Who Is the Identifiable Victim? Caste and Charitable Giving in Modern India

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I. Introduction

A. Overview

A recent advertisement, fund-raising for a nonprofit in an Indian magazine, features a smiling girl: "Sarita . . . Age 10, Muzaffarpur, Bihar." This ad is psychologically sophisticated in at least two ways. First, although the organization presumably helps many more children than Sarita, the ad takes advantage of the "identifiable victim effect": people donate more to appeals featuring particular needy individuals than to statistical groups. Second, the ad does not report Sarita's last name. Although there may be many reasons to only use one name in an ad, one effect is to obscure Sarita's caste and to present her as a generically poor Indian girl.

Economists are increasingly applying their analytical tools to the market for charitable giving (List 2011; Name-Correa and Yildirim 2013), including behavioral aspects (Karlan, List, and Shafir 2011). Many experimental demonstrations by psychologists and others have established an identifiable victim effect (Jenni and Loewenstein 1997; Small and Loewenstein 2003; Kogut and Ritov 2005): "People are much more willing to aid identified individuals than unidentified or statistical victims" (Slovic 2007, 88).[1]

Although a psychological account of the identifiable victim effect is beyond the scope of this econometric article, Loewenstein and Small (2007) propose that such helping behavior is explained by the interaction of sympathy and deliberation, where sympathy is "caring but immature and irrational" and

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[1] This behavioral effect on economic decision making was recently highlighted in the economics literature by Banerjee and Duflo (2011). Throughout this article, we follow this literature in referring to hypothetical beneficiaries of donations who are described in experimental prompts as part of anonymous, quantitative groups as "statistical" (e.g., 2 million Biharis) and hypothetical recipients who are personally named or described as "identified" or "identifiable" (e.g., Sarita, age 10). Additionally, although there is no clear mechanism of victimization in our studies, we will often follow the existing literature in referring to recipients as identifiable "victims."

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subject to a range of influences (112). Thus, people give more to the identified victim because they feel sympathy for her plight, but the statistical victim evokes no such emotion. However, emotional reactions to others are not always sympathetic.[2] This suggests an interaction between identifiability and social hierarchy: the reaction to identifiable victims is motivated by sympathy, but members of low-ranking groups in need often do not evoke sympathy.[3]

Our article extends this literature by studying the market for charitable giving within a developing country, and particularly in a society that has been historically stratified through a complex hierarchy of castes. Formerly "untouchable" groups—referred to as "scheduled castes" (SCs) or "Dalits"—remain stigmatized, low ranking, and subject to deep social and economic exclusion. However, the continuing importance of caste prejudice and discrimination in "modern" India is seriously debated among scholars and policy makers (Kapur et al. 2010; Deshpande 2011).[4] Therefore, experimental evidence on how caste shapes attitudes of Indians in economic decisions would be important information.

We study how caste and religious identities in contemporary India interact with the identifiable victim effect: does identifiability encourage donations to low-ranking out-groups? We conduct three randomized experiments with educated, computer-using Indian participants. In order to learn about modern Indian society, we target a special set of participants: our participants use English-language websites, but in the 2005 India Human Development Survey, less than 1% of Indian households owned a computer.[5] In the same survey, only 7% of 8–11-year-old children chose to take a reading test in English, rather than another language.

[2] For example, social psychology's stereotype content model predicts different emotional reactions to different out-groups; the lowest-ranking groups, judged to lack both warmth and competence, evoke disgust (Fiske et al. 2002). Very low-ranking people may not even be mentally represented as eligible for human sympathy. Harris and Fiske (2006) find that when US experimental participants think about extreme out-groups—in particular, homeless people and drug addicts—the medial prefrontal cortex, a part of the brain necessary for social cognition, is not activated; in participants' mental representations, these out-groups are neurally "dehumanized."

[3] Previous papers in the psychology literature have shown that identifiability does not always promote charity. For example, Small and Loewenstein (2005) find that lab participants are more willing to incur a cost to punish identified defectors in a public goods game than unidentified defectors.

[4] Although we drop the quotation marks around "modern" India in the rest of the article, we use them here to emphasize that we are speaking to an active debate about the importance of community groups to social and personal identity and treatment in contemporary India.

[5] To be clear, we do not know that our respondents own the computers they are using, but it is nevertheless a relatively privileged minority of Indians who use English-language websites.

We present three studies: a survey experiment with detailed information about participants; a natural setting experiment with 56,000 purchased advertisements, studying economic behavior in a real market for charitable giving; and an Internet choice experiment in which participants allocate real money. We indicate group membership of identified recipients subtly, using names that connote caste or religion; in a separate "first-stage" experiment, we verify that the names we use connote population groups. In all three studies, we find an identifiable victim effect for generic Indian and high-caste recipients, which is absent or reversed for low-caste recipients. We find that participants in all three studies respond similarly to statistical high- and low-caste recipients but demonstrate the least generous responses to identifiable low-caste named recipients.

These results contribute to three literatures in economics. First, they document the continuing relevance of caste in the modern Indian economy. Second, they contribute to the economics of charitable giving by highlighting a complication of an important and well-known behavioral mechanism, in part by extending this literature to charitable giving within a developing country. This result builds on a literature in empirical social choice which documents that actual allocations made in the lab reject "welfarism" and instead are sensitive to information beyond the mere distribution of well-being (Yaari and Bar-Hillel 1984); in particular, our participants' donations are motivated not merely by poverty or wealth but also by labels of social identity. Third, they advance understanding of the identifiable victim effect, itself an important phenomenon.

Our article is complementary to recent research in the psychology literature by Kogut and Ritov demonstrating interactions between identifiability and other heterogeneity, especially members of dissimilar groups. Kogut and Ritov (2007) find that undergraduate students at Hebrew University in Israel are especially willing to help an identifiable tsunami victim only when the victim is also Israeli, rather than another nationality. Ritov and Kogut (2011) show that the effect of identifiability interacts with membership in a group that is in conflict with a group with which the participant identifies: identifiability increases generosity to people with opposing political attitudes or who support opposing soccer teams. Finally, Kogut (2011) documents that identifiability interacts with belief in a just world: people may be unlikely to help identified victims who are seen as responsible for their situation. Indeed, although rarely studying castes in India, a large literature in psychology documents intuitive or automatic tendencies to treat dissimilar people or out-group members differently from how in-group members are treated (Greene 2013).

B. Caste in Modern India

> Mukund Kamalakar, a successful entrepreneur, who now owns a flourishing solar equipment firm, was originally Mukund Kamble, from a Dalit caste. He changed his surname when he was in college, from the caste-revealing Kamble to the Brahminical Kamalakar. "The attitudinal change in the people and groups that I interacted with then on was remarkable. Access into the competitive world was so much easier." (*Outlook Business*, May 2, 2009, 25)

The caste system, despite its changing manifestations over time, is inherently hierarchical, such that it endows individuals with an underlying sense of superiority (or its converse, inferiority), flowing from their birth into a particular caste. While it has been legally abolished in India since 1950, caste identity continues to define hierarchy and status significantly and is an important marker of economic inequality, although not the only one. Caste hierarchy is neither linear nor fixed, and debates over its changing forms continue; however, there is consensus on which groups constitute the bottom of the system. These are the ex-untouchable castes, traditionally associated with menial, dirty, and degrading occupations (scavenging, handling corpses, etc.). Although untouchability is illegal and punishable, overt and covert instances of untouchability, such as violence, abuse, and humiliation, continue to occur; individuals from these castes suffer from the consequences of their "stigmatized ethnic identity" in their daily lives, even when they are not engaged in their traditional roles. It is not surprising that these groups are also disproportionately poor, with limited access to productive assets or decent employment and lower educational outcomes compared to the upper castes. For more information on caste in India, please see Deshpande (2011).

Our experiment is methodologically similar to labor economists' "correspondence studies" of discrimination that randomly assign names associated with social groups to fictional persons in order to test for an effect of group membership. For example, Bertrand and Mullainathan (2004) sent resumes to prospective employers with typically African American names (e.g., Lakisha) or typically white names (e.g., Emily) and found that white names received 50% more callbacks for interviews. In this spirit, we subtly manipulated the apparent caste status of identifiable victims using names associated with religion and caste rank.

Thorat and Attewell (2007) and Siddique (2011) both use a similar strategy to document caste-based discrimination in Indian labor markets by randomly assigning names to job applications. Deshpande and Newman (2007) found that several of their respondents during job interviews faced comments about their surnames, often followed up by direct questions about their caste; this typically happened in cases in which the names were not upper-caste surnames. There is other evidence which suggests that caste prejudice permeates the modern economy, where caste is often not believed to "matter." For example, Bhagwan Gawai, now a successful Dubai-based Dalit entrepreneur, discusses how he faced caste prejudice in his earlier job as an officer in a public sector firm, which was "subtle, subterranean, never overtly articulated."[6]

Do caste names indeed matter? This article proceeds with three separate studies. Section II presents an Internet survey conducted with young, Internet-savvy participants who participate in Amazon Mechanical Turk. Section III analyzes results from 56,000 displays of an online ad to the Indian Internet-using, English-speaking public. Section IV reports an online choice experiment in which participants allocated real money. Section V concludes.

II. Study 1: Internet Survey Experiment

If the identifiable victim effect operates through sympathy, it could be absent or reversed when recipients are members of low-ranking groups associated with prejudice or unsympathetic reactions. We implemented an Internet survey experiment in September 2011. Participants from India were recruited through an online labor market and completed the experiment online. The experiment randomly assigned each participant to one of nine prompts, each describing poor people in India, and then asked about participants' willingness to donate to help.

A. Empirical Strategy

After agreeing to participate and providing informed consent, participants were first shown the experimental prompt (a few sentences of text describing an opportunity for charitable giving, reprinted in the next subsection), and immediately afterward they were asked to rate their willingness to donate. Next, the participants were asked a set of multiple-choice survey questions. Finally, participants rated the similarity of their family to typical members of 10 groups (e.g., Brahmin, poor, urban). The mean participant took 6.51 minutes to complete the survey experiment; the 25th, 50th, and 75th percentile participants took 4, 6, and 7 minutes, respectively. The informed consent described

[6] He says that "access to capital is critical for an entrepreneur. Banking in India still lacks professionalism. I'd have relocated to India but for this issue" (*Economic Times*, July 18, 2011).

the study as "a five-minute survey about people in India"; the survey was not explicitly about caste or religion.

Participants were recruited and paid US$0.20 through Amazon Mechanical Turk for completing the experiment; this is about Rs 30 at market exchange rates, although purchasing power parity rates suggest that it would be worth about US$0.80. Paolacci, Chandler, and Ipeirotis (2010) and Buhrmester, Kwang, and Gosling (2011) both provide evidence that Mechanical Turk produces high-quality experimental data that replicate well-documented lab findings from behavioral economics. The software was set to only allow participants using computer IP addresses within India and to allow each user to complete the survey only once.

As recommended by Oppenheimer, Meyvis, and Davidenko (2009), the sample was screened for attentiveness using two instructional manipulation checks. Within the survey questions, participants were asked, "How often have you suffered a fatal heart attack?" Only those who selected "never" were included in the analyzed sample. Similarly, participants were prompted with "On many important issues, people have different opinions. Some people agree, and some people disagree, even very strongly. Here in this question, please select the number four in the slider below, to rule out random clicking." Only those who selected 4 were included. Note that these questions were placed within the set of survey questions, after both the experimental treatment and the response of the main dependent variable. In addition to the instructional manipulation checks, the last page of the survey asked participants which country they were in (with a multiple-choice list) and whether they had taken the survey before. Seven participants who reported being in Sri Lanka, rather than India, and one who reported having taken the experiment before were excluded from data analysis; we interpret these responses as extreme markers of inattention. These filters resulted in a sample of 475 participants (318 male, 157 female; 359 participants age 20–34). Because the survey was in English and over the Internet, the sample is not representative of the Indian population.

Crossed Experimental Manipulations: Identifiability and Recipients' Group Identities

Table 1 summarizes the design of Study 1.[7] Each participant was randomly assigned to one of nine experimental treatments, which varied the version of

[7] Our dependent variable measures willingness to donate on a 0–100 scale, and our main effect is 13 points. Standard experimental sample size calculations would recommend a sample of 40.3 participants per experimental group to have an 80% chance of detecting a 10-point effect at the 0.05 level, given our observed standard deviation of 25.0 (Dell, Holleran, and Ramakrishnan 2002). Our achieved sample size of 52.8 members per group is therefore not substantially over or under powered, especially because we were testing interactions.

TABLE 1
STUDY 1: DESIGN AND DISTRIBUTION OF PARTICIPANTS TO TREATMENT GROUPS

	Generic Indian	High Caste	Low Caste	Muslim
Identifiable victims, person's name prompt	$n = 51$	$n = 56$	$n = 50$	$n = 49$
Statistical victims, group description prompt	$n = 59$	$n = 50$	Dalit, $n = 54$; scheduled caste, $n = 57$	$n = 49$

an introductory prompt. Participants read a description of need. The first dimension of randomized assignment was to identified or statistical recipients. Those assigned to a statistical victim treatment read:

> Many GROUP families are very poor. For much of each year, they cannot find work. Thousands of families frequently cannot afford enough basic food to eat. As a result, millions of children go without medicine if they get sick, and often go to bed hungry.

Those assigned to an identifiable victim treatment read:

> The family of NAME is very poor. For much of each year, they cannot find work. His family frequently cannot afford enough basic food to eat. As a result, his children go without medicine if they get sick, and often go to bed hungry.

This treatment was crossed with the second dimension of treatment, the group membership of the recipients. The prompt described one of four social groups: generically Indian (as a control treatment), high caste, low caste, or Muslim.

In the identified recipient case, the identification of the recipient's category was done only implicitly by his name, using well-known names commonly associated with each of the groups. Our experiment used 20 names, five for each of the four groups; each participant assigned to read about an identifiable recipient read only one of these five names, randomly presented. For the control treatment, we used names that are commonly found across caste levels and are unable to be identified with a particular group. The names used are listed in the appendix.

In the statistical recipient case, the group name was substituted into the blank: "Indian," "Brahmin" (high ranking), "Scheduled Caste (SC)" (low ranking), or "Muslim." An additional low-ranking statistical recipient treatment used the word "Dalit"—a common synonym for SC descended from the Sanskrit word for "oppressed"—for a total of nine experimental treatments.

The dependent variable in our estimation was "willingness to donate," which was assessed as follows. Immediately after the experimental prompt, on the same computer screen, all participants were asked, "How much money would you be willing to donate to a charity working with such people?"

Participants answered using a slider bar ranging from 0 to 100, labeled none at all, some, much, and very much at four evenly spaced points. The median participant took 55 seconds to read the experimental prompt and respond.

Observed Heterogeneity among Participants: Ratings of Similarity

The penultimate page of survey questions asked participants to rate their self-perceptions of similarity with 10 groups. Note that this was purposefully placed well after the experimental treatment and dependent variable collection to avoid influencing them. An introductory question asked, "How much do you believe your family is like a typical family of each of the following types?" The 10 groups, as they were written on the survey form, were Brahmin (historically the highest-rank group of castes), Forward/Upper Castes (broad group of castes that are conventionally regarded as high ranking), OBC [Other Backward Classes] (broad group of castes and communities that are low ranking but not as low as Dalits), Dalit/SC (explained above), Adivasi/ST (Scheduled Tribe; marginalized tribal communities), Muslim, Poor, Middle class, Rural, and Urban. The 10 groups were listed in a randomly counterbalanced order. Participants answered on using sliders from 0 to 100, marked with seven evenly spaced labels: not at all like my family, not like my family, not much like my family, neutral, somewhat like my family, like my family, and just like my family.

Using these rankings, we constructed an index of participants' self-perceived identities as high caste, rather than low caste. The computation followed the following formula, in which a participant ranked his or her family's similarity to 10 groups, and standard deviations are over 10 participant responses:

$$\text{caste index} = \frac{\text{high} - \text{low}}{\sigma}, \tag{1}$$

where high and low are a participant's self-perceived similarity to typical high-caste ("Brahmin") and low-caste ("Dalit/SC") families. For the denominator, from the 10 rankings, we constructed a mean and standard deviation, σ, for each participant. Thus, we made the index by subtracting each participant's low-caste score from his or her high-caste score and scaling by the standard deviation to create a z-score among the participant's own responses.

As a verification of the validity of this index, respondents who (in a separate part of the survey) report belonging to a high caste score 1.25 standard deviations more positive on the caste index, on average. Respondents who report being Dalit or OBC score 0.80 standard deviations more negatively on the caste index, on average.

B. *Results*

Description of Participants

Table 2 describes the sample of all three studies and compares them with summary statistics representative of all India from the 2005 India Human Development Survey. Studies 1 and 3 include self-reported Internet survey data. No such data are available for the participants in the natural-setting study 2; we know only whether an online ad was clicked. However, it is clear that all participants in all studies had access to a computer and used English-language websites. As the India Human Development Survey data show, these facts make clear the high socioeconomic status of our participants, relative to the Indian population.

In survey data for studies 1 and 3, participants are much more likely to live in urban residences than the average Indian; this is also true for residence in a large metropolitan city, which was asked about in study 1. Almost all participants in studies 1 and 3 are high school graduates, compared with less than a quarter of all adult Indians. Nearly 30% of Indians are either Dalit or Adivasi, but almost none of the participants in study 1 belong to these groups. Finally, only about a third of participants in study 1 and 3 are female; greater access to computers by males likely reflects broader patterns of female disadvantage in India.

TABLE 2
SUMMARY STATISTICS BY SAMPLE, COMPARED WITH ALL INDIA

	Study 1	Study 2	Study 3	India, 2005
Literate in any language	100	100	100	61.4
Any English ability*	100	100	100	19.3
Computer access	100	100	100	.9†
Female	33.1		35.7	49.9
Urban residence	92.2		75.3	28.5
Large metro residence	33.5			10.6
High school graduate	99.8		99.5	24.76‡
Hindu	69.3		77.0	78.5
Muslim	8.6		5.5	11.5
Brahmin or other upper caste	27.6			21.72
Dalit or scheduled caste	2.3			20.9
Adivasi or Scheduled Tribe	.2			7.3
Sample size	475	56,100	182	≈133,000

Note. All statistics except sample size are expressed as a percentage. All India summary statistics are computed from all persons at least 18 years old in the nationally representative 2005 India Human Development Survey (IHDS). Summary statistics are not available for study 2 because we only observe properties of ads shown, not of the people to whom they are shown.

* For experiment participants, we infer English ability from their participation. English ability in the IHDS is as reported to a surveyor; 3.7% of respondents are claimed to be fluent, 6.6% of children age 8–11 elected to take the IHDS reading test in English.

† In the IHDS, computer access is operationalized as household computer ownership.

‡ In the IHDS, we operationalize high school graduate as having passed tenth grade.

Therefore, this study is most informative about certain high-socioeconomic-status Indians. However, this is the purpose of the study, not a defect: this group is perhaps the population most able or likely to donate to or influence poverty relief, and it is a modern society among whom the continuing presence of caste discrimination is sharply debated today.

Participants' Self-Perceptions Interacted with Recipients' Caste

Did participants, in fact, notice and respond to caste status in the names experimental treatment? To assess whether participants react to caste here, we used the constructed caste identification index. Figure 1 presents local polynomial, kernel-weighted regressions of willingness to donate on this index. Willingness to donate is plotted separately for high- and low-caste participants, pooling data across experimental treatments. The identity of the recipient interacts with the identity of the participant: participants who perceive their family as more similar to typical high-caste, rather than low-caste, families are more willing to donate, on average, to high-caste recipients and less willing to donate to low-caste recipients. This interaction is statistically significant using the full sample (one-sided $p = .043$; $p = .038$ with regression controls for participant's sex and six age categories) and almost significant when the sample is restricted to participants experimentally assigned only to high- or low-caste recipients (one-sided $p = .055$).

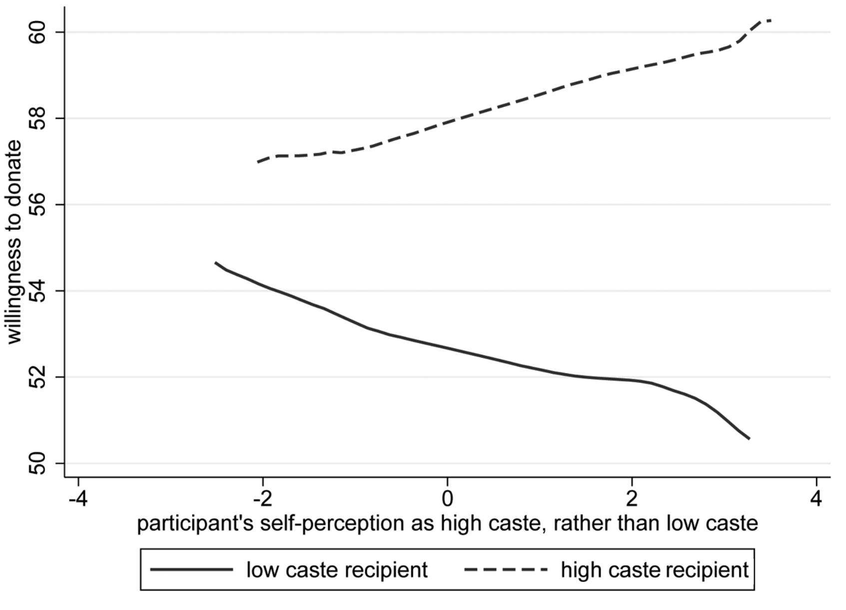

Figure 1. Study 1: Donor's caste interacts with recipient caste, local polynomial regressions

We also tested for the interchangeability of labels "Dalit" and "Scheduled Caste," in order to rule out the possibility that these specific labels or familiarity with one usage or the other was prompting participants' behavior. Half of the participants who were assigned to low-caste statistical recipients read about "Scheduled Caste (SC)" recipients and half read about "Dalit" recipients. These two group names refer to the same people. This difference in terminology had no effect: participants expressed willingness to donate of 54.07 and 54.11, respectively, to the two groups (two-sided $p = .993$). For the rest of this analysis, these two prompts are therefore pooled as one treatment: low-caste statistical recipients.

An Overall Identifiable Victim Effect

Pooling the data over all recipient groups, this experiment replicated earlier findings of an identifiable victim effect. On average, participants reported a willingness of 51.06 to donate to statistical recipients and a willingness of 56.67 to donate to identified recipients. This difference of 0.22 standard deviations is statistically significant, according to a nonparametric Wilcoxon signed rank test (two-sided $p = .014$).

Main Result: The Effect of Identification Reversed for Low-Caste Recipients

The identifiable victim effect found for the entire sample and for the control (generic "Indian") group was reversed for low-caste recipients but not for high-caste or Muslim recipients, as figure 2 shows. Note that willingness to donate to statistical recipients is essentially identical among high-caste and low-caste recipients. The recipients' group matters only in the case of iden-

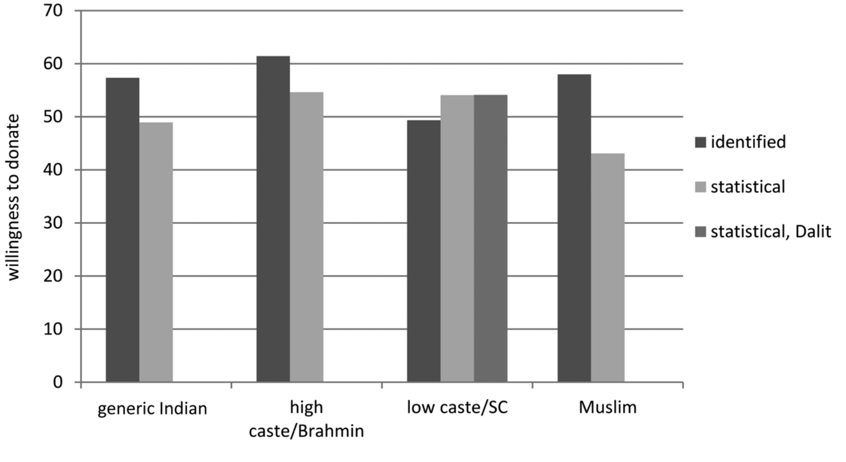

Figure 2. Study 1: Willingness to donate in survey experiment, caste interacts with identifiability

TABLE 3
STUDY 1: CASTE INTERACTS WITH IDENTIFIABILITY

	(1)	(2)
Identifiable recipient	8.401*	8.552*
	(4.730)	(4.753)
High caste	5.708	5.887
	(5.134)	(5.197)
Identifiable × high caste	−1.613	−2.453
	(7.051)	(7.052)
Low caste	5.158	6.293
	(4.101)	(4.147)
Identifiable × low caste	−13.13**	−13.47**
	(6.384)	(6.549)
Muslim	−5.830	−4.116
	(4.460)	(4.534)
Identifiable × Muslim	6.497	5.879
	(6.401)	(6.501)
Controls		✓
Constant	48.93**	45.18**
	(3.383)	(4.108)
n (participants)	475	475

Note. Heteroskedasticity-robust standard errors in parentheses. Controls are an indicator for being female, an indicator for being in the lower half of the sample age distribution, indicators for having high and low education relative to the sample, and a set of four indicators for the size of the participant's city or town. Willingness to donate (0–100). Two-sided p-values.
* $p < .10$.
** $p < .05$.

tified recipients, where participants were much less willing to donate to help members of low-ranked castes.

Table 3 confirms the statistical significance and robustness of the interaction between identifiability and low-cast identity of the recipient. The negative interaction between an identifiable recipient and low caste is statistically significant (two-sided $p = .04$). This is unchanged—as would be expected in a randomized experiment—when controls for the respondent's age, sex, city size, and education are included.

An alternative statistical significance test collapses the data, to verify that the finding is not a spurious effect of a few outlier names. We use the mean willingness to donate to each group—a data set with 20 observations—a nonparametric Wilcoxon rank-sum test finds that willingness to donate to the five low-caste names is statistically significantly lower than willingness to donate to the other 15 names (two-sided $p = .016$), suggesting that the result is not driven by only a few of the names used.

III. Study 2: An Online Marketplace "Field" Experiment

Study 1 documented that the identifiable victim in effect was reversed in a survey experiment that also collected detailed information about participants.

Would this effect also be found in a real-behavior experiment in a real market for charitable giving? Study 2 tests potential donors' responses to real Google ads for charitable donations, randomized among experimental treatments.

A. Empirical Strategy: Randomized Advertisements

We purchased 56,100 displays of online advertisements from Google's AdWords advertising service.[8] These ads were in every way ordinary fund-raising ads, displayed among other ads on Google's websites and purchased in the ordinary marketplaces for Google advertising by a 501(c)(3) nonprofit public charity. UNICEF India, for example, places similar Google ads for donations.

Each participant was randomly assigned one of six types of ad: statistical or identifiable, crossed with high-caste, low-caste, or a generic control group.[9] The Google software was set to only show a particular computer an ad from our experiment one time per day. As before, in the identifiable treatment, group membership was identified by name only. Thus, the statistical recipient ads read:

> ***Help children grow tall***
> Many GROUP families are very poor
> Please donate today to help!

The identifiable recipient ads read:

> ***Help a child grow tall***
> NAME's family is very poor
> Please donate today to help!

Control names were Sunil Chandra and Bharat Das, and the group name was "Indian" in the statistical recipient case. Low-caste names were Nathu Valmiki and Ashok Chamar, and the group name was "Dalit." High-caste names were Mrigank Gupta and Mahesh Pandit. The group name presented for the high-caste statistical recipient treatment was "Brahmin," although note that Gupta, while a high-caste name, is not a specifically Brahmin name.

[8] Although the precise sample size was not under direct control of the experimenters (instead, Google showed ads until enough clicks accumulated that a predetermined budget was exhausted), we can compare our sample to an ex post power calculation. Based on the observed Bernoulli standard deviation of the low probability of clicking, standard sample size calculations suggest a sample size of 9,735 per experimental group to have an 80% chance of observing the effect size we observed at the 0.05 level (Dell et al. 2002). With six treatment groups, this recommends a sample of 58,410, so our study was appropriately powered.

[9] For parsimony and to concentrate statistical power on the interaction of interest, Muslim recipients were dropped from study 2; there was neither a mean nor a interactive statistically significant effect of Muslim names in study 1.

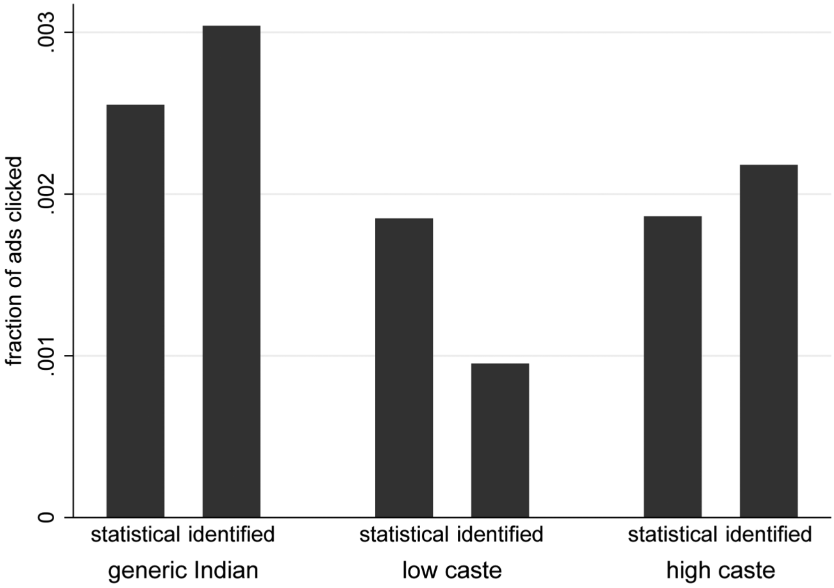

Figure 3. Study 2: Clicks on Google ad for charity, caste interacts with identifiability

The outcome of interest is whether a display of an ad (referred to in the advertising industry as an "impression") resulted in a viewer clicking on an ad.[10] Thus, each impression is an observation, the independent variables indicate the experimental treatment, and the dependent variable is a binary indicator for a participant's click. Note that, in this market, advertisers are charged by Google per viewer click, so this is an economically relevant variable.

B. *Results*

In natural-setting behavior, does the reversal of the identifiable victim effect for low-caste recipients appear? Figure 3 indicates that it does. As was the case in figure 2, an identified victim effect is apparent in the generic case (although regression results show that it is not statistically significant), and participants click on statistical ads mentioning a low-caste group almost exactly as often as on ads mentioning a high-caste group. However, once again, the identified

[10] Ads on Google are shown in response to users' searches. Keywords that activated our ads were rice for the poor, it's deductible, pay pal donations, donations and tax, India, poor, poverty, charity, malnutrition, hunger, children, donation, charity, give to charity, give charity, child, rice, and stunting. Some of these were suggested by Google on the basis of the text of the ad. We used the same keywords for each ad; because Google randomly assigns ads, there is no reason to expect participant pools to vary across ads.

low-caste names generate the smallest response, and in particular no identifiable victim effect is apparent for low-caste recipients.

We verify the statistical significance of this result by estimating regressions of the form

$$\begin{aligned} \text{click}_{is} = {} & \beta_1 \text{identifiable}_{is} + \beta_2 \text{low}_{is} + \beta_3 \text{low}_{is} \times \text{identifiable}_{is} \\ & + \beta_4 \text{high}_{is} + \beta_5 \text{high}_{is} \times \text{identifiable}_{is} + \varphi \text{position}_i \alpha_s + \varepsilon_{is}, \end{aligned} \tag{2}$$

where i indexes individual impressions (ad views), s indexes states, and α_s are state fixed effects that will be introduced as a robustness check. The dependent variable click_{is} is an indicator that the viewer clicked the ad, and high_{is} and low_{is} refer to the caste group of the recipient. The position of the ad among others shown at the same time (1 for first, 2 for second, etc.) will also be added as a control, position_i. The hypothesis being tested is that $\beta_3 < 0$: a reversal of the identifiable victim effect.

Table 4 presents the results: in a real market for charitable giving, the result from study 1 is replicated. Identified-recipient low-caste ads are about half as likely to be clicked on as statistical low-caste ads, a difference that is statistically significant at least at the two-sided .05 level in all specifications. Unsur-

TABLE 4

STUDY 2: CASTE INTERACTS WITH IDENTIFIABILITY, GOOGLE AD CLICKS

	Full OLS			Position ≤ 3 OLS	Full Logit	
	(1)	(2)	(3)	(4)	(5)	(6)
Identifiable	.000480	.000491	.000527	.000818	.171	.188
	(.000972)	(.000985)	(.000991)	(.00102)	(.457)	(.464)
Low caste	−.000725	−.000715	−.000826	−.000833	−.331	−.395
	(.000863)	(.000873)	(.000911)	(.000917)	(.444)	(.479)
Low caste × identifiable	−.00162*	−.00162*	−.00166*	−.00176*	−.997*	−1.030*
	(.000771)	(.000777)	(.000821)	(.000829)	(.411)	(.448)
High caste	−.000692	−.000680	−.000642	−.000343	−.313	−.296
	(.000826)	(.000844)	(.000850)	(.000880)	(.419)	(.428)
High caste × identifiable	−.000357	−.000355	−.000324	−.000258	−.149	−.137
	(.000877)	(.000878)	(.000880)	(.000887)	(.438)	(.440)
Ad position		.0000313	.0000904	.000963		.0323
		(.000438)	(.000516)	(.000799)		(.233)
State fixed effects			✓	✓		✓
Constant	.00258***	.00253*	.00246*	.00120	−5.958***	−5.968***
	(.000726)	(.00103)	(.00106)	(.00138)	(.354)	(.784)
n (impressions)	56,100	56,100	56,100	55,820	56,100	56,100

Note. Averaging over all treatments, 0.16% of ads were clicked. Heteroskedasticity-robust standard errors in parentheses. Two-sided *p*-values. OLS = ordinary least squares.

* $p < .05$.

*** $p < .001$.

prisingly, because assignment to ad treatments was randomized, adding controls for the position of the ad and the state of the viewer do not change the result. No ad in the study was ever clicked beyond the third position (92% of ad impressions were in the first or second position); column 4 omits ads beyond the third position, which does not change the result. Finally, because the probability of a click is close to zero, columns 5 and 6 estimate logistic regression models that, again, find a similar result.

IV. Study 3: Splitting Real Money, Verifying Names

Studies 1 and 2 found interactions between caste of recipients and identifiability across two online settings. In this third study, we use a separate set of participants to rate the caste-ness of candidate names; we replicate our result selecting names recommended by this procedure, to ensure that we are implementing the treatment that we intend. Using these names, we again implement a survey experiment online, in which participants split a real Rs 100 between themselves and a charitable donation. Rs 100 is about US$1.60 at market exchange rates but is between $6 and $7 at purchasing power parity.

A. Selecting Names

Can we be sure that randomized names are indeed having a first-stage effect on perceptions of caste? As a preliminary to our main experiment, we created a categorization task on the online labor marketplace Amazon Mechanical Turk in which Indian workers were requested to categorize a name by social group. Workers were shown a name, such as “Hiraman Chamar,” and asked to categorize the name into exactly one of the following six groups, presented in a random order: Adivasi or Scheduled Tribe, Brahmin or other forward caste, Dalit or SC, Muslim, Other Backwards Caste, other/none of these.[11]

In its entirety, the prompt read:

> Please categorize the Indian name below into one social group. If the name does not clearly belong to one specific group, please select “other/none of these.”
>
> Name:
>
> [followed by the name, the choice among the six categories, and a submit button]

Overall, 60 names (of which we chose 15 to use in the experiment) were categorized 1,200 times, or 20 times each. Participants were paid $0.05 for categorizing a name. Table 5 reports the results; the “rating” is the fraction of

[11] Although we indeed wrote Other Backwards Caste in the survey instrument, the category in Indian law is in fact Other Backwards Class.

TABLE 5

STUDY 3: SOCIAL CONNOTATIONS OF INDIAN NAMES, AS RATED ON MECHANICAL TURK

Name	Rating	Used in Study 3	Name	Rating	Used in Study 3
Low caste:			High caste:		
Hiraman Chamar	.73	✓	Ishan Chaturvedi	1.00	✓
Om Prakash Chamar	.64	✓	Ved Pratap Chaturvedi	1.00	
Nathu Chamar	.60		Kunwar Rajesh Pratap Rathore	.93	✓
Sukhiya Mochi	.55	✓	Ishwar Pandit	.92	✓
Ashok Mochi	.50		Vishnu Shankar Shastri	.92	
Bhimrao Valmiki	.43	✓	Mahesh Pandit	.85	✓
Amit Jatav	.36	✓	Akhilesh Pandit	.83	
Nathu Valmiki	.33		Raghavendra Sharma	.81	
Sukhdev Jatav	.33		Akhilesh Sharma	.75	✓
Ramesh Teli	.29		Krishna Kant Sharma	.73	
Muslim:			Generic Indian:		
Habib Faisal	1.00		Sunil Prakash	.43	✓
Rahamatullah Khan	1.00		Rajiv Kumar	.40	✓
Imtiaz Ali	1.00		Ramesh Kumar	.40	
Mohammad Ansari	1.00		Bharat Kumar	.33	✓
Murad Ali	1.00		Sanjay Kumar	.29	
Mohammad Saeed	1.00		Suresh Lal	.27	✓
Yousuf Saeed	1.00		Joginder Teli	.26	
Majid Siddiqui	.92		Yash Pal	.21	✓
Adil Hussain	.92		Rakesh Kumar	.21	
Rashid Khan	.92		Krishna Kant Sharma	.20	

Note. Rating is the fraction, 0–1, of participants who classified a name into the given category.

participants who put a name into a category. We classify names rated as "other/none of these" as generic. As this article's experimental strategy assumes, some names are strongly associated with particular population groups. For study 3, we use the five most group-specific names, except that we skip some for diversity in first and last names (e.g., after Om Prakash Chamar, Nathu Chamar is skipped for Sukhiya Mochi and then Ashok Mochi is skipped for Bhimrao Valmiki).

B. *Experimental Strategy*

As in study 1, we recruited participants on Amazon Mechanical Turk; this pool of participants had no direct relationship with the participants who were used to select names, and this phase of the study was conducted 3 months after the first stage name-rating study. Participants were offered \$0.08 to complete a 5 minute survey, as well as a chance at winning more money in the experiment; the total expected payment in study 3 was \$0.16 per participant, compared with \$0.20 in study 1. Unlike in studies 1 and 2, participants made decisions about real money. We explained that 5% of participants' decisions would be implemented with real payments, made through a Mechanical Turk system that allows participants to be paid bonuses. In particular, workers first read the general instruction:

> In this study, you will have the opportunity to split 100 rupees between yourself and another recipient. That means that you will get to decide how much of the 100 rupees goes to you, and how much of it goes to somebody else. You could choose to keep all 100 rupees yourself, or to give all 100 rupees away, or to split them in any other way you choose. We will randomly choose 5 percent of the people who complete the survey and implement their choice with real money, using the "bonus" feature in Mechanical Turk to pay according to your choice; any money that you give to somebody else we will give for you, you will only receive the part you assign to yourself. To show that you read these instructions carefully, do not type the word yes below, but instead type the first word in this paragraph. Remember, your choices may be randomly selected to be implemented for real money.
>
> Please type the word yes to indicate that you are ready to proceed:

Then participants were shown a statistical or identifiable victim treatment with text identical to the treatments used in study 1. Next, participants were asked to split Rs 100 between what they would receive themselves and a donation to a charity:

> If you are randomly selected to receive 100 rupees, how much would you keep for yourself, and how much would you donate to a charity working to help such people?
>
> For myself: Rs. (enter 0 to 100)
>
> To donate: Rs. (enter 0 to 100)
>
> Note: these two must add up to 100 rupees.

Finally, participants were asked a few brief survey questions.

Again as in study 1, we verified that participants were attending to our task with an instructional manipulation check. As a careful reader will have noticed, the instructions above ask the participant to type the word "in." We only include in our sample participants who successfully made this demonstration of attention. The Mechanical Turk software was set to allow participants in India only.

C. Results

One hundred eighty-two participants completed study 3.[12] Figure 4 reports the mean donation, out of Rs 100, for each of the six experimental groups: statistical versus identifiable victim crossed with general, low-caste, and high-caste participants. As in the prior two studies, we see a positive identifiable

[12] A power calculation using the observed standard deviation recommends a sample size of 35.4 participants per experimental group to detect a Rs 20 treatment effect. With six groups, this would be a sample of 212 participants; therefore, our study may have been slightly underpowered, which would contribute to the imprecision of our estimated effect sizes.

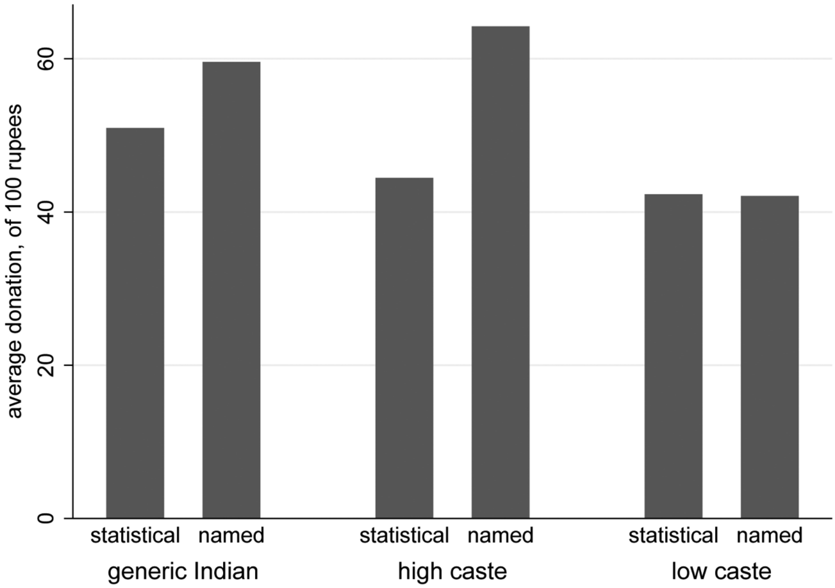

Figure 4. Study 3: Donations to charity, out of Rs 100, by treatment

victim effect for high-caste and general recipients: named recipients in these population groups receive larger donations, on average. Also as in the prior two studies, caste and identifiability interact: identifiability does not increase donations to low-caste recipients—although, in contrast to the prior studies, it does not decrease donation either. Table 6 verifies that this interaction is statistically significantly negative. Unsurprisingly, given the random assignment of experimental treatments, the result is robust to including controls about participants, although an *F*-test verifies that these controls indeed predict donation. Therefore, using names that were rated as caste group specific and using probabilistic real money payments, we again found that identifiability does not confer a charitable benefit for low-caste recipients.

V. Conclusion

Economists are increasingly understanding the market for charitable giving from both traditional and behavioral approaches. To our knowledge, this is the first study to demonstrate the familiar "identifiable victim effect" among participants in India or any developing country.[13] This phenomenon is im-

[13] Thus, our article extends this literature to a non-WEIRD (Western, educated, industrialized, rich, democratic) population (Henrich, Heine, and Norenzayan 2010). India is, of course, democratic, and many of the participants would have been relatively rich within India, but this sample was not Western.

TABLE 6
STUDY 3: CASTE INTERACTS WITH IDENTIFIABILITY, SPLITTING REAL MONEY ONLINE

	(1)	(2)
Low caste	−2.141	−4.316
	(7.659)	(8.308)
Generic Indian	6.518	5.334
	(7.795)	(8.718)
Identifiable	19.77**	19.96**
	(7.666)	(8.288)
Low caste × identifiable	−20.00*	−21.06*
	(10.31)	(10.73)
Generic × identifiable	−11.16	−10.99
	(10.82)	(11.57)
Controls		$F_{13,162} = 7.95$
		$p < .0001$
Constant	44.45***	45.69***
	(5.693)	(15.46)

Note. Dependent variable: rupees donated. Robust standard errors in parentheses. Controls are for eight caste/religion group indicators, a sex indicator, an urban/rural indicator, and six educational group indicators. Two-sided *p*-values. $n = 182$.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

portant in itself if it causes charitable donations to be allocated inefficiently. In its main result, our study shows that identifiability can interact with intragroup social hierarchy.

Despite an identifiable victim effect for generic Indian recipients, participants were the least generous in all three studies to a named low-caste family. Yet, in each case participants indicated being as willing to donate to statistical low-caste victims as to statistical high-caste victims. A psychological account of this outcome is beyond the scope of this econometric article. However, one consistent explanation seems to fit our findings; it is offered by Loewenstein and Small (2007) who suggest that responses to statistical victims are governed by deliberation, while responses to identifiable victims depend on emotion.[14] Also, Fiske et al. (2002) argue that low-ranking out-groups can generate aversive emotion, rather than sympathy, which could explain the behavior of the participants of our study.

The identity of the identified victim matters. In equilibrium, profit-maximizing charities, such as Sarita's, may be unlikely to place costly ads using low-caste names of recipients. Our study establishes that social identity matters in charitable giving, just as it matters in labor, credit, or consumer markets. However, does identity matter because donors have a taste for discrim-

[14] This interpretation would also be consistent with an important dual-process model of moral psychology recently advanced by Greene (2013).

ination, which manifests itself through disgust toward or hatred of the stigmatized groups or through a statistical discrimination channel in which caste identity is seen as a proxy for ability or effort (and therefore deservedness), such that needy upper castes are seen as worthy or deserving of charitable giving? Economists typically address such questions by assessing whether observable characteristics correlated with race can plausibly and statistically account for discrimination—for example, if the evidence showed that employers were equally willing to hire black and white employees known to have the same levels of education, then lower hiring rates of blacks might be interpreted to reflect lack of access to education, rather than discrimination per se. However, in our case this method cannot apply: our unidentifiable victims are experimental fictions. Future studies could measure whether willingness to donate responds differently to charity efficiency or social marginal product for low- and high-caste recipients. Such a study, however, would investigate a triple interaction—caste by identifiability by fictional deservedness information—and would therefore require much larger sample sizes than we study.

Finally, our results contribute to a literature debating the continuing relevance of caste in modern India (Deshpande 2011). Some researchers highlight evidence that caste-based inequality is decreasing or that the well-being of Dalits has improved over time (Kapur et al. 2010). Without addressing the question of change over time, our results suggest that caste prejudice still exists even among Internet-using, English-speaking, young, educated Indians. Other recent randomized studies have made related observations: Hanna and Linden (2012) show that teachers in India give lower grades to the same exams when randomly assigned low-caste student names; Lamba and Spears (2013) document that rural villages randomly assigned a low-caste village leader are less likely to subsequently be recognized with a sanitation prize. Although strategies to resolve caste discrimination are well beyond the scope of this article, our results and others make clear that finding such strategies continues to be of policy importance.

Appendix

Names used in Study 1

For each name, we report the mean willingness to donate and number of participants in parentheses ($\bar{x}$, n). The generic or unidentifiable names were Sanjeev Kumar (46, 10), Sunil Chowdhary (59, 12), Yash Pal (64, 10), Aman Das (55, 12), and Raghav Chandra (66, 7). The high-caste names were Akhilesh Sharma (49, 13), Ishan Chaturvedi (61, 9), Mahesh Pandit (64, 13), Kunwar Rajesh Pratap Rathore (64, 10), and Mrigank Gupta (70, 11). The low-caste names were Nathu Valmiki (43, 12), Rajesh Paswan (53, 8), Om Prakash Chamar (44, 9), Ashok Mochi (59, 11), and Ramesh Teli (47, 10). The Muslim names were Rashid Khan (54, 6), Imtiaz Ali (52, 11), Yousuf Saeed (61, 11), Mohammad Ansari (59, 11), and Imran Hussain (62, 10).

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