

What Determines Giving to Hurricane Katrina Victims? Experimental Evidence on Racial Group Loyalty[†]

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We investigate the role of racial group loyalty on generosity in a broadly representative sample of the US adult population. We use an audiovisual presentation to manipulate beliefs about the race, income, and worthiness of Hurricane Katrina victims. Respondents then decide how to divide \$100 between themselves and Katrina victims. We find no effects of victims' race on giving on average. However, respondents who report feeling close to their racial or ethnic group give substantially more when victims are of the same race, while respondents who do not feel close to their group give substantially less. (JEL D64, J15, Q54)

Many scholars argue that race and racial group loyalty are important determinants of decisions concerning redistribution. This argument is supported by evidence that racial attitudes and the racial composition of cities and states are associated with redistributive attitudes and outcomes. Furthermore, in laboratory experiments, racial and ethnic biases have been found in trust games.¹ On the other hand, several studies have failed to find the expected effects of race and racial group loyalty. For instance, there is no consistent evidence of racial discrimination in dictator games, and a recent experiment finds that respondents report a higher level

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[†] To comment on this article in the online discussion forum visit the articles page at <http://www.aeaweb.org/articles.php?doi=10.1257/app.1.2.64>.

¹ See Alberto Alesina, Reza Baqir, and William Easterly (1999); Alesina, Edward L. Glaeser, and Bruce Sacerdote (2001); and Luttmer (2001) on the effect of the racial composition of cities and states and Martin Gilens (1999); Woonjin Lee and John E. Roemer (2006); and Roemer, Lee, and Karen Van der Straeten (2007) on the effect of racial attitudes in redistributive politics. Daniel Hungerman (2008) finds that the charitable activity of all-white religious congregations decreases as the fraction of blacks in the community increases. See Chaim Fershtman and Uri Gneezy (2001); Catherine C. Eckel and Rick K. Wilson (2003); Jan Bouckaert and Geert Dhaene (2004); Justine Burns (2006); and Daniel Haile, Abdolkarim Sadrieh, and Harrie A. A. Verbon (2006) on racial or ethnic discrimination in trust games. Racial biases have also been documented in attitudes to Katrina victims (Melissa Harris-Lacewell, Kosuke Imai, and Tepei Yamamoto 2007; and Shanto Iyengar and Kyu S. Hahn 2007).

of support for government job training and placement assistance for unemployed blacks than for unemployed whites.^{2,3}

If objective racial group membership is not always a predictor of racial biases in behavior, might there be other easily measured concepts that are? There is a class of easily administered attitudinal measures of a concept known as explicit racism, including the widely used Modern Racism Scale (John B. McConahay, Betty B. Hardee, and Valerie Batts 1981). However, these measures are likely to be prone to social desirability biases. It is typically obvious that they ask about views concerning racial discrimination, so respondents may censor their answers so as not to appear racist. At the other extreme are measures of implicit racial attitudes, including the widely used Implicit Association Test (Anthony G. Greenwald, Debbie E. McGhee, and Jordan L. K. Schwartz 1998). There is much debate among psychologists about what concepts these two types of measures capture, how the measures relate to each other, and what behaviors they should predict.⁴ However, it seems clear that the measures of explicit racism are typically more prone to social desirability bias but are easier to administer than the measures of implicit racism. We examine a third measure: an unusually simple question about subjective closeness to one's racial or ethnic group, which we interpret as a measure of subjective racial identification. It is easy to administer and yet may be less prone to social desirability bias than measures of explicit racism because one can feel close to one's racial or ethnic group without feeling animosity toward other racial groups.

We investigate whether this simple measure is predictive of racial biases in giving behavior using a randomized experiment on giving to victims of Hurricane Katrina. Our experiment was administered by Knowledge Networks, a survey and marketing research firm that maintains a nationally representative panel of respondents who participate by computer or WebTV. The 1,343 respondents who participated in our experiment viewed an audiovisual presentation about Hurricane Katrina victims shown in their natural environments. We manipulate perceptions of the racial composition of victims by presenting respondents with photographs that were mostly of black victims in one treatment condition and mostly of white victims in the other.

² See Fershtman and Gneezy (2001) and Burns (2004) on dictator games and Devah Pager and Jeremy Freese (2006) on unemployment benefits.

³ There are also mixed findings from other economic settings, with many studies documenting racial biases and others finding none. See, for instance, Alicia H. Munnell et al. (1996); Joseph G. Altonji and Rebecca Blank (1999); David M. Cutler, Glaeser, and Jacob L. Vigdor (1999); Marianne Bertrand and Sendhil Mullainathan (2004); and Pager, Bruce Western, and Bart Bonikowski (2006) for evidence of discrimination in housing and labor markets. John A. List (2004) finds statistical discrimination in field experiments on sports card markets. In the political process, racial heterogeneity has been linked to riots (Denise DiPasquale and Glaeser, 1998), lower participation in social activities (Alesina and Eliana La Ferrara 2000) and lower levels of trust (Alesina and La Ferrara 2002). Individuals also prefer to form racially homogenous political jurisdictions (Alesina, Baqir, and Caroline Hoxby 2004). Steven Levitt (2004); Kate Antonovics, Peter Arcidiacono, and Randall Walsh (2005); and List (2006) find little evidence of racial discrimination in behavior on game shows.

⁴ Much of the debate concerns the extent to which implicit and explicit attitudes represent distinct and unrelated constructs. Some relatively recent evidence suggests that implicit and explicit ethnocentrism are distinct concepts but are more strongly correlated than previously thought (William A. Cunningham, John B. Nezlek, and Mahzarin Banaji 2004). Related research has shown that the strength of the statistical association between measures of implicit and explicit racism depends on how they are measured but appears to be stronger when researchers attempt to reduce social desirability biases in the explicit measures (Jason A. Nier 2005). Implicit Association Tests might also be better at predicting bias in decisions taken in a split second (as in NBA refereeing, see Joseph Price and Justin Wolfers 2007) than for more deliberative decisions.

To increase the malleability of respondents' perceptions about the characteristics of victims, our presentation is about victims in cities (Slidell, LA and Biloxi, MS) that were demographically different from New Orleans, were relatively small and unknown, and had less Katrina-related press coverage.⁵ Our design also includes a control condition in which the race of the victims in the pictures is obscured, so that we can control for effects of the backgrounds in the pictures. We also manipulate perceptions of the income and "moral worthiness" of the victims using the audio information in the presentation. We manipulate income by providing information about the city's income level relative to the national average in one condition and no income information in another. We manipulate perceived moral worthiness by varying information that may change respondents' perceptions of how industrious the victims are, and how individually responsible they are for their situation.

Our primary measure of generosity is the amount of money given to the chapter of Habitat for Humanity in the city described in the presentation. We give respondents a 10 percent chance of receiving \$100. Prior to learning the outcome, they are asked to decide how much, if any, of this \$100 they would like to donate to the local chapter of Habitat for Humanity.⁶ The amount of money given to Habitat provides a behavior-based measure of how much various types of donors care about various types of victims. An advantage of this approach is that it allows us to estimate how generosity measured with monetary incentives responds to several tightly controlled and independently manipulated factors. Furthermore, since our sample is representative of the US population, we measure the effects of racial group loyalty among average Americans rather than among a (self-) selected subpopulation.

A disadvantage of our experiment, however, is that the social context of giving differs from that of natural charitable giving. Among other things, respondents are aware that they are under study, which means we must be careful to minimize effects of respondents' tendencies to behave in socially desirable ways when under observation. They are also giving money they just received from the experimenters rather than money they earned, which might affect their generosity levels.⁷ Therefore, we focus on effects of treatment conditions relative to control conditions and infer little from the absolute magnitude of the amount given.

Our experiment yields two main findings. First, on average, the race of Hurricane Katrina victims does not significantly affect the amount given. Thus, we do not find evidence of a significant racial bias on average. Moreover, the amount given was generally insensitive to victims' characteristics except that respondents significantly increased their giving when victims were perceived to be living in a more economically disadvantaged city.

⁵ In the 2000 census, about 28 percent of the New Orleans population was white while the Slidell and Biloxi populations were, respectively, 83 percent and 71 percent white. The populations of New Orleans, Slidell, and Biloxi were about 485,000, 26,000, and 51,000, respectively.

⁶ It is critical to our design that the respondents make the donation decision *before* they learn whether their decision will be implemented. Thus, decisions are not hypothetical because for each respondent there exists a state of the world in which the decision is payoff relevant.

⁷ Respondents may have a stronger sense of entitlement to money they earn by working. Indeed, respondents who earn their endowment through work or by winning a contest tend to play more selfishly than those who are simply given money (Elizabeth Hoffman et al. 1994; E. Elisabet Rutström and Melonie B. Williams 2000; List and Todd L. Cherry 2008). See Levitt and List (2007) for a more general discussion of the external validity of laboratory experiments.

Second, while objective own race is not a significant predictor of racial bias, the simple question, “How close do you feel to your ethnic or racial group?” is a strong predictor of racial bias. Whites who identify with their racial group bias their giving against blacks, while whites who do not identify with their racial group bias their giving in favor of blacks. Similarly, blacks who identify with their racial group bias their giving in favor of blacks, while blacks who do not identify with their racial group bias their giving in favor of whites. This result suggests that subjective identification with one’s racial group is an important determinant of giving, and that objective race, by itself, is not as good a predictor of racial group loyalty.⁸

I. Experimental Design

We contracted with Knowledge Networks to administer our experiment and survey instrument to a sample of their respondents. Knowledge Networks maintains a panel of respondents that it recruits through random-digit dialing. These respondents agree to take a 15–20 minute survey once a week via the Internet using a PC or WebTV in exchange for free Internet and WebTV access. In addition, the panelists often receive incentive payments and rewards through a loyalty program. Knowledge Networks collects basic demographic characteristics for all its panelists, and its panelists are roughly representative of the adult US population according to these characteristics. In addition to demographic characteristics, Knowledge Networks already collects certain other variables (such as some racial attitudes), so we did not need to collect this information as part of our survey instrument.

Respondents participated in one of three variants of our survey instrument, which we describe in detail below. See Web Appendix A, available at <http://www.aeaweb.org/articles.php?doi=10.1257/app.1.2.64>, for the complete wording. In Section ID, we describe how the race-salient and full-stakes variants of the instrument differ from the main instrument.

A. Experimental Manipulations

The instrument consists of four parts. Part I experimentally manipulates the perceived race, income, and worthiness of Hurricane Katrina victims using a brief audiovisual presentation about a small city (Slidell, LA or Biloxi, MS) that was hit by Katrina. The presentation consists of a slide show with eight photos of people after the hurricane accompanied by an audio story about the city’s residents and Habitat for Humanity. Many photos showed devastation caused by Katrina such as extensive flooding or demolished housing. Others showed residents receiving in-kind aid.

⁸ Several other authors have argued that racial discrimination depends on subjective racial identification and that racial loyalties can vary over time and across social situations. See, for instance, Glaeser (2005) and Robert Kurzban, John Tooby, and Leda Cosmides (2001). Our findings are also consistent with findings from a recent study that manipulated perceptions of the race of portrayed Hurricane Katrina victims and then surveyed respondents on their inferences about different types of emotions felt by the Katrina victims as well as their intentions to help the victims. Objective racial group membership had no significant effect on hypothetical willingness to help. However, respondents attributed higher levels of “uniquely human” emotions to racial ingroup members than racial outgroup members and these attributions predicted willingness to help (Amy J. C. Cuddy, Mindi S. Rock, and Michael I. Norton 2007).

We manipulate perceptions of the racial composition of Hurricane Katrina victims by using photos that mostly show white residents in one treatment condition and mostly show black residents in the other.⁹ Across the black and the white picture manipulations, we match the gender, age, and number of people shown, as well as the background and the emotional connotation of the photos as closely as possible. We reduce the resolution of the people in the photos so that their race shows through but their attractiveness and other features are obscured, and refer to these photos as our race-shown treatment conditions. We are primarily interested in estimating the difference in giving in these black and white race-shown conditions.

Because we use real photographs, the backgrounds shown in the photos vary with the race of the victims. To control for this, we create a condition that obscures the race of the people in the photos by filling in their images with blue coloring so they appear as solid blue shapes. We refer to these photos as the race-obscured control conditions. Figure 1 shows examples of the four types of photos used. When analyzing average giving, we can control for the backgrounds in the photos by subtracting the difference in giving in the black and white race-obscured conditions from the difference in giving in the black and white race-shown conditions. Alternatively, we perform conceptually the same estimation in a regression framework, allowing us to control for other experimental manipulations and for respondent characteristics.

We vary the audio information going with the pictures along eight characteristics that we judged to be likely determinants of generosity and plausibly correlated in the public's mind with the racial composition of the city.

These audio manipulations are:

- Whether the city is economically disadvantaged,
- Whether Republicans have a majority in the city,
- Whether many city residents attend church,
- Whether the city has been troubled by crime,
- Whether many city residents helped other victims,
- Whether many city residents received government benefits before Hurricane Katrina hit (as opposed to working),
- Whether recipients had to contribute labor to their home from Habitat for Humanity, and
- Whether many residents prepared for hurricanes.

In addition, we varied the audio along a ninth dimension: Whether or not concerns about looting in the city were mentioned in the audio text. We did this to see whether mentioning a charged topic such as looting would bring out racial biases in giving (it did not).

We took care never to provide incorrect information. Instead, by selectively providing or omitting certain information, we tried to influence respondents' perceptions of the city and of Hurricane Katrina victims who receive housing from Habitat for Humanity in that city. Web Appendix A spells out the exact variations in the

⁹ We did not use pictures of exclusively one race in order to reduce the chance that respondents would infer that our study is about race. Of the eight pictures, six pictures show Katrina victims of the race corresponding to the manipulation, but the third picture shows a Katrina victim of the other race, and the sixth picture shows both black and white Katrina victims.



FIGURE 1. THE PICTURE MANIPULATIONS

Source: David Peterson

audio text that correspond to these nine audio manipulations. In total, the audiovisual presentation contains 12 randomly assigned experimental manipulations: 2 picture manipulations (race and whether race was shown or obscured), 9 audio manipulations, and which city was shown. Details on the randomization procedure are provided in Web Appendix B.

B. Outcome Variables

Our outcome variables consist of four measures of generosity to Hurricane Katrina victims and a set of questions designed to test whether or not our experimental manipulations worked. We summarize the generosity measures first followed by the manipulation check measures.

Our primary measure of generosity is the amount of money that respondents give during the experiment to help Katrina victims. We ask the respondents how they would like to split \$100 between themselves and a charity that benefits Katrina

victims in the city they saw in the presentation. The charity is the local chapter of Habitat for Humanity in the city in question. We implement the decision for 10 percent of the respondents. To credibly convey that each respondent has a 10 percent chance of getting his or her decision implemented, we assign each respondent a random number between zero and nine, and tell respondents that their decision will be implemented if their number is equal to the first digit of the *Pick 3* game of the Louisiana State Lottery on a specified future date. We also tell them that if their number equals the lottery number, Habitat for Humanity will send them a note acknowledging how much they gave.

Next, we measure hypothetical giving by asking: "Suppose that you had not just given [the amount given] to Habitat for Humanity. Instead, suppose that Habitat for Humanity in [city] had mailed a letter to your home describing the effects of Katrina on [city] and had asked you for a donation. How much, if anything, would you have given?" The external validity of this measure may be greater because of the natural social context in which the question is asked, but it has the drawback of measuring hypothetical rather than actual behavior. See part II of our survey (in the Web Appendix) for the exact wording of our actual and hypothetical giving measures.

We also collect measures of attitudinal support for private and public transfers to Hurricane Katrina victims in the city that was featured in the presentation. We ask respondents, on a 7-point scale, whether they think charities should spend more or less on Katrina victims in the city, and whether they think the government should spend more or less on Katrina victims in the city. See part IV of the survey for the exact wording of these questions.

To test whether each of our experimental manipulations produced changes in the corresponding perceptions, we ask respondents about their perceptions of a number of characteristics of Hurricane Katrina victims who receive housing from Habitat for Humanity in that city. We ask most of these perceptions questions in part III of the instrument. However, to avoid biasing responses to attitudinal questions in part IV, we ask about perceptions of the racial composition of the relevant city's residents and the city's Habitat for Humanity recipients at the end of the survey.

C. Measures of Racial Attitudes and Other Respondent Characteristics

We have three measures of racial attitudes. The first is subjective racial identification, which is the answer to the question: "How close do you feel to your ethnic or racial group? Very close, close, not very close, not close at all." This measure has the advantage of having been asked by Knowledge Networks prior to our experiment. It is thus uncontaminated by information presented and decisions made in our experiment. It also seems likely that this measure is less prone to social desirability bias than measures of explicit racism. Causality between subjective racial identification and racially biased behavior can run in either direction. Identifying with blacks might cause people to discriminate less against them. Or, discriminating against blacks for some other reason might reduce subjective identification with them.

The second measure is the frequency of social contact with blacks minus the frequency of social contact with whites. This measure was taken at the end of our survey and thus may be contaminated by information presented and decisions made

in our experiment (see part IV of our survey for exact wording). The expected effect of social contact on racial discrimination is ambiguous. People who are sympathetic to blacks may seek out more social contact with them and discriminate less against them. Alternatively, people may be put into social contact with blacks for exogenous reasons, and this may increase or decrease positive feelings or behavior toward them depending on the nature of the interactions.

The third measure is beliefs about the prevalence of economic opportunities for blacks compared to those for whites (see part IV of our survey for exact wording). This measure was also taken at the end of our survey and may be contaminated by our experiment. Of our three measures, this one is the most similar to the types of questions that are found in measures of explicit racism. It also may be the most susceptible to social desirability bias because respondents are reporting beliefs about a characteristic of blacks that may seem negative and thus socially undesirable to admit.

The respondent characteristics that we collect as control variables consist of prior charitable giving and prior giving to Hurricane Katrina victims. The remaining respondent characteristics that we control for were collected by Knowledge Networks prior to our study.

D. Race-Salient and Full-Stakes Instruments

While 80 percent of the respondents took the main instrument, the rest instead took either a race-salient or a full-stakes variant. Both variants are like the main instrument except in the ways described below. Because we estimate the effect of our race manipulation in these alternative instruments relative to that in the main instrument, all pictures in the alternative instruments showed the race of Hurricane Katrina victims. We administered the variants only to non-black respondents.

We administered the race-salient variant to investigate the concern that respondents who are more aware that the study is about race may be more likely to censor their behavior and discriminate less against blacks. We tried not to make it obvious to respondents in our main instrument that our study was about race, to the extent possible, given media coverage that linked Katrina to race relations. We increased the salience of race in our race-salient instrument by altering our main instrument in two ways. First, in the opening screen, we told respondents that they were participating in a study on “Hurricane Katrina, race relations, and whether the race of Katrina victims mattered for how America responded to Katrina.” To drive this point home, we moved our questions about race perceptions from the end of the instrument to immediately after the slide presentation and before they chose how much to give.

We conducted the full-stakes version of our instrument with the goal of increasing the reliability and validity of our measure of giving. Rather than having a 10 percent chance of having their giving decision implemented, respondents receiving the full-stakes variant had their decision implemented for sure. In order to make the \$100 more “real” in the minds of the respondents, we gave them the \$100 at the beginning of the instrument, before the slide show. After the slide show, we told them they could give away part of their \$100 to Habitat for Humanity to help Hurricane Katrina victims.

II. Results

We fielded our experiment from June 6–19, 2006 and received 1,530 completed surveys.¹⁰ However, 182 respondents reported that they could not hear the audio component of the slide show. We did not administer the giving and perceptions parts of the survey to these respondents and do not use their data in this paper. An additional five respondents failed to report a decision on how much money to give, so we dropped these observations, leaving a usable sample of 1,343 respondents. The main instrument was completed by 1,101 respondents, of which 247 are African American. The race-salient and full-stakes variants were completed by 118 and 124 non-black respondents, respectively. The median completion time was 22 minutes.

The respondents of the main instrument are roughly nationally representative except for an intentional oversampling of black respondents.¹¹ We weight our results to correct for this oversampling. We compared the means of the demographic variables in our data to the means for the same variables in the Current Population Survey and did not find substantial differences (unreported). Among other things, this implies that the demographic means of Knowledge Networks' nonrespondents must also have been similar to the CPS demographic means. Finally, since the first screen of the race-salient variant of the instrument differed from that of the main and full-stakes variants, we note that the nonresponse rates were similar across all three variants.

Table 1 presents selected summary statistics (see Web Appendix Table A1 for the full summary statistics). On average, respondents gave \$65 to Habitat for Humanity, with 44 percent of respondents giving the full \$100, 20 percent giving \$50, and 9 percent giving nothing.¹² Hypothetical giving is notably lower, averaging about \$20. Respondents' subjective support for government spending to help Katrina victims averages 5 on a 7-point scale, and the figure is similar for support for charity spending. The bottom panel presents the three measures of racial attitudes, which we collapse into dummy variables so that about half of the respondents in the overall sample fall in each category. On average, 63 percent of respondents report feeling close or very close to their ethnic or racial group. However, there is a large racial difference in the response to this question, with 90 percent of blacks and only 57

¹⁰ Knowledge Networks invited a total of 2,608 panelists to take the survey. The response rate was 65 percent, with 1,700 respondents opening the survey. The completion rate was 90 percent, yielding 1,530 completed surveys. Completion of the survey does not appear to depend on our experimental manipulations. The hypothesis that our experimental manipulations had no effect on completion of the survey cannot be rejected (p -value = 0.27). We note that the response rate for non-blacks was roughly 75 percent, which is a typical response rate for Knowledge Networks studies, but the response rate for blacks was lower than usual for Knowledge Networks. This occurred because they sent out a large number of invitations to blacks in the last few days of the fielding period in order to achieve the promised number of completed surveys, resulting in less time for these invitees to respond and a low response rate.

¹¹ National representativeness is important because of growing concerns and recent evidence that giving in experiments using college students as subjects misrepresents giving in the broader population. See, e.g., Jeffrey Carpenter, Cristina Connolly, and Caitlin Myers (2007).

¹² This level of giving is quite high compared to average offers in standard laboratory dictator games, which are often around 20 percent of the stakes (Colin F. Camerer 2003), but it is consistent with the finding that offers in dictator games were three times higher to the American Red Cross than to anonymous recipients (Catherine C. Eckel and Philip J. Grossman 1996). Furthermore, W. Kip Viscusi and Richard J. Zeckhauser (2006) present attitudinal data that show a great deal of support for governmental aid to disaster victims.

TABLE 1—SELECTED SUMMARY STATISTICS

| | All respondents | | White respondents | | Black respondents | |
|--|---------------------------------|----------|---------------------------------|----------|---------------------------------|----------|
| | Mean (standard deviation) | <i>N</i> | Mean (standard deviation) | <i>N</i> | Mean (standard deviation) | <i>N</i> |
| <i>Outcome variables</i> | | | | | | |
| Giving to Habitat to help Katrina victims in city (\$ out of \$100) | 65.0 (36.7) | 1,343 | 67.2 (36.8) | 915 | 54.8 (33.9) | 247 |
| Hypothetical giving to Habitat to help Katrina victims in city (topcoded at \$500) | 20.1 (38.9) | 1,341 | 17.5 (34.0) | 913 | 30.2 (53.1) | 247 |
| Subjective support for government spending to help Katrina victims in city (1–7 scale) | 4.9 (1.4) | 1,337 | 4.7 (1.4) | 913 | 5.5 (1.5) | 245 |
| Subjective support for charity spending to help Katrina victims in city (1–7 scale) | 4.9 (1.2) | 1,333 | 4.8 (1.1) | 907 | 5.2 (1.4) | 246 |
| <i>Racial attitude variables</i> | | | | | | |
| Very close or close to own ethnic or racial group | 0.63 (0.48) | 1,126 | 0.57 (0.50) | 749 | 0.90 (0.29) | 219 |
| Equal or more social contact with blacks than with whites | 0.48 (0.50) | 1,328 | 0.38 (0.49) | 903 | 0.97 (0.17) | 245 |
| Blacks have the same or more economic opportunities than other Americans | 0.61 (0.49) | 1,331 | 0.69 (0.46) | 908 | 0.17 (0.38) | 242 |

Note: Sample has been weighted to adjust for oversampling of black respondents.

percent of whites reporting feeling close or very close to their own group. There are also large racial differences for the other two measures. Not surprisingly, social contact with blacks is much higher for black respondents than for whites. Finally, black respondents are much less likely than white respondents to believe that blacks have the same or more economic opportunities compared to other Americans.

In Table 2, we present mean offers in four subsamples defined by crossing the race of the victims in the pictures with whether race was shown or obscured. Here, we use unweighted data from the main instrument. The first column presents mean offers in response to pictures with black and white victims, respectively, in the race-shown treatment condition. Respondents who saw pictures showing black victims gave, on average, \$66.3 to the local Habitat for Humanity chapter, while those who saw pictures showing white victims gave on average \$64.7. Thus, in the race-shown condition, respondents gave about \$1.6 more in response to pictures of black Hurricane Katrina victims, but this difference is not statistically significant. The second column presents mean offers to pictures with black and white victims, respectively, in the race-obscured control condition. In this column, respondents gave \$1.7 more in response to race-obscured photos of black victims. This difference, while not statistically significant, picks up any effect of different backgrounds in the pictures of black victims relative to those of white victims. Subtracting the effect of the backgrounds in the race-obscured condition from the combined effect of race and backgrounds in the race-shown condition yields the estimate of the effect of victim race on giving, $-\$0.1$, which is not statistically significant.

Table 2 suggests that, on average, victim race has little effect on giving. Why might this be the case? One possibility is that the race manipulation failed to change

TABLE 2—MEAN GIVING OUT OF \$100 TO HABITAT FOR HUMANITY TO HELP KATRINA VICTIMS

| | Pictures show race | Pictures obscure race | Difference |
|-----------------------------|--------------------|-----------------------|------------|
| Pictures with black victims | 66.3 (2.2) | 65.6 (2.2) | 0.7 (3.1) |
| <i>N</i> | 280 | 273 | |
| Pictures with white victims | 64.7 (2.2) | 63.9 (2.2) | 0.8 (3.1) |
| <i>N</i> | 280 | 268 | |
| Difference | 1.6 (3.1) | 1.7 (3.1) | -0.1 (4.4) |

Notes: $N = 1101$. Main instrument only. The outcome variable is the dollar amount that the respondent chose to give to Katrina victims via Habitat for Humanity in the city in question. Standard errors are in parentheses. The number of observations is below. Means are not weighted.

perceptions of the racial composition of the victims. In Section IIA, we show that this is not the case. We also show (in Section IID) that the insignificant effect of race on giving is robust, persisting in a variety of samples and specifications. Most important, Section IIE shows that the race manipulation does have a significant effect on giving once we account for subjective racial identity, which provides further evidence that the race manipulation was strong enough to affect behavior.

A. Manipulation Check: Effects of Experimental Manipulations on Perception

Column 1 of Table 3 presents a regression of perceptions of the racial composition of victims on the picture manipulations, audio manipulations, dummies for the variants of the survey instrument, and demographic controls. We measure perceptions of the racial composition by the perceived percentage of Habitat for Humanity recipients in the city in question that are black minus the perceived percentage that are white. The variable *Pictures show black victims* is a dummy variable that equals one only for pictures with black victims in the race-shown treatment condition. The controls for picture backgrounds consist of a dummy variable for the race-obscured condition and a dummy variable for pictures with black victims (whether race was shown or obscured). The coefficient on *Pictures show black victims* therefore measures the causal effect of seeing black victims rather than white victims, controlling for any effect due to differences in picture backgrounds. We now weight observations to correct for the oversampling of black respondents. In order to maximize precision, we also include observations from the race-salient and full-stakes variants, controlling for their main effects on the outcome variable by including dummies for each of these alternative instruments. We show elsewhere that the race effect in these variants is not statistically different from that in the main instrument, so we feel comfortable pooling the main, race-salient, and full-stakes samples (Fong and Luttmer 2007).

The first row of column 1 shows that the black race manipulation increases the perceived fraction of recipients who are black minus the perceived fraction who are white by 16.3 percentage points. This effect is significant at the 1 percent level. The remaining rows show the effects of the audio manipulations. The audio manipulation

TABLE 3—EFFECTS ON PERCEIVED RACE OF KATRINA VICTIMS AND ON GIVING TO KATRINA VICTIMS

| | Perceived percent black – perceived percent white (1) | Giving out of \$100 to Habitat for Humanity to help Katrina victims in city | | | | |
|--|--|--|-------------------|-------------------|-------------------|--------------------|
| | | (2) | (3) | (4) | (5) | (6) |
| <i>Picture manipulations</i> | | | | | | |
| Pictures show black victims | 16.3*** (4.0) | -2.2 (3.8) | -3.8 (4.7) | -17.1** (6.7) | -16.7** (6.9) | -71.5*** (26.9) |
| Pictures show black victims × black respondent | | | 9.6 (9.6) | | | |
| Pictures show black victims × subjective identification with blacks | | | | 30.0*** (9.2) | 29.8*** (10.5) | 87.0*** (29.2) |
| Controls for other picture features | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>Other experimental manipulations</i> | | | | | | |
| Republicans have majority in city | -6.9*** (1.9) | 0.6 (1.9) | -0.5 (2.1) | -1.9 (2.2) | -2.4 (2.5) | 2.1 (4.3) |
| City is economically disadvantaged | 4.9** (2.0) | 4.2** (1.9) | 5.0** (2.0) | 4.7** (2.2) | 6.0** (2.5) | -0.3 (4.3) |
| Many in city received government benefits | 2.1 (2.0) | -1.2 (1.9) | -1.4 (2.0) | -0.3 (2.2) | -0.2 (2.5) | -2.1 (4.3) |
| Many city residents prepared for hurricanes | -0.6 (2.0) | 1.1 (1.9) | 2.3 (2.1) | 3.3 (2.3) | 3.9 (2.6) | 2.7 (4.9) |
| Many city residents attend church | 2.7 (1.9) | -2.6 (1.9) | -0.1 (2.1) | 0.8 (2.2) | 0.9 (2.5) | 4.3 (4.6) |
| City has been troubled by crime | 3.1 (1.9) | -0.2 (1.9) | -0.1 (2.1) | 1.0 (2.3) | 2.0 (2.6) | 0.5 (4.3) |
| Many city residents helped other victims | -0.1 (2.0) | 2.0 (1.9) | 1.8 (2.1) | 1.0 (2.2) | 0.1 (2.5) | 5.7 (4.3) |
| Habitat recipients must contribute labor to house | -2.0 (2.0) | -0.7 (1.9) | 0.3 (2.0) | -0.5 (2.2) | -1.3 (2.5) | 0.5 (4.3) |
| There were concerns about looting | 3.3 [†] (1.9) | -1.5 (1.9) | -2.6 (2.1) | -1.5 (2.2) | -1.9 (2.6) | -0.1 (4.3) |
| Slidell, LA featured in presentation | -3.3 (2.7) | 3.3 (2.6) | 4.5 (2.9) | 3.8 (3.1) | 4.7 (3.5) | 2.9 (6.8) |
| Full-stakes survey variant | -1.5 (3.5) | -14.8*** (3.6) | -16.0*** (4.0) | -13.9*** (4.2) | -14.3*** (4.4) | |
| Race-salient survey variant | -18.0*** (3.9) | -3.1 (3.4) | -2.9 (3.8) | -0.3 (4.2) | -0.1 (4.3) | |
| <i>Respondent racial identity</i> | | | | | | |
| Non-Hispanic black | -19.9*** (3.1) | -10.6*** (2.6) | -5.7 (5.1) | -9.9*** (3.1) | | |
| Other race/ethnicity | -0.8 (3.0) | -0.6 (2.7) | | | | |
| Subjective identification with blacks | | | | -2.1 (4.1) | -3.1 (4.7) | -9.1 (11.5) |
| <i>Other demographic control variables</i> | | | | | | |
| Sample (race of the respondents) | All | All | Blk/Wht | Blk/Wht | White | Black |
| R ² | 0.143 | 0.177 | 0.170 | 0.180 | 0.184 | 0.306 |
| N | 1,321 | 1,343 | 1,162 | 968 | 749 | 219 |

Notes: Numbers shown are OLS coefficients (robust standard errors in parentheses). The dependent variable in column 1 is the perceived percent of the city's Habitat for Humanity recipients who are black minus the perceived percent who are white. The dependent variable in columns 2–6 is giving out of \$100 to the city's Habitat for Humanity chapter. Controls for picture features are the dummy variables "Race obscured" and "Pictures with black victims" and the interaction of these two dummies with "Black respondent" (in column 3) or with "Subjective identification with blacks" (in columns 4–6). Other demographic controls consist of age, age², log household income, log giving to charity in 2005, log prior giving to Katrina relief, and dummies for high school dropout, some college, college or more, dual-income family, married, male, single male, living in the south, employed, disabled, retired, any giving to charity in 2005, and any prior giving to Katrina relief. Regressions are weighted to adjust for oversampling of black respondents.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

stating that Republicans have a majority in the city decreases the perceived fraction of recipients who are black minus the perceived fraction who are white by 7 percentage points (significant at the 1 percent level). This result makes sense if respondents are Bayesian updaters, since blacks are less likely to be Republican. Similarly, when the audio manipulation suggests the city is relatively economically disadvantaged, the perceived fraction black minus the perceived fraction white increases significantly. The effects of the other audio manipulations are smaller and insignificant at the 5 percent level but, by and large, move the perceived racial composition in a fashion that is consistent with Bayesian updating. Finally, in the race-salient variant of the instrument, respondents estimate that fewer victims are black, which is what would be expected if respondents pay closer attention to the race of the people shown in the pictures.

In Web Appendix Table A2, we present the effects of the picture and audio manipulations on respondents' perceptions of nine other characteristics of the Habitat for Humanity recipients or the city they live in. In the large majority of cases, the audio manipulation changes the corresponding perception in the expected direction and is statistically significant at the 5 percent level or better. For example, saying that the city is relatively economically advantaged raises the perceived median household income of Habitat for Humanity recipients by about \$6,800 per year in the full sample.

B. Effects of Race Manipulation and Racial Group Loyalty on Giving

Column 2 of Table 3 presents a regression predicting giving to Hurricane Katrina victims using the weighted and pooled observations from the main, race-salient, and full-stakes samples. As before, respondents do not significantly change the amount they give in response to seeing pictures in which blacks are shown. The point estimate is $-\$2.2$ or about 6 percent of a standard deviation of the amounts given, which suggests that there is little effect of victims' race on giving. However, the 95 percent confidence interval on this estimate ranges from about $-\$10$ to $\$5$ (or between -25 percent to 15 percent of a standard deviation), so we cannot rule out a moderately large racial bias in giving in the overall sample. This establishes our first main result, namely that we find no evidence that Americans, on average, give more or less depending on the race of Hurricane Katrina victims. However, this average result may mask reactions in opposite directions by subgroups of the population. Columns 3 and 4 test whether reactions to our race manipulation differ by objective and subjective racial identity.

In column 3, we estimate objective racial group loyalty by testing whether the effect of the race manipulation on giving differs by the race of the respondent. In this column, we use the sample of non-Hispanic black and non-Hispanic white respondents.¹³ We find that blacks give about $\$9.6$ more in response to pictures showing blacks than white respondents do, but this estimate is not statistically significant. In unreported analyses, we also tested for group loyalty along dimensions other than race, such as religiosity and political identification, and found no evidence of it (results available upon request).

¹³ From now on we will refer to non-Hispanic white respondents and non-Hispanic black respondents simply as white and black respondents, respectively.

In column 4, we test whether the effect of the race manipulation differs by subjective racial identity. In this column, for compactness of presentation, we use a simple coding of the measure of subjective racial identity. Respondents are coded as subjectively identifying with blacks if they are black and report feeling “close” or “very close” to their racial or ethnic group, or if they are white and report feeling “not very close” or “not close at all” to their racial or ethnic group.

The others—namely blacks who feel “not very close” or “not close at all” to their group and whites who feel “close” or “very close” to their group—are coded as not subjectively identifying with blacks. We will report additional results on subjective racial identity in Table 6.

Respondents who do not subjectively identify with blacks give \$17 less after seeing pictures showing black victims rather than pictures showing white victims. This effect is significant at the 5 percent level. Respondents who subjectively identify with blacks react to the race manipulation significantly differently from those who do not, giving \$30 more in reaction to pictures showing black victims compared to those who do not identify with blacks (significant at the 1 percent level). This means that, overall, respondents who subjectively identify with blacks give \$13 more in response to pictures showing black victims than in response to pictures showing white victims (significant at the 5 percent level).

Columns 5 and 6 show the same regression as in column 4 but separately for white and black respondents. We find that, within each group of respondents, giving in response to seeing pictures of black victims is significantly higher when the respondent subjectively identifies with blacks. We note, however, that only 10 percent of black respondents do not subjectively identify with blacks. Thus, it should be kept in mind that only a small fraction of black respondents drive the effect of subjective identification on giving in the regression in column 6.

Columns 3–6 establish our second main result. The effect of the black picture manipulation on giving does not differ significantly by the objective race of the respondent, but the respondent’s subjective identification with blacks has a large impact on the response to pictures showing black Hurricane Katrina victims. Thus, while we do not find significant evidence of objective racial group loyalty, we find strong evidence of what we call subjective racial group loyalty—those reporting not feeling close to blacks biasing their giving against blacks and those reporting feeling close to blacks biasing their giving in favor of blacks. Moreover, we find that subjective racial group loyalty affects giving both among black respondents and among white respondents.

C. Effects of Other Experimental Manipulations on Giving

The effects of the audio manipulations are given by the coefficients on the dummy variables for these manipulations. In columns 2–4 of Table 3, none of the audio manipulations have significant effects, except for the manipulation of the economic situation of the city. Respondents give roughly \$4 to \$5 more when told that the city was relatively economically disadvantaged. This effect is significant at the 5 percent level. Perhaps surprisingly, the manipulations intended to affect perceptions of worthiness, such as whether victims helped others in need or whether victims

took reasonable precautions against hurricanes, do not have statistically significant effects on giving. Finally, we find that the full-stakes variant leads to significantly lower giving.

One might wonder if the lack of treatment effects on giving to Habitat for Humanity in our race and worthiness manipulations might be due to noise in our outcome measure. However, the findings that subjective identification with blacks and our income manipulation have significant effects on giving increases our confidence that giving in our experiment measures something other than pure noise. Furthermore, in unreported results, we find that a history of charitable giving significantly increases giving during the experiment, which gives us additional confidence that our outcome measure corresponds to generosity in the real world.¹⁴

D. Effects of Treatments on Other Measures of Generosity

Table 4 examines whether the findings from columns 2 and 3 in Table 3 carry over when we use alternative measures of generosity and when we use only white or only black respondents. (The findings from columns 4–6 will be analyzed further in Table 6). In particular, Table 4 examines the generalizability of the findings that there is no significant average effect of victim race on giving, there is no significant objective racial group loyalty in giving, that giving is higher for economically disadvantaged victims, and that manipulations affecting perceptions of worthiness have no effect on giving.

Each row in Table 4 presents results from a single regression. The measure of generosity in Panels A–D are actual giving in the experiment, hypothetical giving to Habitat for Humanity in the city, subjective support for charitable giving to help Hurricane Katrina victims in the city in question, and subjective support for government spending to help Hurricane Katrina victims in the city, respectively. Within each panel, there is a regression for the whole sample, the sample of white respondents, and the sample of black respondents.

The columns present the estimated effects of the race manipulation, the income manipulation, and the degree to which the respondent was manipulated to perceive the victims as “morally worthy,” respectively. This worthiness variable was constructed by adding the dummies for the audio manipulations intended to increase perceived worthiness (“many city residents helped other victims,” “many city residents prepared for hurricanes,” and “Habitat for Humanity recipients must contribute labor to house”) and subtracting the dummy for the audio manipulation intended to decrease perceived worthiness (“the city has been troubled by crime”).¹⁵ It is worth

¹⁴ In addition, we investigated the external validity of our giving measure by comparing its sensitivity to respondent demographic variables against the sensitivity of prior charitable giving to the same demographic variables. We find that giving in our experiment is 55 percent to 85 percent as sensitive to demographic characteristics as self-reported prior charitable giving. See Fong and Luttmer (2007) for details.

¹⁵ In constructing this variable, we did not include our manipulations on church attendance, use of public assistance, or looting in the city. Church attendance may be seen as a positive or a negative trait, depending on the respondent’s views. Use of public assistance confounds possible judgments of worthiness with judgments of need. Finally, we did not include the looting manipulation because we originally included it to prime respondents with a racially charged issue. Obviously, the looting manipulation may have affected perceptions of worthiness, so it is reassuring that our results are very similar if we include the looting manipulation in our measure of worthiness.

TABLE 4—RESULTS BY RACE OF THE RESPONDENT AND BY MEASURE OF GENEROSITY

| | Pictures show black victims | | Audio manipulation: economically disadvantaged | | Number of worthiness manipulations | | R^2 | N |
|--|--------------------------------|--------|---|--------|--|--------|-------|-------|
| <i>Panel A: Giving to Habitat to help Katrina victims in city, \$ out of \$100</i> | | | | | | | | |
| All respondents | -2.2 | (3.8) | 4.2** | (1.9) | 0.7 | (0.9) | 0.176 | 1,343 |
| White respondents | -4.0 | (4.7) | 6.1*** | (2.3) | 0.8 | (1.2) | 0.165 | 915 |
| Black respondents | 7.1 | (8.5) | -1.9 | (4.1) | 2.9 | (2.2) | 0.249 | 247 |
| <i>Panel B: Hypothetical giving to Habitat to help Katrina victims in city (\$)</i> | | | | | | | | |
| All respondents | 0.5 | (3.8) | -1.3 | (2.1) | 0.8 | (1.2) | 0.117 | 1,341 |
| White respondents | -2.3 | (4.0) | -2.0 | (2.5) | 1.4 | (1.1) | 0.116 | 913 |
| Black respondents | 7.0 | (13.8) | -2.5 | (6.9) | -6.9* | (4.1) | 0.163 | 247 |
| <i>Panel C: Subjective support for charity spending to help Katrina victims in city (1–7 scale)</i> | | | | | | | | |
| All respondents | -0.21 | (0.13) | 0.09 | (0.06) | 0.03 | (0.03) | 0.059 | 1,333 |
| White respondents | -0.22 | (0.16) | 0.11 | (0.07) | 0.03 | (0.04) | 0.066 | 907 |
| Black respondents | -0.62* | (0.37) | 0.08 | (0.20) | 0.01 | (0.10) | 0.105 | 246 |
| <i>Panel D: Subjective support for government spending to help Katrina victims in city (1–7 scale)</i> | | | | | | | | |
| All respondents | -0.22 | (0.16) | 0.11 | (0.08) | 0.14*** | (0.04) | 0.091 | 1,337 |
| White respondents | -0.44** | (0.20) | 0.10 | (0.09) | 0.16*** | (0.05) | 0.083 | 913 |
| Black respondents | 0.06 | (0.40) | 0.23 | (0.20) | 0.02 | (0.10) | 0.110 | 245 |

Notes: Each row contains results from a single regression. Each panel uses a different generosity measure as dependent variable. The table reports OLS coefficients (robust standard errors in parentheses) for the race manipulation, audio manipulation on the income level of the city, and the number of audio manipulations designed to increase perceptions of victims' worthiness, respectively. The number of worthiness manipulations is equal to the sum of the dummy variables for the audio manipulations "Many city residents helped other victims," "Habitat recipients must contribute labor to house," and "Many city residents prepared for hurricane" minus the dummy for the audio manipulation "City has been troubled by crime." Results for all respondents are weighted to adjust for oversampling of blacks. Control variables are the same as in Table 3, column 2. Hypothetical giving is topcoded at \$500, which affected 4 observations.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

noting that the explanatory power for the regressions predicting actual giving is markedly higher than the explanatory power for any of the hypothetical or subjective measures of generosity. This suggests that actual giving behavior is a less noisy measure of generosity than our subjective or hypothetical measures.

In panel A, the regression for the whole sample repeats the regression that was presented in column 2 of Table 3. The second and third rows show that the estimated response to pictures showing black victims is $-\$4.0$ among whites and $\$7.1$ among blacks, but both estimates are statistically insignificant. Thus, also within the sample of whites (where one might have expected racial bias to be most likely), we find no significant evidence of racial bias. Moreover, the estimated response is not statistically significantly different between black and white respondents (p -value: 0.25), which confirms that we do not detect significant objective racial group loyalty. The second column of panel A shows that the significant positive effect of economic disadvantage in the city is driven by white respondents. Column 3 shows a strikingly small and insignificant effect of the number of worthiness manipulations in all three samples. As we show in Table A2, almost all of the worthiness manipulations have statistically significant effects on the perceptions that they were designed to affect, so the weak effect of the worthiness manipulations on giving is not due to manipulation failures. Further, as we discuss shortly, the number of worthiness manipulations

does have highly significant effects on support for public spending to help Hurricane Katrina victims.

Panels B and C show no statistically significant treatment effects at the 5 percent level or better on the measures of hypothetical giving to and subjective support for charitable spending on Hurricane Katrina victims in the city. This is not too surprising because the measurement reliability of these measures is probably lower than that of actual giving.

The results on subjective support for government spending on Hurricane Katrina victims differ from the results on private giving. Panel D shows a highly significant positive effect of perceived worthiness on subjective support for public assistance to Hurricane Katrina victims in the city in question in the overall sample and sample of white respondents, but not among black respondents. We also find a significant negative effect (at the 5 percent level) of the black picture manipulation among whites, but not in the whole sample or the sample of blacks. It is noteworthy that these significant results occur despite the fact that the dependent variable is an attitudinal measure, and thus may have lower measurement reliability than the behavioral measure used in panel A.

The results in panel D are consistent with the literature on determinants of support for public redistribution, which has shown that both recipient race and perceptions of worthiness play important roles.¹⁶ In view of the widely reported effects of race and worthiness in support for public assistance, the fact that they have no significant effects on private generosity may seem surprising. One possible explanation is that respondents believe that Habitat for Humanity chooses to help only worthy individuals, while the government cannot select its recipients. Some of the open-ended comments that we received hint at this. For example, one respondent wrote:

The people who receive help from Habitat are hard-working families, but the people on public assistance seem to be several hundred pounds overweight. I have trouble putting food on my table and [paying my] expenses. These people are living high on the hog at our expense.

If there is a difference in beliefs about the worthiness of recipients of charity and recipients of government assistance, it could also explain the presence of a race effect in public generosity to Hurricane Katrina victims and its absence in private generosity. This could occur if the effect of race operates through perceptions of worthiness, as some have argued (Gilens 1999).

E. Robustness

Table 4 suggests four noteworthy main treatment results: no effect of the race manipulation on measures of private generosity presented in panels A–C, a significant effect of the manipulation of the city's economic situation on giving (in panel A), and significant effects of the worthiness and race manipulations on support for public aid to Katrina victims (in panel D). In Table 5, we examine the

¹⁶ See, for instance, Luttmer (2001) on racial group loyalty; Fong (2001), and Giacomo Corneo and Hans Peter Grüner (2002) on fairness; and Alesina, Glaeser, and Sacerdote (2001) for a review.

robustness of these results for white respondents. We show the robustness for the sample of white respondents because the absence of racial bias is more surprising in the sample of whites than in the overall sample, and the negative effect of seeing pictures of black victims on attitudinal support for government spending only shows up for whites. The results for the whole sample and because the black subsample are also robust to the alternative specifications shown in Table 5 (results available on request).

The organization of Table 5 is similar to that of Table 4. There is one panel for each of the outcome measures of generosity, and the columns present coefficients and standard errors for the race manipulation, the manipulation of the city's economic situation, and the number of worthiness manipulations, respectively. Within each panel of Table 5, the first row repeats a baseline regression for the sample of whites from Table 4. Each subsequent row is like the first row except that one aspect of either the specification or the sample is changed. Within each panel, row 2 excludes the race-salient and the full-stakes samples. Rows 3 and 4 use only the sample that was shown photos of Slidell or Biloxi, respectively. Rows 5 and 6, respectively, drop or add demographic controls relative to the baseline regression. Row 7 presents censored regressions when the outcome measure is dollars given and ordered probits when the outcome measure is a 1–7 scale. Row 8 presents the effect of the race manipulation using only the subsample of whites who saw pictures in which race was shown, thus dropping controls for the backgrounds of the pictures.

Panels A–C confirm that there is no significant effect of the race manipulation on the three measures of private generosity, except for a marginally significant effect in two specifications in panel C. By and large, panel A confirms that respondents give more money to victims in economically disadvantaged cities. Panel D confirms that the effect of the number of worthiness manipulations on support for public aid to Hurricane Katrina victims is robust. The number of worthiness manipulations has significant effects at the 1 percent level in 6 robustness checks and at the 5 percent level in the remaining 2 robustness checks. The effect of the race manipulation on support for public aid to Hurricane Katrina victims is significant at the 5 percent level or better in 5 of the 8 robustness checks.

F. Effects of Subjective Racial Attitudes on Racial Bias

In Table 3, we showed that subjective racial identification is a strong predictor of racial bias in giving. In this section, we present a more comprehensive investigation of heterogeneity in racial bias according to measures of racial attitudes. Table 6 presents effects of interactions between our race manipulation and the three measures of subjective racial attitudes described in Section I on our four measures of generosity.

Table 6 has four columns, each one explaining one of the four generosity measures. Panels A and B present the results for white and black respondents, respectively. The rows labeled A1 and B1 present the interaction results for the subjective racial identification dummy for whites and blacks, respectively. In both panels, there is a strong interaction between racial identification and our race manipulation in regressions explaining actual giving. The rows in A1 show that whites who report being “close” or “very close” to their ethnic or racial group give roughly

TABLE 5—ROBUSTNESS CHECKS ON SAMPLE OF WHITE NON-HISPANIC RESPONDENTS

| | Pictures show black victims | | Audio manipulation: economically disadvantaged | | Number of worthiness manipulations | | R^2 | N |
|--|--------------------------------|--------|---|--------|--|--------|-------|-----|
| <i>Panel A: Giving to Habitat to help Katrina victims in city, \$ out of \$100</i> | | | | | | | | |
| Baseline | -4.0 | (4.7) | 6.1*** | (2.3) | 0.8 | (1.2) | 0.165 | 915 |
| Main sample only | -3.6 | (5.1) | 6.0** | (2.6) | 0.6 | (1.3) | 0.150 | 717 |
| Slidell sample only | 1.8 | (6.6) | 6.8** | (3.2) | -1.3 | (1.7) | 0.209 | 446 |
| Biloxi sample only | -8.6 | (6.6) | 4.4 | (3.3) | 2.7* | (1.6) | 0.188 | 469 |
| No demographic controls | -2.9 | (4.9) | 6.3*** | (2.4) | 1.1 | (1.2) | 0.028 | 915 |
| Additional control variables | -4.7 | (4.6) | 5.5** | (2.3) | 0.8 | (1.1) | 0.212 | 900 |
| Censored regression | -4.8 | (10.5) | 13.1** | (5.1) | 1.0 | (2.6) | 0.030 | 915 |
| Race-shown sample only | -2.3 | (2.9) | 6.3** | (2.9) | 1.0 | (1.5) | 0.209 | 554 |
| <i>Panel B: Hypothetical giving to Habitat to help Katrina victims in city (\$)</i> | | | | | | | | |
| Baseline | -2.3 | (4.0) | -2.0 | (2.5) | 1.4 | (1.1) | 0.116 | 913 |
| Main sample only | -2.1 | (4.2) | -2.9 | (2.6) | 2.0 | (1.3) | 0.133 | 715 |
| Slidell sample only | -3.9 | (5.4) | -3.6 | (4.0) | 0.5 | (1.1) | 0.145 | 444 |
| Biloxi sample only | -0.7 | (6.0) | -0.9 | (3.6) | 2.7 | (1.9) | 0.147 | 469 |
| No demographic controls | -1.8 | (4.3) | -1.0 | (2.4) | 1.4 | (1.1) | 0.006 | 913 |
| Additional control variables | -2.5 | (4.0) | -2.5 | (2.5) | 1.5 | (1.2) | 0.125 | 899 |
| Censored regression | -4.3 | (7.8) | -0.9 | (3.8) | 3.2* | (1.9) | 0.025 | 913 |
| Race-shown sample only | -0.8 | (3.4) | -1.7 | (3.5) | 0.7 | (1.4) | 0.142 | 553 |
| <i>Panel C: Subjective support for charity spending to help Katrina victims in city (1–7 scale)</i> | | | | | | | | |
| Baseline | -0.22 | (0.16) | 0.11 | (0.07) | 0.03 | (0.04) | 0.066 | 907 |
| Main sample only | -0.29* | (0.17) | 0.05 | (0.08) | 0.05 | (0.04) | 0.075 | 709 |
| Slidell sample only | -0.24 | (0.23) | 0.14 | (0.11) | 0.00 | (0.05) | 0.114 | 441 |
| Biloxi sample only | -0.23 | (0.22) | 0.09 | (0.11) | 0.06 | (0.06) | 0.064 | 466 |
| No demographic controls | -0.22 | (0.16) | 0.12 | (0.07) | 0.03 | (0.04) | 0.035 | 907 |
| Additional control variables | -0.27* | (0.16) | 0.10 | (0.07) | 0.03 | (0.04) | 0.093 | 893 |
| Ordered probit | -0.20 | (0.15) | 0.12* | (0.07) | 0.03 | (0.04) | 0.024 | 907 |
| Race-shown sample only | 0.09 | (0.09) | 0.25*** | (0.09) | 0.03 | (0.05) | 0.087 | 550 |
| <i>Panel D: Subjective support for government spending to help Katrina victims in city (1–7 scale)</i> | | | | | | | | |
| Baseline | -0.44** | (0.20) | 0.10 | (0.09) | 0.16*** | (0.05) | 0.083 | 913 |
| Main sample only | -0.45** | (0.22) | -0.03 | (0.11) | 0.20*** | (0.05) | 0.079 | 715 |
| Slidell sample only | -0.55* | (0.28) | 0.17 | (0.13) | 0.15** | (0.06) | 0.123 | 444 |
| Biloxi sample only | -0.33 | (0.28) | 0.08 | (0.14) | 0.19*** | (0.07) | 0.082 | 469 |
| No demographic controls | -0.45** | (0.19) | 0.10 | (0.09) | 0.17*** | (0.05) | 0.041 | 913 |
| Additional control variables | -0.50*** | (0.19) | 0.09 | (0.09) | 0.17*** | (0.05) | 0.120 | 899 |
| Ordered probit | -0.31** | (0.15) | 0.08 | (0.07) | 0.13*** | (0.04) | 0.026 | 913 |
| Race-shown sample only | -0.14 | (0.12) | 0.20* | (0.11) | 0.15** | (0.06) | 0.112 | 553 |

Notes: Each row contains results from a single regression. Each panel uses a different generosity measure as dependent variable. Unless otherwise noted, the table presents OLS coefficients (robust standard errors in parentheses) for the race manipulation, audio manipulation on the income level of the city, and the number of audio manipulations designed to increase perceptions of victims' worthiness, respectively. Baseline control variables are the same as in Table 3, column 2. The additional controls include subjective assessments of the effectiveness of Habitat for Humanity, how much the respondent values helping others, and how much the respondent cares about money. Results are weighted to adjust for oversampling of black respondents.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

\$17 less when seeing pictures that show black victims rather than white ones. In contrast, whites who say they are “not very close” or “not close at all” give roughly \$13 more in response to pictures showing black victims. These two coefficients are significantly different from each other at the 1 percent level. The rows in B1 show that blacks who feel close to blacks give \$16 more in response to pictures showing

black victims. Blacks who do not feel close to blacks give \$72 less in response to pictures showing black victims. These two coefficients are significantly different from each other at the 1 percent level.

In unreported analyses, we find that the interaction between subjective racial identification and our race manipulation is very robust. For example, when we conduct four separate regressions for each response category of subjective racial identification, there is a clear pattern of heterogeneity. Among whites who are, respectively, “not close at all,” “not very close,” “close,” and, “very close” to their ethnic or racial group, the racial biases toward blacks are \$26 (significant at the 10 percent level), $-\$4$, $-\$9$, and $-\$33$ (significant at the 5 percent level). Furthermore, when subjective racial identification is measured as a continuous variable, it has a highly significant (at the 1 percent level) negative interaction with the race manipulation.

For the other measures of generosity, there are no interaction effects of subjective racial identification and the race manipulation that are significant at the 5 percent level or better. Thus, we only find clear evidence of subjective racial group loyalty when we measure generosity by actual amount given, but find no significant evidence if we measure generosity by hypothetical giving or attitudes toward charity or government spending on Hurricane Katrina victims. Part of this difference might be explained by respondents’ preferences for generosity depending on the means by which Katrina victims are helped (via Habitat for Humanity, via any charity spending, or via government spending). However, we also note that actual giving is the only measure of generosity that is behavior based (i.e., not “cheap talk”) and for which hiding any racial biases would be costly to the respondent. We therefore place the most weight on the results using actual giving as an outcome measure.

Rows A2 and B2 present the interaction results for the dummy variable measuring frequency of social contact with blacks relative to whites, in the white and black samples, respectively. The first column of A2 shows that whites who report having equal or more social contact with blacks give about \$18 less in response to pictures showing black victims while those who have less social contact with blacks give about \$3 more in response to pictures showing black victims. These two effects are significantly different from each other at the 5 percent level.¹⁷ The remaining columns of A2 show no significant interaction effects between frequency of social contact and the race manipulation on the other outcome measures. Row B2 shows that, for black respondents, we find no significant interactions between social contact and the race manipulation on any of the outcome measures. Finally, the rows in A3 and B3 present interactions between the race manipulation and the belief that blacks get at least as many economic opportunities as whites on each outcome variable in the white and black samples, respectively. This interaction effect is insignificant in all cases. This implies that there are people who discriminate against blacks in their

¹⁷ Because social contact was collected after the respondents had decided how much to give to Katrina victims, it is possible that some white respondents, realizing that their giving decision might have been racially biased when questions involving race were asked in Section IV of the survey, try to compensate for this behavior by reporting more social contact with blacks.

TABLE 6—EFFECTS OF INTERACTIONS BETWEEN RACE MANIPULATION AND SUBJECTIVE RACIAL ATTITUDES ON RACIAL BIAS

| | Giving to Habitat to help Katrina victims in city, \$ out of \$100 (1) | Hypothetical giving to Habitat to help Katrina victims in city (\$) (2) | Subjective support for charity spending to help Katrina victims in city (1–7 scale) (3) | Subjective support for government spending to help Katrina victims in city (1–7 scale) (4) |
|--|--|--|--|---|
| <i>Panel A: Effect of "Pictures show black victims" on white respondents' generosity</i> | | | | |
| (A1) By respondent's closeness to his or her ethnic or racial group | | | | |
| Very close/close | -16.7** (6.9) | -3.3 (6.7) | -0.28 (0.24) | -0.33 (0.29) |
| Not very close/not close at all | 13.0* (7.8) | 0.0 (6.8) | -0.14 (0.26) | -0.52 (0.33) |
| <i>p</i> -value on test of equal coefficients | 0.0049 | 0.7352 | 0.6833 | 0.6477 |
| Observations | 749 | 748 | 742 | 747 |
| (A2) By frequency of social contact with blacks compared to whites | | | | |
| Equal or more social contact with blacks | -17.7** (8.2) | -5.7 (7.0) | -0.43 (0.28) | -0.61* (0.34) |
| More contact with whites than blacks | 2.8 (5.8) | -2.1 (4.3) | -0.12 (0.19) | -0.35 (0.24) |
| <i>p</i> -value on test of equal coefficients | 0.0432 | 0.6547 | 0.3593 | 0.5227 |
| Observations | 903 | 902 | 896 | 902 |
| (A3) By belief about number of economic opportunities for blacks compared to whites | | | | |
| Blacks have at least as many opportunities | -3.6 (5.9) | 0.9 (5.5) | -0.22 (0.20) | -0.47** (0.24) |
| Blacks have fewer opportunities | -6.7 (7.8) | -8.4 (6.4) | -0.23 (0.25) | -0.42 (0.32) |
| <i>p</i> -value on test of equal coefficients | 0.7571 | 0.3031 | 0.9802 | 0.8951 |
| Observations | 908 | 907 | 902 | 908 |
| <i>Panel B: Effect of "Pictures show black victims" on black respondents' generosity</i> | | | | |
| (B1) By respondent's closeness to his or her ethnic or racial group | | | | |
| Very close/close | 15.5 (9.8) | 16.5 (16.6) | -0.68 (0.42) | -0.19 (0.47) |
| Not very close/not close at all | -71.5*** (26.9) | -133.7* (74.8) | -0.57 (1.28) | 1.06 (1.48) |
| <i>p</i> -value on test of equal coefficients | 0.0032 | 0.0591 | 0.9396 | 0.4306 |
| Observations | 219 | 219 | 218 | 217 |
| (B2) By frequency of social contact with blacks compared to whites | | | | |
| Equal or more social contact with blacks | 6.0 (8.5) | 5.7 (15.9) | -0.67* (0.37) | -0.10 (0.40) |
| More contact with whites than blacks | -12.6 (40.5) | 31.1 (49.1) | -0.06 (1.48) | 3.19* (1.77) |
| <i>p</i> -value on test of equal coefficients | 0.6482 | 0.6623 | 0.6862 | 0.0679 |
| Observations | 245 | 245 | 244 | 243 |
| (B3) By belief about number of economic opportunities for blacks compared to whites | | | | |
| Blacks have same or more opportunities | 11.8 (21.4) | -0.4 (23.3) | -1.19 (0.89) | -0.28 (1.14) |
| Blacks have fewer opportunities | 4.6 (9.2) | 3.3 (16.7) | -0.55 (0.42) | -0.05 (0.44) |
| <i>p</i> -value on test of equal coefficients | 0.7558 | 0.8837 | 0.5259 | 0.8521 |
| Observations | 242 | 242 | 241 | 240 |

Notes: Each column/row cell contains results from a single regression. Panels A and B present results for non-Hispanic whites and non-Hispanic blacks, respectively. Rows A1–A3 and B1–B3 present interaction effects between "Pictures show black victims" and exhaustive dummy variables for racial attitudes. Regressions also control for the direct effect of the racial attitude as well as all controls included in Table 3, column 2. Numbers shown are OLS coefficients (robust standard errors in parentheses) with *p*-values on the test of equality of coefficients and number of observations below.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

giving (namely, whites who feel close to their ethnic or racial group) but who do not appear to be biased against blacks in their response to the question about economic opportunities for blacks.

Table 6 shows that both black respondents and white respondents exhibit significant amounts of subjective racial group loyalty in giving. Thus, the answer to the simple question “How close do you feel to your ethnic or racial group?” is a significant predictor of racial bias in giving. The other two measures of racial attitudes do not predict racial bias in giving as well. Beliefs about the economic opportunities of blacks have no predictive power whatsoever, while social contact with blacks relative to whites is only predictive for white respondents but not for black respondents. The fact that only one of the three measures of racial attitudes is a clear predictor of racial bias in giving may seem surprising. We offer two potential explanations. First, the response to the question about closeness to one’s ethnic or racial group was collected in an earlier survey by Knowledge Networks and can therefore not be contaminated by our experiment. The other two measures were asked in our survey after the respondents had made their giving decisions. Second, the racial identification question asks about closeness to their own ethnic or racial group, while the other two measures involve answers about other racial groups. The latter seems more likely to trigger social-desirability biases.

III. Conclusion

In this paper, we examine the role of race and racial group loyalty in generosity toward Hurricane Katrina victims using a design with three important features. First, we used a behavior-based measure of generosity, namely the amount of money given during the experiment. Second, we experimentally varied perceptions of the race and other characteristics of Hurricane Katrina victims in order to obtain causal estimates of the effect of victim characteristics on giving. Third, we ran the experiment on a sample that is broadly representative of the US adult population so that our estimates should reflect any racial bias and group loyalty present in the general population.

The experiment yields two main findings. First, in the overall population, we find no evidence that giving differs by race of the victims. Moreover, respondents do not condition their giving on victim characteristics that may indicate worthiness, though they significantly increase the amount given when victims come from an economically disadvantaged area.

Second, we find very strong evidence of subjective racial group loyalty. Respondents who report feeling close to their ethnic or racial group give significantly more when they see pictures of victims of their own racial group, whereas we find the opposite effect for respondents who do not report feeling close to their group. In other words, we find that subjective identification with a racial group is a powerful predictor of bias in giving toward that group, and we refer to this effect as subjective racial group loyalty. Interestingly, while the point estimates indicate some group loyalty based on the actual race of the respondent, these estimates are not statistically significant. Thus, we find that subjective racial identification is a stronger predictor of racial bias in giving than the objective race of the respondent. We do not find clear evidence that our two other and more explicit measures of racial attitudes predict racial bias in giving.

We speculate that two factors can help explain the power of the simple question “How close do you feel to your ethnic or racial group?” in explaining racial bias in giving. First, the question does not ask the respondent to pass judgment on other

groups, and therefore is less likely to suffer from social desirability effects. Second, subjective racial identity may matter more than objective race, which makes sense in view of the rich array of social experiences that accompany interracial and interethnic families, education, and neighborhoods. Since our evidence was gathered in the context of giving to Hurricane Katrina victims, more research on the role of objective and subjective racial identity in different institutional settings would be valuable for a broader understanding of racial discrimination.

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