Anticipation of Discrimination, Misperceptions, and Trust: Application to Affective Polarization *

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Abstract

Does anticipation of discrimination, beliefs individuals hold about the behavior of others toward them, undermine trust and cooperation? Is this anticipation misperceived? We develop a new design to isolate the role of anticipation from other confounding motives. We randomly match trustors with ingroup or outgroup trustees, and vary whether the trustor's own identity is revealed or concealed. We capture anticipation as the double difference between the amount transferred to outgroup vs. ingroup trustees when the trustor's own identity is revealed vs. concealed. We apply our design in the context of affective polarization in the UK using a large representative sample. Our findings reveal that anticipation undermines inter-partisan cooperation by the same magnitude as taste-based and statistical discrimination. However, this anticipation is misperceived because trustees rarely discriminate along partisan lines. This results in cooperation failure. Our design can be used to study anticipation of discrimination across other societal divisions.

JEL: C91, C93, J15, D72, Z13

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I. Introduction

Many economic interactions require trust between individuals from different groups.[1] A large body of evidence shows that trust can be undermined because of *taste-based* (Becker, 1957) and *statistical discrimination* (Arrow, 1970; Phelps, 1972). However, we know little about the role of another factor which can also undermine trust – *anticipation of discrimination*. Individuals anticipate discrimination if they believe that others will engage in taste-based or statistical discrimination toward them. A classical example concerns minorities, who might be reluctant to invest in human capital if they anticipate discrimination from employers. Other examples include a reluctance to migrate despite better opportunities and withholding cooperation to provide public goods in anticipation of discrimination. As is evident from these examples, anticipation can result in self-fulfilling prophecies, which can undermine trust by reinforcing taste-based and statistical discrimination (Arrow, 1973; Coate and Loury, 1993). Anticipation of discrimination can also result in coordination failures if people misperceive others to engage in taste-based or statistical discrimination.

Despite this, there is hardly any evidence on the importance of anticipation of discrimination for trust. Progress in this area has been hampered by the difficulties in separating the effect of anticipation from other confounding motives operating at the same time. For example, individuals might be reluctant to migrate or provide public goods not because they anticipate discrimination but because they themselves engage in taste-based and statistical discrimination.

In this paper, we first develop a new experimental design to overcome these difficulties and highlight the role anticipation of discrimination plays in the erosion of trust. Second, we investigate whether or not the anticipation of discrimination is misperceived (see Bursztyn and Yang, 2022; Alesina, Miano and Stantcheva, 2023). Third, we apply our design to shed new light on the poorly understood motives behind an imperative global problem – affective polarization, which is defined as the hostility between members of opposing political camps that transcends questions of politics (Iyengar et al., 2019) (Iyengar et al., 2019). Recent studies document widespread affective polarization across many countries (Boxell, Gentzkow and Shapiro, 2020) and suspect that it plays a crucial role in the erosion of trust (see Guriev and Papaioannou, 2022).

Our study takes place in the UK, which has witnessed high levels of affective polarization, especially since Brexit (Hobolt, Leeper and Tilley, 2021). We recruit a large sample of about 1600 participants via Facebook and ensure that these participants have similar socio-economic characteristics as the adult population of the UK.[2]

Our experimental design is based on trust, which is a vital ingredient for economic

[1] Platteau (2000); Tabellini (2010); Alesina and Giuliano (2015); Enke (2024, see)

[2] Our sample size is similar to that of (Bursztyn, Egorov and Fiorin, 2020) who use 1600 participants for their online experiment.

exchange and development (Arrow, 1972; Fukuyama, 1996; Knack and Keefer, 1997; Algan and Cahuc, 2010; Putnam, 2000). We use a trust game, which is the cornerstone of cooperation dilemmas and for measuring trust as a cultural trait (Berg, Dickhaut and McCabe, 1995; Glaeser et al., 2000; Guiso, Sapienza and Zingales, 2006; Fehr, 2009; Alesina and Giuliano, 2015). The game has two players, a trustor and a trustee. We give both players the same endowment at the start of the game. Crucially, the players take decisions sequentially: the trustor decides first to send any amount between zero and her endowment to the trustee, which is tripled by the experimenter. The trustee then decides whether and how much to transfer back to the trustor. The total payoffs are maximized if the trustor sends her entire endowment in the expectation that the trustee reciprocates. However, if the trustee does not, the trustor loses money. This creates a cooperation dilemma.

The trust game has three implications for isolating the role of anticipation from other confounding motives: (a) since the trustor moves first, her decision could reflect anticipation of discrimination, as well as taste-based and statistical discrimination; (b) since the trustee is the second mover, she can condition her decision on the trustor's transfer. This means thar the trustee's decision can only reflect taste-based discrimination, but not statistical discrimination or anticipation of discrimination; (c) since the trustee does not need to anticipate what the trustor anticipates and the trustor knows that, the game rules out higher-order beliefs from playing a role. Thus, in our design, we need to separate anticipation of discrimination from other motives only for the trustor, whereas the behavior of trustees can be used to study whether anticipation of discrimination by trustors is misperceived.

To separate different motives underlying a trustor's behavior, we augment the trust game as follows. We randomly match individuals with an ingroup or outgroup person, so that individuals are aware that there is a 50 % chance of matching with an outgroup person. Then we experimentally vary the information structure of the game. We flip a coin for each individual within a match to determine whether her own identity is revealed or concealed to the other individual, such that the outcomes of the coin flip are common knowledge. This generates four key treatments: outgroup-concealed, ingroup-concealed, outgroup-revealed, and ingroup-revealed. We take the *first* difference between the amount sent by the trustors to outgroup vs. ingroup trustees, when the trustor's own identity is *concealed*. We then take the *second* difference between the amount sent by the trustors to outgroup vs. ingroup trustees, when the trustor's own identity is *revealed*.

The first difference captures the importance of taste-based and statistical discrimination but cannot be influenced by anticipation of discrimination, as the trustor's own identity is concealed. However, the second difference additionally captures the importance of anticipation because now the trustor's own identity is revealed. Since taste-based and statistical discrimination are unlikely to depend on whether one's own identity is concealed

or revealed, a double difference allows us to estimate the importance of anticipation of discrimination over and above taste-based and statistical discrimination. In addition, we also elicit a trustor's expectation of the amount they think the trustee will return. This allows us to go beyond the reduced-form evidence and confirm whether anticipation of discrimination indeed undermines cooperation.

To investigate whether anticipation of discrimination is misperceived, we test if trustees display taste-based discrimination. In the standard trust game, researchers observe only one decision of trustees that is in response to a specific transfer by the trustor. To obtain a richer characterization of trustees' preferences, we implement the trust game using the strategy method (Selten, 1965). This allows us to elicit a trustee's conditional response function, that is, the amount the trustee sends back to the trustor for all possible amounts that a trustor could have sent. We then test for taste-based discrimination by comparing conditional response functions across ingroup and outgroup treatments.

We apply our design in the context of affective polarization in the UK. Our study was conducted in 2019, when Boris Johnson was the prime minister of the UK. We used self-reported support for Boris Johnson to classify participants into two groups: "supporters" and "opponents" of Boris Johnson. The participants then took part in the trust game explained above.

We find an economically large and statistically significant effect of anticipation of discrimination over and above taste-based and statistical discrimination. The magnitude of the effect turns out to be as large as the combined effect of taste-based and statistical discrimination. These results arise because trustors anticipate significantly higher defection from outgroup trustees but higher cooperation from ingroup trustees when their own identity is revealed. Crucially, in our context, this anticipation is misperceived, as trustees rarely discriminate between ingroup and outgroup individuals. In fact, the vast majority of trustees in our experiment are perfect reciprocators irrespective of whether they are matched with an ingroup or outgroup trustor. We find similar patterns when we use data on self-reported feelings from the post-experimental survey. These results highlight the importance of (misperceived) anticipation of discrimination in cooperation failures, leaving societies worse off.

Our paper contributes to several strands of literature. First, we complement the literature on the determinants of trust and cooperation. Previous studies provide evidence only on the importance of taste-based and statistical discrimination (see reviews by Lane, 2016; Charness and Chen, 2020). We highlight the role of anticipation of discrimination over and above taste-based and statistical discrimination by using a new experimental design. A recent study by Ortiz (2023) examines anticipation of discrimination between Muslims and Christians in a Nigerian city using multiple dictator games and a simultaneous move coordination game. He uses dictator games to infer preferences towards the outgroup and then deduces anticipation of discrimination by comparing choices in

the dictator games to choices in the simultaneous move coordination game. However, this game does not allow for isolating the role of anticipation of discrimination from the role of statistical discrimination and higher-order beliefs. Our design circumvents these concerns by leveraging a sequential trust game. Moreover, our design is simple, since it is based on a single game, which allows for easy implementation across different contexts.

Second, our paper contributes to the literature on misperceptions (Bursztyn and Yang, 2022). Previous studies have documented misperception of social norms concerning women's participation in labor markets (Bursztyn, González and Yanagizawa-Drott, 2020) and wrong facts about immigration (Alesina, Miano and Stantcheva, 2023). We document that misperceptions can lead people to expect discrimination when there is none, leading to avoidable cooperation failures.

Third, our paper adds to the literature on affective polarization. Many studies have examined the patterns of affective polarization (Boxell, Gentzkow and Shapiro, 2020; Guriev and Papaioannou, 2022) but few have examined the underlying motives (Iyengar et al., 2019). Bursztyn, Egorov and Fiorin (2020) study the change in social norms related to xenophobia in the wake of the election of Donald Trump. Dimant (2023) randomly pairs individuals with supporters and opponents of Donald Trump in a public goods game and then records the change in behavior to study affective polarization. Our paper complements these findings by leveraging experimentally induced variation in the information structure of the game to separate the effect of anticipation of discrimination from taste-based and statistical discrimination.

Fourth, our paper also relates to the nascent literature in behavioral and labor economics that presents mixed evidence on the role of anticipation of discrimination. Charness et al. (2020) use a laboratory experiment to show that female students are less likely to reveal their avatar in math-related tasks because they expect discrimination. Similarly, Lepage, Li and Zafar (2022) find that when offered a choice between revealing the actual grade or pass/ fail status, female candidates are more likely to reveal their actual grade in anticipation of discrimination. In contrast, Aksoy, Chadd and Koh (2023) and Angeli, Matavelli and Secco (2023) find limited evidence on anticipation of discrimination in revealing LGBTQ+ identity and residential location, respectively. A common feature of these studies is that they consider situations in which only one party can discriminate, while the other side can only anticipate discrimination. However, in many social interactions, such as cooperation, there is no such clean separation, and both discrimination and anticipation jointly determine behavior. Our study offers a design to isolate the role of anticipation of discrimination in these settings.

The paper is organized as follows. Section II describes the experimental design and Section III our conceptual framework and strategy. Section IV presents the results from the application of our design to the context of affective polarization in the UK, and Section V offers concluding remarks.

II. Experimental Design

Our design is based on a two-player sequential game, which offers several advantages over a two-player simultaneous move game. Table 1 summarizes the motives underlying players' decisions in both these games. It is evident from the table that in the simultaneous move game both player 1's and player 2's decisions reflect anticipation of discrimination, as well as taste-based discrimination, statistical discrimination, and anticipation of each others' anticipation. In contrast, the sequential move game throws out anticipation of each others' anticipation from playing a role. It thus requires us to isolate the role of anticipation of discrimination by player 1 from her taste-based and statistical discrimination, whereas for player 2 only her taste-based discrimination plays a role. We achieve this through a trust game, which we describe below.

Table 1: Motives Underlying Decisions

Simultaneous Move Game		Sequential Move Game	
P1's decision reflects	P2's decision reflects	P1's decision reflects	P2's decision reflects
Anticipation of **taste & statistical** discrimination by P2	Anticipation of **taste & statistical** discrimination by P1	Anticipation of of **taste-based** discrimination by P2	
Taste-based discrimination	Taste-based discrimination	Taste-based discrimination	Taste-based discrimination
Statistical discrimination	Statistical discrimination	Statistical discrimination	
Anticipation of P2's Anticipation	Anticipation of P1's Anticipation		

Notes: The table shows motives behind Player 1 (P1) and Player 2 (P2) decision in a two-player simultaneous move and a two-player sequential move game.

We describe our experimental design, which relies on the trust game and random assignment of participants to treatments.

II.A. Trust Game

There are two players in a trust game. Following the convention in the literature, we label the first player as "trustor" and the second player as "trustee". Both players receive the same endowment X at the start of the game.[3] The players take decisions sequentially. First, the trustor decides to send an amount t between 0 and X to the trustee. Any amount sent by the trustor is tripled by the experimenter. The trustor then decides to send any amount between 0 and $X + 3t$ back to the trustee. Each player takes two decisions in the following order:

[3]In the original trust game only the trustee is endowed, resulting in kindness as a plausible motive behind transfers to trustees. Our design follows Cox (2004) to rule this out.

- *Conditional* transfer as a trustee for each possible transfer by the trustor. We implement this using the strategy method (Selten, 1965).
- *Unconditional* transfer as a trustor.

We randomly decide ex post which one of these two decisions is payoff relevant. Since the participants are not aware ex ante which decision will be chosen, both are payoff relevant. The payoffs from the game are calculated as follows:

$$\pi_{trustor} = X - t + \text{amount received from the trustee}$$
$$\pi_{trustee} = X + 3t - \text{amount sent back to the trustor}$$

In this game, the total payoffs are maximized if the trustor sends her entire endowment in the expectation that the trustee reciprocates. However, if the trustee refuses to do so then the trustor loses money. This creates a cooperation dilemma.

We additionally elicit beliefs of the trustors by asking them to guess the conditional transfer of the trustee in response to different possible amounts sent by the trustor. For each correct guess we pay individuals an additional amount.

II.B. Treatments

We use two-stage randomization to assign participants to treatments. First, we randomly pair participants with an ingroup or outgroup person, such that the participants are aware that there is a 50 percent chance of being paired with an outgroup individual.[4] Second, for each individual within a pair, we flip a coin to determine whether her own identity is revealed or concealed to the other individual, such that the outcomes of the coin flip are common knowledge.[5] The random assignment to ingroup or outgroup together with random assignment to the information structure of the game generates six treatments.

[4]Depending on the context, these pairings could be male-female, Democrat-Republicans, Christians-Muslims, upper caste-lower caste, etc.

[5]To achieve a balance across treatments, we used the "evenly present elements" feature in Qualtrics built-in randomizer.

Table 2: Treatments

	Information Structure		Trustee is
	Trustor's identity	Trustee's identity	
1	Revealed	Revealed	In-group
2	Revealed	Revealed	Out-group
3	Concealed	Revealed	In-group
4	Concealed	Revealed	Out-group
5	Revealed	Concealed	Anonymous
6	Concealed	Concealed	Anonymous

Notes: The table shows six treatments. Our focus is on treatments 1-4.

III. Conceptual Framework

We use our framework first to assess the importance of anticipation of discrimination for transfers by trustors. We then assess whether this anticipation is misperceived by studying the transfers by trustees.

III.A. Anticipation of Discrimination

We focus on treatments 1-4 in which the identity of the trustee is revealed to the trustor. In these treatments, all three motives behind discrimination could affect a trustor's decision. We exclude treatments 5 and 6 from this analysis because in these treatments the identity of the trustee is concealed, implying that trustors cannot discriminate on the basis of any of the three motives under consideration.

Table 3 lists the different possible motives behind a trustor's decision in each of the four treatments. In Treatments 1-2, the trustor's own identity is revealed, so her decision may reflect taste-based discrimination, statistical discrimination, as well as anticipation of discrimination. However, in Treatments 3-4, the trustor's own identity is concealed, which means anticipation of discrimination is ruled out, but taste-based and statistical discrimination still play a role.

Table 3: Conceptual Framework: Behavior of Trustors

	Information Structure		Trustee is	Motives
	Trustor's identity	Trustee's identity		
1	Revealed	Revealed	In-group	Taste, Statistical, Anticipation
2	Revealed	Revealed	Out-group	Taste, Statistical, Anticipation
3	Concealed	Revealed	In-group	Taste, Statistical
4	Concealed	Revealed	Out-group	Taste, Statistical

Notes: The table shows plausible motives behind trustor's behavior in four main treatments.

We separate the effect of anticipation of discrimination from taste-based and statistical discrimination by computing three differences:

D_1 is the difference in transfers between Treatment 2 and Treatment 1, that is, the difference in transfers to outgroup vs ingroup individuals when the trustor's own identity is *revealed*. In this case, the trustor's choice of transfer reflects taste-based and statistical discrimination, as well as anticipation of discrimination.

$$D_1 = E\left[Transfer \mid \underbrace{outgroup,\ Revealed}_{\text{Treatment 2}}\right] - E\left[Transfer \mid \underbrace{ingroup,\ Revealed}_{\text{Treatment 1}}\right] \quad (1)$$

D_2 is the difference in transfers between Treatment 4 and Treatment 3, that is, the difference in transfers to outgroup vs ingroup individuals when the trustor's own identity is *concealed*. In this case, the trustor's choice of transfer may reflect taste-based and statistical discrimination, but anticipation of discrimination is no longer possible.

$$D_2 = E\left[Transfer \mid \underbrace{outgroup,\ Concealed}_{\text{Treatment 4}}\right] - E\left[Transfer \mid \underbrace{ingroup,\ Concealed}_{\text{Treatment 3}}\right] \quad (2)$$

We argue that the double difference, $D_3 = D_1 - D_2$, captures the role of anticipation of discrimination. This is because revealing the trustor's identity is unlikely to cause any change in the trustor's taste-based and statistical discrimination, which is already captured by D_1. To the extent trustors anticipate discrimination by outgroup individuals, we expect D_3 to be negative. This means, an estimate of D_3 allows us to capture the extent to which anticipation of discrimination undermines cooperation over and above taste-based and statistical discrimination.

We go beyond a reduced-form interpretation and investigate how beliefs over transfers from the trustees differ across the treatments. Additionally, we carry out a similar exercise using data from a post experimental survey on feelings towards ingroup and outgroup members. We discuss these in detail in the section IV.C.

Econometrically, we estimate the effect of anticipation of discrimination using the following linear specification:

$$y_{ij} = \alpha_0 + \beta_1 outgroup_j + \beta_2 Revealed_i + \beta_3 (outgroup_j * Revealed_i) + \mathbf{X'}_i\delta + \epsilon_i \quad (3)$$

where y_{ij} is the transfer made by trustor i to trustee j. The omitted category is transfer to *ingroup* when trustor's identity is *concealed*. With respect to the omitted category, β_1 captures the difference in transfer when the trustee is *outgroup* and the trustor's identity is concealed. β_2 captures the difference in transfer when the trustee is *ingroup* and the trustor's identity is *revealed*. β_3 captures the marginal effect when the trustee is *outgroup* and the trustor's identity is *revealed*. The coefficient of interest is β_3, which captures the effect of anticipation of discrimination and is the same as D_3. β_1 captures the role of taste-based and statistical discrimination, and is the same as D_1. $\mathbf{X}$ is a vector of individual characteristics that are expected to affect behavior in the trust game. These include generalized trust in strangers, gender, age, education, income, geographic location, religion, and political party.

III.B. Is Anticipation of Discrimination Misperceived?

We use the conditional decisions of trustees from the strategy method to elicit their taste for discrimination. Previous studies show that responses in the strategy method are not biased, relative to responses in a simple sequential game (Brandts and Charness, 2000; Fischbacher and Gächter, 2010). We focus on treatments where the identity of the trustor is revealed to the trustee. Ashraf, Bohnet and Piankov (2006) find that the transfer by trustee's depend on the amount sent by the trustor. Accordingly, we infer taste-based discrimination by studying differences in the conditional responses across the relevant treatments, as follows:

$$D_4 = E\left[Amount\ returned \mid t,\ outgroup\right] - E\left[Amount\ returned \mid t,\ ingroup\right] \quad (4)$$

D_4 captures the differences in conditional responses when matched with an outgroup vs ingroup trustor, and reveals whether the trustee has taste for discrimination or not. We estimate D_4 separately for each possible transfer (t) by the trustor. If estimates of D_4 turn out to be economically small for all levels of transfers then anticipation of discrimination is misperceived.

IV. Application: Affective Polarization in the UK

We illustrate an application of our design in the context of affective polarization. Our design is not specific to our context and can be applied to different domains. Affective polarization is defined as the hostility between members of opposing political camps that transcends questions of politics (Iyengar et al., 2019). Recent studies have documented affective polarization in many countries (Boxell, Gentzkow and Shapiro, 2020; Guriev and Papaioannou, 2022). However, we know little about what motives underlie affective polarization. For instance, Iyengar et al. (2019) writes:

> "There has been little to no research identifying the mechanisms underlying affective polarization. On the one hand, distaste for opposing partisans could be couched in raw, reflexive emotion. [On the other hand,] the aversion to economic transactions with opposing partisans may stem not from a visceral emotional response but from a perception that opponents are untrustworthy."

Motivated by these gaps, we run our study in the UK, which witnessed strong trends in affective polarization in the aftermath of Brexit. Our focus is on political leaders and their supporters to characterize affective polarization. Following Guriev and Papaioannou (2022) who emphasize a strong relationship between trust and affective polarization, we use the trust game between supporters and opponents of Boris Johnson, a polarizing figure in British politics and a prominent proponent of Brexit. Our study took place in 2019, when Johnson was the Prime Minister of the UK.

IV.A. Study Design

Our study proceeds as follows. First, we recruited 1560 participants from the UK via Facebook. These individuals were assigned to six treatments using stratified randomization based on gender, age, and geographical location. This ensures balance across treatments along these variables. Table A.1 in Appendix A shows that the treatments are also balanced across other variables, such as income, education, religion, and political party.

Second, we invited these participants to take part in a trust game (see Appendix B for procedures and instructions). In the game, both players received an endowment of £24 each, which was more than twice the minimum hourly wage in 2019. The participants took decisions in the role of both the trustor (first mover) and the trustee (second mover). As the trustor, the participants could send any amount between £0 and £24 to the trustor in the increments of £4. Similarly, the trustee could send back any amount to the trustor in the increments of £4.

Third, we gauged an individuals' support or opposition to Boris Johnson using an adaptation of a standard question used in the literature on affective polarization (Druck-

man and Levendusky, 2019; Boxell, Gentzkow and Shapiro, 2020). The precise question we asked is: "How do you feel personally about Prime Minister Boris Johnson. Please rate on a scale from 0 to 100, where 0 means you strongly oppose Boris Johnson and 100 means that you strongly support Boris Johnson." We used this information to sort participants into two political camps. Individuals whose rating were above 50 were classified as "supporters", whereas individuals whose rating was below 50 were classified as "opponents".[6]

Finally, we carried out a two-stage randomization to assign participants to our six treatments using the protocol described in Section II.B (see Appendix B for visualization).

Before the participants took part in the game, we tested for their game comprehension. The participants were allowed to proceed only once they have understood the game and answered the control questions correctly. Overall, 5 percent of the participants were selected randomly for payment (participants were informed of this in the beginning). After the game, we implemented a post-experimental survey to collect data on basic socio-demographic characteristics, as well as data on standard questions on affective polarization, such as feelings towards political ingroup and outgroup members. On average, the entire study lasted 15-20 minutes and the selected participants earned £36 (three times the minimum hourly wage).

IV.B. Results

We start by documenting descriptive results on transfers by trustors in each of the four treatments in Figure 1.[7] When the trustor's identity is concealed, the transfer to ingroup and outgroup trustees are £12.14 and £10.08 respectively, a raw difference of £2. This difference reflects both taste-based and statistical discrimination by the trustors. However, when trustor's identity is revealed, the transfer to ingroup trustee increases but that to the outgroup trustee declines, such that now the difference is £3.6. The double difference of £1.6 suggests that anticipation of discrimination by trustors plays an important role over and above taste-based and statistical discrimination.

We test these results econometrically using equation (3) and report the results in Table 4, which provides estimates of β_1, β_2, and β_3. Column 1 controls only for the stated support for Johnson, as this is our basis of classifying individuals into ingroup or outgroup. Relative to the omitted category, three findings are noteworthy. First, when trustor's identity is concealed, the transfer to outgroup trustee declines by nearly 2 pounds and this difference is statistically significant at the 1-percent level. Second, when trustor's identity is revealed, the transfer to ingroup trustee rises by over £1.2 pounds and the difference is statistically significant at the 10-percent level. Finally, the

[6]We excluded participants who rated their support at exactly 50 and could therefore not be assigned to either of the political camps. This was the case for 6.9% of the potential participants.

[7]For results on transfers in the remaining two treatments, see Figure A.1.

interaction term, which captures the effect of anticipation of discrimination, shows that when trustor's identity is revealed, the transfer to outgroup trustee additionally declines by close to £1.7 pounds and the difference is statistically significant at the 10-percent level ($p = 0.056$).

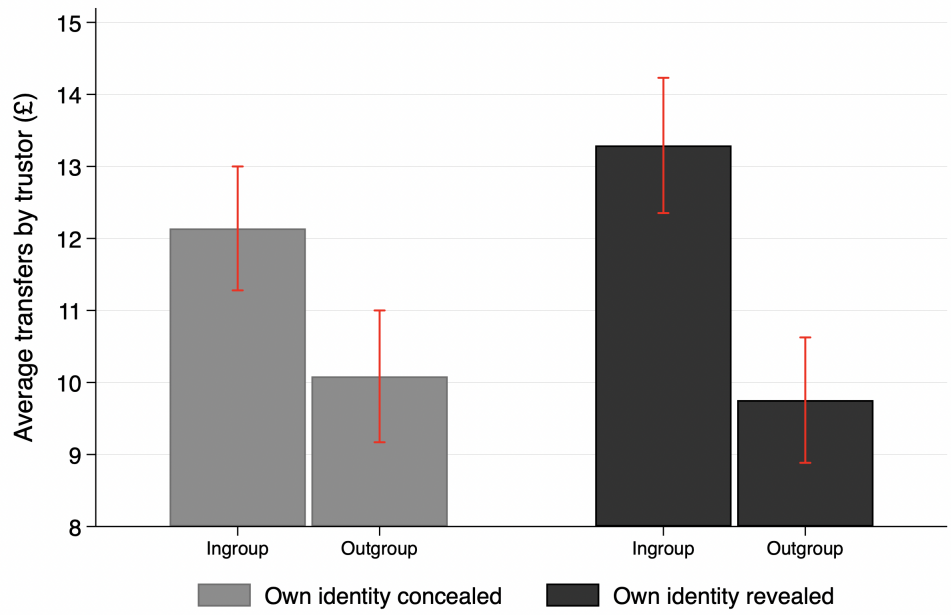


Figure 1: Transfers by Trustors (£)

Notes: The figure plots transfers by trustors in the four main treatments, which from left to right are: Treatments 3, 4, 1, and 2.

Table 4: Transfer by Trustors and Anticipation of Discrimination

	Transfer by Trustor (£)				
	Johnson support	Trust in strangers	Stratification units FE	Income & education	Religion & politics
	(1)	(2)	(3)	(4)	(5)
Outgroup (Concealed)	-1.975	-1.670	-1.710	-1.814	-1.787
	(0.641)	(0.633)	(0.634)	(0.630)	(0.631)
Revealed (Ingroup)	1.244	1.514	1.543	1.536	1.535
	(0.645)	(0.630)	(0.634)	(0.629)	(0.630)
Outgroup×Revealed	-1.666	-2.049	-2.053	-1.907	-1.961
	(0.916)	(0.897)	(0.889)	(0.886)	(0.888)
Baseline Mean			12.14		
R^2	0.03	0.07	0.10	0.11	0.12
N	1536	1536	1536	1536	1536

Notes: OLS estimates with robust standard errors in parentheses. Baseline mean refers to transfer to ingroup members when trustor's identity is concealed (Treatment 3). We progressively include control variables. Column 1 controls for support for Johnson, column 2 additionally for a survey measure of trust in strangers, column 3 for stratification unit fixed effects (area, gender, and age-groups), column 4 for income and education, and column 5 for religion and political party.

We progressively include control variables that are likely to predict transfers by trustors

in columns 2-5. In column 2, we include a general tendency to trust strangers, which enters with a positive coefficient that is statistically significant at the 1-percent level. The coefficient on the interaction term increases slightly in absolute magnitude but its standard error declines, such that it is now statistically significant at the 5-percent level. In column 3, we introduce fixed effects for our stratification units: gender, age, and indicator for ten geographical regions of the UK. This does not lead to any change in the coefficient on the interaction term, which retains its magnitude and significance. In column 4, we control for income and education. These variables enter with positive coefficients that are statistically significant, still the coefficient on the interaction term remains robust in magnitude and retains its statistical significance. Similar results are obtained when we introduce controls for religion and politics in column 5. Our results hold when we carry out a randomization inference test with 5000 replications (p-value < 0.001).

In the model with the full set of controls (column 5), the magnitude of the coefficient on the interaction term implies that anticipation of discrimination leads to a decline in transfer by close to £2 pounds, which is 16 percent of the transfer in the benchmark category and thus economically large. Importantly, the marginal effect of anticipation of discrimination is similar in magnitude to the combined effect of taste-based and statistical discrimination (coef. 1.787, s.e. 0.631). This implies that, in our context, anticipation of discrimination is as important as other motives behind discrimination.

IV.C. Beliefs

The above results offer reduced-form evidence on the importance of anticipation of discrimination. We now go beyond and provide further evidence in support of this result by using data on beliefs from the experiment, as well as the post-experimental survey.

Experiment.– We use data on the beliefs of trustors over expected transfers from the trustees. We define anticipation of defection as the expectation that sending any positive amount to the trustee will result in a payoff which is less than the original endowment.[8]

Figure 2 shows the share of trustors expecting defection in each of the four treatments. When the trustor's identity is concealed, the share of trustors expecting defection is 13 percent when the trustee is ingroup but 18 percent when the trustee is outgroup. This difference of 5 percentage points is, however, not statistically significant. In contrast, when the trustor's identity is revealed, the share expecting defection declines when the trustee is ingroup but rises steeply when the trustee is outgroup. Now, the difference turns out to be 14 percentage points and is statistically significant at the 1-percent level. The double difference of 9 percentage points is economically large and statistically significant. It provides strong support to the reduced-form result on anticipation of discrimination.

[8] As an example, if the trustor sends £12 but expects to receive $<$ £12 back, then she is better off sending nothing because £24 $>$ £12+£8.

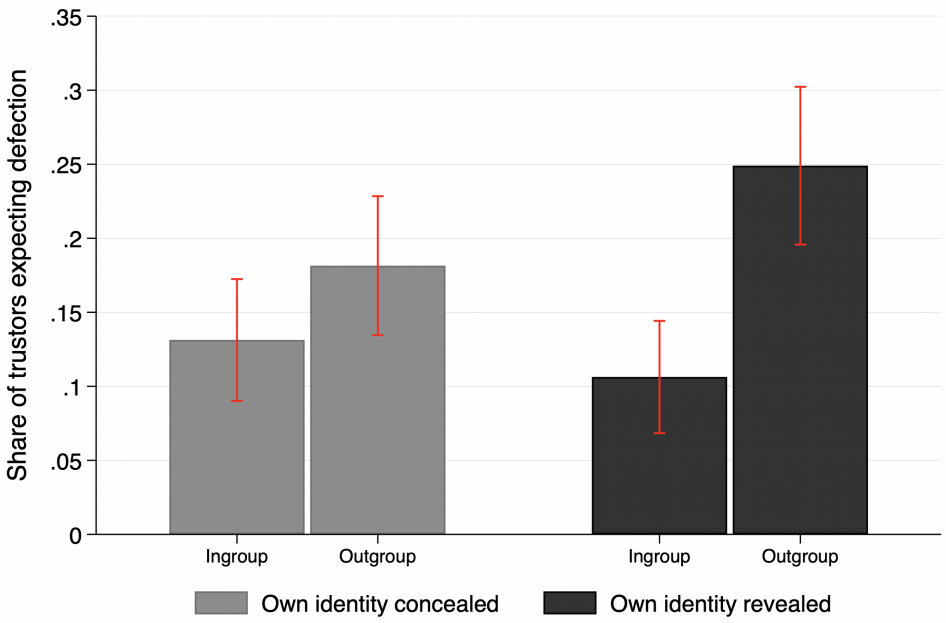


Figure 2: Trustor's Belief over Defection by Trustees

Notes: The figure shows share of defections expected by trustors in the four treatments, which appear from left to right as Treatments 3, 4, 1, and 2.

Results in Table 5 confirm these findings. Regardless of the specification we choose, the coefficient on *Outgroup* × *Revealed* turns out to be close to 0.09 in magnitude and is always statistically significant. The magnitude of the coefficient is slightly smaller than twice the magnitude of expected defections in the baseline category (13 percent).

Table 5: Trustor's Anticipation of Defection from Trustees

	Anticipation of Defection				
	Johnson support	Trust in strangers	Stratification units FE	Income & education	Religion & politics
	(1)	(2)	(3)	(4)	(5)
Outgroup (Concealed)	0.053	0.042	0.041	0.042	0.046
	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Revealed (Ingroup)	-0.022	-0.031	-0.032	-0.032	-0.030
	(0.029)	(0.028)	(0.029)	(0.029)	(0.028)
Outgroup×Revealed	0.086	0.099	0.101	0.100	0.094
	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)
Baseline Mean			0.13		
R^2	0.02	0.04	0.06	0.06	0.7
N	1536	1536	1536	1536	1536

Notes: OLS estimates with robust standard errors in parentheses. Baseline mean refers to expected defection when trustor's identity is concealed and trustee is ingroup (Treatment 3). We progressively include control variables. Column 1 controls for support for Johnson, column 2 additionally for a survey measure of trust in strangers, column 3 for stratification unit fixed effects (area, gender, and age-groups), column 4 for income and education, and column 5 for religion and political party.

Post-Experimental Survey.– We use standard questions from the literature on affective polarization to elicit participants' beliefs about how *other people* feel towards members of the participants' political camp. Specifically, we asked supporters of Boris Johnson to assess the typical feelings of supporters and opponents towards people who *support* Boris Johnson. Similarly, we asked opponents of Boris Johnson to assess the typical feelings of supporters and opponents towards people who *oppose* Boris Johnson. We achieved this through the following questions: a) "In your opinion, how does a typical person who supports (opposes) Boris Johnson feel about other people who also support (oppose) Boris Johnson?"; b) "In your opinion, how does a typical person who supports (opposes) Boris Johnson feel about other people who oppose (support) Boris Johnson?" In both the questions, we asked respondents to rate on a scale of 0-100, where 0 implies strong dislike and 100 implies strong liking.

Figure 3 shows the distribution of responses to these questions. Consistent with the findings from beliefs in the trust game, the participants believe that people have on average warm feelings towards members of their own political camp (an average score of 66) whereas beliefs about the feelings of outgroup members are much more pessimistic (an average score of 33); these differences are statistically significant at the 1-percent level. These findings lend further support to the results on anticipation of discrimination.

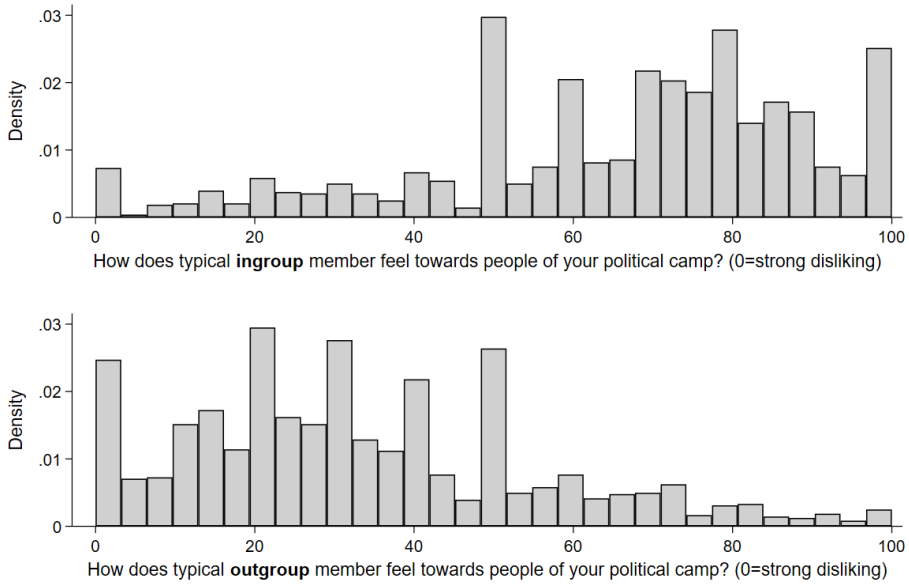


Figure 3: Beliefs about feelings of others towards people of respondent's political camp

Notes: The figure shows a histogram of self-reported beliefs from the post-experimental survey. The top panel shows the distribution of beliefs about the feelings ingroup members have for people of their own political camp (pooled across beliefs of supporters towards other supporters, and beliefs of opponents towards other opponents). The bottom panel does the same using beliefs about the feelings that outgroup members have for people of the opposite political camp (pooled across beliefs of supporters towards opponents, and beliefs about opponents towards supporters of Boris Johnson).

IV.D. Is Anticipation of Discrimination Misperceived?

Thus far, our results reveal a robust negative effect of anticipation of discrimination on transfers by trustors to trustees, which are further backed by data on beliefs from the game and the post-experimental survey. But, is this anticipation warranted or is it misperceived? To test this, we use data from both the experiment and the survey.

Experiment.– As outlined in Section III, we start by comparing conditional transfers by trustees to trustors, when the identity of the trustor is revealed as ingroup or outgroup. Figure 4 reports the results by plotting average conditional transfers by trustees in response to the transfers made by ingroup trustors (blue line) and outgroup trustors (red line). The black dotted line indicates perfect reciprocity – the amount that the trustee would need to return for the final payoffs to be equalized.

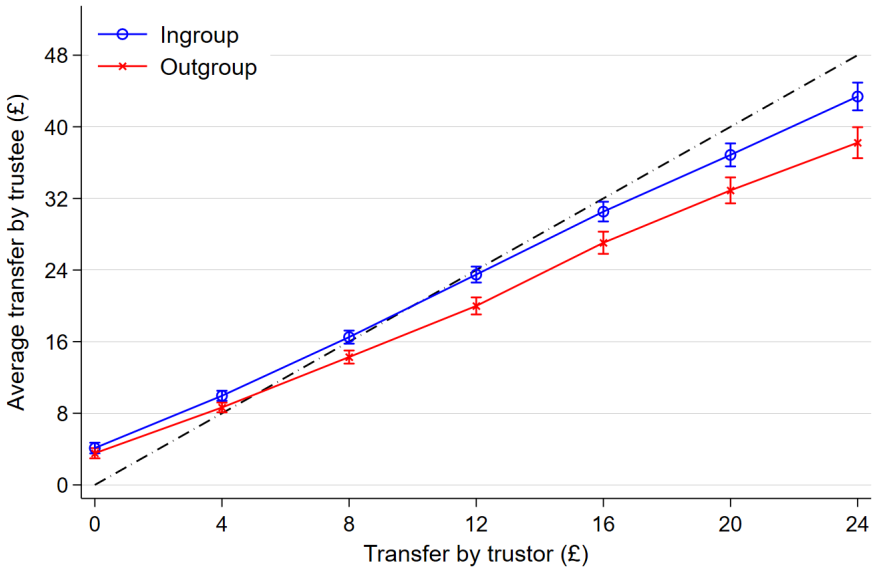


Figure 4: Conditional Transfer by Trustees

Notes: The figure shows conditional transfers by the trustees in response to every possible transfer by the trustor. The blue line indicates conditional transfers to ingroup trustors, whereas the red line indicates conditional transfers to outgroup trustors. The dashed line indicates payoff equalizing transfers by trustees to trustors.

It is evident from the figure that regardless of ingroup or outgroup identity, the conditional transfers by trustees closely follow the dotted line. This means, the trustees return similar amounts to trustors regardless of whether they are ingroup or outgroup. While the amount returned to ingroup trustors is slightly larger than the amount returned to outgroup trustors, the difference is not economically large enough to justify anticipation of discrimination. In fact, the conditional response to both the ingroup trustors and the outgroup trustors is monotonically upward sloping, suggesting the trustors would have

maximized their expected final payoffs by transferring their entire endowments regardless of whether the trustee is ingroup or outgroup.

We analyze these patterns econometrically in Table A.2, were we regress the amount sent back by trustees on each level of amount received from trustors and interact it with an indicator of outgroup. In line with the figure, we see that as the amount sent by trustors increases, trustees reciprocate by sending a larger amount when the trustor is ingroup. When the trustor is outgroup, the amount sent back by trustees declines but only slightly. As an example, when the trustor sends her entire endowment of £24, trustees on average reciprocate by sending £43 to an ingroup trustor and £38 to an outgroup trustor. This means, a risk-neutral trustor is always better off by sending everything to the trustee regardless of their identity.

We dissect this result further by looking at (i) the share of trustees who defect, and (ii) conditional transfers by trustees who do not defect. A trustee defects if she returns less that what the trustor had sent, resulting in the trustor having a final payoff lower than her original endowment. Figure 5 reports the results. Although the share of defectors rises when the other person is out-group, the magnitude of the difference (4 percentage points) is small. Furthermore, when we restrict the sample to trustees who do not defect, we find no difference in the average amount sent back by trustees to ingroup vs. outgroup trustors. These results suggest that the findings in Figure 4 are primarily driven by an extensive rather than an intensive-margin effect.

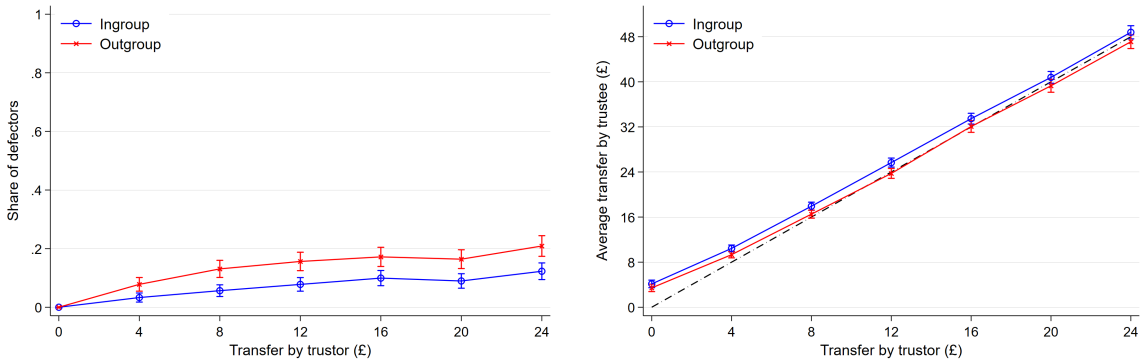


Figure 5: Share of Free Riders and Conditional Transfer by Trustees

Notes: The left figure shows the share of trustees who defect. The right figure shows the conditional transfers by trustees who do not defect in response to every possible transfer by the trustor. The blue line indicates the share of trustees defecting when the trustor is ingroup (left figure) and conditional transfers to ingroup trustors (right figure). The red line indicates the share of trustees defecting when the trustor is outgroup (left figure) and conditional transfers to outgroup trustors (right figure). The dashed line in the right figure indicates payoff equalizing transfers by trustees to trustors.

Past-Experimental Survey.– We use a standard question to elicit participants' feelings toward people from the opposing political camp. We ask supporters (opponents) the

following question: "How do you feel personally about people who oppose (support) Boris Johnson? Please answer on a scale from 0 to 100, where 0 means strong dislike and 100 means strong liking." Figure 6 shows that feelings towards outgroup members are not extreme but moderate, with the most common score being 50. This result is in line with the experimental findings and bolsters the confidence that anticipation of discrimination is to a large extent misperceived.

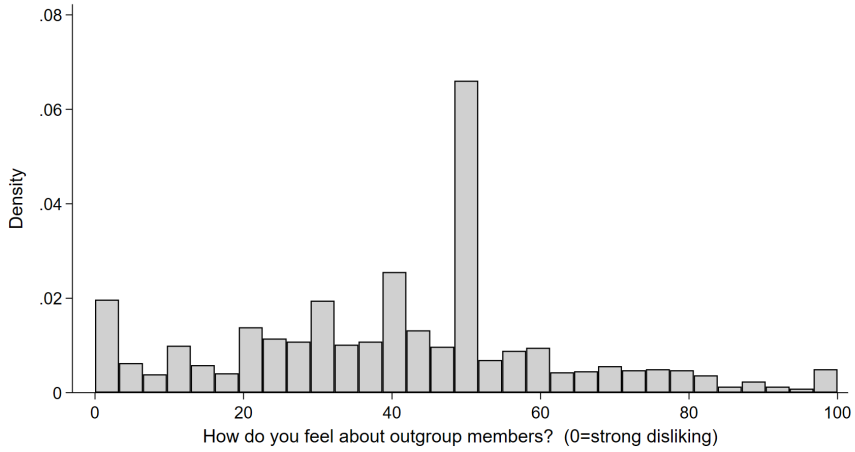


Figure 6: Reported feelings towards outgroup members

Notes: The figure shows a histogram of reported feelings towards outgroup members in the post-experimental survey. For the supporters of Johnson, we asked about their feelings towards the opponents of Johnson, and vice versa. We pooled data across feelings of supporters towards opponents, and feelings of opponents towards supporters of Boris Johnson.

IV.E. Most Polarized Sample

We lend further credibility to our results by showing that anticipation of discrimination widens in the most polarized sample where people express extreme support (score of 100) or opposition (sore of 0) to Boris Johnson. Table A.3 reports the results and shows that the anticipation of discrimination increases from £ -2 in the full sample to £-4 in the most polarized sample (see column 5). Furthermore, results in Figure A.2 and columns 3-4 of Table A.2 suggest that this anticipation is largely misperceived even in the most polarized sample.

V. Conclusions

Many economic activities require generalized cooperation between ingroup and outgroup individuals. However, such cooperation can be difficult to achieve. A large number of studies have documented that taste-based and statistical discrimination can hamper cooperation. However, we know little about the extent to which anticipation of discrimination

affects cooperation. This is in part because of the difficulties in isolating the importance of anticipation of discrimination over and above taste-based and statistical discrimination.

This paper offers a new experimental design to fill this gap. It relies on a trust game in which players decide sequentially on their transfers. This not only allows for a clean separation of motives behind transfers by trustors and trustees, but also shuts down higher-order beliefs from playing a role.

To separate the motives underlying transfers by the trustors, we randomly vary the identity of the other player, as well as the information structure of the game. We achieve this by concealing the identity of the trustor in some treatments and by revealing it in the other treatments. We then take the double difference between the transfers sent to ingroup versus outgroup trustees when the trustors own identity is revealed versus concealed. Since anticipation of discrimination is possible only when trustors own identity is revealed, the double difference allows us to estimate the effect of anticipation of discrimination over and above other motives. To test whether this anticipation is misperceived or warranted, we elicit the preferences of trustees, whose transfers are affected only by taste-based discrimination. For this purpose, we use the strategy method, which allows us to obtain conditional transfers of trustees for all possible amounts that the trustor could have sent.

We apply this design in the context of affective polarization, which is on the rise in many countries but motives underlining it are poorly understood. Our study takes place in the UK which provides an ideal setting because of strong affective polarization in the aftermath of Brexit. We play the trust game between the opponents and supporters of Boris Johnson in the UK. We find that anticipation of discrimination undermines cooperation. The magnitude of the effect is as large as the combined effect of taste-based and statistical discrimination. This is further confirmed by data on beliefs that show trustors expect significantly higher discrimination from outgroup trustees when their own identity is revealed. However, we show that this discrimination is misperceived, as the vast majority of trustees does not discriminate by political identity. This results in cooperation failure.

Our design is general and can be applied to many different contexts to study discrimination along the lines of gender, caste, ethnicity, religion, politics. It can be a valuable tool to study anticipation of discrimination is a variety of different countries and settings.

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ONLINE APPENDIX

Anticipation of Discrimination and Misperceptions in Cooperation Dilemmas

Devesh Rustagi Matthias Schief

Appendix A

I. Summary statistics and balance check

Balance Check

Table A.1 shows that the treatments are balanced across a number of additional covariates that we did not target with our stratified sample protocol (age, gender, region).

Table A.1: Balance Table

	Treatments							
	1	2	3	4	5	6	Mean	Prob>F
Income	1.587	1.621	1.595	1.668	1.641	1.563	1.613	0.365
	(0.568)	(0.562)	(0.592)	(0.608)	(0.596)	(0.599)	(0.588)	
Education	0.516	0.462	0.541	0.560	0.533	0.488	0.517	0.253
	(0.501)	(0.500)	(0.499)	(0.497)	(0.500)	(0.501)	(0.500)	
Religion	0.480	0.522	0.498	0.444	0.502	0.528	0.495	0.445
	(0.501)	(0.501)	(0.501)	(0.498)	(0.501)	(0.500)	(0.500)	
Politics	0.386	0.375	0.351	0.421	0.382	0.321	0.373	0.288
	(0.488)	(0.485)	(0.478)	(0.495)	(0.487)	(0.468)	(0.484)	
Trusting	0.378	0.407	0.471	0.367	0.425	0.401	0.408	0.194
	(0.486)	(0.492)	(0.500)	(0.483)	(0.495)	(0.491)	(0.492)	
N	254	253	259	259	259	252	1536	

Notes: Columns 1-6 shows the means and standard deviations for the variables listed in the rows across the six treatments. Column 7 shows the mean in the full sample. Income is measured as an index with 1 indicating lower income, 2 indicating middle income, and 3 indicating upper income. Education is an indicator variable taking a value of 1 if the respondent has a university degree. Religion is an indicator variable taking a value of 1 if the respondent reports not being religious and 0 otherwise. Politics is an indicator variable taking a value of 1 if the respondent is a voter of the Labour Party, 0 otherwise. Trusting is an indicator variable taking a value of 1 if the respondent thinks that "most people can be trusted", and 0 otherwise. The last column shows the p-value of an F-test that tests for differences in means across treatments.

II. Results

Transfers by Trustors

Figure A.1 shows transfers by trustors in Treatments 5 and 6, that is, when their own identity is revealed but that of the trustee is concealed (Treatment 5) and when their own identity as well as that of the trustee are concealed (Treatment 6).

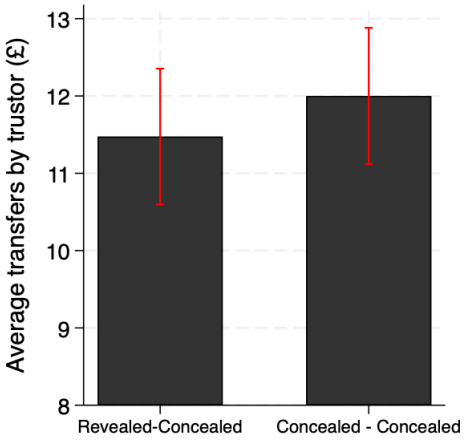


Figure A.1: Transfers by Trustors (£)

Notes: The figure plots transfers by trustors in treatments 5 and 6.

Misperceptions in Anticipation of Discrimination

Table A.2 documents misperception by showing the differences in amount sent by trustees for every possible transfer by the ingroup vs. outgroup trustor in game. The coefficients on amount sent back to outgroup trustors though statistically significant are very small in magnitude to warrant strong anticipation of discrimination.

Table A.2: Anticipation of Discrimination and Misperception

	Conditional Response of Trustees by Identity (£)			
	Full Sample		Most Polarized Sample	
	No controls	All controls	No controls	All controls
	(1)	(2)	(3)	(4)
In-group				
Decision 4	5.840	5.840	5.493	5.493
	(0.256)	(0.256)	(0.487)	(0.490)
Decision 8	12.390	12.390	12.028	12.028
	(0.334)	(0.335)	(0.677)	(0.681)
Decision 12	19.376	19.376	19.634	19.634
	(0.431)	(0.431)	(0.871)	(0.876)
Decision 16	26.409	26.409	27.211	27.211
	(0.540)	(0.541)	(1.060)	(1.066)
Decision 20	32.741	32.741	34.648	34.648
	(0.636)	(0.637)	(1.211)	(1.218)
Decision 24	39.267	39.267	41.380	41.380
	(0.785)	(0.787)	(1.480)	(1.489)
Out-group				
Decision 0	-0.578	-0.529	-1.599	-1.056
	(0.423)	(0.443)	(0.801)	(0.944)
Decision 4	-0.731	-0.731	0.101	0.101
	(0.396)	(0.397)	(0.759)	(0.764)
Decision 8	-1.648	-1.648	-1.953	-1.953
	(0.490)	(0.490)	(0.987)	(0.993)
Decision 12	-2.923	-2.923	-3.664	-3.664
	(0.658)	(0.659)	(1.293)	(1.301)
Decision 16	-2.902	-2.902	-3.662	-3.662
	(0.816)	(0.817)	(1.640)	(1.651)
Decision 20	-3.381	-3.381	-6.979	-6.979
	(0.974)	(0.976)	(1.918)	(1.930)
Decision 24	-4.580	-4.580	-7.275	-7.275
	(1.184)	(1.186)	(2.347)	(2.362)
Baseline Mean		4.12		
R^2	0.51	0.53	0.51	0.54
N	7175	7175	1925	1925

Notes: OLS estimates with robust standard errors clustered on the individual in parentheses. Baseline mean refers to the amount sent back by trustees when the in-group trustor sends 0. We progressively include control variables. Column 1 controls for support for Johnson, column 2 additionally for a survey measure of trust in strangers, column 3 for stratification unit fixed effects (area, gender, and age-groups), column 4 for income and education, and column 5 for religion and political party.

Results Using the Most Polarized Sample

Table A.3: Transfer by Trustors and Anticipation of Discrimination
Most Polarized Sample

	Transfer by Trustor (£)				
	No controls	Trust in strangers	Stratification units FE	Income & education	Religion, politics & ethnicity
	(1)	(2)	(3)	(4)	(5)
Out-group (Concealed)	-2.836	-1.983	-1.733	-1.984	-1.845
	(1.400)	(1.365)	(1.407)	(1.366)	(1.360)
In-group (Revealed)	2.668	2.868	3.020	3.046	3.049
	(1.270)	(1.197)	(1.234)	(1.237)	(1.249)
Out-group×Revealed	-3.341	-3.839	-4.247	-3.954	-4.147
	(1.906)	(1.823)	(1.821)	(1.800)	(1.815)
Baseline Mean			12.77		
R^2	0.07	0.13	0.22	0.24	0.25
N	405	405	404	404	404

Notes: OLS estimates with robust standard errors in parentheses. Baseline mean refers to expected defection when trustor's identity is concealed and trustee is in-group (Treatment 3). We progressively include control variables. Column 1 controls for support for Johnson, column 2 additionally for a survey measure of trust in strangers, column 3 for stratification unit fixed effects (area, gender, and age-groups), column 4 for income and education, and column 5 for religion and political party.

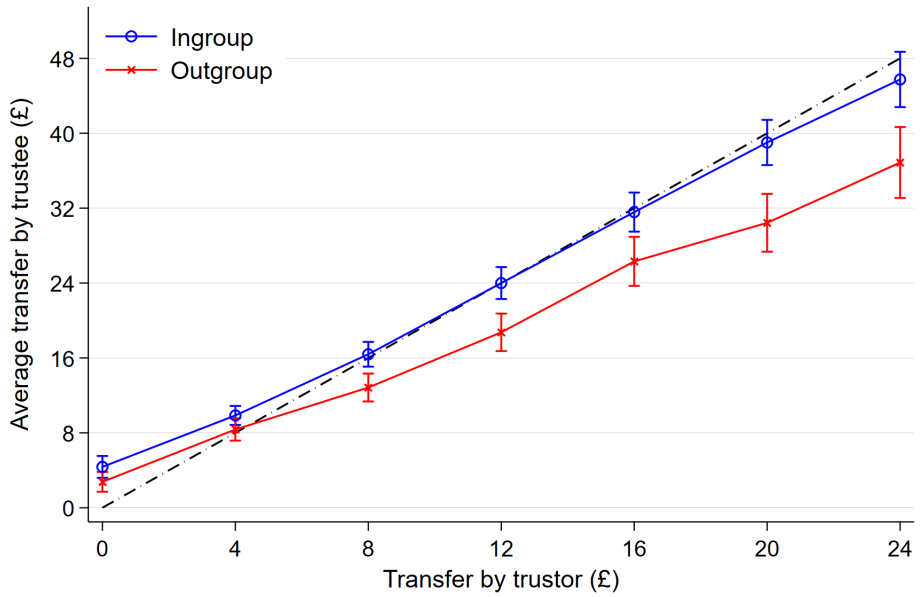


Figure A.2: Conditional Transfer by Trustees in the Most Polarized Sample

Notes: The figure shows conditional transfers by the trustees in response to every possible transfer by the trustor in the most polarized sample. The blue line indicates conditional transfers to ingroup trustors, whereas the red line indicates conditional transfers to outgroup trustors. The dotted line indicates payoff equalizing transfers by trustees to trustors.

Appendix B Experimental Instructions

I. Introduction

In this study, we will match you with another person in the United Kingdom who has also decided to participate in the study. You do not know anything about this person. Similarly, the other person knows nothing about you. You will interact with the other person. Your earnings will depend on the decisions made by you and the other person. Therefore, please carefully read the instructions on the next page.

Decision Situation

We will now introduce you to the basic decision situation. You and the other person will interact in the roles of player A and player B. At the start of the study, each player has £24 in the pocket. This money is provided by us.

Player A moves first and has the opportunity to send all, some, or none of this money to Player B. Any amount sent to Player B will be tripled. Example: if Player A decides to send £20, it will be tripled to £60 when it reaches Player B.

Player B can keep the money or decide to send some money back to Player A. The amount sent back to Player A is not tripled.

Note: Player A must decide how much money to send to Player B without knowing whether Player B will send money back. In contrast, Player B can take Player A's decision into account when deciding how much to send back.

Earnings

Earnings of Player A
£24 in pocket - amount sent to Player B + amount received from Player B

Earnings of Player B
£24 in pocket + 3 x amount sent by Player A - amount sent to Player A

Examples

Example I: Player A sends £0 to Player B. Player B sends £0 back to Player A. Hence, Player A earns £24 and Player B also earns £24

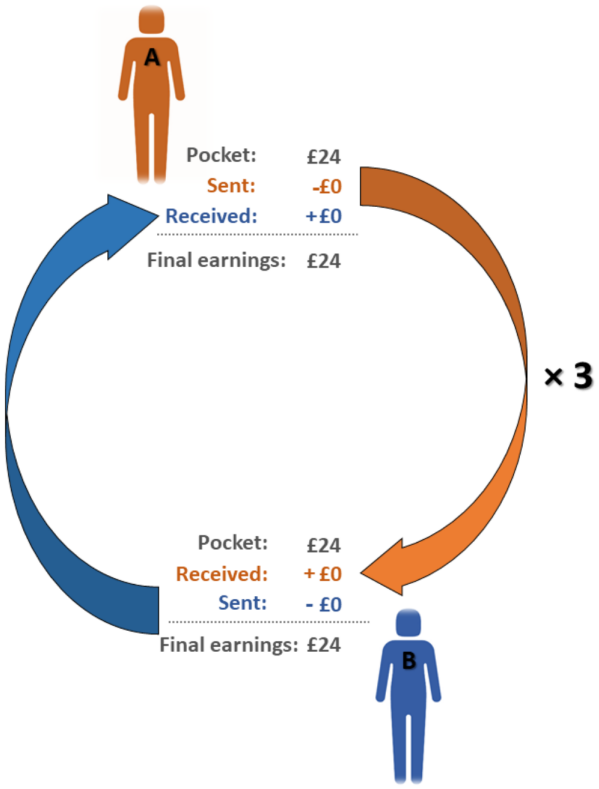


Figure A.3: Example I

Example II: Player A sends £20 to Player B. Player B sends £40 back to Player A. Hence, Player A earns £44 and Player B also earns £44.

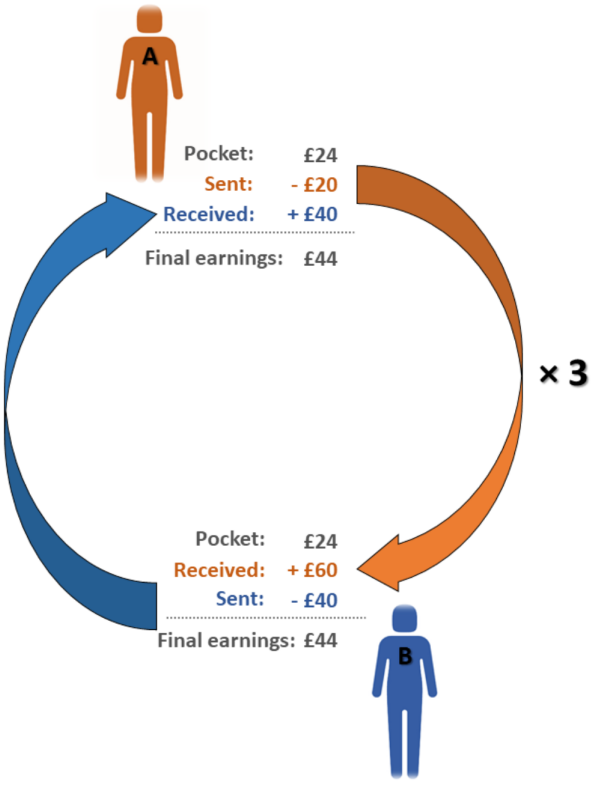


Figure A.4: Example II

Example III: Player A sends £20 to Player B. Player B sends £0 back to Player A. Hence, Player A earns £4 and Player B earns £84.

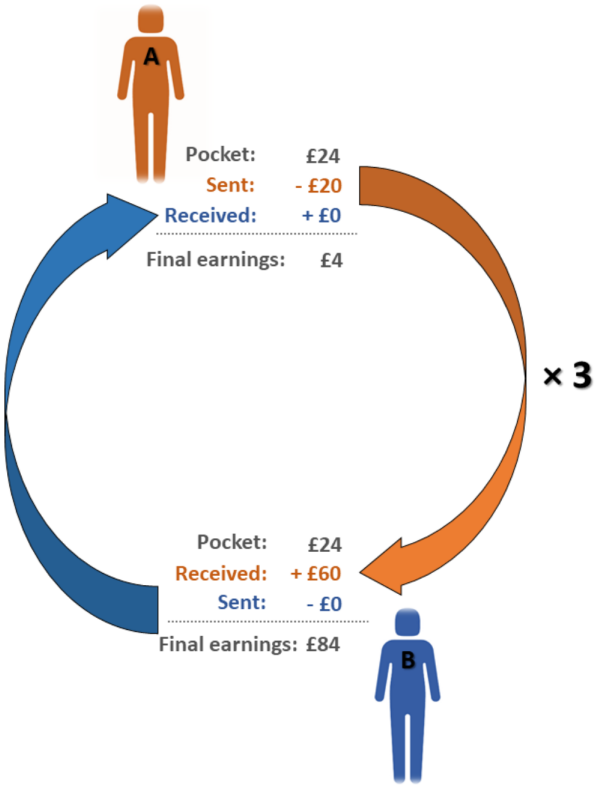


Figure A.5: Example III

Assignment of Roles

You will take decisions both in the role of Player A and Player B. When you decide in the role of Player A, the other person will decide in the role of Player B and vice versa. We will pay you for your decisions in either the role of Player A or Player B. We will toss a coin to decide on this. Please take all decisions carefully.

Game comprehension questions

To make sure that you have understood the decision situation, please answer the questions below.

Case 1: You are Player A and the other person is Player B. Out of the £24 in your pocket, you send £8 to the other person. After tripling, how much does the other person receive from you?

○ £0	○ £12	○ £24
○ £4	○ £16	○ £28
○ £8	○ £20	○ £32

Figure A.6

The other person decides to send £8 back, what will be your and the other person's final earnings?

- ○ You earn £0, the other person earns £40
- ○ You earn £24, the other person earns £40
- ○ You earn £40, the other person earns £24

Figure A.7

Case 2: You are Player B and the other person is Player A. The other person sends £24 to you. After tripling, you receive £72. What will be your final earnings if you decide to send £48 back to the other person?

○ £0	○ £64
○ £16	○ £80
○ £32	○ £96
○ £48	

Figure A.8

What will be your final earnings if you decide to send £0 back to the other person?

○ £0	○ £64
○ £16	○ £80
○ £32	○ £96
○ £48	

Figure A.9

Introduction to Matching

Congratulation, you are ready to take your decisions!

We have now matched you with another person

Before you take your decisions, there is an opportunity for both you and the other person to learn one thing about each other: Whether you support or oppose Prime Minister Boris Johnson. There is a 50% chance that you are matched with someone who supports Boris Johnson and a 50% chance that you are matched with someone who opposes Boris Johnson.

We will toss a coin to decide on whether the other person learns about your political opinion.

- If you get Heads, we will **tell** the other person whether you support or oppose Boris Johnson.
- If you get Tails, we will **not tell** the other person whether you support or oppose Boris Johnson.

Similarly, we will toss another coin to decide on whether you learn about the other person's view on Boris Johnson.

Treatments

[Note for the reader: After this, we tossed coins to assign participants to one of the treatments. Here we are reproducing only one treatment as an example for the sake of illustration.]

We tossed coins and both got Heads!

- We can tell you that the other person opposes Boris Johnson.
- We have TOLD the other person that you support Boris Johnson.

You and the other person are both aware of this situation (visualised below).

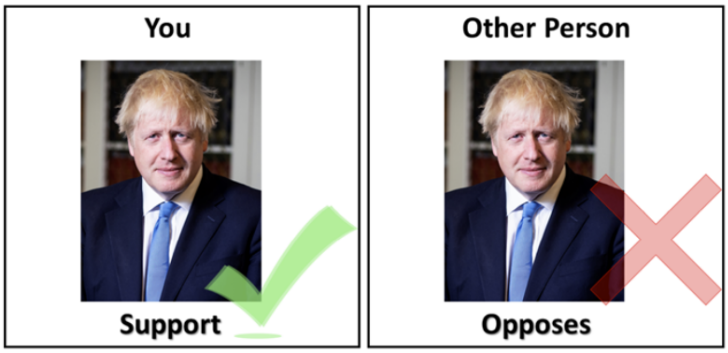


Figure A.10: SCREEN SHOT OF ONE OF THE TREATMENTS

ACTUAL DECISIONS

Decision as Player B

You will take this decision in the role of **Player B**. The other person is in the role of Player A.

You do NOT know the other person's views on Boris Johnson. Similarly, the other person does NOT know your views on Boris Johnson.

As of now we do not know the amount that the other person has sent to you. Therefore, we request you to take a decision for each possible amount sent by the other person. We will display the earnings associated with your decisions (in blue) when you click on the options below.

If the other person sends **£0**, how much will you send back?

- ◯ £0
- ◯ £4
- ◯ £8
- ◯ £12
- ◯ £16
- ◯ £20
- ◯ £24

Figure A.11

You earn:
Other person earns:

If the other person sends £**4** to you, it is tripled to £12. Now, the other person has £20 and you have £36. How much would you like to send back to the other person

- ◯ £0
- ◯ £4
- ◯ £8
- ◯ £12
- ◯ £16
- ◯ £20
- ◯ £24
- ◯ £28
- ◯ £32
- ◯ £36

Figure A.12

You earn:
Other person earns:

If the other person sends £**8** to you, it is tripled to £24. Now, the other person has £16 and you have £48. How much would you like to send back to the other person?



Figure A.13

You earn:
Other person earns:

If the other person sends £**12** to you, it is tripled to £36. Now, the other person has £12 and you have £60. How much would you like to send back to the other person?



Figure A.14

You earn:
Other person earns:

If the other person sends £**16** to you, it is tripled to £48. Now, the other person has £8 and you have £72. How much would you like to send back to the other person?

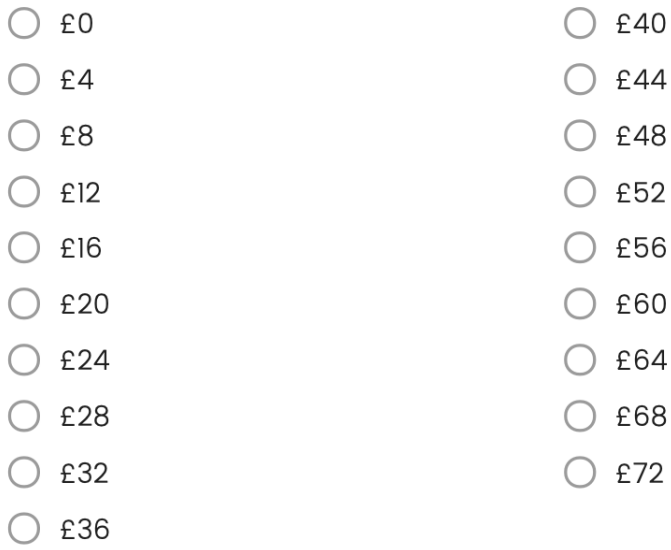


Figure A.15

You earn:
Other person earns:

If the other person sends £**20** to you, it is tripled to £60. Now, the other person has £4 and you have £84. How much would you like to send back to the other person?

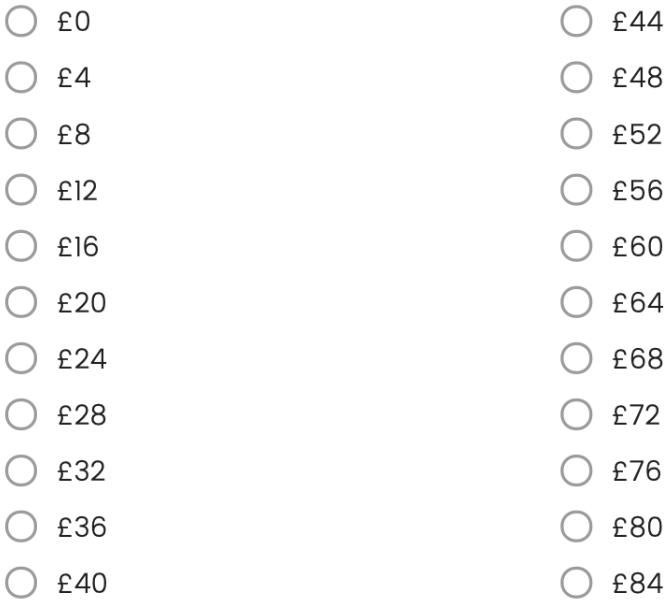


Figure A.16

You earn:

Other person earns:

If the other person sends £**24** to you, it is tripled to £72. Now, the other person has £0 and you have £96. How much would you like to send back to the other person?

○ £0	○ £52
○ £4	○ £56
○ £8	○ £60
○ £12	○ £64
○ £16	○ £68
○ £20	○ £72
○ £24	○ £76
○ £28	○ £80
○ £32	○ £84
○ £36	○ £88
○ £40	○ £92
○ £44	○ £96
○ £48	

Figure A.17

You earn:
Other person earns:

Decision as Player A

You will take this decision in the role of **Player A**. The other person is now in the role of Player B.

You do NOT know the other person's views on Boris Johnson. Similarly, the other person does NOT know. your views on Boris Johnson.

Out of the £24 in your pocket, how many pound would you like to send to the other person?

£0 £4 £8 £12 £16 £20 £24

Now make a guess and increase your earnings

We have requested the other person in the role of Player B to decide on how much money to send back to you for each possible amount that you could have sent as Player A. Can you guess how much the other person will send back to you? For each correct guess we will increase your final earnings by 10 percentage points.
Out of £24 in your pocket, if you send £**0**, how much do you think the other person will send back?

- ○ £0 (You and the other person each earn £24)
- ○ £4 - £24 (You earn between £28 and £48, the other person earns between £0 and £20)

Figure A.18

Out of £24 in your pocket, if you send £**12**, how much do you think the other person will send back?

- ○ £0 - £8 (You earn between £12 and £20, and the other person earns between £52 and £60)
- ○ £12 - £20 (You earn between £24 and £32, and the other person earns between £40 and £48)
- ○ £24 (You and the other person both earn £36)
- ○ £28 - £60 (you earn between £40 and £72, and the other person earns between £0 and £32)

Figure A.19

Out of £24 in your pocket, if you send £**24**, how much do you think the other person will send back?

- ○ £0 - £20 (You earn £0 and £20, and the other person earns between £76 and £96)
- ○ £24 - £44 (You earn between £24 and £44, and the other person earns between £52 and £72)
- ○ £48 (You and the other person each earn £48)
- ○ £52 - £96 (You earn between £52 and £96, and the other person earns between £0 and £44)

Figure A.20

If you would like to revise your "Decision as Player A" at the top of this page, please feel free to do so now.