Discrimination in a Deprived Neighbourhood: An Artefactual Field Experiment*

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Abstract

We present a field experiment designed to examine the discriminatory motives of an understudied demographic: the poorest people in England. Subjects are first asked to divide £10 between two strangers, and then play a £10 dictator game with another stranger. We subtly vary the ethnicity of the receivers by providing subjects with surnames randomly drawn from the electoral register, including treatments that allow us to parse behaviour into either in–group favouritism or out–group negativity, an important behavioural distinction that is typically overlooked in the discrimination literature. Our results suggest that the observed discriminatory attitudes are the result of out-group negativity rather than in-group favouritism. We advance the literature on discrimination through the estimation of a structural model of group–contingent social preferences, which we exploit to perform counterfactual simulations. Our results provide insights into the behaviour of this unique demographic and provide a rationale for why they may support discriminatory policies in their voting behaviour.

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"They're not coming to this country if I'm president"

— Donald J. Trump, 2015

1 Introduction

Donald Trump, the recently inaugurated 45^{th} President of the United States, was branded a bigot and a racist for the proposed policy of banning Muslims from entering the United States during his presential campaign (Milibank, 2015). His comments were described as "hate speech," and the United Kingdom's House of Commons debated banning him from entering the country after a petition calling for his ban was signed by 500,000 people.[1] Similar anti–Muslim sentiment was at the heart of the recent election of the London Mayor, Sadiq Kahn and the 'Vote Leave' campaign during the British referendum on EU membership. For example, those standing in opposition to Sadiq Kahn made a number of allegations linking him to Muslim extremist groups, and Vote Leave directed significant attention to potential increased immigration from Muslim countries in order to promote anti–EU sentiment.[2]

The similarity between the Trump and Vote Leave campaigns is likely not a coincidence, as supporters of both Trump and Vote Leave appear to share similar views. As highlighted by a number of pre and post–referendum polls, voters most likely to have supported Vote Leave are white, have low levels of education and earn low incomes.[3] The same appears to be true for the supporters of Donald Trump (Thompson, 2016). Further, surveys suggest that 30% of British voters would support policies that reduced the Muslim population (Townsend, 2012), and around 27% believe Islam is incompatible with the values of British democracy. This latter view is found to be correlated with respondents' incomes and level of education (Page, 2009).

However, little is known about *why* individuals from this particular demographic might support policies that discriminate against those with Islamic heritage. This is largely because there is little research into the behaviour of the poorer, less educated population. Previous work into discrimination against Muslims has typically focused on better educated and wealthier individuals that are less likely to be supporters of such policies. For example, Ahmed (2010*b*) reports evidence of discrimination in trust and dictator games against non–Europeans in an experiment utilising Swedish university students. Ahmed & Hammarstedt (2008) find that landlords in the Swedish

[1] The petition, parliamentary debate and discussion can be viewed through the UK's Parliamentary Petition website: https://petition.parliament.uk/petitions/114003.

[2] A number of newspaper articles demonstrate this point. For example, Hinsliff (2016) reports on Sadiq Kahn's competitors trying to link him to an ISIS sympathiser. The former UKIP party leader, and prominent figure in the Vote Leave campaign, Nigel Farage, posed with a billboard poster of Muslim refugees with the slogan, 'Breaking Point' (Wright, 2016), and Vote Leave 'appealed to prejudice' by claiming Turkey was about to join the EU and that its citizens posed a threat to UK national security (Boffey & Helm, 2016).

[3] Three separate studies support this. The *Lord Ashcroft Poll* information on voter demographics: http://lordashcroftpolls.com/2016/06/how-the-united-kingdom-voted-and-why/; the *Telegraph* newspaper poll: http://www.telegraph.co.uk/news/2016 /06/22/eu-referendum-which-type-of-person-wants-to-leave-and-who-will-b/; and a *Guardian* report: https://www.theguardian.com/news/datablog/2016/jun/24/the-areas-and-demographics-where-the-brexit-vote-was-won.

housing market discriminate against tenants with Muslim sounding names in comparison to 'native' names. Ahmed *et al.* (2009) find that small business sellers are less likely to contact potential buyers if they have Muslim sounding names. Kaas & Manger (2012) report similar evidence of discrimination by hiring committess in the German labour market towards those with Turkish sounding names. Booth *et al.* (2012) report comparable results from the Australian labour market against those with names of Middle Eastern origin. However, the strategic nature of the interactions in these studies means that individuals may be using ethnic stereotypes in order to inform their decisions, making preference (Becker, 1971) and statistical (Phelps, 1972) explanations of discrimination difficult to disentangle.

In contrast to field studies, lab experiments provide a more tightly controlled environment in which statistical and taste–based explanations of discrimination can be more easily parsed. Experimenters have employed a range of strategic and non–strategic games in order to distinguish between the two competing explanations, and also to develop new behavioural theories of discrimination. Recent studies suggest that individuals' social preferences are group–contingent, i.e. that the extent to which individuals care about others depends on the degree to which they identify with them (Akerlof & Kranton, 2000; Chen & Li, 2009). Individuals have been found to behave more charitably (Chen & Li, 2009; Whitt & Wilson, 2007), cooperatively (Drouvelis & Nosenzo, 2013; Falk & Zehnder, 2013; Goette *et al.*, 2006; Ruffle & Sosis, 2006) and coordinate more efficiently (Chen & Chen, 2011) when interacting with those they perceive as the 'in–group', in comparison to the 'out–group'. A stylised interpretation of these findings is that individuals exhibit favouritism towards the in–group, rather than negativity towards the out–group, what Bernhard *et al.* (2006) call *parochialism*. As noted by Becker (1971), this is an important behavioural distinction, and as suggested by Ahmed (2007), many studies that conclude that individuals exhibit in–group favouritism omit a treatment where interactions are 'group neutral' (Falk & Zehnder, 2013; Fershtman & Gneezy, 2001; Goette *et al.*, 2006; Ruffle & Sosis, 2006; Whitt & Wilson, 2007), and so are unable to parse behaviour into in–group favouritism or out–group negativity.

The purpose of this paper is to shed light on the potential motives underpinning the behaviour of an understudied demographic: poor and less educated white-British individuals. We focus on this demographic because, as previously discussed, opinion polls suggest them to be most supportive of discriminatory policies. This paper aims to determine if their support for anti–Muslim policies could be a result of taste–based discrimination (Becker, 1971). This is achieved using a door–to–door field experiment conducted in the poorest areas in England, two housing estates in the town of Rochdale. The residents of these estates were chosen because they are highly representative of the demographic of interest: they are white, have received little formal education and have very low incomes. Many of those who took part in our experiment live on incomes as low as £57 per week, with some subjects living in neighbourhoods that endure unemployment rates of 100%. The experiment was conducted almost two years prior to the actual UK referendum, and around 18 months before the announcement that a referendum would be held. Analysing post referendum voting data shows that the voter turn out in Rochdale was 65% with 60% voting to leave the European Union. This compares with the national average voter turnout of 72%, of which 52% voted to leave.

Subjects were first asked to make distributional choices in Other–Other games, where they had to divide £10 between two strangers. Following the social psychology experiments of Turner (1978), this was done in order to make their ethnicity salient. They then played a single Dictator Game, dividing £10 between themselves and a receiver. Similar to Fershtman & Gneezy (2001), and the lost letter experiments of Ahmed (2010*a*), the 3×1 between–subject design subtly varies the ethnicity of the individuals that subjects are allocating money to by providing them with surnames taken from the local electoral register.[4] The surnames were categorised into either *English* or *Muslim* ethnic origin using the taxonomy of Mateos *et al.* (2007). These allocation decisions are then compared to the decisions from an *Anonymous* baseline treatment, where the receiver's surname is withheld, allowing us to distinguish between in–group favouritism and out–group negativity. As we study a non–standard subject pool, the experiment satisfies the artefactual field experiment criteria of Harrison & List (2004).

In the Dictator Game, we find that subjects give around £5 to receivers with surnames of *English* origin, £2 to those with surnames of *Muslim* origin and £5 to those in the *Anonymous* treatment. Individuals with surnames of *Muslim* origin are treated worse than someone who is *Anonymous*, whilst those with surnames of *English* origin are not treated more favourably. Thus, in contrast to the conclusions of the majority of the literature we find no evidence of in–group favouritism, but instead report evidence of out–group negativity. This is a particularly interesting finding, as anti–immigration and nationalist groups often associated with this demographic employ slogans or names that focus on the in–group, and imply that they are promoting in–group favouritism, rather than supporting out–group negativity.[5]

To link our empirical analysis to behavioural theory, we assume that subjects give in the Dictator Game because they have social preferences, and that these preferences are group–contingent (Chen & Li, 2009). Structural parameter estimates of the Cox *et al.* (2007) social preference utility function reveal that the utility weight that dictators place on the payoff of a receiver varies with the ethnicity of the receiver. This weight is an astounding 87% smaller when the receiver has a surname of *Muslim* origin in comparison to the weight given to receivers' payoffs in the *Anonymous* treatment and to those with surnames of *English* origin. In Section 6 we conduct a counterfactual simulation using the preference parameters we estimate, and discuss how they could provide an explanation for our subjects' potential support of discriminatory policies. Our conclusions may also provide insight into the results of Ahmed & Hammarstedt (2008), Booth *et al.* (2012), and the discrimination faced by Muslims reported in the press.[6]

In the Other–Other games we find that when allocating money between two individuals with surnames of contrasting ethnic origin, one *English* and one *Muslim*, subjects allocate around £1 more to the individual with the surname of *English* origin

[4]Experiments into gender differences conducted by Holm (2000) preceed those of Fershtman & Gneezy (2001) and use a similar method, but utilise forenames rather than surnames in order to study gender.

[5]For example, consider the right–wing British organisation, *Britain First*, that promotes nationalist policies, and other campaigns in the UK such as 'Buy British' or phrases such as 'British Jobs for British Workers'.

[6]For example, Syal & Topping (2014) report on taxi customers in Rochdale requesting 'local' (white British) drivers over the phone, rather than Asian–Muslim drivers.

regardless of the ordering of the surnames. Although this result resonates with the minimal group literature (Turner, 1978; Chen & Li, 2009), we cannot determine if this behaviour is driven by in–group favouritism or out–group negativity as there is no group–neutral baseline from which to compare behaviour.

While discrimination against minorities is not a new finding *per se*, this study makes a number of contributions. First, we contribute to the discrimination literature by examining the behaviour of an understudied population: deprived, white–British individuals, the demographic most likely to support discriminatory policies. Second, we provide the first evidence of the potential motives underpinning the behaviour of this demographic. Finally, we provide a potential behavioural rationale for *why* these individuals might discriminate by modelling ethnic discrimination as being a consequence of group–contingent social preferences.

The remainder of this paper is organised as follows. Section 2 gives details of the study population, Section 3 outlines the experimental design, Section 4 presents the results, Section 5 estimates a structural model, Section 6 provides a counterfactual analysis, and Section 7 concludes.

2 Subject pool

The subjects in our experiment are drawn from two population areas, *Falinge* and *Kirkholt*, two housing estates situated in Rochdale, England, a town located in a wider region that has a recent history of ethnic tensions (Carter & Midlane, 2012; Syal & Topping, 2014). The Rochdale area has an electorate of 156,621, and in the referendum had a voter turnout of 65%. Of the 103,319 registered voters who turned out to vote, 60% voted to leave the European Union. Whilst turnout was lower than the national average (71.8%), the number of voters in favour of leaving was much higher (52%).

The extent of unemployment in the areas of interest is most evident from the English Indices of Deprivation, a five yearly publication from the UK Government's Office of National Statistics (ONS). The report ranks small highly localised populations in terms of relative deprivation. At the time the study took place (Summer 2014) the *Falinge* and *Kirkholt* housing estates were ranked in the top 0.3% of the most income and employment deprived populations in the country. The *Falinge* estate was ranked first out of 32,482. It was determined to be the most deprived area in both these domains.[7]

Table 1 provides information on the housing estates we study. Each estate is divided into different areas by the UK Census, and the Table outlines the number of households, the percentage of people without qualifications, the percentage of people out of work and simplified ethnic demographics for each of these Census Areas. It is evident that a significant number of residents are out of work, and a large proportion have not obtained any formal qualifications. Deprivation rankings in the income and employment domains are given in the final two columns.

Although a majority of residents from the two housing estates are white British

[7]In the latest 2015 report, other locations have become relatively more deprived. However, *Falinge* and *Kirkholt* are still ranked in the top 1% of the most income and employment deprived populations in England.

Estate	*Census Area*	*No. Households*	*Claiming Benefit*[a]	*No Education*[b]	*White*	*Deprivation Domain*[c]	
						Income	*Employment*
Falinge	F1	177	85%	31%	40%	1st	1st
	F2	215	100%*	44%	60%		
Kirkholt	K1	132	75%	44%	82%	98th	50th
	K2	120	65%	31%	66%		
	K3	150	85%	36%	79%		
	K4	136	55%	36%	80%		

Source: Office of National Statistics, English Indices of Deprivation 2010, UK Census 2011 and own calculations. The Output Area codes used within the Census are removed for anonymity reasons. *The Census reports this figure as 120%, which is potentially attributed to fraudulent benefit claims. [a]Percent of population claiming out of work benefits. [b]Percent of population with no formal qualifications. [c]Income and employment deprivation ranks out of 32,482. Higher ranks imply greater levels of deprivation.

Table 1: Population Demographics

nationals, Table 1 highlights that a large number of residents are from non-white minority ethnic groups. In one area in *Falinge* (area F1), non–white residents constitute a majority of the population (60%). In contrast, far fewer minority ethnic groups are present in the population of the *Kirkholt* Estate. The non-white populations in Rochdale are predominantly categorised as being Asian: although this category is very broad, incorporating many different ethnicities, the vast majority of this population in Rochdale are of Pakistani or Bangladeshi origin, with the second most commonly stated religious belief after Christianity being Islam.

3 Experimental design

We study the other–regarding behaviour of white British nationals and how their behaviour is influenced by the *English* and *Muslim* ethnic origins of those they interact with. This was achieved by conducting a door-to-door artefactual field experiment administered to the population areas outlined in Section 2. All subjects first completed a series of Other–Other Games, dividing £10 between two other people (Part 1). They then played a single Dictator Game, where they allocated £10 between themselves and a receiver (Part 2). All choices were made in whole pounds. Subjects then completed a post experimental questionnaire.[8]

To vary the ethnicity of the individuals that subjects are allocating money to, we provided them with their surnames. In an attempt to avoid any experimenter bias in surname selection, we classified surnames taken from the Edited Electoral Register for the Rochdale area into different groups of 'ethnic and cultural' origin using the 'Cultural, Ethnic and Linguistic' taxonomy of Mateos *et al.* (2007). Only those surnames which were classified as 'Western European, English' in origin (for example, *Smith*) and those of 'Muslim' origin (for example, *Islam*) were used.[9]

[8]All experimental materials are included in Appendix A.

[9]These names are examples, and were not necessarily used within the study. The surnames, and actual

Households that answered the door were read out a fixed script that outlined who the caller was, and were asked if they would like to take part in an 'Economic Decision Making Study'. They were told they would receive £2.50 for taking part, and that they had the opportunity to earn additional money. If a resident agreed to take part the experiment was conducted at the door–step. Once finished, subjects were paid in cash. The order in which streets were approached was randomised and only one person per household was permitted to take part.

Subjects were told that once the study was completed those they were allocating money to would receive payment in cash through the post, which they did. Subjects were aware that they were not allocating money directly to their neighbours as the housing estates studied make up only a tiny fraction of the entire town of Rochdale. Subjects were also told that they were allocating money to people who would not be required to make a decision, and that these people were not even aware that they were involved in the study. Any money they received would be a surprise. This was emphasised in an attempt to mitigate the effect that subjects' first order beliefs (their belief about the receiver's choice) and second order beliefs (their belief about the receiver's expectation of their choice) might have on their behaviour. These beliefs have been highlighted as important for reciprocity in a number of studies (Falk & Fischbacher, 2006; Yamagishi & Kiyonari, 2000). In order to control for the effects stemming from the commonality of social group affiliation (Guala *et al.*, 2013), subjects were assured they would remain anonymous to the receivers and the receiver would not be informed about the dictators' identity.

In Part 1 subjects were asked to allocate £10 between two other people ('*Person A*' and '*Person B*'), but were not able to allocate any money to themselves, and therefore received no payment for their decisions. They were required to do this under three schemes in which they were provided with the surnames of the two people they were allocating money to. Subjects completed what we label an *In–In*, *Out–Out* and *In–Out* scheme, in a random order. In the *In–In* scheme, subjects allocated £10 between two people with surnames of *English* origin and in the *Out–Out* scheme the money was allocated between two people with surnames of *Muslim* origin. In the *In–Out* scheme money was allocated between one person with a surname of *English* origin, and one with a surname of *Muslim* origin, the order of which was randomised between subjects to control for any order effects. Subjects were informed that one scheme would be selected for payment at random and that the two individuals from that scheme would receive payment through the post. Subjects did not learn the scheme that was selected until the experiment was completed. Part 1 is motivated by the minimal–group findings of Turner (1978), who reports that Other–Other allocation choices can enhance the salience of subjects' identities in subsequent decisions.[10]

In Part 2 subjects were asked to allocate £10 between themselves and a receiver, an individual randomly selected from the Edited Electoral Register. The between–subject design varied whether the receiver had a surname of *English* origin (*English* Treatment), a surname of *Muslim* origin (*Muslim* Treatment) or if the surname was withheld (*Anonymous* Treatment). Each subject was randomly assigned to a treatment, and the surname was unknown to the experimenter. Once they had made

examples, are not given for anonymity reasons.

[10]This may not be a robust finding, as Chen & Li (2009) found that Other–Other games have no effect on identity salience.

their choice they were paid in cash. A £10 endowment was chosen as it is the 'standard' dictator amount (Engel, 2011), and thus allows for a comparison with previous studies.

To elicit background characteristics, subjects completed a post experimental survey and self–reported a number of characteristics. However, the main survey question of interest was one which aimed to measure group attachment and to check the experimental manipulations were successful. In the post experimental survey, in a manner similar to Yamagishi & Kiyonari (2000), subjects were asked, '*How close did you feel to your match in Part 2, based on their surname alone?*'.[11] Subjects were asked to make a choice on a scale from 1 to 10, with 1 being '*Not at all*' and 10 being '*Very much so*'. If ethnicity is important to subjects, then this measure of *Closeness* should register increases (or decreases) relative to the *Anonymous* baseline.[12] Table 2 provides a summary of the experimental design.

Treatment	*Part 1*	*No Surname*	*English*	*Muslim*	*Survey*	*Observations*
Anonymous	✓	✓			✓	38
English	✓		✓		✓	42
Muslim	✓			✓	✓	42

Table 2: Experimental Design – Part 2

The experiment was conducted between the hours of 12pm and 6pm during the summer of 2014. A total of 16 full days across 4 weeks were required to collect all the observations. The experiment was conducted by a single experimenter, who was a white British male. A total of 828 individual addresses from the two housing estates were approached, 341 residents answered the door, and 132 agreed to take part.[13] We were unable to recruit any additional residents: of the 487 addresses that did not answer the door, all were approached an additional time at a later date. The 209 addresses which refused to take part were not approached again.[14]

4 Results

This section outlines the experimental results. A number of common features are present throughout: where non–parametric tests are given, the p–value and test used are both presented in parentheses. Unless otherwise stated, the null hypothesis is always that there is no difference in behaviour between treatments and all reported

[11] In the *Anonymous* Treatment subjects were asked '*How close did you feel to your match?*'

[12] This question is a variation of a question used by Turner (1978), "*How much did you like the people in your group?*", or of that used by Chen & Li (2009), "*Please rate how closely attached you felt to your own group throughout the experiment*".

[13] While this participation rate seems low, it is comparable to the response rate of students to email communication inviting them to participate in laboratory experiments. At the FEELE laboratory at the University of Exeter, we frequently send out six email invitations in order to get a single participant.

[14] We do not include the responses from 10 residents who were either non–white, non–British or both. However, these 10 residents were paid for their decisions, and their allocations were sent to the receivers.

tests are two sided.[15] Only one person per household took part and each individual observation is treated as independent. As the experiment was conducted over four weeks, subjects may have heard about the study from neighbours or through social media, causing behaviour to differ over time. No evidence is found of a trend in behaviour over the course of the experiment ($p = 0.76$, Cuzick's Wilcoxon–like test for trend) (Cuzick, 1985) so all observations are pooled.[16]

4.1 Dictator game

Table 3 presents summary statistics of the subjects' choices and self reported demographics.[17] Figure 1 displays box plots of amounts given in each treatment.

		% of subjects:		*Median*	*Mean*	*Mean*
Treatment	*Male*	*Employed*	*No Education*	*Income*	*Closeness*[a]	*Amount Given*[b]
Anonymous	57%	26%	26%	≤£10,000	2.95 (2.78)	4.05 (2.88)
English	43%	22%	44%	≤£10,000	3.78 (2.78)	4.88 (3.15)
Muslim	35%	19%	29%	≤£10,000	2.61 (2.65)	2.62 (2.47)

Note: Standard deviations are given in parentheses. *Male*, *Employed*, *No Education*, *Income* and *Closeness* summarize the self–reported answers to the post experimental questionnaire. [a] *Closeness* was measured on a Likert Scale as the answer to the question, "*How close did you feel to your match (based on their surname alone)?*" with 1 being "*Not at all*" and 10 being "*Very much so.*" [b]Amount given in the dictator game in pounds.

Table 3: Summary Statistics

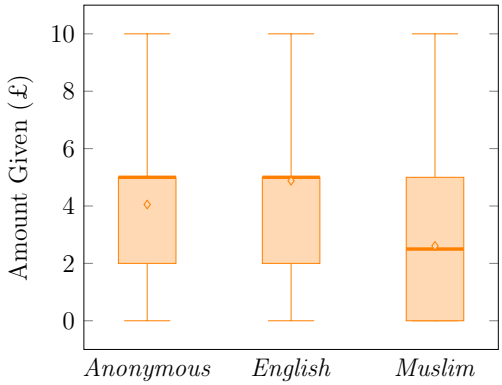
Result 1 (Out–Group Negativity): *In the Dictator Game subjects give around half as much to those with surnames of Muslim origin in comparison to those with surnames of English origin, and in comparison to those who are Anonymous.*

Support. Table 4 presents the test statistics and p–values for the pairwise comparisons of distributions and medians between treatments. As shown, no significant differences are reported when comparing how much was given to those with surnames of *English* origin to how much was given to those who are *Anonymous*. However, when comparing giving to those with surnames of *Muslim* origin to giving to those with surnames of *English* origin and to those who are *Anonymous*, significant differences are found in both distributions and medians.

[15] We use non–parametric Robust Rank Order tests instead of Wilcoxon-Mann-Whitney tests following the analysis of Feltovich (2003).

[16] While this test is commonly used in the medical literature, it is less common in the economics literature. The Cuzick Test is an extension of the Wilcoxon-Rank-Sum Test which allows to test for trend across three or more ordered groups.

[17] Randomisation into treatments was done by putting instructions relating to each treatment into a random order. At the door, the experimenter then used the instructions in that random order. See Appendix B for a summary of all self reported demographics.



Note: ⋄ represents the mean. Thick horizontal bars represent the median.

Figure 1: Box Plots of Amounts Given in the Dictator Game by Treatment

Comparison	*Alternative Hypothesis*	*Test Statistic*	*p–value*	
Distributions[a]	H_A: *English* ≠ *Anonymous*	χ^2=1.366	0.266	
	H_A: *Muslim* ≠ *Anonymous*	χ^2=5.541	0.019	**
	H_A: *Muslim* ≠ *English*	χ^2=11.637	0.001	***
Medians[b]	H_A: *English* ≠ *Anonymous*	Ú=-1.115	0.265	
	H_A: *Muslim* ≠ *Anonymous*	Ú=2.341	0.019	**
	H_A: *Muslim* ≠ *English*	Ú=3.63	0.000	***

Note: In each comparison the null hypothesis is always that there is no difference between treatments. ***, ** denote significance at the 1% and 5% level.

[a] Compared using Kruskall–Wallis Tests. Test statistics reported with ties.

[b] Compared using Robust Rank Order Tests. The p-values are identical for the equivalent Wilcoxon-Rank-Sum Tests.

Table 4: Pairwise Comparisons of Dictator Giving

Dependent variable:	Amount Given			
	(*i*)	(*ii*)	(*iii*)	(*iv*)
English[a]	1.169	1.16	0.423	-1.51
	(0.907)	(0.91)	(0.852)	(1.323)
Muslim[a]	-2.051**	-2.072**	-2.06**	-2.58**
	(0.919)	(0.938)	(0.891)	(1.258)
Male		-0.165	-0.541	-0.529
		(0.757)	(0.691)	(0.679)
Area		0.118	0.927	0.716
		(0.753)	(0.692)	(0.689)
Income			0.322	0.367
			(0.667)	(0.659)
Employed			-0.49	-0.519
			(0.893)	(0.888)
High School			0.972	1.217
			(0.828)	(0.837)
A-Level			3.63***	3.586***
			(1.01)	(0.994)
Degree			0.692	1.229
			(2.08)	(2.059)
Closeness			0.733***	0.424
			(0.156)	(0.263)
Closeness×*English*[a]				0.74*
				(0.401)
Closeness×*Muslim*[a]				0.233
				(0.373)
Constant	3.846***	3.878***	0.917	1.592
	(0.657)	(0.847)	(1.143)	(1.192)
Observations	122	122	96	96

Note: Observations left censored at 0 and right censored at 10. Standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% level. The number of observations differs between models due to missing entries. Identical estimates are obtained with robust standard errors. [a]*English* and *Muslim* correspond to dummies for the *English* and *Muslim* treatments respectively.

Variables: *Male*, 1 if male, 0 otherwise; *Area*, 1 if *Kirkholt*, 0 otherwise; *Employed*, 1 if employed, 0 otherwise; *High School*, *A-level* and *Degree*, take 1 if that education level is the highest obtained by the subject, and 0 otherwise. *Income*: self reported income. *Closeness* is self reported level of closeness: with 1 being "*Not at all*" and 10 being "*Very much so*".

Table 5: Determinants of Amount Given – Tobit Regressions

Table 5 presents estimates from a number of Tobit regressions. In each regression the amount given is the dependent variable, the *Anonymous* treatment is taken as the baseline and we include dummies for the *English* and *Muslim* treatments. To examine the robustness of the estimates, in each subsequent model additional dummies which take values of 1 for each of the following (and zero otherwise) are also included: if the subject was male (*Male*), employed (*Employed*), educated to GCSE level (*High school*), A-level (*A-level*), University level (*Degree*) and if the subject was from the *Kirkholt* estate (*Area*); when the dummies controlling for education are all zero, the subject has no formal qualifications. Income (*Income*) and the subjects' self reported level of Closeness (*Closeness*) are also included, along with *Closeness* interacted with the treatment dummies. Supporting the non–parametric analysis, Table 5 outlines how the coefficient on the *Muslim* dummy is always estimated to be negative and significant ($p < 0.05$, in all regressions), with its magnitude robust to specification changes.

Although Result 1 resonates with the results of previous experiments, it does not support the typically reported result that subjects exhibit in–group favouritism. Many previous studies do not include a group–neutral baseline treatment from which to compare behaviour, with Chen & Li (2009) and Chen & Chen (2011) being notable exceptions. For example, Ruffle & Sosis (2006) suggest that they find, "strong in–group favouritism in cooperation" because individuals treat in–group and out–group members differently. Falk & Zehnder (2013) frame the results of their field experiment in terms of, "in–group effects" rather than out–group effects. Similarly, Ioannou *et al.* (2015) posit that one of their experimental treatments is, "sufficient to invoke an in–group bias" in trust and dictator games, when only comparing behaviour from in–group and out–group interactions. As Goette *et al.* (2006) highlight, in–group favouritism and out–group negativity produce the same predicted outcome, and are indistinguishable when a baseline or group–neutral comparison is excluded.

One explanation for why we do not observe in–group favouritism could be that the *English* treatment is not a strong enough experimental manipulation, or it was unsuccessful in inducing an in–group sense of identity. To shed light on this, we examine self–reported levels of group attachment, or *Closeness*, as is standard in the literature (Chen & Li, 2009; Ioannou *et al.*, 2015; Yamagishi & Kiyonari, 2000).

Subjects are found to report feeling closest to those with surnames of *English* origin, with *Closeness* in the *English* treatment found to be significantly different from that in the *Anonymous* and *Muslim* treatments ($p = 0.07$ and $p = 0.011$, Robust Rank Order Tests). We find this even though behaviour is found to be identical in both the *English* and *Anonymous* treatments, as outlined in Result 1. No differences in levels of *Closeness* are reported when comparing the *Muslim* treatment to the *Anonymous* treatment ($p = 0.65$, Robust Rank Order Test). This suggests the *English* treatment was successful in inducing an in–group sense of identity.

An alternative explanation may relate to the implicit and explicit attitudes of the subjects. Subjects giving to an *Anonymous* receiver may have unconsciously believed the receiver was 'like them', and therefore the same as someone with a surname of *English* origin. Only when they are explicitly prompted to consider how 'close' they feel to someone with a surname of *English* origin do they exhibit favouritism. This is likely captured by the significant, but small effect of the interaction between *Closeness*

and the *English* treatment (*Closeness*×*English*) reported in Table 5 ($p = 0.078$, model *iv*). Thus, at least in this setting, in–group favouritism is only observed when explicitly prompted.

4.2 Other–other games

Prior to the Dictator Game all subjects played three Other–Other games where they were required to allocate money between two other people, Person A and Person B. They made their choices under three schemes in a random order: the *In–In*, *Out–Out* and *In–Out* scheme. In the *In–Out* scheme, subjects completed either an *In–Out* or *Out–In* ordering. As shown in the experimental instructions given in Appendix A, Person A's surname was presented first and to the left, whilst Person B's surname was presented second and to the right. We disaggregate the *In–Out* scheme data by orderings to identify any potential presentation effect.

Table 6 presents the results from each scheme, outlining the average amounts allocated to Person A, Person B and the mean difference between these allocations. The mean differences between amounts allocated to Person A and Person B for all schemes are presented graphically in Figure 2.

Scheme	*In–In*	*Out–Out*	*In–Out*	
Ordering			*Out–In*	*In–Out*
Person A Allocation	£5.58	£5.73	£4.62	£5.85
Person B Allocation	£4.42	£4.27	£5.39	£4.19
Difference	£1.16***	£1.46***	-£0.77*	£1.7***
	(2.34)	(2.43)	(2.92)	(3.29)
Observations	122	122	69	53

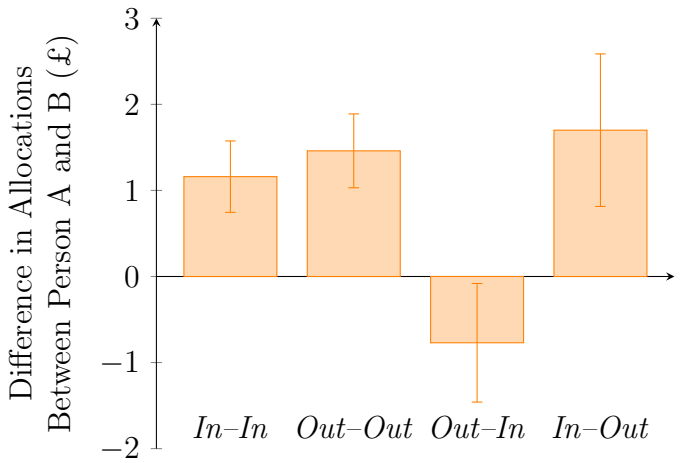
Note: Standard deviations in parentheses. Reported allocations are means. ***, * denote significance at the 1% and 10% level. *p*–values calculated from two–sided Sign Tests.

Table 6: Allocations in the Other–Other Games

Result 2 (Presentation Effect): *When the surnames are of the same ethnic origin, subjects allocate approximately £1 more to the person listed first (Person A) than to the person listed second (Person B).*

Support. From Table 6, in both the *In–In* and *Out–Out* schemes, subjects give significantly more to Person A than to Person B ($p < 0.001$ in both cases, Sign Tests).

Result 3 (Discrimination): *When Person A and Person B have surnames of different ethnic origins, subjects allocate more to the individual with the surname of English origin. This is true even when the English surname is presented second. However, we can only distinguish between the presentation effect and discrimination in the Out–In scheme.*



Note: Vertical bars represent 95% confidence intervals.

Figure 2: Difference in Allocations in the Other–Other Games

Support. It can be seen from Table 6 that subjects give more to the person with a surname of *English* origin in the *In–Out* scheme, with the difference in both *In–Out* and *Out–In* orderings being significantly different from zero ($p = 0.001$ and $p = 0.08$, Sign Tests).

Further support for Result 2 and Result 3 is presented in Table 7, which outlines estimates from OLS and Tobit regressions. In each regression, the difference between amounts allocated to Person A and Person B is the dependent variable. Observations from the *In–In* scheme are taken as the baseline, and explanatory variables include dummies controlling for choices made in the *Out–Out* scheme, and the orderings of the *In–Out* scheme, taking values of 1 in each case (and 0 otherwise).

Dependent Variable:	Difference in Allocations to Persons A and B			
	(*i*)	(*ii*)	(*iii*)	(*iv*)
Out–Out Scheme		0.295		0.3
		(0.241)		(0.242)
In–Out Ordering		0.534		0.548
		(0.488)		(0.492)
Out–In Ordering		-1.932***		-1.942***
		(0.419)		(0.421)
Constant	0.975***	1.164***	0.978***	1.164***
	(0.163)	(0.213)	(0.164)	(0.212)
Observations	366	366	366	366

Note: Observations from the *In–In* scheme are taken as the baseline. ***, ** and * denote significance at the 1%, 5% and 10% level. Reported standard errors are clustered at the individual level. Models (*i*) and (*ii*) are OLS regressions, (*iii*) and (*iv*) are Tobit regressions.

Table 7: Order Effects in Other–Other Allocation Decisions

In support of Result 2, Table 7 outlines how the constant is estimated to be approximately 1 across models. The OLS regression results of model (*ii*) suggest that Person A is allocated £1 more than Person B on average. No differences are found between the *In–In* and *Out–Out* schemes, with the coefficient on the *Out–Out* dummy never being significantly different from zero at conventional levels ($p > 0.1$, all cases). In support of Result 3, the coefficient on the *Out–In* ordering dummy has a negative and highly significant coefficient ($p < 0.01$). The estimates of model (*ii*) outline how, in the *Out–In* ordering, a person of *English* origin is allocated around £2 more than the individual with the surname of *Muslim* origin, once the 'Presentation Effect' is accounted for.

Both Results 2 and 3 are in line with previous findings in the literature. The presentation bias is well documented in the psychology literature. In particular, there is considerable evidence of subjects choosing options presented on the left more frequently than those presented on the right (Friedman *et al.*, 1994; Weng & Cheng, 2000). However, although Result 3 is a replication of the findings of Chen & Li (2009) and Turner (1978), and outlines how subjects differentiate between individuals conditional on their ethnicity, it is not conclusive that they are discriminating. First, as there is no *Anonymous* baseline from which to compare behaviour, the results could equally imply either in–group favouritism or out–group negativity. Second, as behaviour in the *In–Out* ordering is indistinguishable from behaviour in the *In–In* and *Out–Out* scheme, we can only conclusively say that discrimination is observed in the *Out–In* ordering. It is possible that this is a result of a ceiling effect in how comfortable subjects feel when implementing inequitable outcomes. If subjects already hit a 'ceiling' with respect to inequitable choices in the *In–In* scheme, then it is unlikely we would observe differences between this scheme and the *In–Out* ordering, as subjects would be unwilling to tolerate any additional inequality. Therefore, it is likely that we only observe discrimination in the *Out–In* scheme as the motive to discriminate works in the opposite direction to the presentation effect.

5 Structural model

To link our empirical analysis to behavioural theory, we model subjects' behaviour structurally. Following the model of Cox *et al.* (2007), it is assumed that each subject has utility

$$u(x, y; \alpha, \theta, \epsilon) = \begin{cases} \alpha^{-1}(x^{\alpha} + \theta(e, m)y^{\alpha}) & \text{if } \alpha \in [-\infty, 0) \cup (0, 1] \\ xy^{\theta(e,m)} & \text{if } \alpha = 0 \end{cases}, \tag{1}$$

which is derived from her own payoff, x, and the receiver's payoff, y. Her own payoff consists of her participation fee, $s = 2.5$, plus the amount that she keeps for herself: her initial endowment, $\omega = 10$, minus the amount given to the receiver, $y \in \{0, 1, 2, ..., 10\}$. The receiver's payoff, y, is the amount the subject decides to give. The social preference parameter, $\theta(e, m)$, is a function that captures the utility weight the subject places on the receiver's payoff. We can rewrite Equation 2 as follows

$$u(y;\alpha,\theta,\epsilon) = \begin{cases} \alpha^{-1}((s+\omega-y)^{\alpha} + \theta(e,m)y^{\alpha}) & \text{if } \alpha \in [-\infty,0)\cup(0,1] \\ (s+\omega-y)y^{\theta(e,m)} & \text{if } \alpha = 0 \end{cases}. \quad (2)$$

Implicit in our analysis is the assumption that subjects are making their decision in isolation, or that they are 'narrowly bracketing' their decisions, and thus, are not taking into account their own annual income, or the income of the receiver (Read *et al.*, 1999). Following Chen & Li (2009), we assume this parameter is a function of the ethnicity, or identity, of both the subject and the receiver,

$$\theta(e,m) = \theta(1 + ae + bm) + \epsilon, \quad (3)$$

where e and m are dummy variables, with $e = 1$ when the receiver has a surname of *English* origin, and $m = 1$ when the receiver has a surname of *Muslim* origin, and 0 otherwise. Following the behavioural literature, we interpret parameter θ as capturing the utility weight placed on the payoff of the receiver in a group neutral interaction (Chen & Li, 2009). Thus, parameter θ represents baseline social preferences, or preferences when the receiver is *Anonymous*. Parameters a and b, the *English* and *Muslim* identity parameters, measure the additional effects of the receiver's *English* or *Muslim* ethnic origin on this weight. The function $\theta(e,m)$ is assumed identical across subjects, except for an idiosyncratic error term, ϵ, which we assume to be normally distributed with variance σ^2. Thus, following the analysis of Apesteguia & Ballester (forthcoming), we assume a random preference model rather than a random utility model.

Specifying utility in this way is advantageous in comparison to more restricted forms, as the model nests many commonly assumed functional forms: when $\alpha < 1$, indifference curves are strictly convex, when $\alpha = 1$ indifference curves are linear and subjects are altruistic. Indifference curves converge to Cobb-Douglas preferences as $\alpha \to 0$. When $\theta(e,m) > 0$, as $\alpha \to \infty$ preferences are Leontief.[18] Utility reverts to standard selfish preferences when $\theta = 0$.[19] Parameters could be obtained from each of these nested forms by estimating the model with restrictions. However, we let the model pick the parameter values that best fit the data and then test to see if such restrictions would be valid. Appendix C describes the strategy employed to estimate the parameters in Equation 2 structurally, closely following the procedure of Cox *et al.* (2007).

Table 8 outlines the parameter estimates and standard errors. We begin by testing a number of parameter restrictions. First, note that α is estimated to be both positive and highly significant ($p < 0.01$, Wald Test). It is found to be significantly different from one ($p < 0.01$, Wald Test), suggesting that linear, altruistic preferences do not provide a good fit for the data. Similarly, θ is estimated to be positive and is highly significant ($p < 0.01$, Wald Test), rejecting the notion of selfish preferences. While θ is estimated to be larger than 1, we cannot reject the null hypothesis that it is equal to 1 ($p = 0.769$, Wald Test).

[18] See Cox *et al.* (2007) for the proof.

[19] If $\alpha < 0$, giving $y = 0$ would imply a payoff of $u = -\infty$. As we observe a high number of $y = 0$ in the data, we assume $\alpha \geq 0$.

Parameter		Estimate	Standard Error
Social Preference	θ	1.11***	0.39
English Identity	a	0.65	0.67
Muslim Identity	b	-0.87**	0.36
Convexity	α	0.72***	0.03
Standard Deviation	σ	2.31***	0.28
Observations		122	
Log-likelihood		-422.53	

Note: ***, ** denote significance at the 1% and 5% level.

Table 8: Structural Parameter Estimates of Equation 2

From Table 8, the estimate of the *English* identity parameter, a, is not significantly different from zero at conventional levels ($p = 0.33$, Wald Test), although the *Muslim* identity parameter, b, is negative and highly significant ($p = 0.014$, Wald Test). The null hypothesis that $a = b$ can be rejected at the 5% level ($p = 0.03$, Wald Test). Social preferences towards receivers with surnames of *Muslim* origin are estimated to be around 87% smaller than social preferences towards those with surnames of *English* origin and those who are *Anonymous*. As a is not estimated to be significantly different from zero, subjects gain no additional utility from the payoff of individuals with a surname of *English* origin in comparison to those who are *Anonymous*. However, as b is estimated to be less than zero, subjects derive less utility from the payoffs of individuals with surnames of *Muslim* origin in comparison to those who are *Anonymous*. The estimates of a and b support Result 1 and the idea that subjects exhibit out–group negativity rather than in–group favouritism. The estimates suggest that out–group negativity is a consequence of group–contingent social preferences.

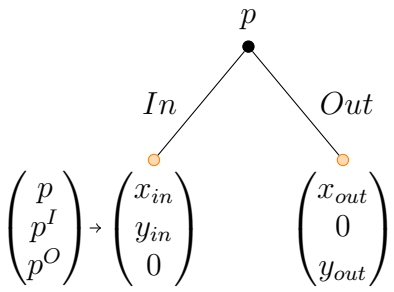
6 Counterfactual analysis

A number of important questions arise from our results: to what extent *could* our structural model explain the attitudes of the demographic of interest? Could our results provide an explanation for the reported discrimination against Muslims in both the press and other studies? For example, the audit study of Ahmed & Hammarstedt (2008) finds Swedish landlords accepting potential tenants with Swedish names more frequently than those with Muslim names. There is also anecdotal evidence from Syal & Topping (2014) that British taxi customers request white British drivers ("local" drivers) instead of Asian–Muslim drivers. In each case, the individual incurs some cost, or trades–off some of her income, in order to complete a transaction with someone they view as in–group, rather than someone from the out–group.

To address this question, consider an individual, p, who must choose with whom to interact. They must select a strategy, s_1, from the strategy set $S_1 = \{In, Out\}$, where strategy $s_1 = In$ implies they interact with someone they perceive as an in-group player (of *English* origin), p^I, and $s_1 = Out$ with someone they perceive as an out–group player (of *Muslim* origin), p^O. We assume that p^I and p^O are identical

except for p's perception of their ethnicity.

From strategy $s_1 = In$, p earns $x_{in} = x - c$, and from $s_1 = Out$ she earns $x_{out} = x$, where $x \geq 0$ and $c \geq 0$. We assume it may be costly for p to choose an in–group interaction, having to incur a cost in order to do so. This cost, c, can be thought of as the cost of discriminating, or the amount of income individual p is willing to forego in order to pursue in–group interactions. For example, when $c > 0$, the cost could represent additional search costs associated with locating a shop run by an in–group member, or the additional wait incurred when requesting an in–group taxi driver. It could represent the cost associated with voting for a policy that is personally costly, but would allow the individual to avoid out–group interactions. Alternatively, it could represent the cost a landlord faces while waiting for an in–group tenant when only an out–group tenant is currently available. Thus, c could range from very small to very large; when $c = 0$, the decision is analogous to the Other–Other game. If $s_1 = In$, p^I earns a payoff of $y_{in} > 0$ and p^O earns zero. We also assume if $s_1 = Out$, p^I earns zero and p^O earns $y_{out} > 0$. Figure 3 represents the decision problem graphically.



Note: p denotes the individual making the decision, p^I denotes the in–group player, p^O denotes the out–group player. x and y denote the respective payoffs.

Figure 3: Extensive Form Representation

Assuming individual p's utility, $u : s_1 \to \mathbb{R}$, takes the form outlined in Equation 2, with parameters equal to those estimated in Table 8, $\theta = 1.11$, $\alpha = 0.72$, $b = -0.87$, $a = 0$, the extent to which she is willing to forego income in order to pursue in–group interactions can be determined. Denoting s_1^* as the utility maximising strategy choice, $s_1^* = In$ when $u(In) \geq u(Out)$,

$$\alpha^{-1}((x-c)^\alpha + \theta(1+a)y_{in}^\alpha) \geq \alpha^{-1}(x^\alpha + \theta(1+b)y_{out}^\alpha). \tag{4}$$

Rearranging Equation 4 for c as a function of y_{out} and y_{in} gives the following two cases,

$$c \leq \begin{cases} x - [x^\alpha + \theta\,(y^\alpha(b-a))]^{\frac{1}{\alpha}} & \text{if} \quad y_{in} = y_{out} = y \qquad (5) \\ x - [x^\alpha + \theta\,(y_{out}^\alpha(1+b) - y_{in}^\alpha(1+a))]^{\frac{1}{\alpha}} & \text{if} \quad y_{in} \neq y_{out}. \qquad (6) \end{cases}$$

We can now consider when p will choose $s_1^* = In$ for given discriminatory costs and other players' payoffs. It is particularly interesting to consider how this choice differs between individuals with group–contingent social preferences and those without discriminatory tastes. Figure 4 plots Equations 5 and 6 graphically. Figure 4a plots

the other players' payoff y against the costs c. Figure 4b plots the difference between the other players' payoffs, d, where this difference is defined as $d = y^{\alpha}_{in} - y^{\alpha}_{out}$, against costs c. In each figure the light shaded areas characterise where $s^*_1 = In$ is chosen when the identity parameters are $a = 0$ and $b = -0.87$, as estimated in Section 5. The dark shaded areas characterise choices of $s^*_1 = In$ when the identity parameters are equal, $a = b$, or when social preferences are not group–contingent. Thus, we can consider how behaviour diverges by comparing light and dark shaded areas.

When $y_{in} = y_{out} = y$, as shown in Figure 4a, p will only choose $s^*_1 = In$ when $a = 0$ and $b = -0.87$. In this case there are no dark shaded areas because p is always indifferent between whom she interacts with when the identity parameters are equal. However, as characterised by the areas that are not shaded, even when p has group–contingent social preferences, there still exist costs at which she would not be willing to discriminate.

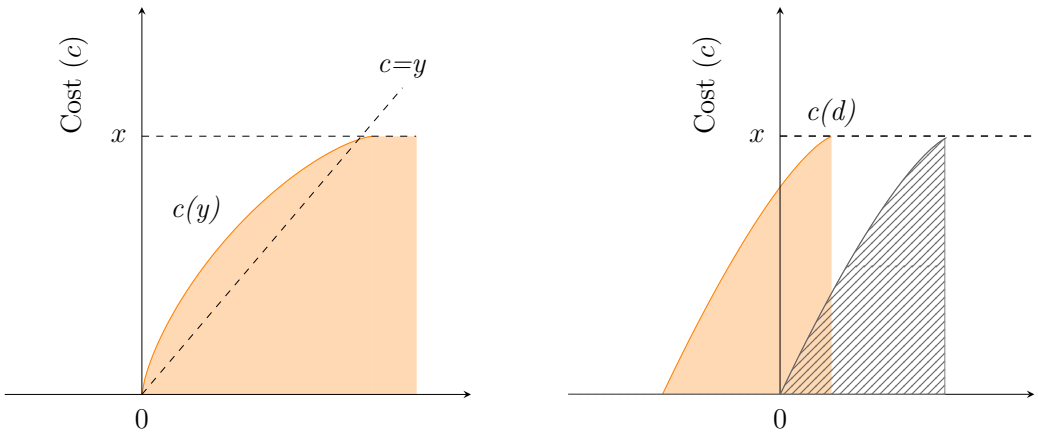
When $y_{in} \neq y_{out}$, as shown in Figure 4b, p may be willing to choose $s^*_1 = In$ even when $a = b$, as highlighted by the dark shaded area. This is due to the positive estimate of θ, which means that p would select the interaction that gave the highest payoff to the other player, regardless of their group identity. Most interesting about Figure 4b is that the light shaded area highlights how p would choose $s^*_1 = In$ for negative values of d and even when the cost is high. This means that p will choose an in–group interaction over an out–group interaction even when the interaction would be more beneficial for an out–group player, and when the cost of selecting the in–group interaction is high.

The conclusions drawn from this counterfactual simulation could provide an explanation for why this particular demographic may support discriminatory policies, even when those policies may produce outcomes that are costly to them, and could provide insight into prior work that outlines discrimination towards Muslims. Although statistical explanations cannot be ruled out in these examples, the analysis highlights how individuals with group–contingent social preferences may be willing to incur significant costs, or be prepared to trade–off large amounts of their own income, in order to avoid out–group interactions.

7 Conclusion

We report evidence of individuals from an understudied subject pool – the poorest people in England – discriminating against those with surnames of Muslim origin and demonstrate this to be a consequence of social preferences being group–contingent. Although discrimination against minorities is not a new finding *per se*, in the past it has largely been attributed to in–group favouritism. Our study allows for the disentangling of the mechanisms underlying such discrimination. We find that the observed discrimination is most likely a result of out–group animosity rather than in–group favouritism.

We focus on behaviour in a deprived neighbourhood because opinion polls have shown that individuals with such demographics are most likely to support discriminatory policies, and little was known about the motivation for such support. Our research suggests that this may be because the social preferences of the deprived are group–contingent, resulting in taste–based discrimination. Determining where these



Note: $c(y)$, if $y_{in} = y_{out} = y$

(a) Illustration of Equation 5

Note: $c(d)$, if $d = y^{\alpha}_{in} - y^{\alpha}_{out}$

(b) Illustration of Equation 6

Note: Light shaded regions characterise where p chooses strategy In, $s^*_1 = In$ when $a \neq b$. Dark shaded regions characterise where p chooses strategy In, $s^*_1 = In$ when $a = b$.

Figure 4: The Relationship Between Discrimination and the Other Player's Payoff

preferences come from, and when they start to develop, is beyond the scope of this paper.

We advance the literature through the estimation of a structural model which we exploit to perform a counterfactual simulation, modelling how these group–contingent social preferences may cause individuals to incur costs in order to avoid out–group interactions. In doing so we provide a potential explanation for the documented attitudes of individuals from the demographic we study.

Whilst the results are suggestive, we acknowledge that care should be taken when trying to generalise the results to the behaviour of other populations and identities. We deliberately did not conduct the study in affluent neighbourhoods because individuals with such demographics are not normally associated with support for discriminatory policies. Further, affluent individuals are more likely to be at work during day light hours, introducing a selection bias in addition to the obvious income effect that could play a role in decision making.

We also acknowledge that little can be said about those who opted out of the experiment. For example, it may be that those who agreed to take part in the experiment were more inclined to discriminate than those who did not. Alternatively, as suggested by List (2006), those who participated in the experiment may be more sensitive to experimental cues unaccounted for here. However, such selection bias is unlikely to explain why we do not observe in–group favouritism. The results do, however, serve as a sign that ethnic identities embedded within surnames can have a significant effect on social preferences and behaviour.

Alternatively, the surnames we used could have signalled something else to the subjects which we have failed to account for (Heckman, 1998). For example, although we have focused exclusively on surnames as revealing ethnicity, Mitra & Ray (2014)

outline how group conflict between Hindus and Muslims in India and Bangladesh may instead be the result of status differences. In our study population Muslims are a minority, and it may be that they are perceived as being of a lower social status. Instead of ethnicity, subjects may identify with people along these status lines. Whilst determining if this is the case is beyond the scope of this paper, the result that subjects exhibit group-contingent social preferences would still hold; the only aspect of the analysis that would change is what constitutes the social group.

In conclusion, we report evidence that the discriminatory behaviour of a previously understudied demographic is a consequence of out–group negativity, rather than in–group favouritism. We are able to determine this because of the inclusion of an *Anonymous* baseline treatment, a crucial design aspect that is typically omitted. Our results highlight how simple experiments can be used to understand the drivers of discrimination and the willingness of individuals to engage in pro–social acts.

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A Experimental Appendix

A.1 Experimental instructions - Part 1

Instructions (Part 1)

In Part 1 you will be asked to make three choices. These choices will be referred to as **Choice 1**, **Choice 2** and **Choice 3**. In each **Choice**, you will be randomly **matched** to two people chosen from a list compiled from the Edited Electoral Register for the Rochdale area. These two people will be called **Person A** and **Person B**. **Person A** and **Person B** will not be required to make a decision.

In each **Choice**, the surnames of **Person A** and **Person B** will be revealed. No other information about these people can be given. Even if the surnames are identical between **Choice**s, the people will be different.

In each **Choice**, you will be asked how to split **10 pounds** between **Person A** and **Person B**. You can allocate the 10 pounds however you like, as long as the allocation adds up to 10 pounds and the amount given to each person is in whole pounds. **You cannot allocate money to yourself**.

Once the study is complete, one of these three **Choices** will be chosen at random, and **Person A** and **Person B** from that **Choice** will receive payment in cash.

The **Person A** and **Person B** that are chosen to receive payment, will not know they have taken part in this experiment until they receive a letter with the amount of money you have allocated them. The amount you decide to send will be placed in an envelope and will be delivered through **Person A's** and **Person B's** door. **Person A** and **Person B** will not learn any information about you.

You will not earn any money in Part 1.

When a **Choice** is chosen, the amount of money **Person A** and **B** will earn in pounds will be:

The amount that you allocate them.

To make sure you fully understand what is asked of you, please complete the three practice questions below. Once you have finished, please let the instructor know so that they can check your answers:

1. How much will **you** earn in Part 1?_____
2. If you allocate **Person A** 2 pounds:
   (a) How much will **Person A** earn?_____
   (b) How much will **Person B** earn?_____
3. If you allocate **Person A** 8 pounds:
   (a) How much will **Person A** earn?_____
   (b) How much will **Person B** earn?_____

A.2 Other–other decision sheets

Part 1 Answer Sheet
Choice 1

You must allocate £10 between Person A and Person B.

Person A's surname is_____

Person B's surname is_____

Write the amount of pounds you would like to allocate to each Person in the boxes below. The amounts allocated must add up to 10.

Amount Allocated to Person A	Amount Allocated to Person B

Once you have made your decision please turn over.

A.3 Experimental instructions - Part 2

Instructions (Part 2)

In Part 2 you will be given £10. You will then be **matched** to a person randomly chosen from a list compiled from the Edited Electoral Register for the Rochdale area. This person will be called **your match**. **Your match** is **not** the same person as anyone you allocated money to in Part 1. Before you make your decision I have to tell you the surname of **your match**.

Their surname is _____. I can't reveal any other information about **your match**.

You will be required to make a single decision. **Your match** will not be required to make a decision.

You will be asked how many of the 10 pounds that you have been given you would like to **send** to **your match**. You can make any choice between 0 and 10. Your choice must be in whole pounds and not pence. The amount you decide to send will be placed in an envelope and will be delivered through **your match's** door with an accompanying letter. **Your match** will not know they have taken part in this experiment until they receive a letter with the amount of money you decide to send them. **Your match** will not learn your surname or any information about you.

The amount of money **you** earn in Part 2 in pounds will be :

10 minus the number of pounds you send.

The amount of money **your match** will earn in pounds will be:

The amount that you send them.

To make sure you fully understand what is asked of you, please complete the three practice questions below. Once you have finished, please let the instructor know so that they can check your answers:

1. If you send £0 how much will:
   (a) **you** earn in Part 2?_____
   (b) **your match** earn?_____
2. If you send £5 how much will:
   (a) **you** earn in Part 2?_____
   (b) **your match** earn?_____
3. If you send £10 how much will:
   (a) **you** earn in Part 2?_____
   (b) **your match** earn?_____
4. Does **your match** know they are taking part?_____

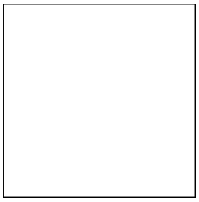
A.4 Dictator game decision sheet

Part 2 Answer Sheet

You have been given **£10.**

Your match's surname is_____

Write the amount of pounds you would like to **send** to **your match** in the box below.



Once you have made your decision please turn over.

B Statistical Appendix

B.1 Survey and responses

Variable	Observations	Mean	Std. Dev.
Male, 1 if yes	122	0.443	0.499
Married, 1 if yes	121	0.372	0.634
No. Children	113	0.628	0.485
Employed$^{\diamond}$	119	0.429	0.696
Income*	100	0.36	0.612
Education†	117	0.906	0.83
Housing benefit, 1 if yes	121	0.678	0.469
Years Living in Rochdale	120	26.142	16.353
Beliefs**	103	3.738	3.106

Note: Observations differ due to missing entries.

$^{\diamond}$0 if unemployed, 1 if employed, 2 if retired.

*0 if income $<$ £10000, 1 if £10000 $<$ income $\leq$ £20,0000, 2 if £20000 $<$ income $\leq$ £30,0000 .

†0 if no qualifications, 1 if GCSE level, 2 if A–level, 3 if Degree, 4 if postgraduate.

** Subjects' belief about the experimenter's expectation of their behaviour in the Dictator Game.

C Structural Appendix

C.1 Constructing the likelihood function

Fixing the parameters α, θ, a, b, for each amount sent $y \in \{0, 1, ..., 10\}$ we can determine the critical values of ϵ where the subjects utility maximising choice changes, ϵ_y. This is because the utility maximising choice of y, y^*, varies with ϵ. The dictator will choose to send an amount y over $y+1$ up until,

$$u(y; \alpha, \theta, a, b, \epsilon_y) = u(y+1; \alpha, \theta, a, b, \epsilon_y). \tag{7}$$

Rearranging for ϵ_y gives

$$\epsilon_y = \frac{(s+\omega-y)^\alpha - (s+\omega-y-1)^\alpha}{(y+1)^\alpha - y^\alpha} - \theta(1+ae+bm) \tag{8}$$

Dividing through by σ gives,

$$\frac{\epsilon_x}{\sigma} = \frac{1}{\sigma}\left(\frac{(s+\omega-y)^\alpha - (s+\omega-y-1)^\alpha}{(y+1)^\alpha - y^\alpha} - \theta(1+ae+bm)\right) \tag{9}$$

When $\epsilon \in (\epsilon_{y-1}, \epsilon_y)$, $y^* = y$. The probability of choosing $y^* = y$ can then be determined from the cumulative distribution function of the error term. Where $f(z)$ is the density function, and $F(z)$ the cumulative distribution, the probability that the dictator chooses $y^* = 0$ is the probability that $\epsilon \in (-\infty, \epsilon_0)$, or

$$\Pr[y^* = 0|\alpha, \theta, a, b, \sigma] = \int_{-\infty}^{\epsilon_0} f(z)dz = F(\epsilon_0). \tag{10}$$

The probability the dictator chooses $y^* = y \in \{1, 2, ..., 9\}$ is

$$\Pr[y^* = y|\alpha, \theta, a, b, \sigma] = \int_{\epsilon_{y-1}}^{\epsilon_y} f(z)dz = F(\epsilon_y) - F(\epsilon_{y-1}), \tag{11}$$

and the probability of choosing $y^* = 10$ is

$$\Pr[y^* = 10|\alpha, \theta, a, b, \sigma] = \int_{\epsilon_9}^{\infty} f(z)dz = 1 - F(\epsilon_9).$$

The likelihood function, as we have $k = 122$ observations, is therefore

$$L(\alpha, \theta, a, b, \sigma) = \prod_{k=1}^{122} Pr[y_k = y; \alpha, \theta, a, b, \sigma]. \tag{12}$$