



Contents lists available at ScienceDirect

Journal of Economic Behavior & Organization

journal homepage: www.elsevier.com/locate/jebo

The white-man effect: How foreigner presence affects behavior in experiments[☆]



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ARTICLE INFO

Article history:

Received 15 February 2014

Received in revised form 20 March 2015

Accepted 26 March 2015

Available online 2 July 2015

JEL classification:

C91

C93

O19

Keywords:

Africa

Aid

Behavior

Dictator games

Lab-in-the-field experiment

Race

ABSTRACT

We experimentally vary white foreigner presence in dictator games across 60 villages in Sierra Leone, and find that the simple presence of a white foreigner increases player contributions by 19 percent. To separate the impact of the white foreigner's race and nationality from other characteristics, we test additional predictions. First, the white foreigner's presence may heighten demand effects, prompting players to try to impress the white foreigner by being more generous. This should make behavior in the game less indicative of true generosity. Consistent with this, we find that game contributions are no longer predicted by real-world public good contributions when the white foreigner is present. Second, those more familiar with aid may perceive the games as a form of means-testing, and therefore give less to signal that they are poor. Consistent with this, in the presence of the white foreigner, players in more aid-exposed villages give less, and are more likely to believe that the games are testing them for aid suitability. Together, these results suggest that players' giving decisions respond to the white foreigner's race and nationality. Behavioral measures are increasingly used to infer cross-national differences in social preferences or to assess aid effectiveness—our results suggest that we should be cautious in these uses.

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1. Introduction

Lab-in-the-field experiments are increasingly conducted in developing countries (Fig. 1). The actions of players in these experiments tend to be interpreted as measures of human behavior, and are used to offer answers to questions such as whether aid affects social capital (Fearon et al., 2009, 2015; Casey et al., 2012; Humphreys et al., 2012; Avdeenko and Gilligan, 2015), or to infer cross-national differences in social preferences (Henrich et al., 2006). In practice, these experiments are

[☆] We are grateful to Sendhil Mullainathan for numerous discussions and suggestions. We also thank Abigail Barr, Erwin Bulte, Jessica Gottlieb, Rachid Laajaj, Ted Miguel, Anand Rajaram, and Debraj Ray, as well as seminar participants at Stanford, Oxford, NYU, Wageningen, the Princeton Conference on Causal Inference and the Study of Conflict and State Building, the Northeast Universities Development Consortium (NEUDC) Conference, the Berkeley Symposium on Economic Experiments in Developing Countries (SEEDEC), and the Centre for the Study of African Economies' Annual Conference for comments. Jessica Creighton, Anthony Mansaray, Ali Ahmed, Joshua McCann and Jeffrey Steinberg provided excellent research assistance. The International Initiative for Impact Evaluation (3ie) provided funding for the research. Siddiqi acknowledges support from the AFOSR under Award No. FA9550-09-1-0314. Any opinions, findings, and conclusions are those of the authors and do not necessarily reflect those of any supporting institution.

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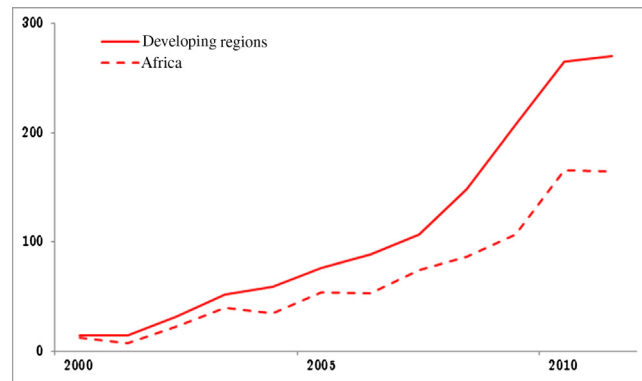


Fig. 1. The increasing use of dictator games in developing countries. *Note:* This graph shows the number of mentions of dictator games in developing countries (solid line) and in Africa (dashed line), based on Google Scholar counts.

frequently conducted by foreigners whose ethnicity differs starkly and visibly from that of local participants. Could the presence of these foreigners affect behavior in these experiments and thereby complicate using them for these purposes?

While the results from such experiments are interpreted as unbiased metrics of human behavior, in both the experimental and aid contexts, differences in race and nationality strongly signal differences in wealth, power and authority. Understanding the extent to which these differences bias individual behavior is important, given the growing use of behavioral techniques, and the implications of such bias for aid targeting and evaluation.

Our paper directly examines this issue. We study the effect of researcher race and nationality on individual behavior in a developing country. Specifically, we conduct a lab-in-the-field experiment that randomizes the presence of a white foreigner across behavioral games in 60 villages across Sierra Leone. We assess how the presence of this ‘white-man’¹ impacts generosity as measured by player contributions in dictator games.² We find that the mere presence of the white foreigner on the research team increases the amount given by a substantial 19 percent.³

A challenge in interpreting this effect as arising from the white foreigner’s race and nationality lies in the fact that the experiment varied the presence of two individuals—one white and one black—across the games. Thus, players may have responded to some other characteristic of the white foreigner, besides his race and nationality. For example, if he was perceived to be especially friendly, this personality trait may have increased giving. Alternatively, if he seemed more educated, this may have commanded greater respect and influenced player allocations. In short, many channels are possible.

We deal with this challenge in two ways. The first is by design. The behavioral games were implemented by teams of five individuals, four of whom were black Sierra Leoneans, including the team leader. The random variation is in the identity of the fifth member: in control areas, he too was a black Sierra Leonean, but in treatment areas, the fifth member was a white foreigner. The team leader—who was the same person across all games—played an active role and instructed the players. In contrast, the protocol specified a silent, limited role for the fifth member. He was not allowed to talk to the players in either treatment or control areas. His specific task was to hand out money at the start of the game, and he was not to interact otherwise with game participants. In this regard, our experiment examines how the mere *presence* of a white foreigner affects measured generosity in the games.

This design only partly mitigates concerns that other characteristics could drive the observed effects. To further address this issue, we lay out explicit hypotheses about why the white person’s presence could plausibly influence player contributions, and generate additional, testable predictions implied by the hypotheses.⁴ We test these predictions by drawing on rich household and village-level survey data collected from game participants, which were collected as part of a baseline for a separate study on post-conflict reconciliation.

Our key hypothesis is that the result can be explained by the ‘experimenter demand effect’, which arises when research subjects change behavior to conform to the perceived desire of the researcher (Masling, 1966; Levitt et al., 2011; Nichols

¹ Non-Africans in Sierra Leone are typically referred to in English and Krio as “white-man”, and in other dominant local languages as its literal equivalent: “opoto” in Temne, “poomui” in Mende, etc. Even though Lebanese, Chinese, or Indians may sometimes be identified as such when they are compared to one another, they are generally referred to as “white” when discussed in comparison to Sierra Leoneans or Africans. Thus we refer to our treatment as the ‘white-man’ effect.

² Player contribution in a dictator game is the common measure of altruism in behavioral experiments (Cardenas and Carpenter, 2008).

³ Our paper relates to studies in the social psychology literature that have examined the impact of surveyor and experimenter characteristics, including race, on relevant outcomes—see Rosenthal (1963) for a summary. Others have also examined how researcher characteristics including race, education, income, gender, and religion influence subject response in survey data (Miyazaki and Taylor, 2007; Cotter et al., 1982; Finkel et al., 1991; Hyman, 1954; Mensch and Kandel, 1988; Reese et al., 1986; Anderson et al., 1988; Webster, 1996; Bailar et al., 1977; Blaydes and Gillum, 2012).

⁴ By focusing on the experimenter’s ethnicity, our paper differs from previous studies which have examined the import of players’ racial, ethnic and national identities in determining behavioral game allocations (e.g. Burns, 2006, 2012; Whitt and Wilson, 2007; Habyarimana et al., 2007; Cappelen et al., 2013; Adida et al., 2012).

and Maner, 2008).⁵ We posit two types of demand effects. Each implies opposite effects on giving. First, players may give *more* if they perceive that the researcher wants them to display generosity and they have a stronger desire to impress a white foreigner. Demand effects have been shown to arise from perceived power differentials (Schultz, 1969), and in our setting, there are stark disparities in wealth and authority between black locals and white foreigners.⁶ Second, players may perceive that the white foreigner is there to assess whether they need aid. As a result, they may modify their behavior to appear aid-worthy. Specifically, if subjects believe aid is targeted to the poor, they will have an incentive to act poor by giving *less* in the games where he is present. This second phenomenon is more likely to arise in communities that have more experience with aid, where people have had a chance to learn about means-testing and aid-targeting methods. Thus, these two forces—the desire to impress the white foreigner by displaying generosity, and the desire to appear aid-worthy—imply countervailing effects on giving.

We generate two key empirical predictions implied by these two types of demand effects. First, if people assume that the white foreigner wants them to give more, his presence should erode the relationship between giving in the games and giving in the real world. Past work suggests that the desire to look good in the lab setting can induce people to act more altruistically than they would outside this setting (Benz and Meier, 2008; List, 2005; Levitt and List, 2007), and more so when their actions are known to the researcher (Andreoni and Bernheim, 2009; List et al., 2004; Hoffman et al., 1996). Taken together, these studies suggest that larger experimenter demand effects in presence of the white foreigner will reduce the correlation between player's game allocations and their real world behavior.

We use our baseline surveys to measure how much players contributed to public goods in their villages prior to the games. We find that these contributions strongly predict contributions in the games where the white foreigner is absent, but do not where he is present. This pattern is consistent with stronger demand effects eroding the relationship between behavior and measured generosity in the white-man sample.

Second, if players give based on perceptions that the white foreigner is testing them for aid, they should give less in his presence, particularly in villages that have more experience with development aid. We present two pieces of evidence that support this mechanism. We find that the white-man treatment effect falls in villages that have had longer exposure to NGOs dispensing aid. In fact, players in the top 20% most aid-exposed villages give *less* in the presence of the white foreigner. We also draw on a post-game question that asked players their perception regarding the purpose of the games. We find that players in these highly aid-exposed villages were also more likely to believe that the games conducted in the presence of the white foreigner were to test them for aid, rather than to give out money or conduct research.

These results suggest that players act based on their perceptions of the experimenter as a white foreigner. Also, they seem inconsistent with other potential channels. For example, if other characteristics such as his demeanor drove the results, we should not observe differential impacts in either giving or aid-testing beliefs based on aid exposure.

We also conduct a third empirical test. Traditional village leaders are typically the intermediaries between aid-givers and the community; thus if players give based on perceptions of aid-testing, then leaders should also give less, as they are more likely to be aware of how aid is given out. Consistent with this account, we find that players from households of traditional leaders also give less in the presence of the white foreigner, and are more inclined to believe that the purpose of the games where he is present is to test them for aid. This suggests a desire to signal the need for aid on their part. In addition, since these are also higher-status persons, they may also give relatively less because they perceive less difference in power between themselves and the white, and hence a smaller demand effect foreigner, and hence a smaller demand effect. Thus both forces—the desire to signal need and smaller demand effects among powerful people—may contribute to the lower average giving by leader households in response to the white foreigner.

We also formulate a fourth empirical prediction to examine the primacy of other characteristics. If giving responded to another personality trait correlated with the white foreigner, we would plausibly expect the treatment effect to vary based on this trait among players. However, we find no differential impacts based on characteristics such as player friendliness proxied by their friendships, or psychometric measures of depression and PTSD; nor do we find differential effects based on gender and education. This casts doubt on the importance of characteristics in inducing differential giving in the presence of the white foreigner.

In the sections below, we provide institutional context, describe the empirical strategy and data, present the results on generosity and mechanism, and conclude by discussing the implications of our findings for research and policy design.

2. Institutional context

As our study explores the role of differences in power and aid exposure in moderating subject responses to foreigner presence, this section provides a brief overview of Sierra Leone's extensive exposure to foreign aid, and its traditional authority system, including the strong local power base of chiefs, religious leaders, and secret societies.

⁵ Experimenter demand effects are related to the idea of 'Hawthorne effects'—the finding that participants alter their behavior in experimental settings because they are being observed (Adair et al., 1989; Hoffman et al., 1994; Diaper, 1990; Haley and Fessler, 2005; McCarney et al., 2007; Levitt et al., 2011; Zwane et al., 2011).

⁶ In the wake of Sierra Leone's long civil war, foreigners affiliated with the U.N. provided peacekeeping and sponsored the special courts that prosecuted war crimes. Foreign aid workers continue to dispense resources and restructure governance, while foreign businessmen trade in diamonds.

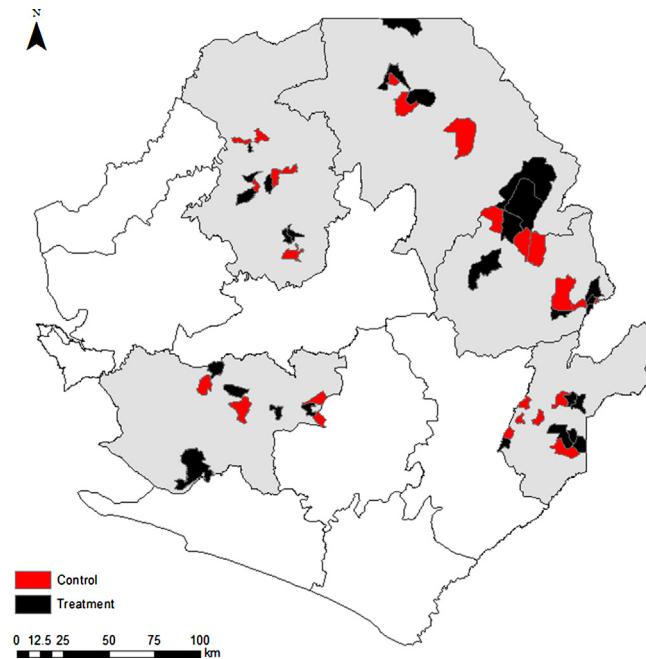


Fig. 2. Treatment and control communities. *Note:* This figure shows the five districts of Sierra Leone included in the experiment (gray), the 30 treatment sections (black) and the 30 control sections (red). One pre-designated village from each of these sections comprises the treatment and control communities in the study. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of the article.)

Sierra Leone experienced a dramatic influx of foreign aid following its long and brutal civil war, which lasted from 1991 to 2001. The war contributed to the escalation of poverty, leaving the nation amongst the ranks of the world's poorest and least developed—in 2011, the country ranked 180th out of 187 countries on the United Nation's Human Development Index, up from 187th out of 187 in 2003. With the end of the conflict, the country experienced a surge in foreign aid (Fanthorpe, 2003). Official Development Assistance as a proportion of GNI peaked at 43 percent in 2001 and remained over 24 percent in 2010, making it the 11th most aid-dependent nation in the world (World Bank, 2012). However, Sierra Leone's exposure to foreign aid dates back far longer due to religions missions, the earliest of which were established in the beginning of the 17th century (Fyfe, 1968). Given this extended dependence, exposure to development aid is highly likely to shape locals' perception of white foreigners. Thus Sierra Leone provides an ideal setting to explore the questions that motivate this study—how foreigner presence effects measured generosity, and how this effect is moderated by aid expectations.

Sierra Leone has a strong chieftaincy system, a key institutional legacy of British indirect rule. With the creation of the protectorate in 1898, the Paramount Chiefs—the highest-level traditional chiefs—were given substantial powers over their subjects (Fanthorpe, 2001; Jackson, 2006; Acemoglu et al., 2014). Despite postwar attempts to create more accountable, formal sources of administrative authority, *de facto* power still remains with the chiefs: “chiefs are regarded as legitimate traditional rulers, with broader political power bases than the council chairperson” (Jackson, 2006). They are still called upon to settle disputes (Sawyer, 2008), and they retain the power to tax, request labor, administer justice, and allocate land. Informal power also resides in secret societies, cultural institutions or “cult associations” (Richards, 2005) that regulate social behavior through secret rituals and practices (Fanthorpe, 2007). Most Sierra Leoneans are members of secret societies and their influence permeates all aspects of social and political life in the country (Murphy, 1980). Traditional and missionary religions overlap with these institutions to some degree. Most of the population identifies as either Muslim or Christian, though the practice of these religions is intertwined with traditional religious beliefs (Conteh, 2009). Religious organizations such as Bible and Koranic study groups play an important role in community life. Local religions imbue higher powers with chief-like authority—the word for god in Limba means, literally, “The Great Chief”. The chief's powers are also intricately linked to the workings of the secret societies (Jackson, 2006).

3. Experimental design

To examine the impact of white foreigner presence on measured generosity, we randomized the presence of a white foreign ‘supervisor’ in the administration of behavioral games. We play three variants of the dictator game. However, we aggregate proceeds from these games and do not attempt to draw conclusions from each of the games individually. The experiment was carried out in 60 villages across five districts in Sierra Leone: Bombali, Kailahun, Koinadugu, Kono, and Moyamba. Randomization was stratified by district: 12 villages were selected in each district and randomized into treatment

and control. Thus there are six treatment and six control villages in each district. Fig. 2 shows these treatment and control locations.

3.1. Baseline survey

The subjects in our study were recruited in the following manner. We conducted baseline surveys of households and villages, on average, 1.3 months prior to when the games were conducted. These surveys were part of a broader study that examines post-conflict reconciliation in Sierra Leone, though the survey was modified for the purposes of the white-man experiment. For the surveys, we randomly sampled 12 respondents within each village. First, households were sampled using in-field randomization within the village population. Within each sampled household, the respondent was also randomly selected from the roster of household members. These twelve individuals typically served as the players in the games. However, if the original survey respondent could not be found, he or she was replaced by someone else from the same household, and of the same gender as the original respondent. Replacement occurred for 7.5 percent of the sample, and we do not have baseline covariates on these individuals.

3.2. Game sessions

All games were administered by a core team of black Sierra Leoneans comprising one trained facilitator, two assistants, and a local translator recruited from the area where the games were played. Besides the facilitator, the three other core members remained constant across all the games, in the treatment and control areas. Thus all the games were run by the same facilitator, who read out instructions to the player, and interacted extensively with them. The assistants played relatively minor roles such as preparing the envelopes in which players made their allocations and remaining in the room to ensure players did not talk with one another.

In addition, the team included a ‘supervisor’ whose identity varied systematically across control and treatment villages. In the control villages, the supervisor was a Sierra Leonean from Freetown, the national capital. In the treatment villages, the supervisor was a white American. A clear concern is that some other unobserved characteristic of the two individuals may have driven participants’ responses. Our design took several precautions against this. First, neither ‘supervisor’ was made aware of the experiment being conducted in order to minimize the chance of expectancy effects (Rosenthal and Rubin, 1978). At the same time, to prevent any potential difference in behavior, such as proactivity, from confounding the ‘white-man’ treatment, all games were conducted according to a strict protocol and set script that specified a silent, background role for the supervisors. Their sole tasks were to take notes and distribute money to players in the beginning of each game. They did not talk during the administration of the game, or otherwise interact with game participants in any way. They were also not allowed to be in the room when players made their allocation decisions. In other words, though they were called supervisors, their actual role was extremely limited. Thus, our experiment examines how the mere presence of a white foreigner affects giving in these games.

Second, both supervisors were chosen for similarity on key characteristics: they were both young, college-educated males in their 20s. Importantly, Sierra Leone’s rural population is homogeneously black African and it is therefore reasonable that the starkly visible difference in the race of the white supervisor is the most salient difference between them. Though it is not possible for our design to disaggregate the impact of race from that of nationality, in research and aid settings in developing countries this tends to be a common bundle—in our setting, it would be rare to for example, find a “white local”.

However, the design of the experiment cannot fully rule out the possibility that other characteristics besides race and nationality could give rise to observed differences in giving across the two games. Therefore, we also formulate several additional empirical predictions that should hold if players act based on their perceptions of the researcher as a white foreigner. We draw on these additional tests to provide evidence of the white-man mechanism.

Our measure of generosity aggregates giving in three versions of the standard dictator game. The advantage of the dictator game is that it provides a clean measure of other-regarding preferences, since payoffs to play are independent of beliefs over other players’ expected behavior.

In each of the specific games, the player was given 4000 Leones (approximately \$1), which is an amount slightly higher than the average daily income in Sierra Leone. The player could decide how much of this money to keep and how much to ‘give’ to recipients.

The three specific types of the dictator game were played in the following order:

- 1 Game 1: Anonymous Own-village. The recipients were 12 additional individuals randomly sampled from the players’ village. The players were told who these 12 recipients were, but not which recipient they would be matched to. Players were also informed that they would be re-matched to a different recipient in each game. Player contributions were strictly anonymous, i.e. no player was informed how much any other player had chosen to contribute.
- 2 Game 2: Non-anonymous Own-village. Here, we relaxed the between-subject anonymity condition. Instead, players were told that their giving decision would be announced at the end of the session, after all games had been played. All other game elements of Game 1 and 2 remained the same. Recipients were the same individuals from Game 1, and as in that game, players were not told who they were matched to. To minimize feedback across games, the announcement was

made in front of the players and recipients at the end of the session, after all games had been played. Recipients were not present during the game but arrived at the end after all games were played to receive their allocations.

3 Game 3: Anonymous Other-village. Players were told that we had identified 12 recipients in “another village” who would receive the amount they chose to give. All other game elements were the same as in Game 1—no player was informed of any other player’s contribution, who they were matched to, etc.

We sum the proceeds from each of these specific games and examine the impact on total giving. Since the order of the games was not randomized, this raises potential concerns that ordering affects how much was contributed in each game. However, we do not aim to identify different effects across games, and only examine aggregate giving.

Games were conducted in two rooms, a public room where the group received instructions, and a private room where they individually entered to make their allocation decision. No talking was allowed during the explanation of the games, to prevent strategic interaction and framing. To ensure that players understood the game, the facilitator repeated the explanation of the game in the private room, allowing players to ask questions. After the second explanation, the facilitator would exit the room, allowing the player to make her or his allocation decision privately. The players were instructed to place their allocations in an envelope, and place the envelope into the slit of a locked box.

Finally, directly after the games were conducted, we asked participants to respond to a question about what they believed to be the purpose of the research team’s visit. These questions were administered in private to each respondent.

To maximize players’ comprehension, the games were also translated into seven local languages in three of the five districts.⁷ To promote quality and consistency across translations, the games scripts were translated into the local languages and then back-translated again into English. However, the translation quality may have varied across districts to some degree. In three of districts, there is just one major local language, and thus one translator had to be hired. In our samples in Bombali and Koinadugu districts, there are two and three local languages, respectively, requiring the equivalent number of translators. Since changes in translator may have lowered the quality of communication, we verify the robustness of our results to dropping these two districts (in Table A.5 of the Appendix). To account for the possibility that the teams, as a whole, may have improved their communication skills over time, we varied the ordering of treatment and control villages, so that games were conducted first in treatment villages in half the districts, and first in control villages in the other half.

3.3. Empirical strategy

Based on this experimental design, we use the following specification to estimate the impact of the white-man effect:

$$y_{ivld} = \alpha_d + \delta_l + \beta(\text{white-man}_{vld}) + \mathbf{Z}_{vld} \rho + \mathbf{X}_{ivld} \phi + \omega_{ivld} \quad (1)$$

where i is individual, v is village, l is majority language group, and d is district. y_{ivld} is the amount given in the dictator games, white-man_{vld} is an indicator of treatment to assignment, α_d are district fixed effects, and δ_l are majority-language fixed effects. We include district fixed effects to account for substantial regional heterogeneity in economic development, urbanization, war exposure, and other factors that may influence generosity. We include majority-language fixed effects to control for variation in underlying generosity levels across ethnic groups, since language groups correspond closely to ethnic/tribal affiliation in Sierra Leone. Majority-language fixed effects also reduce noise created by the varying quality of communication across games. Specifically, they absorb potential variation in the quality of the translator, since there was translator per language group. \mathbf{Z}_{vld} and \mathbf{X}_{ivld} are additional village and individual level covariates included in some specifications. β captures the white-man treatment effect. The randomization took place at the village level. Thus in all specifications, we cluster the standard errors at this level.

4. Data

The behavioral games data used in the analysis is supplemented by household and village level survey data collected prior to implementation of the games.⁸ We have village-level data for all 60 villages, and household-level data for 715 out of 720 participants, though the sample size varies for particular measures.

To account for ethnicity, we construct an indicator variable for each language spoken by the majority of the respondents in each village. Ethnicity is geographically concentrated and identified by language. The language spoken by the majority of villagers therefore provides a measure of the majority ethnic group in the village. There were seven ethnic groups among our sampled villages (Fullah, Kono, Koranko, Loko, Mende, Temne and Yalunka). However, there was only one Yalunka majority village, and thus, for robustness, we constructed two additional measures of ethnicity using broader linguistic parent groups from the third and fourth levels in the Atlantic language hierarchy, details of which are provided in the Appendix.

We control for violence-related effects by using a measure of the number of buildings burnt in each village during the war. We also measure village size based on the number of households residing in the village, and the level of market integration,

⁷ The facilitator spoke the local language in the other two districts.

⁸ Data collection was part of a larger research project in Sierra Leone, although we adapted the questionnaire to address the specific research questions of this study.

with an indicator for whether there is a market in the village. We also use two proxies for village cooperation: an indicator of whether the village has communal farms and an indicator of whether it has a labor gang, which refers to a group of people who work on each others' farms.

To measure each village's exposure to foreign aid, we construct various measures of NGO involvement in the provision of local public goods.⁹ The broadest indicator is based on whether a NGO either owns a school or clinic, or currently provides resources to the local school or clinic, or has contributed to the construction of public facilities (including schools, clinics, wells, latrines, or the palava hut¹⁰). This variable is called "NGO aid", and 54 villages in our sample (all but six) have some form of aid engagement under this metric.

Since the NGO aid indicator has little variation, we also construct two continuous measures of the extent of aid exposure. The first variable, "Years of aid with NGO-owned facility," refers to the number of years that a NGO has owned either a school or clinic in the village. It is worth noting that NGOs own these public facilities in 31 (or just over half) the sample villages. We also construct "Years of aid with NGO activity," which is the number of years a NGO has either owned a school or a clinic, or the number of years since a NGO contributed to the construction of the school or clinic, if a NGO provides current support to the school or clinic. This variable has the advantage of being comprehensive, capturing NGO activity in five additional villages where they do not own facilities. Therefore, we use it as our primary measure of aid exposure. However, since we cannot observe if the NGO engaged each year between the time it built the facility and its current continued support, we present robustness checks with the NGO owned facility and NGO aid variables in the appendix.

To measure household socioeconomic status, we develop an asset index. As is standard in the literature (Vyas and Kumaranayake, 2006), we use principal component analysis to construct an aggregate measure based on ownership of a broad range of appliances and equipment,¹¹ the material of the roof and floor of the house in which the respondent lives, as well as ownership of land.¹² Data on the respondent's educational attainment is used to generate a continuous measure of years of education completed.

We are also able to examine the player's past exposure to white foreigners, as the survey tracked how often the respondent has met a white person before. In addition, we asked the respondent if someone in their household is a paramount, town, or village chief/headman; a religious leader including reverends, imams or pastors; or the leader of a secret society. We use this to generate a measure of whether the participant is from a household of a traditional authority leader.

To test for the correlation between giving in the games real-world behavior, we construct two indices of individuals' contributions to community groups and public goods. The "Monetary contributions" index aggregates the size of monetary contributions to different groups in the village over the past 3 months (religious, women, parent-teachers association, village development committee, and youth group), as well as an indicator whether a respondent has contributed towards building or maintaining public facilities. The broader "All contributions" index additionally adds in labor contributions to community groups and road brushing. We use the methodology of Kling et al. (2007) to aggregate these different types of measures.¹³

To gauge differential impacts based on player characteristics, we used household survey measures of psychological health to create a depression index based on 5 questions drawn from the Zung (1971) index and a measure of Post-traumatic Stress Disorder (PTSD) based on a list of 11 questions from the 4th Diagnostic and Statistical Manual of Mental Disorders (DSM-IV), as developed by (Foa et al., 1997). We also asked each respondent to list which of the other respondents they consider a very good friend, within their village. We use the number of times each player was named by someone else as a good friend as a proxy of their friendliness.

In the post-game questions, participants were asked: "Why do you think the researchers were playing these games? Choose the statement you agree with most." The following three statements were then read out to them: "To give money to the community in a fun and educational way", "To test the community, to see which community is more deserving of aid", and "To find out more about how members in this community think about each other, interact with each other, and treat each other." The last option corresponds most closely to the stated purpose of the research team's visit prior to the start of the games, which was to find out about the community. In the text and tables, we refer to the first statement as 'Give money', the second as 'Aid test' and the third as 'Research'.

4.1. Descriptive statistics

Tables A.1 and A.2 in the Supporting Information Appendix present descriptive statistics of key variables. Our main dependent variable is total giving, which sums the proceeds from the three dictator games. The average total giving was 3216 Leones out of 12,000 given to players in all three games, or an average 1079 (= \$0.25) per game, which is about a quarter of the daily wage in Sierra Leone.¹⁴ Table 1 examines key individual and village level characteristics in the control

⁹ In the Sierra Leonean context, nearly all large NGOs involved in providing public goods in rural villages are foreign or have foreign funders.

¹⁰ The palava hut is often the only public space in the village for community members to congregate.

¹¹ These include: iron, refrigerator, television, radio, mobile phone, generator, fan, bed, clock, sewing machine, modern stove, bicycle, motorcycle, car/truck, and pushcart.

¹² We cannot use measures such as income or wages to control for wealth since the majority of respondents are subsistence farmers.

¹³ The Kling et al. (2007) method standardizes each of the indicators and sums across them after imputing missing values.

¹⁴ The Anonymous Other-village game is only available for 708 out of 720 observations owing to enumeration error, since the values for this game were not recorded in one of the villages.

Table 1
Key individual and village-level characteristics.

	Treatment	Control	Difference	Std. Err.
<i>Individual</i>				
Years of education	2.06	1.90	0.160	(0.318)
Female	0.55	0.54	0.007	(0.039)
Ethnic majority	0.87	0.91	-0.039	(0.034)
Household asset index	0.00	-0.00	-0.003	(0.232)
Age	41.23	43.43	-2.228*	(1.172)
Customary authority	0.23	0.28	-0.053	(0.032)
Monetary contr. index	0.02	-0.00	0.020	(0.055)
All contr. index	0.04	-0.00	0.037	(0.041)
<i>Village</i>				
NGO aid	0.93	0.87	0.067	(0.076)
Years NGO-owned facilities	10.13	14.23	-4.100	(5.633)
Years NGO activity	11.13	15.70	-4.567	(5.570)
White aid visitors	0.34	0.37	-0.012	(0.116)
White non-aid visitors	0.41	0.37	0.046	(0.127)
Number of households	334.23	234.30	99.933	(94.736)
Market community	0.17	0.10	0.067	(0.087)
Buildings burned during war	6.10	5.43	0.667	(3.559)
Labor gang	0.93	0.97	-0.033	(0.054)
Communal farm	0.27	0.47	-0.200*	(0.106)
<i>Dominant ethnic group</i>				
Fullah	0.03	0.03	-0.000	(0.044)
Kono	0.20	0.20	-0.000	(0.000)
Koranko	0.13	0.17	-0.033	(0.050)
Loko	0.07	0.07	-0.000	(0.055)
Mende	0.40	0.40	0.000	(0.000)
Temne	0.13	0.13	-0.000	(0.055)
Yalunka	0.03	0.00	0.033	(0.032)

Note: Columns 1–2 present treatment and control group means, respectively. Column 3 shows coefficients from a regression of each variable on the white-man treatment indicator. Column 4 displays robust standard errors clustered at the village level.

* Significant at the 10% level.

and treatment groups. There are no statistically significant differences in these characteristics across the two groups with the exception of respondent age and whether the village has a communal farm. Though these differences are only significant at the 10 percent level, in Appendix Table A.4 we show that the results are robust to controlling for these characteristics.

5. Result: white-man presence and giving

We begin with a simple graphical exploration of whether the white-man treatment induced differential giving. Fig. 3 shows the CDF of total giving in treatment and control regions. The treatment distribution first-order stochastically dominates the control distribution: a higher proportion of the treatment group gave more, evaluated at each level of giving, indicating that the white-man treatment induced an upward shift in the distribution of giving in the dictator games. The Kolmogorov–Smirnov equality of distribution test rejects the null hypothesis that these two distributions are equal.¹⁵ While the CDF presents visualization of the raw data, Table 2 presents regression estimates of Eq. (1). Column (1) presents estimates on total giving without ethnicity fixed effects. The coefficient indicates that players gave 537 leones more in the games where the white man was present. Column (2) adds in ethnicity fixed effects, which increase the precision of the estimates. Here, the coefficient of 564 indicates a 19 percent increase in total giving, relative to the control mean (2944 Leones).

In column (3), we show that this main result is not affected by controlling for replacements, who played the games but were not surveyed at baseline. Column (4) shows that the treatment effect is not significantly different for this group while column (5) demonstrates the robustness of the effect to dropping these 55 individuals. This is an important check since we are not able to include replacements in several of the remaining empirical tests, which require baseline covariates. Thus Column (5) represents our preferred specification.¹⁶

We conduct a number of additional robustness checks. Appendix Table A.4 shows that the results are robust to the inclusion of respondent age and communal farm indicator, as well as alternative ethnicity fixed effects, generated on the basis of broader linguistic categories. Table A.5 also demonstrates that the effects are robust to dropping the two districts

¹⁵ The test statistics reject the null that giving in the control group is smaller than giving in the treatment group with a p -value of 0.043, and rejects the null that the samples are drawn from the same distribution with a p -value of 0.079.

¹⁶ Table A.3 in the appendix examines effects for each specific variant of the dictator game. The effect appears to be most precisely estimated for the anonymous own-village dictator game. However, Table A.6 also shows that when we pool the games together, we are not able to discern statistically distinguishable treatment effects across the three specific game types.

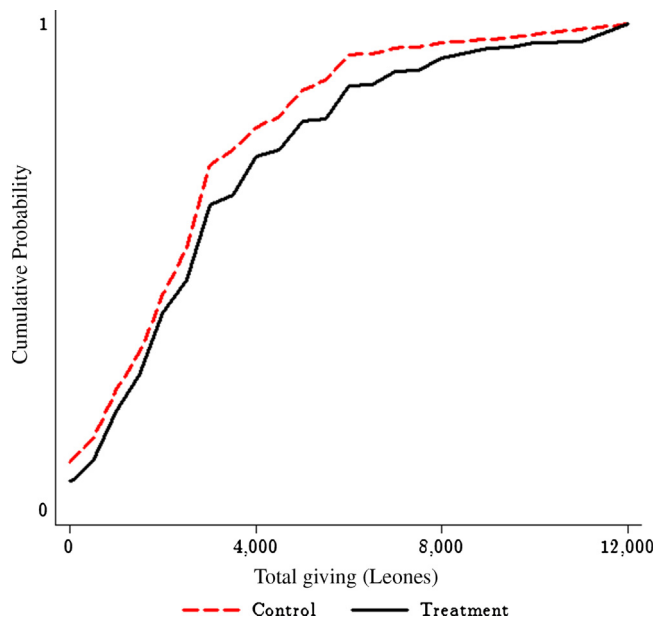


Fig. 3. CDF of total giving in control and treatment groups. *Note:* This figure shows the cumulative distribution function of total giving in the control and treatment groups.

Table 2
White man presence and measured generosity.

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving	(5) Total giving
White-man	537.320*	564.943*	564.681*	555.390*	548.941*
Replacement	(312.028)	(296.734)	(297.182)	(293.607)	(292.644)
Replacement × white-man				-40.539 (406.934)	127.623 (630.268)
Control group mean	2943.966	2943.966	2943.966	2943.966	2943.966
Ethnicity fixed effects	No	Yes	Yes	Yes	Yes
Replacements dropped	No	No	No	No	Yes
Observations	708	708	708	708	653

Note: Robust standard errors clustered at the village level in parentheses. All specifications include district fixed effects. Columns (2)–(5) include ethnicity fixed effects. Replacement equals one if the game participant was a replacement household member.

* Significant at the 10% level.

(Bomabali and Koinadugu) where translation quality may have been lower because larger linguistic diversity required different interpreters to join the teams intermittently.¹⁷

In the sub-sections below, we additionally draw on our household and village survey data to explore mechanisms through which white foreigner presence affects measured generosity.

6. Mechanism

We hypothesize that giving increases in treatment areas because players act according to their perceptions of the white person—they give based on what they perceive he wants them to do, and based on their beliefs about what he is there to do. There are two potential demand effects that imply opposite effects on giving. First, if players perceive that the white person wants them to give more, they may increase their game allocation to please him. Second, if they perceive he is testing them to see if they need aid, they may give less in order to signal need. This latter ‘need-signaling’ effect is more likely in villages that have received aid in the past, as this is where people have had a chance to learn about means testing. In the sub-sections below, we present two empirical predictions designed to test this hypothesis.

¹⁷ Our results are also robust to iteratively dropping each individual village with 12 players, which suggests that outliers do not drive the result. When we re-estimate the treatment effect dropping each village, the *p*-value exceeds 0.10 in only three out of 60 cases, and marginally so, rising to 0.102, 0.106, and 0.11. These three villages are also in different districts, suggesting that there is no systematic bias at the district level.

6.1. Giving to impress the white man

In this sub-section, we explore the first potential demand effect. We develop the following implied empirical prediction: if people give more to impress the white person, this should reduce the correlation between real world contributions and giving in the games.

Why would we expect a weakening of this correlation? In the white foreigner's absence, players give more in the games if they have more pro-social preferences. In his presence, however, people may also give out of a desire to impress him. Behavior in the experiment thus becomes a noisier measure of true generosity, as selfish people may now also act generously. This weakens the correlation between their past contributions and giving in the white-man games.¹⁸ To test this hypothesis, we devise a raw prediction test. Our aim is to see if real-world contributions predict giving in the games where the white foreigner is absent, but not where he is present. To implement this, we draw on several different measures of how people contribute to community groups and public goods in their communities. As shown in Appendix Table A.1, the most common forms of contributing are giving resources to the construction of public facilities; contributing labor toward road-brushing; and making monetary contributions to religious organizations, with 47%, 35% and 31% contributing in these ways, respectively.¹⁹ Individually, each of these variables is a noisy measure of people's willingness to give. We therefore aggregate them together into two indices. The "Monetary contributions index" includes the amount of money contributed to each of the community groups, as well as the indicator for contributions to public facilities. The "All contributions index" additionally adds in labor contributions to community groups and road brushing.²⁰

Table 3 presents these results. The coefficients in the first two columns indicate that both indices significantly predict giving in absence of the white foreigner, but this significant positive correlation does not hold in the treatment villages. For example, the coefficient on the monetary contributions index in column (1) tell us that a 1 standard deviation increase in this variable is associated with 453 higher giving in the games where he is absent. The significant coefficient on the interaction term with the white-man treatment indicates that the predicted correlation is smaller in the games where he is present. Finally, a F-test of the sum of these two coefficients ($453 - 429 = 24$) is insignificant, indicating contributions have no predictive power in the white-man sample. The same account holds for the more aggregate measure in column (2). Here the sum of the two coefficients on the contributions index and its interaction is -51 and statistically insignificant—again, indicating no predictive power of behavior on giving in the white man sample.

We place the most weight on these aggregate measures since the index helps reduce noise underlying any one measure. This can be seen when we present the same specification for the three most important contributions comprising the index in Table 3. In each case, the indicator predicts giving in the games where the white foreigner is absent. And, in each case, the sum of the coefficient on the indicator and its interaction with white-man indicate no predictive power in his presence. However, among the individual indicators, the interactions are insignificant, though they are all negative and substantial in magnitude.²¹

Overall, this prediction exercise demonstrates that individuals deviate from giving based on their real world behavior in the presence of the white foreigner.

6.2. Giving based on perceptions of aid testing

We posit a second demand effect arises from the perception that the white person is there to assess their need for aid. In this case, players may act out of a desire to seem aid-worthy. If they know aid is means-tested, they would act poor to signal that they cannot give money away easily—they would give less in the games where the white man is present. This type of need-signaling is more likely to arise when people have had more experience with aid, and had a chance to learn about means-testing. This serves as our second empirical prediction. If people act based on perceptions that the white person is testing them for aid, we should observe less giving in his presence among more aid-exposed villages. We should also observe that they are more likely to believe the games where he is present aim to test them for aid. To examine this prediction, we draw on both giving in the games as well as on questions asking respondents what they believed to be the purpose of the games.

We begin with a simple, but powerful test of this prediction in Table 4. We ask, is there any observed relationship between aid and giving in the control sample where the white man is absent, and in the treatment sample where he is present? We use our most comprehensive measure of aid engagement, the years of NGO activity.

¹⁸ Even if preferences were additive, so the desire to please the white person were added to their own pro-social preferences, we would still expect a weakening of the correlation based on ceiling effects: there isn't much room to increase giving in response to the white person, among those who already give a lot because they are generous.

¹⁹ After that there is a steep drop-off with 20% contributing labor to religious groups and 10% or less contributing to each of the other groups.

²⁰ In creating these indices, we drop one outlying observation, who was reported to give 600,000 Leones to religious groups. This is 22 standard deviations above the mean and 10-fold larger than the value contributed at the 99th percentile. It is also twice the maximum of any other giving variable (300,000 for PTAs). In Table A.5, we verify that dropping this outlier doesn't affect our main effect on total giving.

²¹ The other indicators, which are relatively unimportant sources of contributions, are not significantly correlated with giving even in control areas. (This is not shown but available from the authors upon request). Thus individually, they are uninformative as tests of how the correlation falls in treated areas.

Table 3
Predicting measured generosity with real-world contributions.

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving	(5) Total giving
Monetary contributions index	453.211 [*] (241.460)				
Monetary contributions index × white-man	−429.704 [*] (245.217)				
All contributions index		582.818 ^{**} (263.763)			
All contributions index × white-man		−633.684 ^{**} (297.104)			
Public facility			540.712 [*] (272.658)		
Public facility × white-man			−183.594 (419.680)		
Brushed road				729.763 ^{**} (301.932)	
Brushed road × white-man				−367.474 (417.499)	
Religious money					0.037 ^{**} (0.016)
Religious money × white-man					−0.013 (0.025)
White-man	540.930 [*] (291.159)	542.827 [*] (292.115)	633.846 [*] (318.936)	648.127 [*] (326.096)	601.237 ^{**} (280.947)
Proportion who contribute	–	–	.47	.348	.312
Observations	652	652	647	644	652

Note: Robust standard errors clustered at the village level in parentheses. “Religious money” is the amount of financial contributions to religious groups over the past three months; “Monetary contributions” is an index of financial contributions to all village groups; “Public facility” indicates whether the respondent has contributed labor or time towards construction or maintenance of facilities; “Brushed road” indicates if someone has brushed a village road the past month; “All contributions” is a broader index of all financial and labor contributions.

^{*} Significant at the 10% level.

^{**} Significant at the 5% level.

Table 4
Aid exposure and generosity in control and treatment areas.

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving	(5) Total giving	(6) Total giving
Years NGO activity	5.433 (6.467)	−20.152 ^{**} (9.488)	5.577 (7.667)	−24.879 ^{**} (11.984)	4.248 (6.746)	−26.208 ^{**} (12.202)
Met white person 1–10 times					−212.215 (243.837)	173.181 (425.442)
Never met white person					−572.625 (453.383)	−340.905 (458.827)
Individual controls	No	No	No	No	Yes	Yes
Village controls	No	No	Yes	Yes	Yes	Yes
White-man sample	No	Yes	No	Yes	No	Yes
Observations	324	329	324	329	313	324

Note: The odd (even)-numbered columns reports regression coefficients on a restricted sample of the control (treatment) communities. All specifications include district and ethnicity fixed effects. Village-level controls include village size, buildings burned during the war, indicators for whether there is a market, a labor gang, and a communal farm, and their interactions with the white-man treatment. Columns (5) and (6) control for respondents' socio-economic status (asset index and education) and his/her previous exposure to white foreigners. Robust standard errors clustered at the village level in parentheses.

^{**} Significant at the 5% level.

We find a clear pattern in columns (1)–(2). Aid exposure has no significant relationship on giving in control areas. But it has a significant negative relationship with giving in treatment areas. Those residing in more aid-exposed villages give less, but only when the white person is present.

Villages receiving aid for longer periods have other characteristics that affect measured generosity. For example, these communities might be larger, less integrated, more cooperative, richer, or have more exposure to white foreigners. These features could serve as confounds to the observed pattern if they also lead people to give less in the presence of the white person. For example, people in more cooperative societies may give less because they respond negatively to the presence of an outsider. To account for these potential effects, in columns (3)–(4), we control for a number of village-level characteristics, including: village size and the presence of a market, which are important indicators of economic development and the degree of market integration; whether the village has communal farms or a labor gang, which correlate with societal cooperation;

Table 5
Aid and the white man effect on generosity.

	(1)	(2)	(3)	(4)	(5)	(6)
	Total giving	Total giving	Total giving	Total giving	Total giving	Total giving
White-man	794.805** (332.336)	675.298 (963.310)	730.540 (965.073)	1103.483** (381.450)	855.183 (896.331)	972.255 (922.845)
Years NGO activity × white-man	-22.357** (9.757)	-21.302** (9.771)	-22.227** (10.020)	-70.006* (35.154)	-93.031* (46.571)	-83.731* (42.509)
Years NGO activity	6.057 (6.673)	5.512 (7.091)	4.259 (6.299)	50.097 (31.970)	72.195 (45.253)	61.548 (41.329)
Met white person 1–10 times × white-man			362.405 (500.050)			291.325 (511.249)
Never met white person × white-man			125.847 (647.737)			-234.626 (656.409)
Met white person 1 to 10 times			-203.631 (251.762)			-174.842 (252.847)
Never met white person			-495.643 (429.583)			-189.658 (454.282)
Individual controls	No	No	Yes	No	No	Yes
Village controls	No	Yes	Yes	No	Yes	Yes
Outliers dropped	No	No	No	Yes	Yes	Yes
Observations	653	653	637	620	620	604

Note: See Table 4 for notes on control variables. Robust standard errors clustered at the village level in parentheses.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

and the number of buildings burnt during the civil war, as post-conflict aid may have been targeted to areas with higher levels of historical violence.

In columns (5)–(6) we also introduce four key individual-level covariates. These include two proxies for economic wellbeing—a household asset index and years of formal education attained.

It is important to consider the possibility that those residing in aid-dependent villages may have had more previous exposure to white persons, since aid is mainly administered by foreigners. This could serve as an alternative explanation of our results, since familiarity with white persons could also lead to a diminished generosity response.

To account for this potential confound, we also include two indicators measuring past exposure to foreigners—never met a white person before, and met a white person between one and 10 times (both defined relative to the omitted category of met a white person more than 10 times). It is interesting to note that these two variables exert no significant impacts on our generosity measures.²² Moreover, the inclusion of the controls do not affect the pattern that giving decreases with aid exposure, but only when the man is present.

This pattern is also difficult to square with alternative accounts that giving may have increased owing to some other characteristic of the white-man. For example, consider the possibility that allocations increased in treatment areas because the white person had a more pleasant demeanor, and not because of his race and nationality. Then, it is hard to understand why aid exposure would have a negative impact on giving in the presence of a more pleasant persona.

In Table 5, we continue building on the results in Table 4. We introduce interaction terms between aid exposure and the white-man treatment to demonstrate that the effects observed in the previous table are significantly different from one another. Column 1 presents the results without controls. Column 2 includes the village level controls, and columns 3 include both the village and main individual level controls. The control variables are also included interactively with the white-man treatment indicator.²³ The significant coefficient on the Years NGO activity × white-man term tells us that across these specifications, the increase in giving is relatively smaller among places that have received aid for a longer period of time.

These effects are consistent with the idea that the two types of demand effects exert opposite effects on giving. For example, consider the specification in column 1. The coefficient on the White-man variable tells us the treatment effect among villages that had zero years of NGO aid, which represents about 40% of our sample. In these places, giving increased by 795, or 32% (relative to the overall control group mean of 2944). We can think of this effect as arising from the first type of demand effect—people give more because this is what they believe the white person wants them to do. But there is a second, offsetting force captured by the negative interaction term. The coefficient on this term indicates that each

²² These questions about meeting white persons before were administered in the baseline survey in treatment and control areas. The survey, on average, preceded the games by 1.3 months, though there is variation from under a week to nearly three months. If answering these questions increased the salience of issues related to white foreigners, we would expect to see that an interaction between the treatment effect and time between survey and games. Table A.4 shows that there is no significant effect of this interaction.

²³ Since these controls are only available for a sub-sample of the observations, column (4) of Table A.5 verifies that our main treatment effect holds in this sub-sample.

Table 6
White-man presence and participant beliefs.

	Logit	Multinomial Logit		
	(1) Aid Test	(2) Aid Test over Give Money	(3) Aid Test over Research	(4) Give Money over Research
White-man	0.439** (0.156)	0.455** (0.161)	0.392** (0.155)	0.862 (0.159)
Observations	664	664	664	664

Notes. Robust standard errors clustered at the village level in parentheses. Columns 1–4 display odds/relative risk ratios for a unit increase in the independent variable. Column 1 displays the odds ratio from a logit regression on the “Aid Test” indicator variable. Columns 2–4 display the relative risk ratios for each pair of choices from a multinomial logit regression on the categorical variable of participant beliefs over the ‘aid test’, ‘give money’ and ‘research’ choices. All specifications include district and ethnicity fixed effects.

** Significant at the 5% level.

year of aid engagement lowers the treatment effect by 22. We can think of this effect as arising from the second type of demand effect—people give less to signal need because they believe the white person is testing them for aid. Given these two opposite forces, the second effect comes to dominate in the most highly aid exposed villages. Those residing in villages that have received aid for 36 years or longer (above the 86th percentile of the aid distribution), give *less* in the presence of the white person.

Columns 4–6 repeat these three specifications, but dropping the top 5 percent most aid-exposed areas, comprising 3 villages. The results are robust to their exclusion. The coefficients in column (4) indicate that the treatment effect becomes negative for those residing in villages that have received 16 or more years of aid exhibit lower giving in treatment.²⁴ This corresponds to the 80th percentile of the aid distribution excluding outlier villages. In other words, in this specification, the top 20 percent most aid-exposed villages give *less* in the presence of the white foreigner. This is again consistent with the idea those who have the most experience with development aid face an incentive to act poor, and signal that they have relatively little to give away by lowering their game allocations when the white foreigner is in the room. The first two columns of Appendix Table A.7 also show that the pattern of results remains the same using two alternate measures of aid exposure—the NGO aid indicator and the years of aid based on NGO ownership.

If this behavior reflects players’ beliefs that the white person is there to assess their need for aid, then individuals from highly aid-exposed areas should also be more inclined to believe that the purpose of the games with the white foreigner present is to test them for aid suitability. To examine this prediction, we next turn to data on perceived beliefs regarding the purpose of the games.

First, we examine overall respondent choice across treatment and control communities (not disaggregated by aid exposure). Column (1) of Table 6 uses a simple logit model where the dependent variable equals one if the respondent chooses ‘Aid test’ and zero otherwise. We find a significant treatment effect of 0.439, equivalent to 56.1% lower odds of choosing this option in the presence of the white researcher. However, since this logit specification groups together the money and research options, we next employ a multinomial logit model, using a categorical dependent variable of participant belief that includes all three choices. Columns (2)–(4) show the relative coefficients for each option pair: ‘Aid test’ over ‘Research’, ‘Aid test’ over ‘Give money’ and ‘Give money’ over ‘Research’. The results are consistent with those in the first column—in the presence of the white foreigner, the relative risk ratio of choosing ‘Aid test’ is 54.5% lower than choosing ‘Give money’, and 60.8% lower than choosing ‘Research’.²⁵ However, the treatment has no significant effect in the choice between ‘Give money’ and ‘Research’. Thus, in the presence of the white foreigner, players on average were more inclined to believe that the games aimed to find out about the community, which was the stated intent of the team’s visit, or to believe that the games were designed to hand out money, which is what the team actually did. This is in contrast to perceiving an underlying agenda that the team was there to test them for aid-worthiness.

However, the pattern of beliefs regarding aid-testing vary substantially depending on the village’s past exposure to development aid. Table 7 examines this heterogeneity by introducing interaction terms between the white-man treatment and years of aid with NGO activity. We present these results for the full sample, but Appendix Table A.8 shows that the results are robust to dropping the three most aid-exposed villages. Columns (1)–(4) present the results without any controls, while columns (5)–(8) show the results with the village level controls and core individual controls for wealth and past exposure to foreigners.

The results show a clear pattern: while communities with no exposure are less likely to choose the ‘Aid test’ option, this effect is countered with additional years of aid experience.

Column (1) presents the logit on ‘Aid test’. The coefficient on the white-man term implies that people living in communities with no aid exposure have a 76% lower odds of believing that the purpose of the game is to test people for aid, in the presence of the white foreigner. However, each additional year of aid erodes this effect by 4.8%. Thus, communities that have received more than 16 years of aid (representing the top 23% of the aid distribution), associate the white foreigner positively with the

²⁴ The treatment effect in non aid-exposed villages is 1103, and the coefficient on the interaction terms tells us each year of aid reduces it by 70.

²⁵ The relative risk ratio in the multinomial logit specification is analogous to the odds ratio in the logit specification.

Table 7
White-man presence and beliefs around aid testing.

	Logit	Multinomial logit			Logit	Multinomial logit		
	(1) Aid Test	(2) Aid Test over Give Money	(3) Aid Test over Research	(4) Give Money over Research	(5) Aid Test	(6) Aid Test over Give Money	(7) Aid Test over Research	(8) Give Money over Research
White-man	0.237 ^{***} (0.091)	0.253 ^{**} (0.010)	0.189 ^{***} (0.077)	0.744 (0.168)	0.662 (0.450)	0.596 (0.415)	0.647 (0.572)	1.086 (0.637)
Years NGO activity × white-man	1.048 ^{**} (0.020)	1.047 ^{**} (0.020)	1.051 ^{**} (0.022)	1.004 (0.006)	1.060 ^{**} (0.018)	1.057 ^{**} (0.018)	1.062 ^{**} (0.021)	1.004 (0.006)
Years NGO activity	0.968 ^{***} (0.010)	0.970 ^{**} (0.010)	0.964 ^{***} (0.011)	0.994 [*] (0.004)	0.972 ^{**} (0.01)	0.972 ^{***} (0.010)	0.967 ^{***} (0.011)	0.994 ^{**} (0.003)
Met white person 1–10 times × white-man					1.682 (1.682)	1.71 (0.939)	1.506 (1.023)	0.88 (0.363)
Never met white person × white-man					8.01 [*] (8.505)	7.586 [*] (8)	9.485 [*] (11.74)	1.21 (0.827)
Met white person 1–10 times					1.415 (0.490)	1.371 (0.473)	1.619 (0.717)	1.181 (0.338)
Never met white person					0.604 (0.540)	0.6 (0.531)	0.643 (0.674)	1.072 (0.554)
Individual-level controls	N	N	N	N	Y	Y	Y	Y
Village-level controls	N	N	N	N	Y	Y	Y	Y
Observations	652	652	652	652	636	636	636	636

Notes. Robust standard errors clustered at the village level in parentheses. See Tables 4 and 6 for additional notes and text for specification and controls.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

Table 8
Authority differentials.

	OLS	OLS	Logit	Multinomial Logit		
	(1) Total giving	(2) Total giving	(3) Aid Test	(4) Aid Test over Give Money	(5) Aid Test over Research	(6) Give Money over Research
White-man	918.656*** (314.997)	45.131 (1631.26)	0.6338 (0.940)	0.57 (0.815)	0.616 (1.203)	1.081 (1.364)
Customary authority × white-man	-1,411.335*** (472.147)	-1366.726*** (500.895)	2.445 (1.751)	2.007 (1.50)	4.688* (3.751)	2.335 (1.247)
Customary authority	948.297*** (289.489)	800.867** (306.241)	1.342 (.471)	1.365 (0.470)	1.23 (0.584)	0.9 (0.299)
Met white person 1–10 times × white-man		323.952 (484.825)	1.23 (0.806)	1.248 (0.828)	1.115 (0.846)	0.894 (0.383)
Never met white person × white-man		153.234 (630.370)	1.23 (0.806)	5.345 (5.784)	5.427 (6.561)	1.015 (0.664)
Met white person 1–10 times		-62.055 (248.416)	5.389 (5.764)	1.261 (0.412)	1.512 (0.610)	1.198 (0.325)
Never met white person		351.916 (401.087)	0.469 (0.397)	0.457 (0.384)	0.504 (0.498)	1.103 (0.537)
Individual-level controls	N	Y	Y	Y	Y	Y
Village-level controls	N	Y	Y	Y	Y	Y
Observations	653	634	634	634	634	634

Notes. Robust standard errors clustered at the village level in parentheses. Customary authority indicates that the respondent is from the household of a chief or secret society leader. Other individual-level controls include respondent age, gender, past exposure to white persons, ethnic majority group dummy, years of education attained, a household asset index, and their respective interactions with the white-man variable. Village-level controls include village size, buildings burned during the war, indicators for whether there is a market, a labor gang, and a communal farm, and their interactions with the white-man treatment. All specifications include district and ethnicity fixed effects.

* Significant at the 10% level.

** Significant at the 5% level.

*** Significant at the 1% level.

'Aid test' option. In other words, individuals from highly aid-exposed villages are more inclined to believe that the purpose of the games is to test them for aid, when the white man is present.²⁶

Columns (2)–(4) show that the results are similar with the equivalent multinomial logit specification: each year of aid exposure increases the relative risk of choosing 'Aid test' over 'Give money' by 4.7 percent, and over 'Research' by 5.1 percent, in treatment versus control areas. Again, the associated relative risk of choosing the research versus money options are not significantly different. Columns (5)–(8) also indicate that the results are robust to the controls.²⁷

The coefficients in Appendix Table A.8 show that the implied effects are similar when we drop the outlier villages. For the logit specification, the coefficients suggest that the top 20% of the aid exposure distribution excluding outliers are more inclined to believe that the purpose of the games is to test them for aid when the white man is present. Table A.7 also verifies same pattern of results hold with two alternative measures of aid exposure (the NGO aid indicator and Years of aid with NGO activity). Overall, these results provide robust evidence that the those in the most aid exposed communities gave less in the presence of the white person and were more inclined to believe that the games were designed to assess them for aid when he was present. This is consistent with the idea that those who have experience with aid disbursement have an incentive to act poor by giving less in the games, as a signal that they are in need of further assistance. They also help rule out the alternative account that the treatment induces smaller effects on generosity in more aid-exposed villages simply because individuals there are more familiar with white people.

These differential impacts are again difficult to square with the idea that some other characteristic induces the white-man effect, beyond his identity as white foreigner. If another characteristics were salient, it would be surprising to observe heterogeneous impacts of past aid exposure on both aid-test beliefs and measured generosity.

²⁶ In table A.10 we also show the fraction of individuals in treatment and control areas opting for the three belief options, in the highly aid exposed villages (top 20% of the aid distribution) and the other villages. The fractions confirm the same pattern: in the aid exposed areas, the white-man treatment is associated with a higher fraction of individuals who believe that the purpose of the games is to test for aid suitability.

²⁷ The evidence on beliefs also help us rule out an alternative interpretation of the results, that respondents give more in response to the white man treatment to signal they are altruistic and thus more deserving of aid. Under this "altruism-signaling" account, the less aid-exposed participants display differential levels of generosity in the hopes of securing future aid from the white foreigner. However, if this account held, then the less aid-exposed participants should also be differentially inclined to believe that the purpose of the games is to test them for aid. Instead, we find precisely the opposite result.

Table 9
Alternative accounts.

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving	(5) Total giving
White-man	555.193 (339.984)	667.584 (473.190)	679.957 (539.930)	786.885 (524.731)	378.466 (347.408)
Female × white-man	23.836 (397.531)				
Female	−738.990*** (275.378)				
No. of friends × white-man		−74.942 (159.803)			
No. of friends		−57.865 (106.043)			
Depression × white-man			−10.045 (88.491)		
Depression index			−131.128*** (44.922)		
PTSD × white-man				−25.405 (54.626)	
PTSD				−14.078 (33.143)	
Years of education × white-man					93.191 (65.370)
Years of education					6.165 (44.908)
Observations	649	653	636	615	645

Note: Robust standard errors clustered at the village level in parentheses.

*** Significant at the 1% level.

Next, we conduct a third, related empirical test. If players give based on perceptions of aid-testing, then village leaders should also give less, as they are more likely to be aware of how aid is given out. If, indeed, this is related to their beliefs about being tested for aid by the white foreigner, we should again observe that they are more inclined to believe that the purpose of the games is to test them for aid, when he is present.

To examine this account, we use our household survey to generate an indicator of whether the respondent comes from the household of a customary authority leader, which includes members belonging to the three groups that traditionally symbolize power and authority in Sierra Leone: chiefs, religious leaders, and leaders of secret societies. In Table 8, we interact the treatment with this “customary authority” variable. In column (1), the coefficient on the White-man variable indicates that those who are not from leadership households give 919 more in treatment areas, which represents a 31 percent increase over the control mean. The sum of this coefficient and the coefficient on the interaction term tell us that leadership households, in contrast, give 492 less in the presence of the white foreigner.

It is possible that this sub-group may be richer, and may have been exposed more to white persons in the past. It is also likely that they are from the dominant ethnic group in the village, and that older males are over-represented among this group. To account for these potential confounds, we incorporate our main individual controls, and also include three additional individual-level variables—gender, age and whether the respondent is from the village’s ethnic majority, all interacted with the white-man treatment. In addition, we retain our village level controls. Column (2) shows that the interaction effect remains large and significant with the addition of these other covariates.

Here, both types of demand effects may be at play. First, leadership households have a relatively high status and may not perceive much of a power difference between themselves and the white person. If the demand effect to give more in the presence of the white man comes from perceived power differentials, we expect the response to be relatively smaller among higher status players. However, this by itself would imply a smaller positive response, rather than a net negative response. In addition, leadership households may also give less in response to the white person because they perceive he is there to test them for aid, and they are more familiar with how aid disbursement works. In that case, they may give less to signal that their community is in need of assistance. Columns (3)–(6) of Table 8 provide some evidence favoring this account. Here, we examine how the customary authority interactions affect beliefs regarding the purpose of the games. The evidence here is weaker, in that we see no significant effect on the ‘Aid test’ option relative to the ‘Give money’ option. However, column (5) indicates that individuals from leadership households disproportionately believed that the games conducted in the presence of the white person were designed to test them for aid relative to the research option.

6.3. Ruling out alternative accounts

In this section, we devise another empirical test to address the account that other characteristics of the white person, besides his race and nationality, lead to the observed effect.

If giving responded to another personality trait in the white person, we would plausibly expect the treatment effect to vary based on this personality trait among players. For example, a more friendly player may respond to a more friendly researcher, if friendliness were the key factor. Or, happier players may respond more to a happier researcher, with a more smiling countenance, if happiness and demeanor were at play. To test these effects, we construct psychometric measures of depression and PTSD, and a measure of the number of good friendships each player has within the community, as a proxy for friendliness. Importantly, all of these measures are collected from players in the baseline survey prior to the games. In addition, we look at two demographic traits—gender and education. If the white man was perceived to be more physically attractive, we may expect differential responses among female players. Or, if he conveyed he was more educated, even in non-speaking role, we might expect differential impacts among players who are educated. However, Table 9 shows that there are no significant effects based on any of these traits. This range of null interactions casts doubt on the importance of these other characteristics in accounting for increased giving in the presence of the white person.

7. Conclusion

This paper has examined whether researcher identity affects measured generosity. We use a lab-in-the-field experiment which varied the presence of a white foreigner across behavioral games in 60 Sierra Leonean villages. This white foreigner played a silent role, without directly interacting with the players. Yet, his mere presence boosted total giving by 19 percent in the dictator games.

Our findings suggest that players act based on their perception of this individual as a white foreigner. We find results consistent with two types of demand effects. First, players appear to give more because they perceive this is what he wants them to do. For example, we find that real-world contributions strongly predict giving in games where the white foreigner is absent, but do not in the games where he is present. As such, giving is less indicative of true generosity in the white foreigner's presence.

Second, we find that players from more aid-exposed villages and households of village leaders give less in the presence of the white person. When asked about the purpose of the games, they are also more likely to report that the games where he is present test them for aid suitability. This is consistent with the idea that those knowledgeable about aid perceive the games as a form of means-testing and give less to signal they are poor and in need of further assistance.

These results support the white-man mechanism: if measured generosity increased due to other researcher characteristics besides nationality and race, we would not expect the treatment effect to vary with exposure to development aid. We also present evidence against alternative mechanisms by examining if the effects vary based on other player traits. We find no differential impacts based on characteristics such as player's friendliness or gender. Taken together, these results suggest that the researcher's identity of a white foreigner, rather than other traits such as demeanor, explain the main result. Future work replicating these findings will help us understand how broadly these results hold in other contexts.

Our study holds direct implications for how we interpret current measures of generosity and evaluate aid effectiveness in the developing world. First, it shows that measures of generosity may be upward biased relative to actual other-regarding preferences, depending on who is carrying out the measurement. In addition, if there is heterogeneity in how players respond to foreign researchers, this makes it challenging to interpret variation in cross-national measures of generosity. For example, Henrich et al. (2006) find large differences in measured generosity across developing countries and attribute this to differences in cultural practices. However, if communities vary in their response to foreigners based on factors such as previous exposure to aid, then the results may be driven in part by different responses to foreigners rather than cultural differences *per se*.

In addition, aid evaluations increasingly use behavioral games and experiments to measure economic and social capital outcomes. These evaluations often compare outcomes in communities that received aid to those that did not. However, these approaches may be biased if participants behave strategically in response to white foreigners present in treatment communities. For example, if communities receiving aid respond strategically to signal need, we may underestimate the impact of aid on social capital. This is important in light of the fact that a number of recent studies have found insignificant or mixed effects of CDD programs on social capital outcomes (Casey et al., 2012; Avdeenko and Gilligan, 2015; Fearon et al., 2015).

Finally, imbalance in the racial and national composition of evaluators across treatment and control areas could generate bias. For example, if foreigners implementing a program take greater interest in observing behavioral games in treatment areas, then their mere presence could lead to the spurious conclusion that program has engineered changes in pro-social behavior. Overall, by demonstrating how the presence of a foreign researcher can affect measured altruism, our analysis points to the import of considering researcher identity broadly when measuring and interpreting behavioral outcomes.

Appendix A. Supplementary Information

Supplementary information associated with this article can be found, in the online version, at <http://dx.doi.org/10.1016/j.jebo.2015.03.015>.

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