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Crowding-out the in-group bias: a nationalist policy paradox?

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Abstract

Using a dictator game experiment, we investigate if a policy of introducing material incentives to favour one's own group members will be effective in raising the in-group bias in behaviour. It is not: the introduction of the material incentives in our experiment crowds-out the in-group bias in our subjects' social preferences. Specifically, we find evidence that is consistent with the social identification with own group members weakening through the introduction of material incentives towards the in-group bias. This result potentially creates a nationalist policy paradox whereby policies like tariffs and discriminatory employment regulations designed to encourage materially the employment of home rather than foreign workers will, on the evidence of this experiment, weaken individuals' preferences for favouring home over foreign workers.

JEL Codes: C72, C91, D31, D63, D91, J70, Z18

Keywords: experiment, dictator game, social identification, in-group bias, incentives, crowding-out

People are frequently nicer to members of their own group than those who belong to a different one. This in-group bias in pro-sociality has, for example, been frequently observed in social psychology and economics (e.g. see Chen and Li, 2009, and Hargreaves Heap and Zizzo, 2009, for experimental evidence). In this paper, we examine with an experiment whether this revealed in-group bias in social preferences is crowded-out by the introduction of material incentives designed to encourage in-group biased behaviour.

In general, the possible crowding-out of social preferences is important because it can affect the efficacy of policy interventions that turn on tweaking material incentives in favour of pro-social behaviour: the weakening of social preferences tends to offset the effect on behaviour of the change in the material incentives. In our particular case, a crowding-out of the in-group bias in social preferences through the introduction of pro-in-group material incentives would have a paradoxical policy implication. We call it the nationalist policy paradox. This is because common nationalist policies, like tariffs and tougher employment regulations for foreign workers, that materially encourage the employment of home rather than foreign workers, would, with crowding-out, paradoxically mean that the motivating belief or social preference for such policies of treating home workers better than foreign ones would actually become weaker.

The background to this question is a large literature on the crowding-out of social preferences when material incentives designed to encourage pro-social behaviours are introduced (see Bowles and Polania-Reyes, 2012, for a survey). Gneezy and Rustichini (2000), hereafter G&R, famously illustrate this possibility and the associated policy concern. They report on an experiment where a fine, introduced to deter late pick-ups at day care nurseries, backfires spectacularly because the numbers of late pick-ups actually increases after the introduction of the fine. We qualitatively replicate the G&R experiment, but, in a different laboratory setting, to test for the possible crowding-out of the in-group biased character of social preferences.

To our knowledge, we are the first to test for this possibility and its associated implication of a nationalist policy paradox. This is one of our contributions and its relevance stretches beyond that of nationalist policies. Companies or teams, for example, that compete with each other might naturally wish to encourage their employees/team members to behave more nicely and more cooperatively with each other than with their competitors' employees/team members. Would a strengthening of the material incentive towards being especially nice to own group members be an effective way of encouraging this difference in behaviour or should they fear crowding-out?

There are also examples where policy interventions could take the opposite form because the in-group bias appears to be unwarranted discrimination. For example, there is no good reason for a doctor or lawyer to care more about a patient or a client simply because they belong to the same group as themselves. So, the question arises: would a tweak in the material incentives, this time against the in-group bias, be effective in reducing the in-group bias in behaviour? Or might there be some countervailing crowding-out, in this instance, of the social preference for equal treatment that will tend to offset the effect of the change in material incentives? (In an ancillary experiment that we report in the appendix, we test, in manner analogous to the crowding-out of the in-group bias in social preferences in our main experiment, whether there is also crowding-out of the equal treatment social preferences when material incentives are introduced to discourage the in-group bias.)

The experimental methodology enables us to identify whether there is crowding-out. It also allows us to test a particular explanation of the in-group bias and its possible crowding-out. This is our second contribution: we test whether the in-group bias arises because people socially identify more strongly with own group members than others; and we test whether, if there is crowding-out, this can be explained because own group social identification weakens with the introduction of the material incentives. Social identification theory provides a plausible explanation of the in-group bias (e.g., see Tajfel and Turner, 1979), but it is not the only one. Out-group hate is another possible explanation of the bias and, in so far as there is crowding out, it could also arise from a weakening in out-group hate. This difference in the possible origin of the in-group bias that we test can also be expressed slightly differently: does the in-group bias arise from positive or negative discrimination (see Hargreaves Heap and Zizzo, 2009)?

Our second contribution in this respect is also potentially important, partly because social identification theory has become an increasingly popular explanatory vehicle in economics (see Akerlof and Kranton, 2000 & 2005, and Shayo, 2020). It is also important because in so far as there is crowding-out and it can be connected to weakening social identification, then it points to a more general conclusion: the preferences that are revealed through the influence of social identification on behaviour are not fixed. That is, they cannot be taken to be exogenous. This matters because preferences are often regarded as a bedrock in economics. For example, Stigler and Becker (1977) famously suggest that 'de gustibus non est disputandum' and Lucas (1976) notably argues for and establishes a programme in macroeconomics that is based on individual preferences precisely because they are presumed to be stable.

In the experiment, our subjects make dictator decisions in three phases either in a baseline control where there is no group affiliation or in group treatments where subjects are randomly assigned to either a Yellow or Green group. In the group treatments, each subject makes two dictator decisions in each phase: one where the co-player comes from own group and the other where co-player belongs to the other group; and the group affiliations are common knowledge.[1]

Our background assumption is that individuals decide how much to allocate to their co-player by weighing their selfish preference for own pay-offs against their social preference for the pay-offs of the co-player. In the first phase of these dictator decisions this is the only consideration because the dictator simply has an endowment and makes the allocation to the co-player (there is no policy of a fine or a subsidy to provide an extra material incentive either towards or away from an allocation to the co-player). We further conjecture from social identification theory that in the group treatments the social preference weight attached to the co-player's pay-offs is higher when the other person belongs to the same group than when there are no group affiliations; whereas the weight attached to a co-player from the other group is no higher in the group treatments than when there are no group affiliations in the baseline control.

Aggregate behaviour in the first phase is consistent with this prediction, but we find individual differences. Roughly half our subjects behave in this way and reveal the in-group bias in social preferences and half either make no such distinction by giving the same amount to both types of co-player or give more to a co-player from the other group. Although the balance between these two groups is somewhat different in our experiment, this is not unlike the G&R first phase because they start from a position where some people reveal a social preference for 'good' behaviour with timely pick-ups and others reveal with late-pick-ups either no such social preference or, indeed, a social preference for 'bad' behaviour.

In the second phase, G&R introduce a fine on 'bad' behaviour in some day care centres and not others. We do the same in phase 2. We have one Group treatment (Group-Fine) where the subjects in phase 2 are fined if they do not exhibit an in-group bias in their allocation in the second phase and another group treatment (Group) where there is no fine. In other words, our

1 Thus, the group affiliations are artificial and minimal and so provide a 'tough' test of social identification in the sense of Popper.

in-group biased social preference is analogous to the 'good' behaviour social preference in G&R and we attempt to encourage the behaviour associated with this preference by fining those who do not behave in this manner in Group-Fine, just as G&R do.[2] Despite the fine on those who do not exhibit the in-group bias, the aggregate in-group bias does not change in the phase 2 of Group-Fine as compared with either that in the first phase or when compared with the phase 2 in-group bias in Group (the group treatment where there is no fine). Thus, although the policy does not spectacularly backfire in the way of G&R, the fine policy is nevertheless ineffective in our experiment and this points to the existence of crowding-out.

This aggregate evidence of crowding-out is what G&R present in favour of crowding-out. Our laboratory design, however, improves over the G&R test for crowding-out because we can also test for crowding-out at the individual level and the possible mechanism behind it. In particular, we find that those who reveal the in-group bias social preference in the first phase of Group-Fine reduce the extent of their bias in the second phase. This is important because those who have revealed a social preference for the in-group bias in the first phase, have no material reason to adjust their behaviour in the second phase when the fine is introduced. The reduction in their in-group bias can only have arisen because their social preference for the in-group bias diminished: i.e. it was crowded-out. Furthermore, and this is the part that explicitly refers to social identification mechanism, we find that the reduction in the in-group bias occurs because the allocation to own group members falls. The weight given to the pay-offs of a co-player from own group falls and this is consistent with the fine actually weakening the dictator's social identification with own group members.

Finally, in the third phase in Group-Fine, like G&R, we remove the fine and examine whether the crowding-out in phase 2 persists. Again, we can test for persistence in the aggregate data like G&R and, in addition, through individual level data that also allows us to test the social identification mechanism. Unlike G&R, the crowding-out does not persist in our experiment.

In the next section, we define in a dictator decision the in-group bias, an in-group bias in social preferences and their crowding out that we will test and we develop the hypotheses we use to test the possible role of social identification theory in explaining this social preference bias and its change. Section 2 explains the experimental design and Section 3 gives the results. Section 4 concludes.

2 Our electronic appendix describes a complementary experiment where we instead fine in-group bias behaviour so as to discourage this kind of behaviour. We focus in the main part of the paper on the fine to encourage in-group biased behaviour for technical reasons that we explain later.

1. Theory and hypotheses

We ask our subjects in the group treatment to make dictator decisions with co-players who either belong to the same group or the other group. We define the possible varieties of biased and non-biased behaviour by the relations between allocations to the co-player who is a member of the same group (= $CP(own)$) and the co-player who belongs to the other group (= $CP(other)$). These supply the tests for whether there is an in-group bias and whether it changes.

Definition: In–group biased behaviour (IGB) arises when $CP(own) > CP(other)$ and its extent is measured by $CP(own) - CP(other)$.

Definition: Equal treatment behaviour (EQB) arises when$CP(own) = CP(other)$.

Definition: Out–group biased behaviour (OGB) arises when $CP(own) < CP(other)$ and its extent is measured by $CP(own) - CP(other)$.

We choose the gap between own and other allocations as the index of the in-group bias, but recognise that a ratio measure could have been used. Accordingly, the Appendix gives the corresponding results for the ratio measure. There are no qualitative differences.

We assume in general that individuals value their own pay-off (*OP*) and (possibly) their co-player's pay-off (*CP*) as in (1).

$$U = f(OP, CP) \qquad (1)$$

In the first phase dictator decision, an individual decides how to divide a sum *X* between *OP* and *CP*. This is the constraint on maximising (1). Since the relative 'price' of *OP* in terms of *CP* is 1 in this constraint, it follows that utility maximisation will be achieved when the ratio of marginal utilities from *OP* and *CP* is equal to this relative price of 1. The chosen allocation *OP/CP* is thus given by the elasticity of substitution between *OP* and *CP* in (1). The smaller the elasticity (i.e. the larger the % change in *CP* is required to compensate for a unit % change in *OP*), the bigger is the share of *OP* relative to *CP*.

As an illustration consider a Cobb-Douglas utility function as in (1'), where '*a*' and '*b*' are the weights given respectively to each type of pay-off in the individual's utility function, and *A* is a constant. In effect, this follows the Charness and Rabin (2000) representation of preferences when they test for the character of social preferences revealed in dictator like decisions. They consider discrete choices between pairs of allocation and so can use a linear utility function in own and co-player pay-offs. As we have a range of options between 0% and 100% of *X*, this linearity would produce corner solutions and to avoid this we assume log-linear preferences.

$$U = A * OP^a * CP^b \qquad (1')$$

Maximising (1) subject to the constraint $OP + CP = X$ yields the following:

$$OP = a * X/(a + b)$$

$$CP = b * X/(a + b) \qquad (2)$$

In the simple dictator game above, we note that there is no material incentive in this utility maximisation dictator decision to treat co-players differently on the basis of their group membership because a one-unit allocation to a co-player costs that individual one unit in terms of *OP* whether the co-player comes from own or the other group. Thus, in so far as $CP(own) > CP(other)$ (i.e. IGB is observed), it reveals in-group biased social preferences (IGBSP). For example, in the Cobb-Douglas illustration '*b(own)*' > '*b(other)*'. By the same reasoning in this simple dictator decision, EQB reveals an equal treatment social preferences (EQTSP) and OGB reveals an out-group biased social preferences (OGBSP).

In this way the relation between *CP(own)* and *CP(other)* tells us whether IGBSP, EQTSP or OGBSP are revealed by subjects when they make the simple dictator decision above.

We assume for the purpose of testing social identification theory that it predicts that individuals who identify more closely with a group weigh co-player's pay-offs from that group more highly than they do co-player's from groups they identify with less closely. Thus for example, '*b(own)*' > '*b(other)*', $'b'(the\ value\ in\ the\ no - group\ control)$ in the Cobb-Douglas representation of preferences. Such social identification, together with (2) implies $CP(own) > CP(other), CP(when\ there\ are\ no\ groups)$.[3] In so far as social identification is weak or does not apply, then $'b(own)' \cong 'b(other)' \cong 'b'(the\ value\ in\ the\ no - group\ control)$ and $CP(own) \cong CP(other) \cong CP(when\ there\ are\ no\ groups)$.

Social identification theory provides one reason why IGBSP might be revealed in behaviour in the simple dictator decision, but it is not the only possible cause. An alternative explanation of the bias in behaviour is that the introduction of explicit groups triggers out-group hate. In this case, we assume *CP(other)* falls relative to *CP* when there are no groups (i.e. '*b(other)'* falls

3 In the general case, we assume social identification theory predicts that the elasticity of substitution between OP and CP in an individual's utility function is higher when the co-player is from own group: i.e., it requires a smaller % change in an own group co-player's pay-offs to compensate for a unit % change in own pay-offs.

relative to '*b*'(where there are no groups). Further since there is no reason to suppose *CP(own)* is different to *CP* on this account (i.e., '*b(own)*' is any different to '*b*'), a gap is opened up between *CP(own)* and *CP(other)* because *CP(other)* falls (i.e., '*b(own)*' > '*b(other)*' because *b(other)* falls).

Thus our basic test of social identification versus out-group hate in the explanation of IGB is whether the gap between *CP(own)* and *CP(other)* opens up because $CP(own) > CP$ or because $CP(other) < CP$. Thus an alternative way of expressing this difference is whether the in-group bias arises from positive discrimination in favour of own group members or negative discrimination against out-group members.

It is, of course, possible that subjects feel some identification with the group as a whole when there are no explicit group affiliations in our Control---so *CP* may reflect some social identification. This is less likely in our online experiment than in laboratory ones. Nevertheless, such a whole group identification with everyone in the experiment, if it exists, cannot be as strong as the identification with own group when there is explicit assignment to either a Yellow or Green group: thus, with group identification, *CP(own)* will be greater than *CP* (i.e., *b(own)* will be greater than '*b*'), and how much greater depends on whether there is any whole group identification when there are no explicit groups. In so far as there was any whole group identification supporting *CP*, then this means that social identification theory might also predict that *CP* will be greater than *CP(other)* (i.e. '*b*' could be higher than '*b(other)*') and this, of course, is what the out-group hate hypothesis predicts. So in these circumstances what distinguishes the social identification account from out-group hate is that *CP(own)* exceeds *CP* (i.e., '*b(own)'* will be greater than '*b*'). H1 follows.

> **H1** (social identification and in-group bias): *CP(own)* is greater than *CP(other)* because relative to *CP* when there are no groups, the introduction of explicit groups leads *CP(own)* to rise and *CP(other)* does not rise.

Now, let us consider how material incentives could influence these social preferences. There is a large social psychology literature following Deci (1975) arguing that the 'intrinsic' reasons for taking an action can be crowded-out by the introduction of 'extrinsic' reasons to take that action. 'Intrinsic' reasons have often been taken in economics to mean having a preference for that action (or its outcome) and the 'extrinsic' reasons for action come from material incentives towards an action (e.g. see Frey, 1997). This literature predicts that the introduction of a material incentive towards a behaviour may so crowd-out the intrinsic reasons for the action that the incentive has no or possibly the opposite effect on behaviour in the aggregate. This is the version of crowding-out that G&R test and we do the same by introducing a fine in the phase 2 dictator decisions on those who do not exhibit IGB. H2 follows as an analogous test to G&R of crowding-out in our experiment.

> **H2** (aggregate crowding-out): The introduction of the fine in phase 2 designed to encourage IGB either has no effect on the IGB in the aggregate or a negative effect (i.e., IGB falls).

We are also able to test for crowding-out at the individual level. Consider formally a second dictator decision problem where a fine (F) is introduced on any individual who does not reveal IGB. The fine creates a new constraint for the maximisation problem, given by (3).

$$OP = X - CP \qquad \text{if } CP(own) > CP(other)$$

$$OP = X - CP - F \qquad \text{if } CP(own) \leq CP(other) \qquad (3)$$

We note that, for those who revealed IGBSP in the first phase dictator decisions, this new constraint is not binding on the utility maximizing decision. There is a change in material incentives but that change does not <u>materially</u> impinge on decision makers who revealed IGBSP in phase 1. Thus, the only reason for subjects who reveal IGB behaviour in phase 1 to change their IGB behaviour in phase 2 is if the IGBSP changes in phase 2. H3 follows as a test that it occurs in our experiment.[4]

> **H3** (individual crowding-out): Those who reveal IGBSP in the phase 1 dictator decision reveal lower IGB behaviour in the phase 2 than in phase 1: i.e., their $CP(own) - CP(other)$ falls in the second phase compared with the first.

The alternative hypotheses for this set of individuals with IGBSP in phase 1 are either that there is no change in $CP(own) - CP(other)$ or that there $CP(own) - CP(other)$ rises. We call the latter crowding-in.

Although this reverse possibility has not been theorised in the same way as crowding-out, there is evidence of it in the empirical literature (see Bowles and Polania-Reyes, 2012). It is also not

4 In the complementary experiment where the fine is levied on IGB behaviour, the analogous test would be that those who initially revealed EQTSP and OGBSP (and so were unaffected materially by the fine) nevertheless reduced their out-group dictator allocation. This is a weaker test than the one above because, unlike IGBSP subjects above, the EQTSP subjects in the complementary experiment have no margin of adjustment in their behaviour to reveal such crowding-out while maintaining EQTSP. Crowding-out would only potentially register among OGBSP subjects and they are small in number.

difficult to see why it might occur. When a policy of encouraging a particular behaviour is introduced through tweaking the material incentives, it is possible that this public material endorsement of the behaviour encourages people to re-evaluate positively the 'intrinsic' reasons that they have for engaging in such actions.

If there is crowding-out then it could be explained by the crowding-out of social identification with own group (or equivalently a weakening of positive discrimination). In this case, the reason $CP(own) - CP(other)$ falls in the second phase is because *CP(own)* falls in the second phase (it gets closer to *CP* with the weakening of own group social identification). The contrasting explanation of IGB behaviour that it comes through the triggering of out-group hate would instead have any crowding-out explained by the fall in out-group hate (or equivalently a weakening of negative discrimination): i.e., $CP(own) - CP(other)$ falls because *CP(other)* rises in the second phase (e.g., in the Cobb-Douglas illustration $d\,b(own)/dF < 0$). H4 follows.

> **H4** (individual crowding-out due to weakened social identification): If those who reveal IGBSP in the first phase also reveal a lower IGB ($CP(own) - CP(other)$) in the second phase than the first, it is because *CP(own)* falls in the second phase.

We also consider a possible kind of crowding out/in of IGBSP that might arise with individuals who reveal EQTSP in the first phase. The fine in the second phase dictator decisions <u>does</u> affect their utility maximising decision, it creates a material incentive to move towards IGB behaviour. With a Cobb-Douglas utility function, they should marginally adjust *CP(own)* up and/or *CP(other)* down so that the utility cost of IGB behaviour is minimised by adjusting on both sides of EQB. Thus, in so far as EQTSP individuals in phase 1 reveal larger IGB behaviour in phase 2 than these marginal adjustments, it suggests that they have to some degree gained a social preference for IGB (i.e., IGBSP has been crowded-in).

> **H5** (individual crowding-in of IGBSP among the EQTSP)**:** Those who reveal EQTSP in the first phase and adjust to the fine with IGB behaviour in the second phase, do so with non-marginal changes to *CP(own)* and/or *CP(other)*

To preserve the comparison with G&R, we are finally interested in whether any crowding-out/in of IGBSP persists when the material incentives to IGB behaviour in Phase 2 are removed. Thus, in Phase 3 of the dictator decisions, the fine is removed and the dictator decisions are formally the same as in phase 1. Our tests for persistence follow in natural extension naturally by comparing $CP(own) - CP(other)$ in phase 3 with that in phase 2 and phase 1.

2. Experimental design and procedures

At the beginning of the experiment, each subject received a separate one-time lump sum endowment of 50 tokens. They then made decisions in three Phases.

2.1 Dictator decisions

In each Phase, all subjects independently made decisions in a dictator game. Each subject decided how to split 80 tokens between him/herself and an anonymous subject in the study. The recipient had no say in the allocation. Before making their decisions, dictators were informed that both they and the recipient had an endowment of 50 tokens. The Nash equilibrium is for dictators to allocate 0 tokens to recipients, and keep all 80 tokens for themselves. In the absence of distributional concerns, any allocation of tokens between the two is efficient.

Before making decisions in a Phase, subjects were informed that they would be matched with a randomly chosen participant in the study, and that either their decision or that of the matched coparticipant would be implemented. This payment procedure made it clear that there was an equal chance of being a dictator or a recipient in the Phase. Therefore, it made decisions incentive compatible, i.e., subjects had every incentive to take each decision seriously.[5]

2.2 Treatments

We ran three main treatments. Treatments varied in whether or not subjects were assigned to groups, and whether dictators received incentives to favour members of their own group.

In BASELINE, subjects were not assigned to any groups and did not receive any additional incentives. In each Phase, dictators made one allocation decision where the recipient was a randomly chosen participant in the same treatment. All three Phases were identical.

In Group, subjects were randomly assigned to either a **YELLOW** or a **GREEN** group, and informed of the group assignment at the beginning of Phase 1. In each Phase, dictators made

5 Prior to the main experiment, all subjects independently performed a real effort task for three minutes. The task involved converting a randomly generated three-letter "word" into a numeric string (Erkal et al., 2011). Subjects were paid 3 tokens for every correct code. They received no feedback until the end of the experiment. This task was completely independent of the dictator game.

two allocation decisions: one where the recipient belonged to the same group, and one where the recipient belonged to the other group. All three Phases were identical.

In Group-Fine, subjects were once again randomly assigned to groups and made two decisions in each Phase as in Group and in phase 1 the decision is identical to that in Group. Group-Fine differs in the phase 2 dictator decisions: earnings in Phase 2 were subject to a possible adjustment. In particular, if a dictator's decision was chosen as the allocation relevant for earnings in Phase 2, then the dictator's earnings for the Phase were reduced by 10 tokens if he/she allocated strictly fewer tokens to the recipient from his/her own group than to a recipient from the other group. Equal allocations were also penalised. Thus, there was an incentive to favour, i.e., allocate more to, a recipient from the dictator's own group. If the matched coparticipant's decision was chosen for implementation, then the recipient's earnings were not adjusted. Phase 3 like Phase 1 was identical to those Phases in Group, and earnings in these Phases were calculated as before with no adjustments. Table 1 summarises our treatments.[6]

Table 1. Summary of treatments

<table>
<tr><th rowspan="2">Treatment</th><th rowspan="2">Groups?</th><th rowspan="2"># decisions per Phase</th><th rowspan="2">Earnings reduction in Phase 2?</th><th colspan="3"># subjects</th></tr>
<tr><th>Yellow</th><th>Green</th><th>Total</th></tr>
<tr><td>BASELINE</td><td>No</td><td>1</td><td>No</td><td colspan="2">38</td><td>38</td></tr>
<tr><td>Group</td><td>Yes</td><td>2</td><td>No</td><td>37</td><td>34</td><td>71</td></tr>
<tr><td>Group-Fine</td><td>Yes</td><td>2</td><td>Yes, if no in-group bias</td><td>39</td><td>39</td><td>78</td></tr>
<tr><td colspan="5">Earnings were adjusted only if the dictator's choice was chosen for implementation.</td><td>Total</td><td>187</td></tr>
</table>

2.3 Procedures

The experiment was conducted over two sessions using the online platform Prolific which gave us access to volunteer adult subjects from a number of countries.[7] Upon agreeing to participate in the study advertised on Prolific, subjects were directed to a website that hosted our experiment. Subjects first read a consent statement and, if they agreed, were then presented with instructions for the experiment (available in Appendix A in the Electronic Supplementary

6 As mentioned earlier, we ran an additional complementary treatment. Group-FineProEqual, was procedurally the same as Group-Fine, but differed in the earnings adjustment in Phase 2. In this treatment, dictators were given an economic incentive to *not* favour recipients from their own group. Earnings were reduced by 10 tokens if a dictator's decision was chosen as the relevant one for payment and if he/she had allocated more to a recipient who belonged to their own group. Here, equal allocations were *not* penalised. Phase 1 and Phase 3 were the same as in Group. A total of 79 (41 Yellow and 38 Green) participated in this treatment.

7 We conducted multiple sessions to minimise the chances of server overload during a session and to avoid the whole session crashing. The two sessions were conducted one after the other on the same day. We ran a third session where all subjects were assigned to Group-FineProEqual.

Material). Subjects were randomly assigned to one of the three treatments as they signed up to participate. They then completed the experiment on their own devices at their own pace.[8] The experiment was programmed in oTree (Chen et al., 2016).

Subjects received no feedback during the experiment. Subjects were paid a flat participation fee of USD 1.50 upon completion of the experiment. Within the next two days, they were paid their earnings from each Phase of the experiment. Token earnings were converted to cash at the rate of 200 tokens to USD 1. The average participant took about 12 minutes to complete the experiment and received an additional USD 1.10. The total average payment was USD 2.60, which translates to USD 13 as an hourly rate.

3. Results

Table 2 gives the aggregate dictator allocation to their co-player in our baseline where there are no group affiliations and in the two Group treatments for each of the 3 phases. We focus first on phase 1 and use Wilcoxon ranksum tests to make comparisons across treatments and Wilcoxon signed rank tests to make comparisons within treatments. We first note that there is an in-group bias in behaviour (IGB) in both Group treatments: *CP(own)* is significantly greater than CP(*other*) (respectively in Group and Group-Fine, $p < 0.00001$; $p = 0.0084$) in Phase 1.

Table 2. Mean dictator allocations

		Recipient's group					
		Phase 1		Phase 2		Phase 3	
	Obs.	Own	Other	Own	Other	Own	Other
Baseline	38	29.47 (13.74)		30.39 (15.74)		29.61 (16.54)	
Group	71	37.75 (15.30)	28.83 (15.02)	40.00 (15.17)	28.45 (15.06)	38.87 (17.51)	29.23 (16.64)
Group-Fine	78	34.62 (14.07)	29.94 (14.78)	37.26 (14.78)	29.69 (13.75)	37.08 (15.30)	28.17 (14.56)

Figures in parentheses are standard deviations. Dictators and recipients in the Baseline do not have a group identity. All participants have an endowment of 50 tokens each. The size of the pie the dictator splits is 80 tokens in all cases.

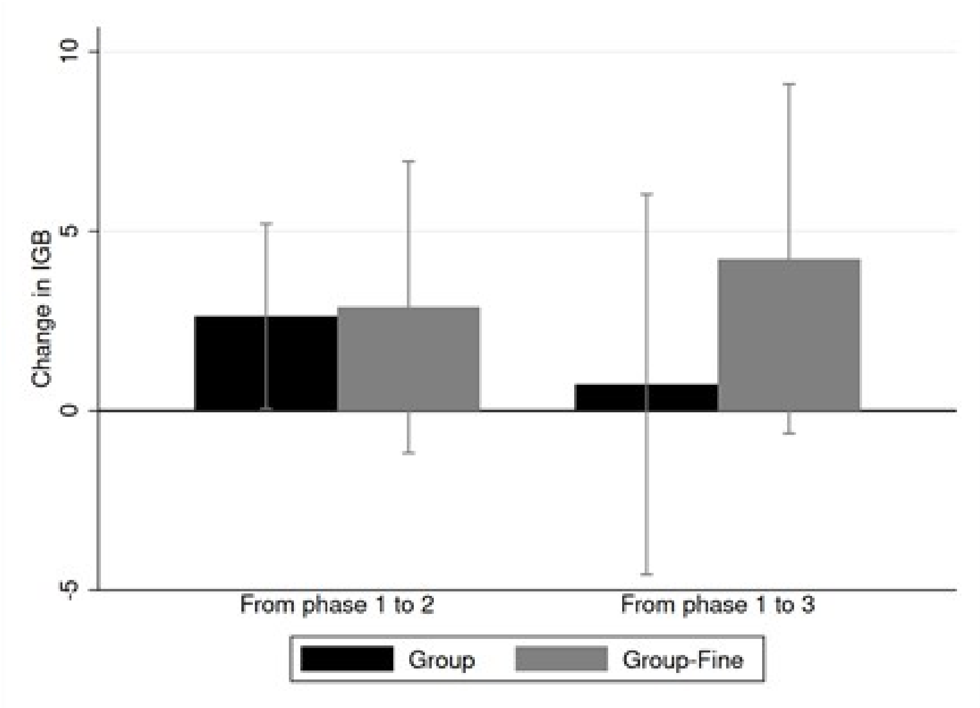
To test H1 on the social identification sources of this bias, we note that the phase 1 baseline allocation *CP* is very similar to *CP(other)* in both the Group treatments. This goes against the

8 There was a maximum time limit of 40 minutes after which subjects who had not yet completed the experiment were automatically ejected from the study by Prolific, and no data from them were recorded.

alternative out-group hate explanation for IGB and is consistent with weak or no overall group identification in the baseline under social identification theory. Furthermore, *CP(own)* is higher than CP as predicted by social identification as the source of the bias. *CP(own)* in Group is significantly higher than the baseline CP (p = 0.0098), but while *CP(own)* in Group-Fine is higher than the baseline *CP* this is not significantly higher (p = 0.2136). During phase 1 there is no reason to distinguish Group and Group-Fine, and when we combine *CP(own)* in these two Group treatments, *CP(own)* is significantly higher than the baseline *CP* (p = 0.0381).

> **Result 1** (support for H1, social identification and the in-group bias): Dictator allocations to own group co-players are higher than the allocation to co-players from the other group, and this is due to higher *CP(own)* in the Group treatments than *CP* in the baseline.

Figure 1. Aggregate change in IGB with 95% confidence intervals



We turn now to the crowding-out/in hypotheses and begin with H2, the aggregate test. The left part of Figure 1 shows the average change in IGB between phase 1 and phase 2 in the two Group treatments, along with the 95% confidence intervals. The average change is similar in the two treatments and the difference between them is not significant (2.63 vs. 2.88, p = 0.6782). Result 2 follows.

Result 2 (in support of H2, no aggregate crowding out): The introduction of the fine has no aggregate effect on the in-group bias in behaviour.[9]

With respect to the individual crowding-out hypothesis H3, the top panel of Table 3 reports summary statistics of the magnitude of the IGB, $CP(own) - CP(other)$, of those who revealed such a bias in phase 1 in the Group and Group-Fine treatments and also how this group of subjects' IGB evolves in phases 2 and 3. We compare the change in $CP(own) - CP(other)$ between phase 1 and 2 in Group and Group-Fine for this group of IGB individuals in phase 1: the change in IGB in Group-Fine is significantly less than the change in IGB in Group (p = 0.0482 when looking at absolute changes, p = 0.0396 when looking at percentage changes).

Table 3. Mean change in favouritism conditional on level of favouritism in Phase 1

		Group favouritism			Change in group favouritism		% Change in group favouritism	
					From Phase		From Phase	
	Obs.	Phase 1	Phase 2	Phase 3	1 to 2	1 to 3	1 to 2	1 to 3
					In-group favouritism (IGB)			
Group	32	22.28	24.53	17.97	2.25	-4.31	0.27	0.09
		(18.01)	(18.59)	(31.13)	(14.08)	(29.84)	(0.91)	(1.89)
Group-Fine	24	21.46	15.38	17.29	-6.08	-4.17	-0.22	-0.04
		(17.48)	(21.79)	(20.27)	(19.78)	(23.62)	(1.27)	(1.2)
					Equal allocations (EQT)			
Group	35	0	1.86	3.43	1.86	3.43	-	-
		(0)	(5.16)	(12.59)	(5.16)	(12.59)		
Group-Fine	45	0	6.24	4.18	6.24	4.18	-	-
		(0)	(16.57)	(14.65)	(16.57)	(14.65)		

Figures in parentheses are standard deviations. There were 4 (9) individuals in Group (Group-Fine) who displayed OGBSP in Phase 1. Given the small number of observations here, we do not conduct any analysis of the behaviour of these individuals.

Parametric individual regressions in Table 4 also support H3 and provide additional evidence in support of H2. Table 4 gives the OLS regressions for the Group and Group-Fine treatments with the change in $CP(own) - CP(other)$ between phase 1 and 2 as outcome variable. In

9 The analogous result in the complementary experiment where the fine is levied on IGB behaviour is the opposite: the fine has an aggregate effect because it significantly reduces IGB behaviour. We cannot conclude from this that there was no crowding-out, but if it exists, it is clearly weaker for the fine on IGB behaviour than is the fine on non-IGB behaviour.

column (1) we just consider a dummy for the Group-Fine treatment, where the Group treatment is the omitted category. Column (2) has the full set of interactions between this dummy and two dummies for subjects who revealed IGBSP and OGBSP respectively in phase 1. EQTSP subjects in phase 1 are the omitted category for these dummies. Column (3) adds socio-demographic controls at the individual level.[10] Results from column (1) show that on average the fine does not have a significant impact on IGB at the individual level (i.e., further support for H2). However, in column (2) the coefficient on the interaction between the Group-Fine treatment and displaying IGB in phase 1 is negative and significant at 5% level in support of H3. This result is robust to the additional controls of column (3).

Table 4. Regression on change in in-group bias between phase 1 and 2

	Change in favouritism		
	(1)	(2)	(3)
Group-Fine	0.251	4.387	4.45
	(2.449)	(3.308)	(3.704)
IGBSP		0.393	-1.999
		(3.589)	(4.052)
Group-Fine × IGBSP		-12.72**	-13.56**
		(5.162)	(5.792)
OGBSP		10.64	12.87
		(7.746)	(8.532)
Group-Fine × OGBSP		-6.887	-9.216
		(9.419)	(10.44)
Constant	2.634	1.857	-7.962
	(1.808)	(2.481)	(16.21)
Individual controls	NO	NO	YES
Obs.	149	149	149

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

10 The controls are gender, age, education, employment status, political and economic opinions and a measure of performance in the previous real effort task. None of these control variables is statistically significant.

Result 3 (in support of H3, individual crowding out): For players who revealed IGBSP in phase 1, IGBSP is crowded-out in Group-Fine in phase 2 as compared with Group.[11]

We turn to whether the crowding-out that we observe can be attributed to a fall in social identification with one's own group (H4). Table 5 presents the average *CP(own)* and CP(other) for subjects who reveal IGBSP in phase 1. In Group-Fine, IGB decreases from Phase 1 to Phase 2 due to a decrease in *CP(own)* and an increase in *CP(other)*. However, neither change is significant when compared with the change in the baseline (*CP(own)*: p = 0.2329; *CP(other)*: p = 0.1890).

Table 5. Mean dictator allocations for subjects who reveal IGB in phase 1

		Phase 1		Phase 2		Phase 3	
	Obs.	Own	Other	Own	Other	Own	Other
BASELINE	38	29.47		30.39		29.61	
		(13.74)		(15.74)		(16.54)	
Group	32	43.91	21.62	46.41	21.88	43.59	25.62
		(16.3)	(12.22)	(15.77)	(13.84)	(20.21)	(19.12)
Group-Fine	24	38.12	16.67	36.54	21.17	35.21	17.92
		(15.24)	(10.39)	(16.47)	(13.26)	(16.97)	(12.76)

Figures in parentheses are standard deviations.

Table 6 gives individual OLS regressions on H4. Columns (1) and (2) present regressions of *CP(own)* in Phase 2 with *CP(own)* in Phase 1, treatment dummies (excluded treatment: BASELINE), dummies for IGB and OGB in Phase 1 (excluded category: EQB) and their interactions with Group-Fine as explanatory variables. Columns (3) and (4) present the corresponding regressions for *CP(other)*.

11 As noted in footnote 5, the test for crowding-out in the complementary experiment is weaker than in the main experiment because EQTSP individuals cannot reveal adjustments downwards in their EQTSP. Nevertheless, there is no evidence of individual crowding out because the size of the bias does not shrink among EQTSP and OGBSP compared with Group (test for the joint p = 0.3799). Together with the analogous result in the complementary experiment to Result 2, reported in footnote 11, this suggests crowding-out was weak at best in the case of fines designed to discourage IGB. This cannot be because EQTSP is more salient as the 'correct' social preference in this experiment because the numbers with IGBSP and EQTSP are about the same. It is possible though that the fine is associated with a market-type intervention (i.e. it puts a price on a particular kind of behaviour) and markets are known from other experiments to encourage equal treatment (see Hargreaves Heap *et al,* 2013). In this way, the fine may actually reinforce the equal treatment behaviour even though it sets up the conditions where intrinsic is no longer necessary to explain behaviour and so might in other circumstances diminish.

Table 6. Regressions on *CP(own)* and *CP(other)* in phase 2

	(1) Own	(2) Own	(3) Other	(4) Other
Allocation to own group in Phase 1	0.723***	0.709***		
	(0.057)	(0.062)		
Allocation to other group in Phase 1			0.740***	0.717***
			(0.059)	(0.063)
Group	11.59*	10.79	-2.134	-3.649
	(6.204)	(6.64)	(5.829)	(6.188)
Group-Fine	15.34**	15.44**	-2.817	-3.564
	(7.135)	(7.598)	(6.707)	(7.084)
IGBSP	4.173	3.632	-2.209	-1.282
	(2.672)	(2.879)	(2.526)	(2.728)
Group-Fine × IGBSP	-9.436**	-10.13**	3.642	3.382
	(3.763)	(4.029)	(3.54)	(3.771)
OGBSP	6.012	5.202	0.58	-1.337
	(5.599)	(5.863)	(5.394)	(5.623)
Group-Fine × OGBSP	-9.407	-7.978	-3.15	-0.919
	(6.856)	(7.233)	(6.487)	(6.813)
Constant	-1.096	9.235	10.22	29.12**
	(6.971)	(13.57)	(6.596)	(12.69)
Individual Controls	NO	YES	NO	YES
Obs.	187	187	187	187

Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$

The interaction between the Group-Fine treatment and IGB in phase 1 on allocations to co-players from one's own group is negative and significant at 5% (column 1), while the same interaction is not significant, although positive, when measured on allocations to co-players from the other group (column 3). This is robust to adding individual characteristics as controls (columns 2 and 4).

Result 4 (in support of H4): There is evidence from individual-level regressions that crowding-out of IGBSP in phase 2 in Group-Fine is driven by a reduction in *CP(own)* more than by an increase in *CP(other)*.

To test H5 (the possibility of crowding-in of IGBSP), we consider the 20 out of 45 individuals who reveal EQTSP in phase 1 and also adjust to the fine in phase 2 by moving to IGB. On average, these subjects move to an IGB of 17.30 in phase 2 in Group-Fine. This is not a marginal change to avoid the fine: we can reject the hypothesis that IGB=1 (i.e., a marginal adjustment) in phase 2 ($p < 0.00001$). Furthermore, those who reveal IGBSP in phase 1 in the Group treatments have an average IGB of 21.92 and the difference between this and 17.30 is only weakly significant ($p = 0.0916$).

> **Result 5** (supporting H5): Those who reveal EQTSP in phase 1 and go on to reveal IGB in phase 2 adjust non-marginally to the fine in Group-Fine.

Finally, we consider how many of these results persist in phase 3. Result 1 translates completely, as sign rank tests reveal that in the Group treatments CP(*own*) is still significantly greater in phase 3 than CP(*other*) (for Group: $p = 0.0002$, for Group-Fine: $p < 0.0001$) and ranksum tests relative to the baseline on CP(*own*) provide evidence that it is still because of social identification rather than out-group hate (for Group: $p = 0.0088$, for Group-Fine: $p = 0.0323$).

Turning to the impact of the fine and our hypotheses on crowding out/in, we now consider aggregate changes between phase 1 and 3. At the aggregate level, the fine still has no significant impact on IGB (see Figure 1: 0.73 vs. 4.23, $p = 0.4301$). To assess Result 3, we examine if the change in IGB for subjects who reveal IGBSP in phase 1 in Group-Fine is statistically significant by comparing it with the equivalent change in the Group treatment (see Table 3). It turns out that individual crowding-out does not persist, both when looking at absolute changes ($p = 0.9498$) and at percentage changes ($p = 0.9230$). Regarding Result 5, we turn to subjects in Group-Fine who revealed EQTSP in phase 1 and IGB in phase 2. In phase 3 they display an average in-group bias of 8.90. A sign rank test on IGB for these subjects between phase 1 and 3 reveals that the difference is significantly positive ($p = 0.0078$). Therefore, we can conclude that Result 5 holds also in phase 3: i.e., the crowding-in of IGBSP still persists among those who initially revealed EQTSP in phase 1.

4. Discussion and conclusion

In our Group treatments, we find that subjects exhibit an IGB on average in phase 1. They give more to someone from their own group than to someone from another group. Since there is no material incentive to treat other people differently depending on which group they belong to, this bias reveals IGBSP. This finding is consistent with that of many experiments where IGB has been found. We also find that this bias might be explained by social identification theory. The bias arises in the Group treatments because the allocation to someone from own group rises relative to the allocation in the BASELINE where there are no group affiliations (and the allocation to someone from the other group is no different to the BASELINE allocation). As would be expected from social identification theory when individuals identify more strongly with members of their own group than those from other groups, subjects treat own group members especially kindly compared with how other people are generally treated. Since social identification theory has been found helpful in explaining other behaviours in economics, this finding too is broadly consistent with the literature (e.g., see Akerlof and Kranton, 2005). In these respects, our experiment coheres with what is known from other studies.

Our contribution is the test for the crowding-out of IGBSP when material incentives towards IGB are introduced. This, to our knowledge has not hitherto been considered or tested. We are the first to examine this possibility. IGBSPs are crowded-out in the aggregate. At the individual level, the picture is more complicated. Those who initially reveal IGBSP in phase 1, exhibit less IGB after the fine is introduced: their IGBSPs are crowded-out by the introduction of the fine. However, there is some evidence that individuals who did not have IGBSP initially and adjusted to the fine develop IGBSP after the fine: i.e., IGBSPs are crowded-in for this set of individuals. Further there is some evidence that this crowding-in persists into phase 3. Nevertheless, although there are these heterogeneous effects on social preferences, the balance both in phase 2 and phase 3 favours the crowding-out effect in the aggregate because the crowding-out of the subjects who revealed IGBSP in phase 1 is sufficiently large and the number of subjects who are willing to pay the fine is sufficiently high (32 out of 54)[12] to compensate the crowding-in of those who adjust to the fine. Furthermore, the crowding-out that we observe is consistent with a weakening of the social identification origins of this bias. It occurs because the special generosity shown to own group members shrinks in phase 2.

These are important results in two respects.

First, they caution against the use of material incentives to encourage the in-group bias because it produces an offsetting crowding-out of the intrinsic motivation (IGBSP) towards such behaviour. In our experiment, this crowding-out is such that there is no effect in the aggregate

12 Out of the 45 (9) subjects who revealed EQTSP (OGBSP) in phase 1, 25 (7) were willing to pay the fine in phase 2, i.e., they displayed EQTSP or OGBSP in phase 2.

from the introduction of the material encouragement towards IGB. The policy is ineffective. This has special relevance to nationalist policies and yields a paradox where such policies undermine their own foundations, but it has relevance for any policy that seeks to influence the in-group bias with a change in material incentives.

Second, we find evidence in support of social identification theory in our experiment: both in the explanation of the IGB and in the crowding-out of social identification through the introduction of material incentives. The latter is important because it suggests that the social preferences that arise from social identification are not always stable. In short, such preferences are not the bedrock that economists sometimes assume preferences are (or ought to be).

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ONLINE ONLY

Electronic Supplementary Material

Crowding-out the in-group bias: a nationalist policy paradox?

Appendix A. Experimental instructions

WELCOME TO THE EXPERIMENT

You are now taking part in an experiment in decision making. Your earnings depend on your decisions and/or the decisions of other participants.

It is therefore important that you read the following instructions carefully. During the experiment, you can earn through your decisions several bonus payments in tokens. Tokens will be converted to cash using the following exchange rate:

200 tokens = $1

All payments will be made privately at the end of the entire experiment. The experiment is divided into two parts. Here, we explain the first part of the experiment. Once the first part is finished, you will receive information about the second part of the experiment.

INSTRUCTIONS FOR PART 1

In the first part, all participants will perform an encoding task for 3 minutes. The task is the same for everyone. You will be presented with a set of three letters that form “words” with no specific meaning and your task will be to encode these letters by substituting them with numbers using the Table located permanently at the top of your computer screen: see below for an example.

A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	P	Q	R	S	T	U	V	W	X	Y	Z
10	15	21	20	28	3	19	16	17	4	5	9	24	26	18	27	23	2	12	30	11	7	6	25	29	14

Example: Suppose you are given the word LFA. The Table shows that L=9, F=3, and A=10. Therefore, you have to enter the number 9310 and click the "GO" button.

Once you submit a code, the computer will prompt you with another word to encode. Once you encode that word, you will be given another word and so on. **This process will continue for 3 minutes**. At the end of Part 2, you will see how many words you have encoded correctly. You will NOT be informed of the number of words coded by other participants **You will be paid a bonus of 3 tokens for each word coded correctly by you**. You will be paid this Part 1 bonus at the end of the experiment.

In addition, you will receive an endowment of tokens in Part 2 of the experiment. This endowment will be part of the bonus payments of Part 2.

EFFORT TASK FOR 3 MINUTES

ENDOWMENT SCREEN

Your endowment will be 50 tokens in Part 2. Your endowment is yours to keep and will be paid to you as a bonus.

INSTRUCTIONS FOR PART 2

<NOT IN BASELINE> Group Assignment: At the beginning of Part 2, all participants will be assigned to either a **YELLOW** group or to a **GREEN** group. This group assignment is completely random.

There are three Phases in Part 2. You will receive information on each Phase at the beginning of that Phase.

PHASE 1 INSTRUCTIONS

Phase 1: decisions

<BASELINE> You make 1 allocation decision. This decision concerns 80 tokens: you are randomly paired with another participant and you decide how many of the 80 tokens to allocate to this coparticipant. The residual goes to you.

<ALL OTHER TREATMENTS> You make 2 allocation decisions. Each decision concerns 80 tokens: you are randomly paired with another participant and you decide how many of the 80 tokens to allocate to this coparticipant. The residual goes to you. You make 2 such decisions: one for each of the two possible types of participant you might be paired with. There are 2 possible types of paired participant because they could belong to **YELLOW** or **GREEN** group.

All participants face the same decision tasks.

Phase 1: bonus payment

After you have made these decisions, your bonus payment is determined as follows.

1. You are randomly paired with another participant.

<NOT IN BASELINE> This determines the group identity of your co-player. One of your 2 decisions concerns a coplayer with this identity: call this decision YOURS. Likewise, one of your coplayer's decisions concerns how to allocate this sum when paired with someone of your identity and endowment: call this decision COPLAYER's.

2. Either YOURS or COPLAYER's allocation decision is randomly chosen for implementation.

If YOURS is chosen, you will receive as a bonus the number of tokens you allocated to yourself in this decision. If COPLAYER's is chosen, you will receive as a bonus the number of tokens they allocated to the other participant in this decision. Both you and the other participant have an equal (50%) chance of being chosen.

CONTROL QUESTION

EXAMPLE OF EARNINGS CALCULATION

Please answer the following question about earnings calculations.

There are 80 tokens to be allocated between you and another participant. You decide to allocate 30 tokens to the other participant.

Suppose YOUR decision is randomly chosen for implementation.

How many tokens would you earn as a bonus in this Phase?

PHASE 1 DECISIONS

<NOT IN BASELINE> You belong to the **YELLOW** group.

<ALL TREATMENTS> You have an endowment of 50 tokens. You have 80 tokens to allocate between you and your coparticipant.

Your coparticipant has an endowment of 50 tokens.

<BASELINE> How many tokens would you allocate to your coparticipant?

<ALL OTHER TREATMENTS> How many tokens would you allocate to your coparticipant if:

He/she belongs to the **YELLOW** group?

He/she belongs to the **GREEN** group?

<Order of decisions randomised across subjects. The order stays the same across Phases. In BASELINE, there is no mention of group and there is only one decision.>

PHASE 2 INSTRUCTIONS

Phase 2: decisions

This has the same decisions as Phase 1. You make the same 2 allocation decisions.

<ONLY IN GROUP-FINE AND GROUP-FineProEqual TREATMENTS> The difference with Phase 2 is the bonus payment.

Phase 2: bonus payment

The determination of the decision that will be implemented is the same as Phase 1.

As in Phase 1, you will be randomly paired with another participant, and either YOURS or your COPLAYER's decision will be randomly chosen for implementation.

************* BELOW TEXT ONLY IN GROUP-FINE TREATMENT ************

The Difference is that if YOURS is chosen, your bonus payment may be adjusted. Your Coplayer's bonus will not be adjusted: it is what you allocated to him or her in YOURS. The adjustment to your bonus depends on how generous you are to members of your own group as compared with members of the other group in otherwise equivalent decisions. Thus, if you have allocated the same or a lower number of tokens to someone who belongs to your group than to someone who belongs to the other group, THEN your bonus payment is reduced by 10 tokens. If you allocated more to someone from your group than the other group, there is no adjustment.

EXAMPLE: Suppose you belong to the **YELLOW** group, YOURS is selected. And your coplayer belongs to the **GREEN** group. If you allocated 30 tokens to this coplayer, and allocated 20 tokens to the coparticipant who belongs to the **YELLOW** group (even though this decision was not chosen), your bonus payment in this Phase will be reduced by 10 tokens.

Your bonus payment is NOT adjusted if COPLAYER's is chosen for implementation.

************* ABOVE TEXT ONLY IN GROUP-FINE TREATMENT ************

********* BELOW TEXT ONLY IN GROUP-FINEPROEQUAL TREATMENT *********

The Difference is that if YOURS is chosen, your bonus payment may be adjusted. Your Coplayer's bonus will not be adjusted: it is what you allocated to him or her in YOURS. The adjustment to your bonus depends on how generous you are to members of your own group as compared with members of the other group in otherwise equivalent decisions. Thus, if you have allocated a lower number of tokens to someone who belongs to the other group than to someone who belongs to your group, THEN your bonus payment is reduced by 10 tokens. If you allocated the same as or more to someone from the other group than your group, there is no adjustment.

EXAMPLE: Suppose you belong to the **YELLOW** group, YOURS is selected. And your coplayer belongs to the **YELLOW** group. If you allocated 30 tokens to this coplayer, and allocated 20 tokens to the coparticipant who belongs to the **GREEN** group (even though this decision was not chosen), your bonus payment in this Phase will be reduced by 10 tokens.

Your bonus payment is NOT adjusted if COPLAYER's is chosen for implementation.

********* ABOVE TEXT ONLY IN GROUP-FINEPROEQUAL TREATMENT *********

PHASE 2 DECISIONS

<NOT IN BASELINE> You belong to the **YELLOW** group.

<ALL TREATMENTS> You have an endowment of 50 tokens. You have 80 tokens to allocate between you and your coparticipant.

Your coparticipant has an endowment of 50 tokens.

<BASELINE> How many tokens would you allocate to your coparticipant?

<ALL OTHER TREATMENTS> How many tokens would you allocate to your coparticipant if:

He/she belongs to the **YELLOW** group?

He/she belongs to the **GREEN** group?

<Order of decisions randomised across subjects. The order stays the same across Phases. In BASELINE, there is no mention of group and there is only one decision.>

PHASE 3 INSTRUCTIONS

<u>Phase 3: decision</u>

This has the same decisions as Phase 1 and 2.

<u>Phase 3: bonus payment</u>

The bonus payment for Phase 3 is calculated in the same way as in Phase 1.

<NOT IN BASELINE AND IN GROUP>That is, your bonus payment will NOT be adjusted in any way.

PHASE 3 DECISIONS

<NOT IN BASELINE> You belong to the **YELLOW** group.

<ALL TREATMENTS>You have an endowment of 50 tokens. You have 80 tokens to allocate between you and your coparticipant.

Your coparticipant has an endowment of 50 tokens.

<BASELINE> How many tokens would you allocate to your coparticipant?

<ALL OTHER TREATMENTS> How many tokens would you allocate to your coparticipant if:

He/she belongs to the **YELLOW** group?

He/she belongs to the **GREEN** group?

<Order of decisions randomised across subjects. The order stays the same across Phases. In BASELINE, there is no mention of group and there is only one decision.>

DEMOGRAPHIC QUESTIONNAIRE

Finally, before ending the experiment, we would like to ask for some information about you. Please answer all questions honestly and accurately. Your answers will stay anonymous.

How old are you?

What is your gender?

- (Male, Female, Other/self-identify, Prefer not to say)

What is your level of education?

- (Below high school, High school, Some university education, Undergraduate degree, Master's degree, Doctorate or professional degree, Prefer not to say)

What is your current employment status?

- (Employed, Unemployed, Retired, Student, Not looking employment, Prefer not to say)

Please describe your political beliefs.

- (Very left, Left, Centre, Right, Very right)

Please describe your economic beliefs.

- (Very left, Left, Centre, Right, Very right)

In which country is your hometown?

How many economics experiments have you participated in before?

FINAL COMPLETION SCREEN

Thank you for participating in our experiment!

The total number of "words" you encoded correctly in Part 1 is ____. After all participants have made their decision, we will let you know your earnings in Part 2 and pay you the bonuses from Part 1 and 2 through Prolific.

To end the experiment, please click the completion link below that will take you back to Prolific:

Appendix B. Analysis of ratio of dictator allocations to co-players

In this appendix we will consider the ratio *CP(own)/CP(other)* in place of the difference $CP(own) - CP(other)$ as our measure of IGB. This is a robustness test for our results and a different way of testing our hypotheses 2, 3 and 5. The downside of this alternative analyses (and the reason why we put it in the appendix) is that we lose subjects who do not donate anything to co-participants from the other group either in phase 1 or 2. Ultimately, we lose 6 observations in treatment Group and 7 observations in treatment Group-Fine.

Figure B1. Aggregate change in IGB ratio with 95% confidence intervals

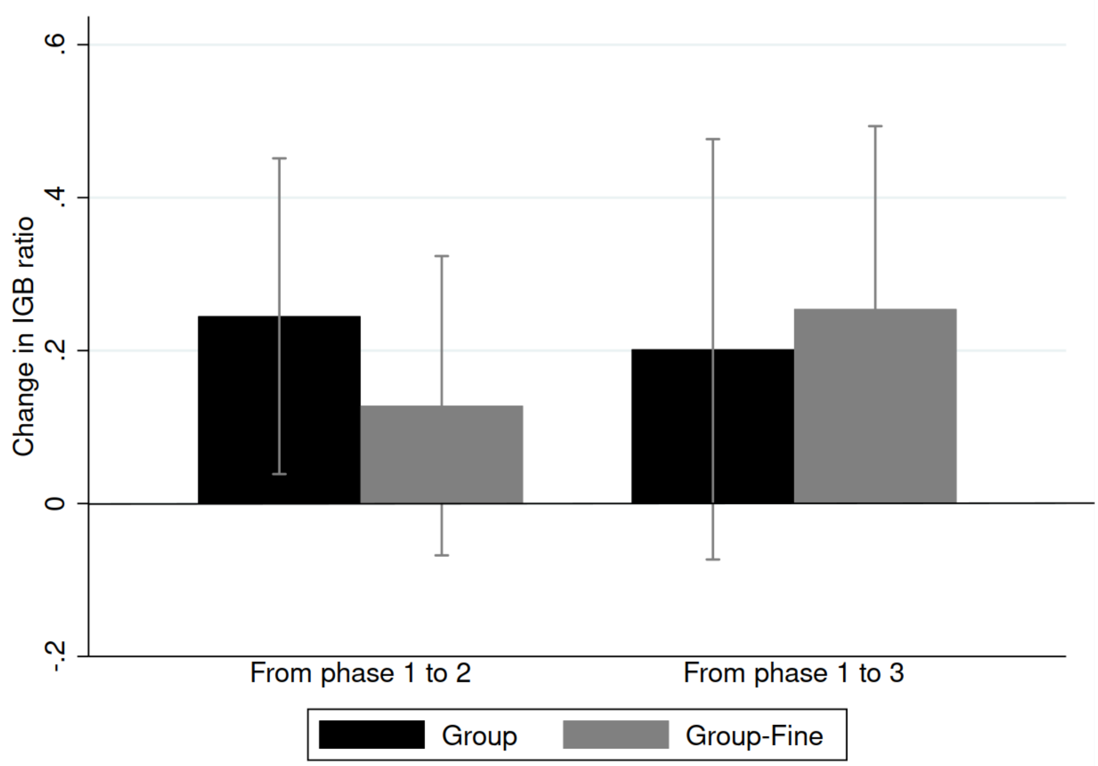


Figure B1 presents the average aggregate change in IGB by treatment along with 95% confidence intervals to try to reproduce Result 2. The average change in ratios (0.24 vs. 0.13) goes again in the direction of crowding-out at the aggregate level. However, the difference between the treatments is not statistically significant (p = 0.9240).

With respect to the individual crowding-out Result 3, the top panel of Table B1 reports the magnitude of *CP(own)/CP(other)* of those who revealed such a bias in phase 1 in the Group and Group-Fine treatments and also how this group of subjects' IGB evolves in phases 2 and 3. We compare the change in *CP(own)/CP(other)* between phase 1 and 2 in Group and Group-Fine for this group of subjects. Even when considering the ratios, the change in IGB in Group-Fine is significantly less than the change in Group (p = 0.0399).

Table B1. Mean change in CP(own)/CP(other) conditional on level of favouritism in Phase 1

		Group favouritism (CP(own)/CP(other))			Change in group favouritism	
					From Phase	
	Obs.	Phase 1	Phase 2	Phase 3	1 to 2	1 to 3
Ingroup favouritism (IGB)						
Group	27	1.72	2.15	1.68	0.43	-0.11
		(0.53)	(1.32)	(1.29)	(1.26)	(1.27)
Group-Fine	19	2.10	1.92	2.49	-0.11	0.39
		(0.90)	(1.51)	(1.96)	(1.42)	(1.77)
Equal allocations (EQT)						
Group	34	1.00	1.09	1.24	0.09	0.24
		(0.00)	(0.23)	(1.05)	(0.23)	(1.05)
Group-Fine	43	1.00	1.20	1.11	0.20	0.11
		(0.00)	(0.49)	(0.38)	(0.49)	(0.38)

Lastly, further evidence in support of Result 5 is that the subjects who revealed EQTSP in phase 1 in the Group-Fine treatment move to a ratio *CP(own)/CP(other)* of 1.548 in phase 2. They donate roughly 50% more to a co-participant of the same group than to a co-participant from the other group. This is significantly more than just a marginal 10% increase ($p = 0.0022$) and it is not far from the level of favouritism (1.88) of subjects who revealed IGBSP already in phase 1 in the Group treatments.

Appendix C. Complementary experiment: Group-FineProEqual

We present here the results from the complementary experiment on crowding-out equal allocation preferences. The only difference with respect to the main experiment is in the Group-Fine treatment. In the Group-FineProEqual treatment adjustments to earnings in phase 2 are exactly the opposite of those in ID-Fine. Earnings were reduced by 10 tokens if a dictator's decision was chosen as the relevant one for payment and if he/she had allocated more to a recipient who belonged to their own group. Here, equal allocations were *not* penalised. Phase 1 and Phase 3 were the same as in Group. A total of 79 (41 Yellow and 38 Green) participated in this treatment. For comparison, we present results from the Baseline, Group and Group-FineProEqual.

Table C1. Mean dictator allocations

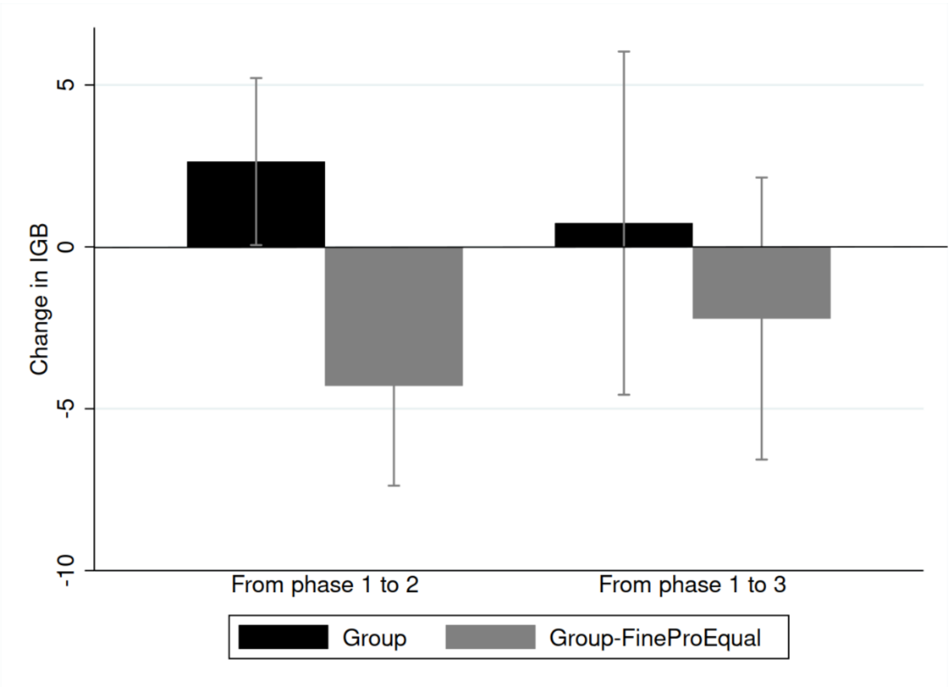
		Recipient's group ID					
		Phase 1		Phase 2		Phase 3	
	Obs.	Own	Other	Own	Other	Own	Other
Baseline	38	29.47		30.39		29.61	
		(13.74)		(15.74)		(16.54)	
Group	71	37.75	28.83	40.00	28.45	38.87	29.23
		(15.30)	(15.02)	(15.17)	(15.06)	(17.51)	(16.64)
Group-FineProEqual	79	36.52	29.11	34.18	31.06	35.76	30.57
		(10.87)	(12.85)	(10.81)	(12.78)	(12.43)	(15.36)

Figures in parentheses are standard deviations. Dictators in recipients in the Baseline do not have a group identity. All participants in the above treatments have an endowment of 50 tokens each. The size of the pie the dictator splits is 80 tokens in all cases.

Table C1 gives the aggregate dictator allocation to their co-player in our baseline where there are no group affiliations, in the Group and Group-FineProEqual treatments with group affiliations for each of the 3 phases. Focusing on phase 1, allocations to co-participants from the same group in Group-FineProEqual are higher than allocations to co-participants from the other group ($p < 0.00001$). This reproduces exactly the same evidence as for the Group treatment. Given that in phase 1 the two treatments are the same, there was no reason to expect otherwise. Consistent with social identification theory, *CP(own)* is higher than *CP* in the baseline ($p = 0.0082$) while CP(other) is not ($p = 0.7628$). Therefore, we reproduce Result 1 in all details.

We turn now to the crowding-out/in hypotheses. There is a technical issue related to testing the hypotheses in this treatment with respect to the Group-Fine treatment. To observe crowding-out in Group-FineProEqual, we would need that subjects who reveal either EQTSP or OGBSP in phase 1 substantially move to revealing IGB in phase 2. This leaves little space for proper testing, because it implies a shift across types, and not just a change in donations. Furthermore, there are only two dictators who display OGBSP in phase 1 in Group-FineProEqual. Hence there isn't a way to statistically test for an adjustment in favour of the in-groups for these subjects. Nevertheless, the left part of Figure C1 shows the average change in IGB between phase 1 and phase 2 in the two Group treatments, along with the 95% confidence intervals. This time the average change between phase 1 and 2 is quite different among treatments, as pointed out in footnote 10 of the main text. When there is a fine on IGB, subjects lower their favouritism by 4.29 on average, while they increase it by 2.63 in the Group treatment (no fine). This difference is strongly significant ($p = 0.0038$). So, at the aggregate level we cannot conclude that there was no crowding-out altogether, but it is certainly weaker than for a fine on the opposite behaviour.

Figure C1. Aggregate change in IGB with 95% confidence intervals



With respect to the individual crowding-out hypothesis H3, the bottom panel of Table C2 reports the magnitude of the IGB, $CP(own) - CP(other)$, of those who revealed EQTSP in phase 1 in the Group and Group-FineProEqual treatments and also how this group of subjects' IGB evolves in phases 2 and 3. The change in IGB is, on average, higher in Group than Group-

FineProEqual, which goes to say that we do not observe the substantial move to IGB that would support crowding-out. Furthermore, by ranksum tests, the difference is not significant (p = 0.4497).

Table C2. Mean change in favouritism conditional on level of favouritism in Phase 1

		Group favouritism			Change in group favouritism		% Change in group favouritism	
					From Phase		From Phase	
	Obs.	Phase 1	Phase 2	Phase 3	1 to 2	1 to 3	1 to 2	1 to 3
				In-group favouritism (IGB)				
Group	32	22.28	24.53	17.97	2.25	-4.31	0.27	0.09
		(18.01)	(18.59)	(31.13)	(14.08)	(29.84)	(0.91)	(1.89)
Group-FineProEqual	39	16.03	7.05	6.54	-8.97	-9.49	-0.55	-0.53
		(8.13)	(14.04)	(17.06)	(14.52)	(18.31)	(1.27)	(1.78)
				Equal allocations (EQT)				
Group	35	0	1.86	3.43	1.86	3.43	-	-
		(0)	(5.16)	(12.59)	(5.16)	(12.59)		
Group-FineProEqual	45	0	-0.76	4.08	-0.76	4.08	-	-
		(0)	(10.17)	(18.08)	(10.17)	(18.08)		

Figures in parentheses are standard deviations

To test H5, we consider subjects who revealed IGBSP in phase 1 and adjusted to the fine in phase 2. In this treatment, we cannot test crowding-in of the equal allocation preferences by testing the magnitude of the shift in phase 2. This is because it is sufficient to reveal EQB in phase 2 to avoid the fine. What we can do to test crowding-in is observe how many of these subjects stick to EQB in phase 3. The average IGB of these subjects moves from 15.91 in phase 1 to 3.40 in phase 3. A sign rank test between phases supports that subjects who display IGB in phase 1 and EQB in phase 2 are still less prone to IGB in phase 3 than in phase 1 (p = 0.0005). Therefore, we can conclude that the equivalent of Result 4 is replicated in Group-FineProEqual.

Appendix D. Demographic characteristics of participants

Table D1 summarises the demographic characteristics of participants in all four treatments.

Table D1. Demographic characteristics of study participants (N = 266)

	Mean	St. dev.	Median	Min.	Max.
Age	26.78	8.99	24	18	72
Economics	2.92	0.83	3	1	5
Politics	2.71	0.82	3	1	5
Employment	2.59	1.54	2	1	6
Education	3.54	1.3	4	1	7
Gender	60.15% Male		38.72% Female		1.13% Other

Economics & Politics: 1 = Very left, 2 = Left, 3 = Centre, 4 = Right, 5 = Very right
Employment: 1 = Employed, 2 = Unemployed, 3 = Retired, 4 = Student, 5 = Not looking employment, 6 = Prefer not to say
Education: 1 = Below high school, 2 = High school, 3 = Some university education, 4 = Undergraduate degree, 5 = Master's degree, 6 = Doctorate or professional degree, 7 = Prefer not to say

The means for education and employment give a misleading picture. It appears that the average participant is unemployed and not terribly well educated. The histograms below give a more accurate picture.

Figure D1. Distribution of employment status and education level