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Searching for religious discrimination among Anganwadi workers in India

An experimental investigation

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Abstract: This paper examines whether, in India, discriminatory practices by government-employed child caregivers along religious lines, lead to differential health outcomes among the care receiving children. Child caregivers participate in a novel allocation game where we incorporate treatments to disentangle statistical and taste-based discrimination. Our findings find no evidence of taste-based discrimination or statistical discrimination among the child caregivers. We also weigh-in on the usefulness of non-incentivized experiments in discrimination experiments.

Keywords: artefactual field experiment, discrimination, health, allocation game, India, religion
JEL classification: C9, D3, I1, O1

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1 Introduction

Social discrimination has a long-standing history and has permeated into every arena of the society—religion, race, caste, gender, and wealth.[1] Repeated and sustained discrimination often leads to violent conflict imposing huge economic costs on society. Recent civil wars such as those in Syria, Iraq, Central African Republic, and Sri Lanka were all fought along sectarian lines. Although there exist theoretical foundations characterizing the sources of discrimination—taste-based and statistical—there is relatively little empirical work that focuses on identifying these different sources of discrimination. Taste-based discrimination is a result of intrinsic preferences for a particular group or observable characteristic (religion, caste, color, or gender) and typically results in animosity towards the minority group (Becker 1971). Statistical discrimination occurs when identity (for example: race, caste, gender) serve as a proxy for other less easily observable characteristics (for example: ability, productivity, delinquency; see Arrow 1972; Phelps 1972).

We focus on searching for discriminatory attitudes among public health workers from the Integrated Child Development Services (ICDS) in India, and on evaluating the sources of religious discrimination if they are found to exist.[2] ICDS is the largest Early Childhood Development (ECD) program in the world, encompassing 1.3 million day care centers where child care workers are responsible for improving children's nutritional status as well as pre-school readiness in India. Our experiment subjects primarily consist of Hindu public health workers who have the access and scope to influence both Hindu and Muslim (majority and minority religions respectively) children's health, and learning outcomes, in multiple ways. We design an experiment to explore the possible sources of religious discrimination using a novel allocation game to elicit preferences that arise from underlying discriminatory norms and attitudes. In particular, our experiment attempts to identify evidence for religious discrimination that rises from preferences for religion only (taste-based discrimination) and discrimination that rises from preferences for an unobserved characteristic for which religion only serves as a proxy (statistical discrimination). Additionally, we compare behavior from incentivized and non-incentivized experiments to evaluate the usefulness of thought experiments in our specific context of religious discrimination.

Traditionally, cross-sectional regressions have been used for identifying discrimination. These experiments control for available observable characteristics (for example: education, income, age, location, and other socioeconomic factors) and have an indicator for identity (for example, being Black in the USA, being a woman, belonging to a lower caste). If the indicator for identity has a significant effect, then it is associated with the presence of discrimination (see Altonji and Blank 1999, for review). The drawback of such a regression is that identity could serve as a proxy for an unobserved characteristic (risk preference, competition, social preference, productivity, ability) and consequently suffers from omitted variables bias. The *resumé* audit methodology is an improvement over controlling for observables, where fictitious *resumés* are sent out for the same job with identical

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[1] See, for example, Hoff and Pandey (2004), List (2004), Hanna and Linden (2012), Bertrand and Mullainathan (2004), and Banerjee et al. (2009).

[2] India has considerable religious heterogeneity. Hinduism is the predominant religion, followed by 80 per cent of the population. Islam has the next largest following with 14 per cent (Census of India 2011). Although a religiously secular country, India has witnessed periods of grave societal unrest fought on religious grounds.

qualifications but differing characteristics (such as gender, ethnicity, and race) in labor and housing markets (Andersson et al. 2012; Banerjee et al. 2009; Bertrand and Mullainathan 2004; Siddique 2009). These studies capture how the response rate from the supply-side changes depending on those characteristics and is often attributable to the presence of taste-based discrimination. Adding dimensions on quality of workers for the different groups has also occasionally been used to allow authors to comment on the presence of statistical discrimination as well. Psychologists and, more recently, economists have used controlled laboratory settings to identify the presence and aspects of discrimination. In a meta-analysis involving 77 studies, Lane (2015) finds that although there is some evidence of taste-based as well as statistical discrimination in about a third of the cases, null results seem to be widely prevalent in the literature. Further, the author notes that, "*relatively few experiments have been designed to distinguish between taste-based and statistical discrimination.*" Extending the experiments to the field, among a non-student population, can be difficult due to the sensitive nature of the investigation.

In contrast to the above literature, our paper contributes to lab-in-the-field discrimination experiments in important ways: First, we aim to identify not just the presence, but also the sources (taste-based and statistical) of religious discriminatory practices. Second, our subject sample is unique in that we implement our experiment among actual Anganwadi workers in the public health sector in India, and do not use the typical method of using a framed experiment among student subjects. This makes the case for our results' external validity. Third, we design a novel allocation game that, in contrast to existing allocation games that have been used to study discrimination (Fershtman and Gneezy 2001; List 2004), holds the personal cost of the allocator fixed, allowing us to focus exclusively on altruistic preferences (or the lack of them) towards different groups.[3]

In our lab-in-the-field experiment, the choices of the Anganwadi workers are observed under three treatments: *information and no-identity*, *information and identity*, and *no-information and identity*. In each treatment, the caregiver needs to choose an allocation of a given endowment between mothers of two randomly chosen pre-school children varying in nutritional status and religious identities.[4] Our findings suggest: (1) The majority of the subjects choose to provide equally in all three treatments indicative of a general aversion towards unequal resource allocation towards the recipient children, regardless of their religious identities; (2) There is no evidence of taste-based discrimination or statistical discrimination among the child care givers as measured in our treatments; (3) Consistent with our findings on no discrimination, we find that, faced with only the knowledge of health status among pre-school children or poverty level of the family, workers tend to prefer allocations that are pro-poor and pro-malnourished; (4) Our incentivized experiment results seem largely robust to a non-incentivized experiment setting as well.

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[3] Another game commonly used in the discrimination literature is the trust game that assumes the absence of altruistic or inequality-averse other-regarding preferences. Cox (2004) shows that single trust-games cannot distinguish between transfers resulting from trust or reciprocity and transfers resulting from other-regarding preferences that are not conditional on the behavior of others. As a result, single trust games cannot disentangle the sources of discrimination. For example, Johansson-Stenman et al. (2009) measure trust between Hindus and Muslims in Bangladesh.

[4] Similar framed allocation games have allowed researchers to elicit choices and decisions under very different environments such as health care, organ donation, charity, etc. (Eckel and Grossman 1996; Kass et al. 2013; Leonard et al. 2013).

2 Experiment

2.1 Role and outreach of the Anganwadi Program

As already mentioned, there are over 1.3 million government-employed Anganwadi workers in India under the ICDS, making it the largest ECD program in the world. These workers influence child health and learning in multiple ways. First, workers are responsible for distributing the government-provided midday meals that are aimed to improve nutrition and consequently foster both improvements in physical health and brain development. Second, workers are responsible for effectively communicating the best practices in nutrition to mothers, whom they are supposed to advise and monitor on a regular basis. Finally, workers are also supposed to enhance children's socio-emotional and cognitive skills necessary for reaching the children's pre-school potential.[5]

One correlate of health could be religion. Brainerd and Menon (2015) show that children from Hindu households are significantly taller and heavier than Muslim children between the ages of 1 and 4 years across the Indian subcontinent. Since it is well documented that poor nutrition under 5 years is strongly related to adult height, completed grades of schooling, grade progression, test scores, and adult income, the observed health disadvantage faced by Muslim children can have long-term implications for their socioeconomic well-being (Behrman et al. 2009; Maluccio et al. 2009; Mani 2012; Stein et al. 2003, 2008; Victora et al. 2008). While some of the observed differences in Hindu and Muslim children's anthropometric outcomes may be due to demand-side choices, such as differential investments in immunization, diet, and health preventive practices, supply-side factors may play a role as well (Singh and Mitra 2013). Differences in health outcomes among pre-school Hindu–Muslim children could plausibly stem from religious discrimination as well, practiced by Hindu workers towards Muslim children.[6] We explore this latter pathway in this paper using experiments in the field.

2.2 The Discrimination Game

In our Discrimination Game, each health care worker (Anganwadi worker) decides the payouts of mothers of two other pre-school children. Each subject's allocation decisions are observed under three treatments: (1) *information and no-identity*, (2) *information and identity*, (3) *no-information and identity*.

In the *information and no-identity* (INFO-NOID) treatment, the worker is told that she will receive Rs. 120 to be allocated between herself and two mothers. She is then asked to choose one of the four allocation bundles described in Table 1. In each choice, the worker's own payoff is fixed at Rs. 40 to prevent selfish payoff-maximizing preferences contaminating discriminatory allocations. In this treatment, the worker has no information on the child's religious identity. The worker is aware that both children (1 and 2) suffer from grade 2 malnutrition or moderate levels of undernutrition as measured by weight-for-age (see Instructions for Task 1, Appendix A1).[7] This treatment was

[5] Numerous studies report leakages and inefficiencies in the ICDS (see Singh 2015 for further details).

[6] In Chandigarh, 80.8 per cent of the population is Hindu and 4.9% is Muslim. In Delhi, 81.2% of the population is Hindu and 12.9% is Muslim (Census of India 2011).

[7] In the context of the weight-for-age indicator used by Anganwadis, moderate malnutrition is defined as the child being between 2 and 3 standard deviations below the WHO specified mean for the reference population of the same age and

implemented to rule out any irrational behavior among our subjects in the presence of no information on religious identities of the recipients, and one would expect that average choices should reflect equal splits.

In the *information and identity* **(**INFO-ID) treatment, the worker has information on both the child's religious identity and nutritional status. Both children in this treatment suffer from moderate levels of undernutrition as measured by weight-for-age. Further, child 1 is assigned a Muslim name and child 2 is assigned a Hindu name (see Instructions for Task 2, Appendix A1).[8] To minimize experimenter demand effects, we do not provide explicit information on the religious identity of the children by stating "mother of Hindu child" or "mother of Muslim child."[9] It is nevertheless assumed that workers would know that Muslim children generally have different sounding names compared to Hindu children. Note that this is not an unreasonable assumption, as noted in Banerjee et al. (2009), who found that among faculty and college students surveyed in New Delhi, "Muslim names were universally recognized as such." In this treatment, showing preference towards a Hindu or Muslim child would be consistent with the presence of taste-based discrimination as the worker is given a choice over two otherwise "identical" children.[10]

In the *no-information and identity* (NOINFO-ID) treatment, there is no information on the child's nutritional status. The workers only receive information about the child's religious identity; where child 1 is assigned a Muslim name and child 2 is assigned a Hindu name (see Treatment 3/Task 3, Appendix A1). In this treatment, if the proportion giving preference to (say) Hindu children exceeds the proportion giving preference to Muslim children, it indicates the presence of discrimination accruing to taste and statistical sources. To disentangle the presence (if any) of statistical discrimination from taste-based discrimination, we have to subtract the differences in allocations between the two types of children in treatment INFO-ID from the NOINFO-ID treatment.

In each of the treatments, the Anganwadi worker was presented with the four options summarized in Table 1 and was asked to choose one of them. The total value of each option is fixed at Rs. 120 (around US$2 at the then exchange rate). Since our treatments focus on the worker's distributional preferences for children with different religious identities, we keep the amount the worker can allocate to herself fixed in each of the four options to control for selfish payoff-maximizing preferences across the treatments and focus exclusively on distribution attitudes towards others.[11]

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sex. In Chandigarh, about 41 per cent of the children enrolled in these centers were malnourished in July 2014 (Singh and Masters 2016).

[8] In the Chandigarh lab experiment, workers were making choices over actual Hindu and Muslim children living in another part of the city.

[9] Framing can generate huge experimenter demand effects in dictator games (see Zizzo 2010 for a recent review).

[10] Fershtman and Gneezy (2001) and List (2004) both use a dictator game where the amount transferred by player A (dictator) to player B (receiver), if affected by player B's background characteristic, is a strong indication of taste-based discrimination.

[11] This is in contrast to other papers that also allow the dictator to keep the money for themselves, allowing them to exploit their dictator position (Fershtman and Gneezy 2001; List 2004).

Table 1: Choices in the Discrimation Game

OPTIONS	
A	I want to keep Rs. 40 for myself I want to give Rs. 40 to the mother of child 1 I want to give Rs. 40 to the mother of child 2
B	I want to keep Rs. 40 for myself I want to give Rs. 0 to the mother of child 1 I want to give Rs. 80 to the mother of child 2
C	I want to keep Rs. 40 for myself I want to give Rs. 80 to the mother of child 1 I want to give Rs. 0 to the mother of child 2
D	I want to keep Rs. 40 for myself I want to give Rs. ……..(write amount here) to the mother of child 1 I want to give Rs. ……..(write amount here) to the mother of child 2 [Note: the three amounts must add up to Rs. 120]

Source: Authors' formulation.

2.3 Experiment design and protocol

Our lab-in-the-field experiments were conducted in Chandigarh and in New Delhi. In Chandigarh, the Anganwadi workers were invited at a pre-specified time and were promised a show-up fee of Rs. 50 for participating in the experiment. The invited workers belong to all possible Anganwadi workers employed in the ICDS' Block 1 slum areas in Chandigarh. In total, over 90 per cent of the invited workers participated. Once workers (all women) arrived at the session, the session participants were either randomized to receive treatment 2 (*information and identity treatment*) or treatments 1 and 3 (*information and no-identity treatment* and *no-information and identity treatment* respectively). While we would have ideally liked to randomize subjects into one and only one of the three treatments, due to sample size constraints we chose to randomize the subjects to receive either treatment 2 only, or treatments 1 and 3.[12] While all participating subjects received the show-up fee, we used a within-subject Random Iincentive System method (Baltussen et al. 2012; Bettinger and Slonim 2007; Bolle 1990) for additional payments in each session, where 25 per cent of the subjects were chosen for additional payouts. If a subject participated in treatments 1 and 3, we randomly chose one of the treatments for additional payments to avoid wealth effects.

In INFO-NOID treatment we choose mothers of two randomly chosen children (from Anganwadis in another geographical block) to receive the payouts chosen by the worker in the experiment. In INFO-ID treatment and NOINFO-ID treatment, where the religious identities of the recipient mothers were disclosed by providing the names of her children, we randomly chose two pairs of

[12] The power of an experimental design gives, for a given effect size and statistical significance level, the probability that we will be able to reject the hypothesis of no treatment effects when it is false. In Figure A1 of the Appendix we show the minimum sample size required to detect a medium effect size of 0.40 standard deviations at the 5 per cent significance level. Figure A1 in the Appendix shows that we need a total of approximately 100 workers in each treatment to secure an 80 per cent chance of rejecting the null of zero treatment effects when it is false. Therefore, we randomize subjects into treatments 1 and 3 vs. treatment 2.

moderately malnourished Hindu and Muslim male children to allow for more than one mother to receive the actual payouts from the experiment. In all treatments, we chose male-sounding names because we did not want to conflate gender inequality with religious inequality in this experiment. The mothers were chosen from a different geographical zone so that it was unlikely that the health care workers would know them personally (thus minimizing the possibility of strategic interactions and in-group preferences).

Our New Delhi experiment sessions were non-incentivized. Our field partners in Delhi did not allow us to pay the Anganwadi workers, wary of implementing differential monetary payments based on religious identities. This made us acutely aware of one of the important constraints that one might come across in running discrimination experiments in the field and the possibility that experimenters might be forced to choose non-incentivized experiments undr such circumstances. Consequently, our New Delhi sessions add to the design-of-experiment literature comparing incentivized and non-incentivized experiments (Coppola 2014; Dohmen et al. 2011; Gneezy and Rustichini 2000). A comparison of results from the incentivized and non-incentivized sessions allows us to comment on the substitutability of the two design choices. All session-related information is reported in Table 2.

Table 2: Experiment sessions in Chandigarh and New Delhi

	Chandigarh	Delhi
Number of Anganwadi workers	75	112
Sample size in treatment 1 and 3	37	50
Sample size in treatment 2	38	62
Number of sessions	2	2
Show-up fee	50	None
Discrimination Game	Yes	Yes
Average payout per subject	120	0
Month of experiment	May, 2015	August, 2015

Source: Authors' formulation.

3 Results

Subject randomization into different treatments was successful. We find no significant difference in the family background characteristics between subjects or Anganwadi workers assigned to each of the treatments in Chandigarh as well as in New Delhi, verifying balance among subject characteristics in each of our treatments (see Column 3, Table 3).

Table 3: Differences in baseline characteristics by treatment and location

	Treatment 2 (1)	Treatments 1and 3 (2)	Difference t-test (standard error) (3)
Panel A: Chandigarh sample			
Age in years	41.09	40.60	0.49
	[9.09]	[7.59]	(2.07)
Years of experience	11.88	10.43	1.44
	[8.82]	[9.13]	(2.51)
Education (= 1 have 12 or more grades of schooling, 0 otherwise)	0.54	0.62	-0.07
	[0.50]	[0.49]	(0.12)
Panel B: New Delhi sample			
Age in years	42.90	45.54	-2.64
	[8.28]	[9.66]	(1.69)
Education (in years)	9.59	10.54	-0.94
	[3.90]	[2.63]	(0.64)
Husband's education (in years)	10.63	11.54	-0.91
	[4.72]	[4.32]	(0.86)
Household size	5.38	4.88	0.51
	[1.97]	[1.87]	(0.37)
Number of children	2.30	2.56	-0.26
	[0.93]	[1.26]	(0.21)
Number of male children	1.30	1.24	0.06
	[0.78]	[0.87]	(0.15)
Number of female children	0.90	1.34	-0.44
	[0.78]	[1.06]	(0.17)
Household monthly income in Rupees	20,516.13	18,820	1,696.13
	[14,622.85]	[12,220.24]	(2,586.00)

Source: Authors' data.

We categorize subjects' behavior in the experiment into three groups: (1) equal split: subject chooses to give equally to both mothers, (2) preference for Muslims: subject chooses to give more to the mother of the Muslim child, and (3) preference for Hindus: subject chooses to give more to the mother of the Hindu child. Note that, for our baseline treatment (INFO-NOID), religious identity was not available to the subjects, and they were asked to choose between giving more to child 1 or child 2, or an equal split. Figure 1 summarizes the average choices made in the experiment. More than 80 per cent of the subjects chose to allocate equally to both mothers in all three treatments.

In the INFO-NOID treatment, where there was no information on religious identities of the recipients, we expect the workers to give equally (barring any irrational choices). In the INFO-ID treatment, when religious identity is provided and the two children are moderately malnourished, presence of taste-based discrimination would suggest giving unequally to one of the two children. In the NOINFO-ID treatment, where only information on religious identity is provided, but no information on the children's nutritional status is revealed, any difference in the preferences for giving to either of the two religious groups can be attributed to either taste-based or statistical discrimination or both. Accordingly, we set up the following three hypotheses respectively on rationality (H1), taste-based discrimination (H2), and statistical discrimination (H3) (see Table 4).

Our findings establish that (1) we are not able to reject the null (H1) that in the absence of information on religious identities, the proportion of subjects giving preference to child 2 is different than the proportion of subjects giving preference to child 1. This suggests that irrational behavior can be ruled out; (2) We are not able to reject the null (H2) that workers do not discriminate against Muslim children; hence there is no evidence of taste-based discrimination in our sample; (3) We are not able to reject the null (H3) that there is no evidence for statistical discrimination.

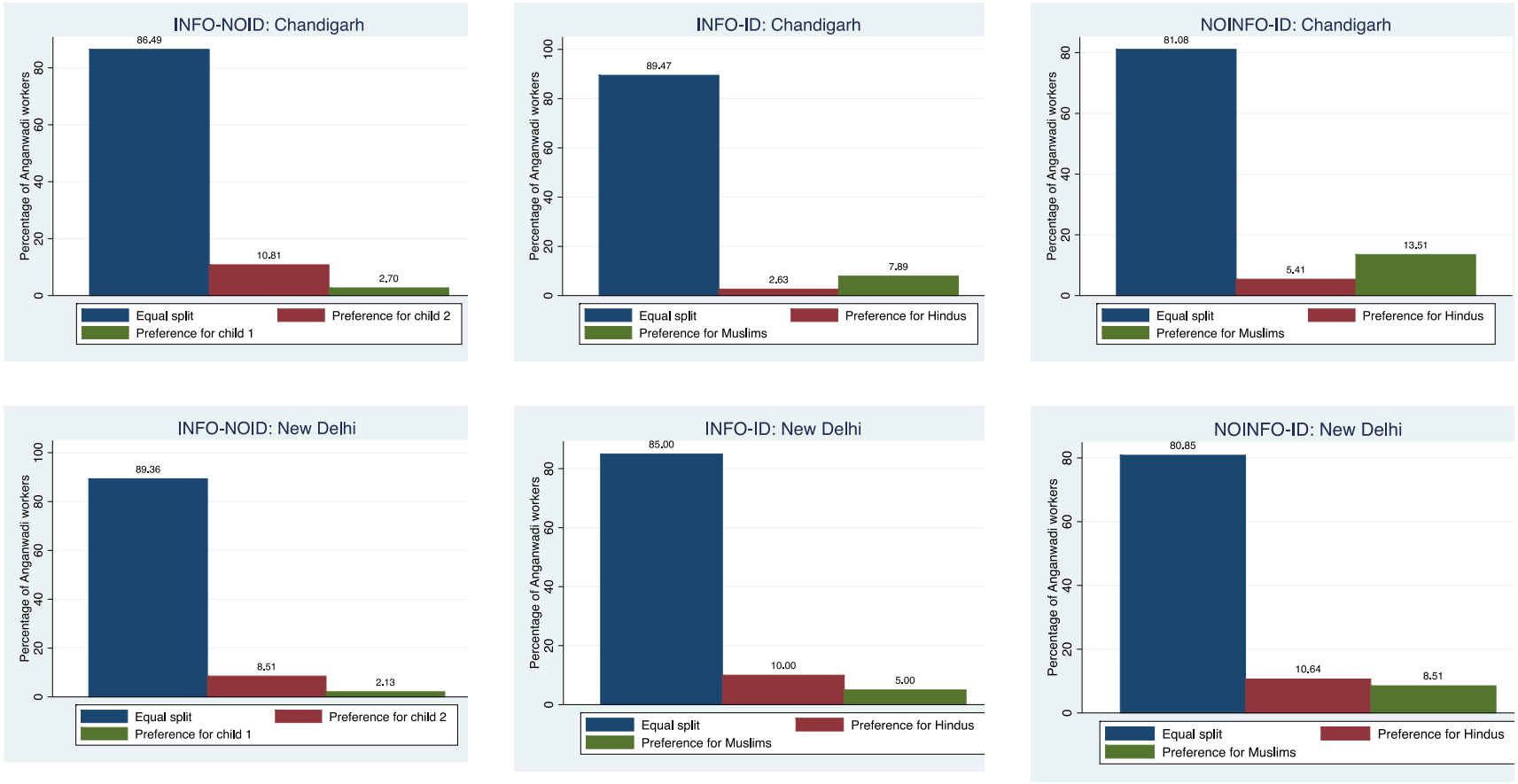

Figure 1: Distribution of choices in the three treatments: Chandigarh and New Delhi

Source: Authors' data.

Table 4: Taste vs. statistical discrimination

	Hypothesis	Decision (p-value) Chandigarh (1)	Decision (p-value) New Delhi (2)
H1:	Test of rationality: In INFO-NOID, proportion giving preference to Child 2—proportion giving preference to Child 1 = 0	Fail to reject (0.16)	Fail to reject (0.17)
H2:	Test of taste-based discrimination: In INFO-ID, proportion giving preference to Hindu child—proportion giving preference to Muslim child = 0	Fail to reject (0.30)	Fail to reject (0.30)
H3:	Test of statistical discrimination: (Proportion giving preference to Hindu child in treatment NOINFO-ID—proportion giving preference to Muslim child in treatment NOINFO-ID)—(proportion giving preference to Hindu child in treatment INFO-ID—proportion giving preference to Muslim child in treatment INFO-ID) = 0	Fail to reject (0.75)	Fail to reject (0.72)

Source: Authors' data.

It is useful to note here that key findings obtained from incentivized experiments implemented in Chandigarh are similar to those obtained from non-incentivized thought experiments implemented in New Delhi (p-value of differences between New Delhi and Chandigarh for H1 is 0.82, for H2 is 0.18, and for H3 is 0.99).

Our results indicate that there is no evidence for taste-based discrimination or statistical discrimination among Anganwadi workers. However, there are two underlying reasons that could cast a shadow on our findings: (a) low statistical power and (b) workers may be indifferent to treatment conditions, and always choose the equal split option. We follow up on these concerns next.

3.1 Saliency issues and weak treatment effects

While our experiment result—that women health care workers in India do not indulge in preferential treatments towards children—is encouraging from a policy point of view, we need to be careful and rule out some possible reasons behind the lack of significant differences across treatments.

Ruling out low statistical power

To rule out sample size related concerns, and lack of statistical power, we provide a discussion on power calculations in footnote 10 as well as report results from the bootstrap technique outlined in Moffat (2016).

We compute bootstrap standard errors using 999 replications for a two-tailed test at the 5 per cent significance level. In Table 5, we replicate our main findings with bootstrap p-values. The p-values reported in Table 5 are consistent with our main findings of no religious discrimination (either taste-based or statistical).

Table 5: Taste vs. statistical discrimination (bootstrap standard errors)

	Hypothesis	Chandigarh	New Delhi
H1:	Test of rationality: In INFO-NOID, proportion giving preference to Child 2—proportion giving preference to Child 1 = 0	Fail to reject (p = 0.15)	Fail to reject (p = 0.13)
H2:	Test of taste-based discrimination: In INFO-ID, proportion giving preference to Hindu child—proportion giving preference to Muslim child = 0	Fail to reject (p = 0.28)	Fail to reject (p = 0.31)
H3:	Test of statistical discrimination: (Proportion giving preference to Hindu child in treatment NOINFO-ID— proportion giving preference to Muslim child in treatment NOINFO-ID)—(proportion giving preference to Hindu child in treatment INFO-ID—proportion giving preference to Muslim child in treatment INFO-ID) = 0	Fail to reject (p = 0.74)	Fail to reject (p = 0.73)

Source: Authors' data.

Ruling out subject indifference to treatments

Since we find no evidence of religious discrimination (taste or statistical) in our samples, it is possible that workers are indifferent to any treatment and the information provided within each and consequently choose the equal split by default. If this is true, workers should be consistent in making indifferent choices in favor of equal split in other informational treatments as well. Additionally, if we had found a significant presence of statistical discrimination in H3, we could test for differential patterns in allocation based on a choice between malnourished (versus normal weight) children. In order to shed more light on choices we ran another set of experiments using a different sample of Anganwadi workers in Chandigarh in January, 2016. We wanted to examine whether workers remain indifferent to other types of information (barring religion) related to children. The choices now included splitting between (A) mothers of a moderately malnourished and a healthy child, (B) a poor and a rich mother, (C) an illiterate and a literate mother, (D) a non-working and a working mother, (E) a mother with low assets and one with high assets, (F) a mother with a physically challenged child and a mother with a child without any handicap, and finally (G) a mother of a boy and a mother of a girl. In a similar experimental design, the subjects were asked to given the same set of allocation choices as before.[13] Table 6 reports the related hypotheses and the results.

We find that workers make allocation decisions based on a number of characteristics and along expected lines. In particular, they provide significantly greater amounts to (1) the mother of a moderately malnourished child, (2) the poor mother, (3) the illiterate mother, (4) the non-working mother, (5) the mother with low assets, and (6) the mother with a physically challenged

[13] All the games were played with all the 49 workers. Only situation (A) was incentivized and the remainder were thought experiments along the lines of the earlier Delhi experiment.

child. Moreover, the differences are twice as large for (2), (5), and (6) compared to (1) and (3). This implies that workers have a very strong belief in giving to mothers who are poorer and "unlucky" and more generally those who are less able to fend for themselves and their children. For (7), we do not find significant allocation by gender and the sign predicts favorable treatment towards girls. These results provide suggestive evidence that workers make Rawlsian allocation decisions based on observable characteristics and appear to be strongly pro-poor but do not discriminate exhibit taste-based preferences for a certain religion or use religion as a proxy for observable differences.[14] These findings are similar to behavior observed in dictator games, where giving increases substantially when the recipient is "deserving," for example can be seen as an object of charity (Engel 2011).

Table 6: Workers allocation decision

	Hypothesis	Decision Difference (p-value) Chandigarh (1)
H1:	In INFO-NOID, amount allocated to the mother of a moderately malnourished child – amount allocated to the mother of a child with normal weight = 0	Reject 25.71 (0.00)
H2:	In INFO-NOID, amount allocated to the mother of a child with a household income of Rs. 2000 per month – amount allocated to the mother of a child with a household income of Rs. 8000 per month = 0	Reject 53.10 (0.00)
H3:	In INFO-NOID, amount allocated to an illiterate mother who has a child – amount allocated to a literate mother who has child = 0	Reject 22.45 (0.00)
H4:	In INFO-NOID, amount allocated to a non-working mother who has a child – amount allocated to a working mother who has a child = 0	Reject 33.67 (0.00)
H5:	In INFO-NOID, amount allocated to the mother of a child whose house does not have fridge and water filter – amount allocated to the mother of a child who has both a fridge and water filter at home = 0	Reject 55.51 (0.00)
H6:	In INFO-NOID, amount allocated to a mother whose child has Polio – amount allocated to mother whose child is normal = 0	Reject 54.89 (0.00)
H7:	In INFO-NOID, amount allocated to a boy child's mother – amount allocated to a girl child's mother = 0	Do not Reject -8.16 (0.18)

Source: Authors' data.

5 Discussion

In this paper, we design an artefactual field experiment to elicit preferences for discrimination along with the aim of identifying the sources of discrimination (statistical versus taste-based if they exist). We implement our experiment among government-employed pre-school caregivers in India. Our results suggest that among child care workers in Chandigarh and New Delhi there

[14] We also check for differential subjective attitudes towards having a Muslim neighbor and also a neighbor from a different caste. The summary of this analysis is provided in Appendix A2 and the results are consistent with the workers having no discriminatory attitude against having Muslims as neighbors.

is no evidence of religious discrimination. Further, there is no evidence of taste-based discrimination or statistical discrimination.[15] Finally, key findings obtained from incentivized experiments are similar to those obtained from non-incentivized thought experiments.

In economics generally, and development economics in particular, there has been an intentional shift towards more evidence-based policies. Policies that address discrimination are no exception. We are bearers of good news – Anganwadi workers in our sample do not discriminate along religious lines, and instead seem to exhibit a pro-social preference towards the poor and the undernourished.[16] Also, subjective attitudes towards having Muslims as neighbors appear to be positive, as shown in Appendix A2. While these are encouraging results, one needs to be cautious in interpreting the implications of our findings on the overall issue of religious discrimination in India. A failure to find taste-based or statistical discrimination along religious lines in our sample does not imply the absence of religion-based discriminatory attitudes in other areas of public health in general, or even among Anganwadi workers in other Indian states where the population is ideologically more polarized, and states where health statistics vary more acutely by religious identity. Only through a series of investigations, possibly using experiment designs similar to ours, can we hope to arrive at a complete picture of the existence, nature, and extent of covert and overt religious discriminatory practices. Some possible domains where our experiment game can be implemented are areas of public services where profit or revenue maximization is often secondary (for example among teachers in education institutes, employees in public sector banks, policemen managing traffic or law and order, and even bureaucrats and diplomats).

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[15] It is possible that taste-based discrimination in a society over time leads to statistical discrimination in the future. This dynamic framework is important in the discrimination context but is beyond the scope of this paper.

[16] These findings may be specific to public health workers who may be intrinsically motivated and different from professionals in other sectors.

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Appendix

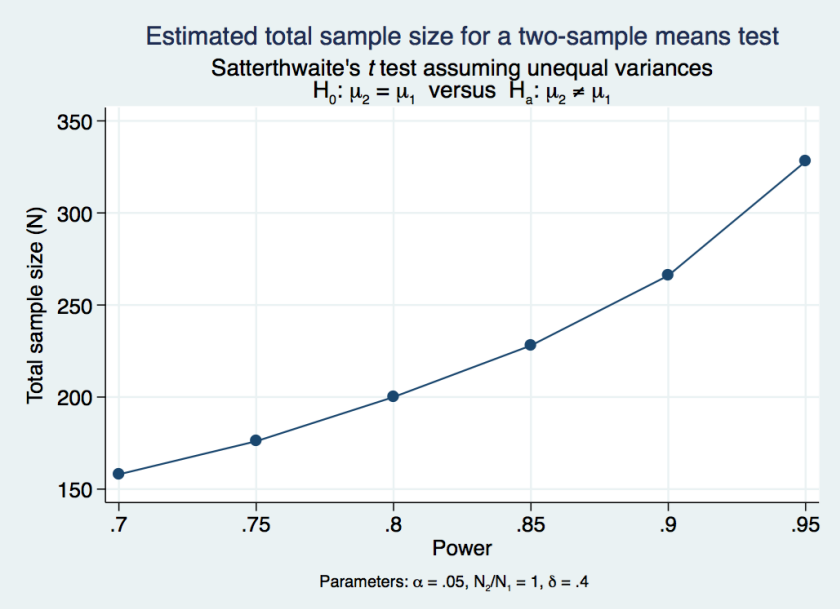

Figure A1: Power calculations

Source: Authors' calculations.

Appendix A1: Subject instructions for Chandigarh

Instructions for Task 2

Thank you for your participation. You will be paid Rs. 50 for your participation.

Each of you will participate in one task in this experiment. At the end of the task, in addition to the fixed fee, 25 per cent of you will be randomly chosen for additional payments. Here is a bag that contains tokens with all subject IDs written on them. One of the workers will be asked to pick a certain number of these tokens and announce the IDs. For example, if there are 40 participants in a session, we will pick 10 IDs and pay the participants who have those IDs. These randomly chosen participants will be paid according to their decision made on this task.

It is important that you read the instructions for the task before making a decision. If you do not understand, you will not be able to participate effectively. In case you have a question while reading the instructions, please raise your hand and we will escort you to the nearby room and clarify your query.

Each page has an ID# on it. Do not show this ID# to any other participant or allow it to be visible to anyone during or after this experiment. There should be no talking or discussion of the task amongst you while waiting for the experiment to begin. We request you to remain seated, and not do anything unless instructed by the experimenter. Also do not look at others' responses once the experiment begins. You might be disqualified otherwise.

If you are ready, then we will proceed.

Each of you will make decisions that will decide your own payouts and the payouts of mothers of two randomly chosen pre-school children. The children will be chosen from our sample through a lottery. They belong to an Anganwadi Center in an urban slum of Chandigarh that is not in your block. Note that only mothers of children identified as "moderately malnourished/suffer from grade 2 malnourishment" are eligible to receive the payouts from the experiment.

All decisions will remain private, and no one will get to know who was matched with whom (either during, or after the experiment due to the code structure of the experiment).

In this task you have inherited Rs. 120. Your task is to decide on how much of that money you want to keep for yourself, how much of it you want to give to give to the two randomly chosen mothers of grade 2 malnourished children.

Please choose only one of the following four options: A, B, C or D. Notice that in all the options, the sum of the allocations between you, mother of child 1, and mother of child 2 should add up to Rs. 120. Note that child 1 and child 2 are both "moderately malnourished/suffer from grade 2 level of malnourishment."

After marking your decisions please fold your response sheet and place it in front of you. The experimenter will collect them.

Child 1's name is [Announced by the experimenter]……..

Child 2's name is [Announced by the experimenter] ……..

CIRCLE THE OPTION YOU WANT TO CHOOSE BELOW:

OPTIONS

A	I want to keep Rs. 40 for myself I want to give Rs. 40 to the mother of child 1 I want to give Rs. 40 to the mother of child 2
B	I want to keep Rs. 40 for myself I want to give Rs. 0 to the mother of child 1 I want to give Rs. 80 to the mother of child 2
C	I want to keep Rs. 40 for myself I want to give Rs. 80 to the mother of child 1 I want to give Rs. 0 to the mother of child 2
D	I want to keep Rs. 40 for myself I want to give Rs.. ……...(write amount here) to the mother of child 1 I want to give Rs. ……..(write amount here) to the mother of child 2 [Note: the three amounts must add up to Rs. 120]

Instructions for Tasks 1 and 3

Thank you for your participation. You will be paid Rs. 50 for your participation.

Each of you will participate in two tasks in this experiment. At the end of the two tasks, in addition to the fixed fee, we will randomly choose one task for which some of you will also receive additional payments.

On the table at the front of the room are 2 cards, each with 1, or 2 written on them. They are kept facing down so that the numbers are not visible. We will ask one of you to pick one of the cards and announce the number written on that card. That particular task will then be used for making the additional payments for this session. For example, if the worker picks a card with 1 written on it, task 1 will be used for making the additional payments. After a specific task has been chosen for payments, 25 per cent of you will be randomly chosen for the additional payments. Here is a bag that contains tokens with all subject IDs written on them. One of the workers will be asked to pick a certain number of these tokens and announce the IDs. For example, if there are 40 participants in a session, we will pick 10 IDs and pay the participants who have those IDs. These randomly chosen participants will be paid according to their decision made on the task that was chosen for the additional payments.

We are about to read the instructions for the two tasks. Please listen carefully. It is important that you read the instructions for the tasks before making a decision. If you do not understand, you will not be able to participate effectively. In case you have a question while reading the instructions, please raise your hand and we will escort you to the nearby room and clarify your query.

Each page has an ID# on it. Do not show this ID# to any other participant or allow it to be visible to anyone during or after this experiment. There should be no talking or discussion of the task amongst you while waiting for the experiment to begin. We request you to remain seated,

and not do anything unless instructed by the experimenter. Also do not look at others' responses once the experiment begins. You might be disqualified otherwise.

If you are ready, then we will proceed.

Instructions for Task 1

Each of you will make decisions that will decide your own payouts and the payouts of mothers of two randomly chosen pre-school children. The children will be chosen through a lottery from an Anganwadi Center in an urban slum of Chandigarh that is not in your block. Note that only mothers of children identified as "moderately malnourished/suffer from grade 2 level malnourishment" are eligible to receive the payouts from the experiment.

All decisions will remain private, and no one will get to know who was matched with whom (either during, or after the experiment due to the code structure of the experiment).

In this task you have inherited Rs. 120. Your task is to decide on how much of that money you want to keep for yourself, how much of it do you want to give to the two randomly chosen mothers of grade 2 level malnourished children.

Please choose only one of the following four options: A, B, C, or D. Notice that in all the options, the sum of the allocations between you, mother of child 1, and mother of child 2 should add up to Rs. 120. Note that child 1 and 2 are both "moderately malnourished/suffer from grade 2 level of malnourishment."

After marking your decisions please fold your response sheet and place it in front of you. The experimenter will collect them.

CIRCLE THE OPTION YOU WANT TO CHOOSE BELOW:

OPTIONS	
A	I want to keep Rs. 40 for myself I want to give Rs. 40 to mother of child 1 I want to give Rs. 40 to mother of child 2
B	I want to keep Rs. 40 for myself I want to give Rs. 0 to mother of child 1 I want to give Rs. 80 to mother of child 2
C	I want to keep Rs. 40 for myself I want to give Rs. 80 to mother of child 1 I want to give Rs. 0 to mother of child 2
D	I want to keep Rs. 40 for myself I want to give Rs. ……(write amount here) to mother of child 1 I want to give Rs. ……(write amount here) to mother of child 2 [Note: the two amounts must add up to Rs. 80]

Instructions for Task 3

Each of you will make decisions that will decide your own payouts and the payouts of mothers of two randomly chosen pre-school children. The children will be chosen from our sample through a lottery. They belong to an Anganwadi Center in an urban slum of Chandigarh that is not in your block.

All decisions will remain private, and no one will get to know who was matched with whom (either during, or after the experiment due to the code structure of the experiment).

In this task you have inherited Rs. 120. Your task is to decide on how much of that money you want to keep for yourself, how much of it you want to give to the two randomly chosen mothers of pre-school children.

Please choose only one of the following four options: A, B, C or D. Notice that in all the options, the sum of the allocations between you, mother of child 1, and mother of child 2 should add up to Rs. 120.

After marking your decisions please fold your response sheet and place it in front of you. The experimenter will collect them.

Child 1's name is [Announced by the experimenter]……..

Child 2's name is [Announced by the experimenter] ……..

CIRCLE THE OPTION YOU WANT TO CHOOSE BELOW:

OPTIONS	
A	I want to keep Rs. 40 for myself, I want to give Rs. 40 to the mother of child 1 I want to give Rs. 40 to the mother of child 2
B	I want to keep Rs. 40 for myself, I want to give Rs. 0 to the mother of child 1 I want to give Rs. 80 to the mother of child 2
C	I want to keep Rs. 40 for myself, I want to give Rs. 80 to the mother of child 1 I want to give Rs. 0 to the mother of child 2
D	I want to keep Rs. 40 for myself, I want to give Rs. ……..(write amount here) to the mother of child 1 I want to give Rs. ……..(write amount here) to the mother of child 2 [Note: the three amounts must add up to Rs. 80]

Please respond to the following additional questions:

Children of which religion do you think are more likely to be malnourished. Please circle the most appropriate option.

1) Hindu

2) Muslim

3) Sikh

4) Christian

5) Other

If your answer to this question matches the modal choice then you will receive an additional Rupees 20 for this question.

Appendix A2: Subjective attitudes towards Muslims as neighbors

Finally, we also tested for the subjective attitudes of workers towards having Muslims as neighbors and benchmark it against having someone from a different caste. This was measured on a 7-point Likert scale stretching from "Very good - 1" to "Very bad - 7". We find that all workers report feelings between 1 and 4 with the box whisker diagram as shown in Figure A2 below. Moreover, the first and third quantiles are between Good (2) and Neither good nor bad (4), and are identical for both the religious identity and caste identity of neighbors. We find a mean score of 2.41 for Muslim neighbors and 2.45 for neighbors belonging to a different caste. Even though true feelings could be different from the responses to this question, we can conclude that these subjective attitudes are consistent with our main findings of no taste-based discrimination against Muslims.

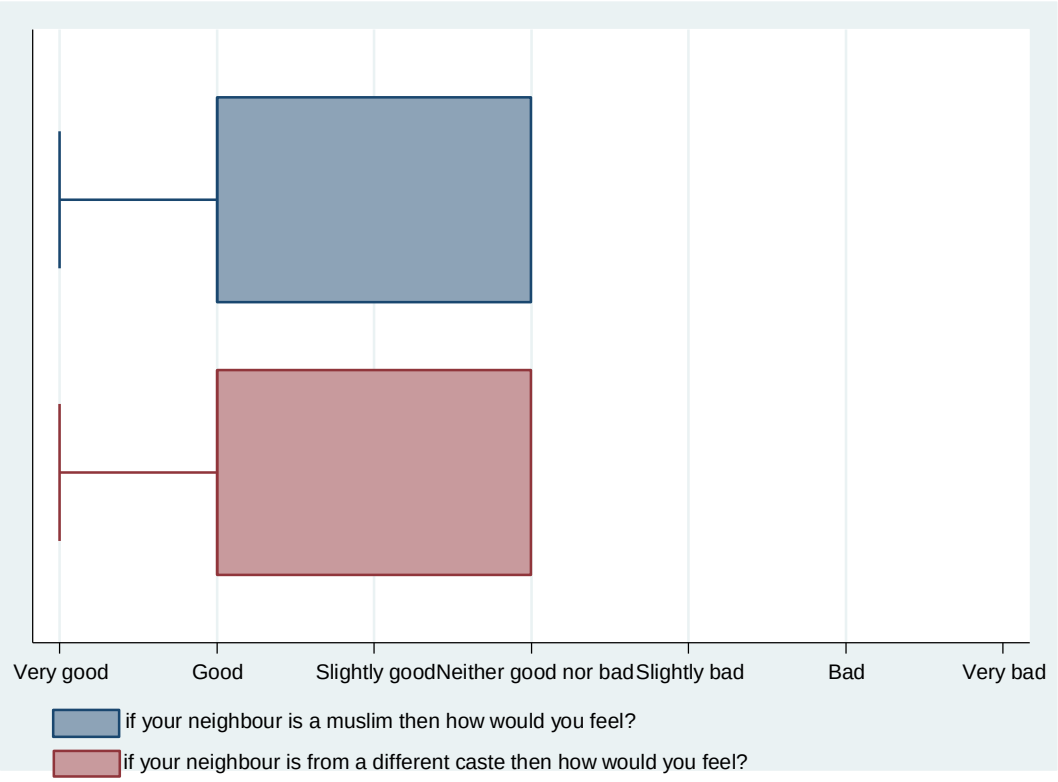

Figure A2: Subjective attitude of workers towards having Muslim neighbors

Source: Authors' calculations.