or having their effort appropriated by others. Intentionally equal treatment by another participant might be valued as a signal and rewarded with acceptance. Equal treatment that is assigned provides no such information.

Other mechanisms could be at work, and it's possible that our findings are unique to our experimental context and subject pool.[16] Future research should build on our example of recruiting large racially and economically diverse subject pools in the US to study mechanisms that drive interactions in small groups.

[16] Bazerman, White, and Loewenstein (1995) and Loewenstein, Thompson, and Bazerman (1989) note that context is likely to influence notions of fairness in numerous ways, generating substantial variation across contexts.

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Table 1: Average Acceptance Rate by Interaction Category

Category	Intentional Offer	Non-intentional Offer
A. Uneven Split		
White Proposer, White Responder	40% (N=30)	56% (N=34)
Black Proposer, White Responder	66% (N=32)	55% (N=33)
White Proposer, Black Responder	49% (N=37)	47% (N=30)
Black Proposer, Black Responder	51% (N=35)	69% (N=36)
B. Even Spilt		
White Proposer, White Responder	82% (N=33)	89% (N=35)
Black Proposer, White Responder	91% (N=33)	97% (N=30)
White Proposer, Black Responder	94% (N=31)	85% (N=33)
Black Proposer, Black Responder	91% (N=35)	89% (N=35)

Notes: Source: Experimental data collected by authors. Panel A groups proposals with relative inequality in {0,0.1} as even splits and of (0.25, 0.3} as uneven splits. Cells represent the share of respondents in each category accepting their offer. N indicates number of completed acceptance decisions.

Table 2: Estimates from Linear Probability Models of Offer Acceptance

	All Respondents	All Respondents	All Respondents	All Respondents	White Respondents	Black Respondents
Offer	0.00	0.00	0.00	0.01	0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
	[0.52]	[0.25]	[0.25]	[0.24]	[0.64]	[0.25]
Relative Inequality	-1.20	-0.99	-0.99	-0.97	-1.16	-0.81
	(0.14)	(0.20)	(0.20)	(0.21)	(0.28)	(0.31)
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.01]
Intentional Offer	-0.03	0.09	0.09	0.10	-0.07	0.24**
	(0.04)	(0.08)	(0.08)	(0.08)	(0.12)	(0.12)
	[0.44]	[0.36]	[0.36]	[0.31]	[0.64]	[0.09]
OfferXIntentional		-0.01	-0.01	-0.01	0.00	-0.01
		(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
		[0.32]	[0.32]	[0.27]	[0.98]	[0.19]
RelativeXIntentional		-0.43	-0.43	-0.48	0.07	-0.82
		(0.29)	(0.29)	(0.30)	(0.44)	(0.42)
		[0.18]	[0.18]	[0.14]	[0.87]	[0.07]
R2	0.16	0.16	0.16	0.18	0.21	0.22
N	532	532	532	532	260	272
Average Acceptance	0.72	0.72	0.72	0.72	0.72	0.72
Type Controls			X			
Demographic Controls				X	X	X

Notes: This table estimates equation 3 using a linear probability model. Model (3) includes type controls to indicate race- and income-based strata, and (4)-(6) include a full set of demographic controls for age, race, marital status, income, education, sex, and party affiliation. The rightmost columns limit the sample to only White and only Black respondents, respectively. Robust standard errors in parentheses. Randomization inference p-values in brackets.

Table 3: Offer Acceptance by Race Composition of Proposer-Respondent Pairs

	White Respondent & White Proposer	White Respondent & Black Proposer	Black Respondent & White Proposer	Black Respondent & Black Proposer
Offer Amount	0.01	0.00	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.46]	[0.81]	[0.22]	[0.61]
Relative Inequality	-0.98	-1.24	-0.77	-0.66
	(0.39)	(0.42)	(0.47)	(0.42)
	[0.04]	[0.01]	[0.10]	[0.16]
Intentional Offer	-0.16	0.04	0.44	0.07
	(0.19)	(0.19)	(0.17)	(0.17)
	[0.45]	[0.84]	[0.03]	[0.72]
OfferXIntentional	0.00	-0.01	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
	[0.79]	[0.62]	[0.12]	[0.69]
RelativeXIntentional	-0.13	0.17	-1.00	-0.68
	(0.64)	(0.70)	(0.62)	(0.65)
	[0.85]	[0.81]	[0.14]	[0.31]
R2	0.26	0.31	0.35	0.28
N	132	128	131	141
Mean Acceptance	0.67	0.77	0.68	0.75
Demographic Controls	X	X	X	X

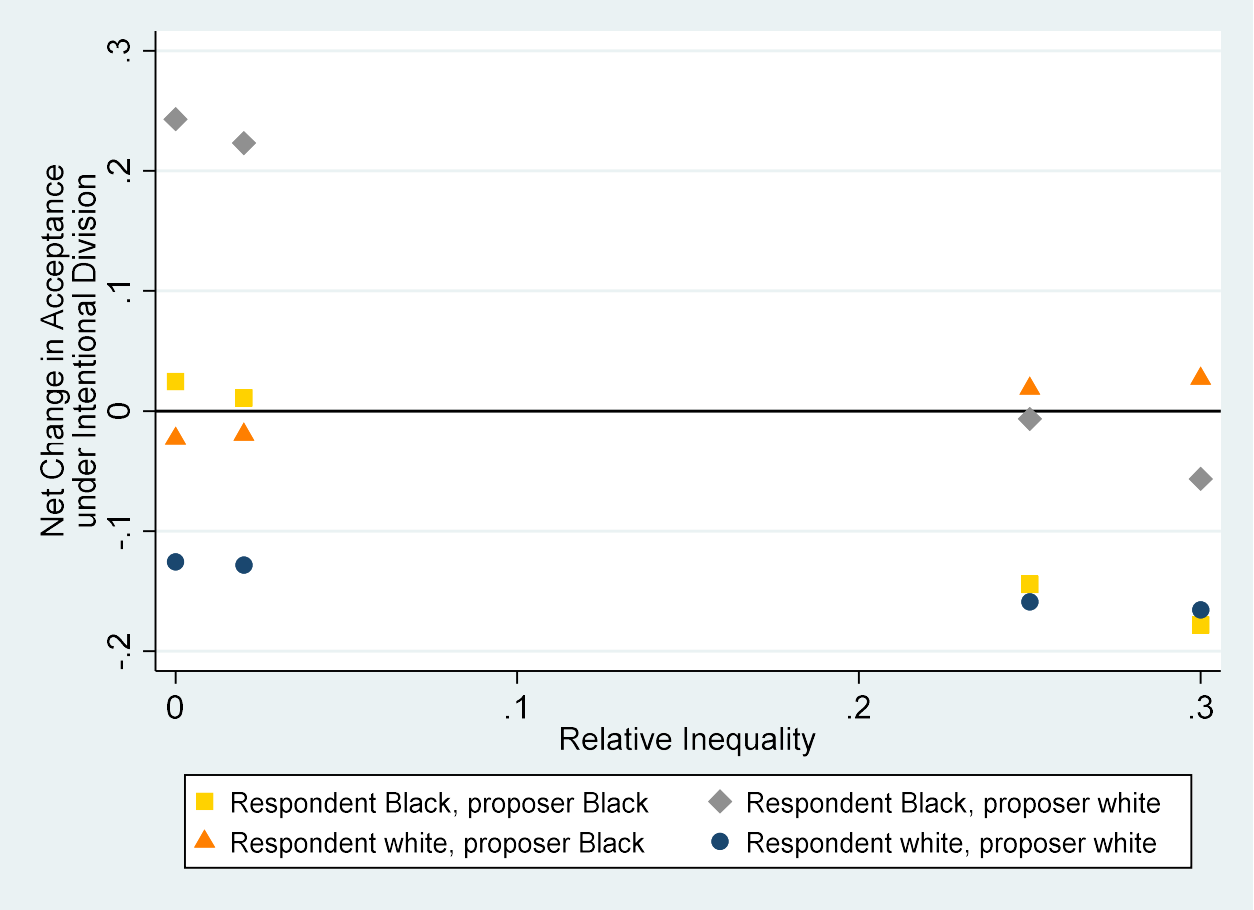
This table estimates equation 3 using a linear probability model, restricting the sample to White respondents and White proposers (1), White respondents and Black proposers (2), Black respondents and White proposers (3), and Black respondents and Black proposers (4), respectively. All specifications use the full set of demographic controls as in Table 2, column 4. Robust standard errors in parentheses. Randomization inference p-values in brackets.

Table 4: Response to Proposer Intentionality by Gender, Age, Political Party, and Income

Group Indicator	Race (Black-White)	Gender (F-M)	Age (>70-young)	Party (Dem-Rep/Ind)	Income (<30k-high)
Indicator interacted with:					
Offer Amount	0.00	0.00	0.01	0.00	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.63]	[0.89]	[0.54]	[0.78]	[0.32]
Relative Inequality	0.35	-0.66	0.15	0.11	-0.44
	(0.42)	(0.44)	(0.42)	(0.41)	(0.42)
	[0.45]	[0.16]	[0.75]	[0.82]	[0.33]
Intentional Offer	0.30*	-0.00	0.05	0.09	0.03
	(0.17)	(0.18)	(0.17)	(0.18)	(0.16)
	[0.13]	[0.99]	[0.82]	[0.66]	[0.87]
OfferXIntentional	-0.01	0.00	-0.01	0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
	[0.35]	[0.94]	[0.69]	[0.92]	[0.33]
RelativeXIntentional	-0.89	0.23	-0.44	0.13	0.44
	(0.61)	(0.63)	(0.61)	(0.64)	(0.60)
	[0.16]	[0.73]	[0.50]	[0.85]	[0.51]
R^2	0.23	0.22	0.21	0.21	0.19
N	532	532	532	532	532
Test Model Equality	p=0.47	p=0.05	p=0.70	p=0.72	p=0.32

Notes: Table 4 estimates an expanded version of equation 3 that includes interactions between the group indicator in the column heading and all the game variables and interactions shown. In the bottom row, we conduct an F test against the hypothesis that all coefficients reported in the column, as well as the level effect of a given group indicator, are jointly zero. All specifications use the full set of demographic controls as in Table 2, column 4. Robust standard errors in parentheses. Randomization inference p-values in brackets.

Figure 1: Net Effect of Intentionality on Offer Acceptance at Different Levels of Relative Inequality



Notes: Difference in probability of acceptance for an intentional offer vs. unintentional, based on estimates in Table 3. Probabilities of acceptance are evaluated at the average offer ($9.5) for each level of relative inequality and pair. The two top left points are statistically significant.

ONLINE APPENDIX MATERIALS

Not for Publication

APPENDIX 1. Design Details and Checks

The experimental design is described in Section II of the paper. This appendix provides additional details on the design implementation. Respondents were selected for surveying by Survey Sampling Inc. from their proprietary database. [17]

First, complete scripts that were read to respondents and proposers are available at the end of this appendix. Respondents were read one of two scripts. The first, Script A, signaled an intentional proposed division on the part of a proposer. The second, Script B, signaled a non-intentional division assigned to the proposer-respondent pair at random by researchers. The portions of the scripts that differ to signal intentional or non-intentional treatment are highlighted in gray. Additionally, respondents heard their assigned partner name three times in both scripts. Proposers were read one of three scripts. An example (Script A) is provided. The scripts varied in the example stakes and division they provided. This was intended to generate variation in proposer offers, but it was not material to the overall experiment.

Tables 1a, 1b, and 1c report results from additional checks on the design implementation. Table 1a shows the distribution of subjects by race across treatment arms of race of the proposer, intentionality, and stakes of the game. Subjects of both races are evenly distributed across the cells (chi-sq = 1.7, d.f. = 6, $p < 0.95$).

It is critical to the research design that subjects are able to intuit the race of the proposer based on the name of the proposer. To verify this understanding, after the game had been completed, we asked subjects "What racial group do you think [Partner first name] [Partner last name] identifies with? Do you think they identify with Whites, blacks, Asians, Hispanics, or something else?" Table 1b presents the results of a linear probability model regressing guessing the proposer identifies as "black" on whether the proposer has a distinctively black name. Under all model specifications, proposers with distinctively black names were 60 percentage points more likely to be identified as black by respondents. Thus, we are confident that this portion of the experimental design was effective in conveying the race of the proposer.

To boost variation in offers within stakes, we gave proposers different anchoring examples in the instructions. The script varied in the example split presented to proposers to explain the ultimatum game. Script A had the most equal example split with $10 for the proposer and $10 for the receiver. Script B had an example split with $14 for the proposer and $6 for the receiver. Script C had the most unequal distribution with $19 for the proposer and $1 for the receiver.

To determine whether the anchoring was effective on our 27 proposers, we estimate the following model:

$$Y_i = \alpha_0 + \beta_1 Script_i + \beta_2 \gamma_i + \epsilon_i \quad \text{(A1)}$$

[17] Survey Sampling, Inc has since been acquired by Dynata. Subjects were called by a survey firm, Sample Solutions, between May 27th and June 9th, 2014.

where Y_i is the amount the proposer decides to keep, $Script_i$ is the script the proposer heard before making their decision, and γ_i is a set of proposer specific demographic controls. The results are presented relative to script A. Table 1c presents the results from estimating equation A1 for stakes $10, $20, and $50 controlling for race and gender of the subject.

Across the three stakes levels, we find that the anchoring vignettes shifted offer amounts on average, but the differences do not cross traditional thresholds of statistical significance. Relative to the perfectly even split illustrated in Script A, the moderately "unfair" splits in presented in Script B did not significant differences in the offers across the $10, $20, and $50 stake games.

In contrast, the more aggressive splits illustrated in Script C may have led proposers to offer less money and retain more for themselves. For all 3 stakes, the proposer offered less money (15% for $10; 18% for $20; and 20% less for $50. The differences for the $20 and $50 game approach traditional thresholds of statistical significance ($p < 0.10$) but are hardly definitive.

Given the small sample size and inconsistency of the result, there is little evidence that the script read to the proposer changed the offer made. Even in the case of script C, the effect on the amount of money the proposer chooses to retain is only weakly significant and only for the highest stakes. Thus, we had to rely on natural variation across proposers for variation in offers made to subjects rather than variation induced by nudges in proposer instructions.

Appendix Table 1a: Distribution of Game Parameters

Game Parameters Distribution

		Stakes	$10	$20	$50	Total
Intentional	Black Proposer		41	48	46	135
	White Proposer		45	39	47	131
Random	Black Proposer		47	41	46	134
	White Proposer		41	45	46	132
Total			174	173	185	532

Notes: This table presents the number of subjects for each treatment of the game. Subjects are divided by stakes ($10, $20, or $50), intentionality (playing against an individual vs. playing against a computer), and the race of the individual proposing.

Appendix Table 1b: Name Treatment Saliency

	Full Sample (1)	White Responders Only (2)	Black Responders Only (3)	Democrat Responders (4)	Republican Responders (5)
Age	-0.00**	-0.00	-0.01**	-0.00**	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female	-0.01	-0.02	0.00	0.02	0.02
	(0.04)	(0.06)	(0.06)	(0.06)	(0.08)
Married	0.00	0.05	-0.03	-0.03	0.09
	(0.04)	(0.06)	(0.06)	(0.06)	(0.08)
Republican	-0.02	-0.01	-0.12		
	(0.06)	(0.06)	(0.13)		
Unaffiliated	0.02	0.02	-0.06		
	(0.08)	(0.10)	(0.16)		
Completed College	-0.12*	0.06	-0.29***	-0.16*	-0.10
	(0.07)	(0.10)	(0.09)	(0.08)	(0.12)
Completed Graduate School	0.03	0.03	0.06	0.06	-0.04
	(0.07)	(0.10)	(0.11)	(0.10)	(0.14)
Attended Vocational/Technical	0.13	0.13	0.15	0.20*	0.06
	(0.09)	(0.11)	(0.13)	(0.12)	(0.12)
Black Proposer Name Implied	-0.23***	-0.24***	-0.22***	-0.12	-0.26***
	(0.06)	(0.06)	(0.06)	(0.10)	(0.08)
Black Responder	-0.13**			-0.15*	0.26***
	(0.06)			(0.08)	(0.07)
Black Proposer and Responder	0.02			-0.04	-0.48**
	(0.08)			(0.12)	(0.20)
Constant	1.02***	0.90***	1.15***	1.06***	0.57**
	(0.13)	(0.17)	(0.22)	(0.19)	(0.22)
N	532	260	272	325	152
Average Match Rate	0.60	0.67	0.53	0.56	0.67

Notes: This table estimates a linear probability model to in which the dependent variable is 1 if responders correctly identified the race of their matched proposer as implied by their name and 0 if not. Specifications are estimated using no controls (1); sex and age controls (2); and sex, age, party affiliation, and education controls (3-7). Columns (4)-(7) restrict the sample by respondent race and political affiliation. All standard errors are robust. *p < 0.10, **p < 0.05, ***p < 0.01

Appendix Table 1c: Anchoring Tests

	$10 Split	$20 Split	$50 Split
Script B	0.04	0.07	0.09
	(0.10)	(0.10)	(0.09)
Script C	0.15	0.18*	0.20*
	(0.12)	(0.11)	(0.11)
Black	0.14	0.20**	0.22**
	(0.09)	(0.09)	(0.09)
Female	-0.06	-0.09	-0.09
	(0.09)	(0.09)	(0.09)
Constant	0.41***	0.39***	0.39***
	(0.11)	(0.09)	(0.09)
N	27	27	27

Notes: This table estimates equation A1 to test whether a particular script affects the amount a proposer will offer out of $10, $20, or $50 stakes. All models are estimated using OLS and robust standard errors, controlling for the proposer's race and sex. *p < 0.10, **p < 0.05, ***p < 0.01

Appendix Table 1d: Average Acceptance Rate by Offers and Stakes

	Acceptance Rate	*N*
A. $10 Stakes		
Offer: $2	0.553***	85
Offer: $5	0.843***	89
B. $20 Stakes		
Offer: $5	0.580***	88
Offer: $10	0.929***	85
C. $50 Stakes		
Offer: $10	0.500***	94
Offer: $24	0.915***	47
Offer: $25	0.909***	44

Notes. This table details acceptance rates and number of observations by offers and stakes for the estimation sample in Table 3a. *p < 0.10, **p < 0.05, ***p < 0.01

Script for Treatment A: Intentional Division by Proposer

Hello, may I speak with [subject first name] [subject last name]?

I'm calling on behalf of researchers at the University of Notre Dame. You have been randomly selected to participate in a short research study. If you participate, you will need to answer three questions today. Then you will be entered in a drawing to win a $500 gift card from Amazon.com. You will also have an opportunity to make some money today.

Would you like to participate in the study? [If yes, proceed, if no, thank them for their time].

Great! Just so you know, your personal information and answers will be kept confidential, used only to mail your winnings, and will be discarded when the project is completed.

Our research study asks you to play a short game.

Q1. You have been selected at random to play the game with a person from the greater Chicago area. To maintain confidentiality, we cannot disclose this person's name. Instead, we use a very similar name.

The rules of the game are simple. [Partner first name] [Partner last name] was asked to propose a split of [Stakes] with you. If you accept the proposal, you will be paid that amount today and [Partner first name] [Partner last name] will keep the rest. If you reject the proposal, neither of you will be paid anything. Would you like to hear the rules again, or are they clear? [If yes, proceed, if no, repeat the rules].

Q2. [Partner first name] has proposed a split in which you receive [Offer] of [Stakes] and [Partner first name] receives [Stakes – Offer]. Do you accept or reject the offer?

[Indicate acceptance or rejection].

1 Accept

2 Reject

BOTH Accept and Reject count as a COMPLETE

Okay.

ASK EVERYONE REGARDLESS OF ACCEPT OR REJECT:

Q3. The final question is, what is the smallest amount you would have been willing to accept as a division of [stakes]?

[Enter amount]

Thank you for your participation. I'd like to remind you that you have also been entered in a drawing for a $500 Amazon gift card.

[IF OFFER WAS ACCEPTED, Q2=1] Can I confirm your name and the address where your payment should be sent?

[IF OFFER WAS REJECTED, Q2=2] Can I confirm your name and the address where the gift card will be sent if your name is drawn?

Script for Treatment B: Non-Intentional Division by Proposer

Hello, may I speak with [subject first name] [subject last name]?

I'm calling on behalf of researchers at the University of Notre Dame. You have been randomly selected to participate in a short research study. If you participate, you will need to answer three questions today. Then you will be entered in a drawing to win a $500 gift card from Amazon.com. You will also have an opportunity to make some money today.

Would you like to participate in the study? [If yes, proceed, if no, thank them for their time].

Great! Just so you know, your personal information and answers will be kept confidential, used only to mail your winnings, and will be discarded when the project is completed.

Our research study asks you to play a short game.

Q1. You have been selected at random to play the game with a person from the greater Chicago area. To maintain confidentiality, we cannot disclose this person's name. Instead, we use a very similar name.

The rules of the game are simple. The researchers have randomly selected a split of [Stakes]. You and [Partner first name] [Partner last name] have been randomly selected to receive the division of the money. If you accept the split randomly selected by the researchers, you and [Partner first name] [Partner last name] will be paid your share today. If you reject the split, neither of you will be paid anything. Would you like to hear the rules again, or are they clear? [If yes, proceed, if no, repeat the rules].

Q2. The randomly selected split would pay you [Offer] and [Partner first name] [Amount – Offer]. Do you accept or reject the split?

[Indicate acceptance or rejection].

1 Accept

2 Reject

BOTH Accept and Reject count as a COMPLETE

Okay.

ASK EVERYONE REGARDLESS OF ACCEPT OR REJECT:

Q3. The final question is, what is the smallest amount you would have been willing to accept as a division of [Stakes]?

[Enter amount]

Thank you for your participation. I'd like to remind you that you have also been entered in a drawing for a $500 Amazon gift card.

[IF OFFER WAS ACCEPTED, Q2=1] Can I confirm your name and the address where your payment should be sent?

[IF OFFER WAS REJECTED, Q2=2] Can I confirm your name and the address where the gift card will be sent if your name is drawn?

Partner Subject Script: SCRIPT A

Thank you for your interest in our study. We are working with researchers at Notre Dame and Northwestern University to study decision-making using a game called the ultimatum game.

Background on Experiment

In the ultimatum game, two people divide a sum of money. We call the first player the "Partner." Partners propose a division of the sum. We call the second player the "Responder." Responders decide whether to accept the proposed division. If the responder accepts, each player gets his or her share of the division. If the responder rejects, neither player is paid anything.

Do you understand the basic idea of the ultimatum game?

Our study interviews Partners and Responders separately.

Your role in the game will be that of Partner. We will ask you to divide several sums of money in different ways. We keep track of your choices, called "splits."

Responders participate in the study in the coming weeks. We will contact them by phone and ask if they are willing to accept one of the splits you chose. We will keep track of the responders' answers. Once our survey is complete, we will randomly choose one of your splits and pay you according to whether a responder accepted that offer. So, every split you propose has some chance of being the one that involves real money for you.

Do you understand how we will chose to pay you based on your choices in the ultimatum game?

We will not use your real name in communicating with responders. But, to make the survey realistic for responders who cannot be physically present, we will use fake versions of names of actual Partner subjects. We will construct fake names by mixing first and last names of real Partner subjects. For example, if one Partner subject is named Jennifer Johnson and one is named Ray Smith, then one fake name could be Jennifer Smith. No real names will be used. Responders will only be told that Partners are from "the Chicago area."

Your real name and responses will be kept confidential by the researchers. This means only the researchers on this project will ever have access to the information. Your name and all identifying information will be discarded as soon as the survey is complete and payments are made.

Do you understand the confidentiality procedures?

Your decision to participate today will help us with important social science research. However, if you decide not to participate, this will in no way affect your relationship with the University of Notre Dame or with any of the researchers working on the study.

Would you like to participate?

If yes, would you please review and sign the consent form?

Experimental Instructions

Here is a sheet with several amounts to divide into splits. Your name is not on the sheet. I've labeled it with a confidential identification number.

Before you decide on your splits, I am going to explain the game in a bit more detail.

In the ultimatum game, two people divide a sum of money. We call the first player the "Partner." Partners propose a division of the sum. We call the second player the "Responder." Responders decide whether to accept the proposed division. If the responder accepts, each player gets his or her proposed share of the division. If the responder rejects, neither player is paid anything.

Your role in the game will be that of "Partner." You can propose any split of the money, to the nearest one dollar.

For example, the sum to be divided might be $20. You might propose a split of exactly $10 for yourself and $10 for the responder. If the responder accepts, you both get $10. If the responder rejects, no one is paid anything.

Notice that if the responder cares about fairness and not just money, he or she may reject an offer that pays them very little while paying you a lot. Some research has found rejection of low offers to be a common occurrence in the ultimatum game. If the responder is likely to reject a low offer, then you can maximize your earnings from this experiment by making higher offers. This is because low offers will be rejected, paying you nothing.

You will be asked to propose splits of three amounts of money. Remember, <u>one</u> of these will be chosen for a "real money" round. This means that if one of the randomly selected responders surveyed later accepts your division, you will be paid the share you proposed.

I will give you a chance to make your decisions privately. When you are done, return the sheet to me.

Ultimatum Game Divisions

ID#_____

If you need a refresher on the ultimatum game rules, please ask your experimenter to read the instructions again.

Propose a division of $10. Amount for you: _____ Amount for responder: _____

Propose a division of $20. Amount for you: _____ Amount for responder: _____

Propose a division of $50. Amount for you: _____ Amount for responder: _____

Finally, have you taken any economics courses? (This does not disqualify you.) Yes No

APPENDIX 2. Assessing Generalizability and Reproducibility

Our experiment builds on earlier work on Griffin, Nickerson and Wozniak (2012); hereafter GNW. The GNW data used a similar sampling frame but surveyed respondents in three states (Georgia, North Carolina and South Carolina). They obtained a sample of 1647 respondents who played the ultimatum game following a design similar to ours. We therefore test whether we can reproduce the main results from that paper. This helps assess the robustness of the findings in both projects and follows the spirit of guidance on enhancing replicability in social science (National Academies 2019, Ch. 6). Because the sample in GNW is much larger and drawn from three US states instead of one, this exercise also informs us about the potential generalizability of our findings.

We first use our 2014 sample of Georgia respondents to re-estimate the equations from which two central findings in GNW were drawn. This allows us to assess whether findings from the larger, broader GNW sample are reproduced in our sample. These GNW findings were: (1) a large and significant impact of relative inequality on ultimatum game acceptance decisions for all respondents, and (2) greater sensitivity of Black respondents to the offer level.

We report results from this exercise in Appendix Table 2a. We first repeat the analysis in GNW's Table 4 using data from our 2014 sample. Results are reported in the first two columns of Table 2a. The second two columns show results from analogous equations estimated using the full GNW data from 2008. The impact of relative inequality on acceptance of a proposed split is qualitatively similar across the two surveys. Coefficients on relative inequality in the GNW sample center on -0.4. In our sample, these coefficients are larger, close to -1.0, but standard errors are large and the result is qualitatively similar. We conclude that our sample reproduces result (1) from GNW.

Turning to the interaction of Black respondent status with the offer level, a comparison of results from the 2014 and 2008 surveys show that this interaction is small and significant in the 2008 data but insignificant in our sample. There are several reasons that our context might fail to reproduce this result. Our 2014 experimental sample is drawn from only one of the three states surveyed in GNW and is approximately one-third the size. GNW (2011) only reported analysis using pooled data from all three states. If respondents from the state from which our sample is drawn (Georgia) differed substantively from those in the other two states (North and South Carolina), or if power is a concern for detecting a relationship between offer size and Black respondents, then a lack of reproducibility on finding (2) could be driven by sample size or composition differences.

To investigate this, we estimate the Table 2a specifications using the original GNW data separately for each state subsample. Results are reported in the three rightmost sets of columns in Table 2a. The interaction between Black respondent status and offer is not significant at conventional levels in any of the subsamples, which are similar in size to our current sample. We therefore conclude that sample size likely contributes to the fact that we do not reproduce the second main result from GNW in the current experiment.

Results in these columns also show that point estimates and statistical significance are similar across the three state subsamples in the GNW data. This gives us confidence that a sample drawn from any one of these three states, as our current sample is, will produce results that generalize to other US state populations.

We also repeated the model selection analysis from GNW's Appendix Table 1. This affirmed that aversion to relative inequality (the deviation of a division from an even split) drives acceptance decisions while absolute inequality (the absolute difference in allocations in a proposed split) does not. Results are available upon request.

Appendix Table 2a. Comparison of Estimates across Surveys and State Subsamples

	2014 GA		2008 All		2008 GA		2008 NC		2008 SC	
Offer	0.00	0.00	0.01*	-0.00	0.01*	0.01	0.01	0.00	-0.00	-0.01
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Relative	-1.21***	-1.18***	-0.45***	-0.39***	-0.47***	-0.44**	-0.54***	-0.48***	-0.33**	-0.26
	(0.15)	(0.21)	(0.08)	(0.09)	(0.16)	(0.19)	(0.12)	(0.14)	(0.15)	(0.17)
Black*O		-0.00		0.02***		0.02		0.02		0.02
		(0.00)		(0.01)		(0.01)		(0.01)		(0.01)
Black*R		-0.06		-0.13		-0.04		-0.10		-0.18
		(0.29)		(0.09)		(0.18)		(0.13)		(0.17)
N	532	532	1598	1598	437	437	662	662	499	499

Notes: Data sources are 2014 survey data described in text and GNW (2011) survey data and associated subsamples, as listed in column headings. Estimates reported are coefficients from linear probability models including the listed covariates plus demographic controls. Columns 3 through 10 use the same covariate set as GNW Table 4 specifications, which reported marginal effects from probit estimation. Robust standard errors in parentheses. *p < 0.10, **p < 0.05, ***p < 0.01

Appendix 3: Additional Tables

Appendix Table 3a: Survey and Comparison Sample Characteristics

	Experimental Sample			Comparison Sample	
	White	Black	Total	Total - GA	Total - US
	Mean	Mean	Mean	Mean	Mean
A. Demographic Indicators					
Female	0.49	0.69	0.59	0.52	0.52
Age	70.24	67.52	68.85	48.40	49.79
Income 10-14k	0.19	0.16	0.17	0.19	0.18
Income 15-19k	0.12	0.14	0.13	0.15	0.15
Income 20-24k	0.11	0.14	0.12	0.14	0.14
Income 25-29k	0.13	0.10	0.12	0.11	0.11
Income 30-34k	0.13	0.16	0.15	0.11	0.11
Income 35-39k	0.10	0.14	0.12	0.09	0.09
Income 40-49k	0.12	0.11	0.11	0.15	0.15
Income 50-54k	0.08	0.05	0.07	0.07	0.07
Married	0.60	0.43	0.51	0.49	0.48
Unmarried	0.34	0.45	0.40	0.51	0.52
Marst. not available	0.06	0.12	0.09	-	-
High school or less	0.48	0.51	0.50	0.45	0.44
Some college	0.12	0.13	0.12	0.33	0.34
Bachelor's or grad	0.16	0.12	0.14	0.22	0.22
Ed. not available	0.06	0.06	0.06	-	-
Democrat	0.36	0.85	0.61	0.43	0.39
Republican	0.54	0.04	0.29	0.50	0.46
Unaffiliated	0.10	0.11	0.10	0.08	0.15
B. Game Variables					
Stakes	27.08	27.24	27.16		
Offer	9.62	9.46	9.54		
Share	0.36	0.35	0.36		
Absolute	7.84	8.33	8.09		
Relative	0.14	0.15	0.14		
Acceptance Rate	0.72	0.72	0.72		
Reservation Offer	9.29	10.06	9.68		
Inconsistent	0.07	0.10	0.08		
Observations	260	272	532	ACS: 38,304	1,272,174
				ANES: 283	10,184

Notes: Table reports the unconditional means for sex, age, game variables (stakes, offer, share, acceptance rate, lowest acceptable offer (reservation offer), marital status, party affiliation, education, and income for White and Black respondents, respectively. The irrational acceptance rate is the percentage of respondents that take an offer that is lower than their reservation offer. The comparison columns present estimates from pooled 2012 and 2016 American National Election Study samples (political affiliation) and the 2014 ACS (all other demographic variables) for GA only and US samples. ACS accessed via IPUMS (Ruggles et al. 2022).

Appendix Table 3b: Models of Offer Acceptance, Logit Specification

	All Respondents	All Respondents	All Respondents	White Respondents	Black Respondents
Offer Amount	0.04	0.04	0.05	0.04	0.06
	(0.03)	(0.03)	(0.03)	(0.05)	(0.04)
Relative Inequality	-5.34***	-5.33***	-5.47***	-7.10***	-4.73**
	(1.25)	(1.25)	(1.29)	(1.98)	(1.84)
Intentional Offer	0.77	0.77	0.82	-0.49	2.18*
	(0.68)	(0.68)	(0.70)	(1.01)	(1.21)
OfferXIntentional	-0.05	-0.05	-0.06	-0.02	-0.10
	(0.04)	(0.04)	(0.04)	(0.07)	(0.06)
RelativeXIntentional	-2.56	-2.57	-2.88	1.25	-6.35*
	(1.99)	(1.99)	(2.06)	(2.91)	(3.55)
N	532	532	532	260	261
Average Acceptance	0.72	0.72	0.72	0.72	0.70

Notes: This table estimates equation 3 using a logit specification. Model (3) includes type controls to indicate race- and income-based strata, and (4)-(6) include the full set of demographic controls as in Table 2, column 4. The rightmost columns limit the sample to only White and only Black respondents, respectively. All standard errors are robust. *p < 0.10, **p < 0.05, ***p < 0.01

Appendix Table 3c: Offer Acceptance Results with Proposer Race

	All Respondents	White Respondents	Black Respondents
Offer Amount	0.01*	0.00	0.01
	(0.00)	(0.00)	(0.00)
Relative Inequality	-0.97***	-1.16***	-0.80**
	(0.21)	(0.28)	(0.31)
Black Proposer	0.08**	0.08	0.10*
	(0.04)	(0.05)	(0.05)
Intentional Offer	0.10	-0.06	0.24**
	(0.08)	(0.12)	(0.12)
OfferXIntentional	-0.01	-0.00	-0.01*
	(0.00)	(0.01)	(0.01)
RelativeXIntentional	-0.47	0.05	-0.81*
	(0.30)	(0.44)	(0.42)
R2	0.19	0.22	0.23
N	532	260	272
Average Acceptance	0.72	0.72	0.72

Notes: This table estimates equation 3 using a linear probability model, with the addition of a dummy variable for the race of the proposer. Columns (2) and (3) restrict the sample to only White and only Black respondents, respectively. All specifications use the full set of demographic controls as in Table 2, column 4. All standard errors are robust. *p < 0.10, ** p < 0.05, *** p < 0.01.

Appendix Table 3d: **Offer Acceptance Models, 50/50 Offers Only**

	All Respondents	All Respondents	All Respondents	All Respondents	White Respondents	Black Respondents
Offer Amount	0.00	0.00	0.00	0.00	-0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.02)
Intentional Offer	-0.00	0.03	0.03	0.02	-0.22*	-0.01
	(0.04)	(0.08)	(0.08)	(0.09)	(0.13)	(0.27)
OfferXIntentional		-0.00	-0.00	-0.00	0.01	0.02
		(0.01)	(0.01)	(0.01)	(0.01)	(0.03)
R2	0.00	0.01	0.02	0.06	0.17	0.21
N	218	218	218	218	131	87
Average Acceptance	0.89	0.89	0.89	0.89	0.89	0.89

Notes: This table estimates equation 3 using a linear probability model and a subsample restricted to only 50-50 offers. Type controls (column 3) and demographic controls (columns 4-6) are included as in Table 2. The rightmost columns limit the sample to only White and only Black respondents, respectively. All standard errors are robust. *p < 0.10, **p < 0.05, ***p < 0.01

Appendix Table 3e: Offer Acceptance by Respondent and Perceived Proposer Race

	White Respondents, Perceived White	White Respondents, Perceived Black	Black Respondents, Perceived White	Black Respondents, Perceived Black
Offer Amount	0.00	-0.00	0.01*	0.00
	(0.01)	(0.01)	(0.01)	(0.01)
Relative Inequality	-0.86**	-2.30***	-0.84**	-0.86
	(0.34)	(0.55)	(0.39)	(0.73)
Intentional Offer	-0.07	-0.35	0.29**	-0.11
	(0.15)	(0.27)	(0.14)	(0.33)
OfferXIntentional	-0.00	0.02	-0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.03)
RelativeXIntentional	-0.31	1.76*	-0.78	-0.15
	(0.54)	(0.89)	(0.51)	(1.16)
R2	0.22	0.43	0.26	0.31
N	185	75	202	70
Average Acceptance	0.72	0.72	0.70	0.76

Notes: This table replicates Appendix Table 3c, but identifies proposers based on whether they were *perceived* to be Black by the respondent. All specifications use the full set of demographic controls as in Table 2, column 4. All standard errors are robust. *p < 0.10, **p < 0.05, ***p < 0.01

Appendix Table 3f: **Offer Acceptance Models with Offer Fixed Effects**

	All Respondents	All Respondents	All Respondents	All Respondents	White Respondents	Black Respondents
Relative Inequality	-1.28***	-1.06***	-1.06***	-1.04***	-1.11***	-0.99***
	(0.16)	(0.22)	(0.22)	(0.22)	(0.30)	(0.33)
Intentional Offer	-0.03	0.09	0.09	0.10	-0.07	0.24**
	(0.04)	(0.08)	(0.08)	(0.08)	(0.13)	(0.11)
OfferXIntentional		-0.01	-0.01	-0.01	0.00	-0.01
		(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
RelativeXIntentional		-0.43	-0.43	-0.48	0.08	-0.84**
		(0.29)	(0.29)	(0.30)	(0.45)	(0.42)
R2	0.16	0.16	0.16	0.19	0.22	0.24
N	532	532	532	532	260	272
Average Acceptance	0.72	0.72	0.72	0.72	0.72	0.72

Notes: This table estimates equation 3 using a linear probability model. Type controls (column 3) and demographic controls (columns 4-6) are included as in Table 2. The rightmost columns limit the sample to only White and only Black respondents, respectively. All standard errors are robust and all models use offer fixed effects. *p < 0.10, **p < 0.05, ***p < 0.01