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Inequality in the minimal group paradigm: How relative wealth and its justification influence ingroup bias[☆]

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ABSTRACT

This article details a registered report for a well-powered ($N = 1500$) experiment examining the influence of wealth inequality between groups on ingroup bias, as well as the potential moderating role of justification for the wealth distribution. Using the Minimal Group Paradigm, in which participants are assigned to groups with anonymous others and asked to allocate resources to ingroup or outgroup members, we randomly assigned participants to a relatively disadvantaged or a relatively advantaged group. Group assignments were ostensibly based on chance (weak justification), performance on a financial decision-making task (strong justification), or an ambiguous combination of the two (ambiguous justification). As expected, we found evidence for an *inequity aversion* hypothesis, with disadvantaged participants displaying heightened ingroup bias compared to their advantaged counterparts. Interestingly, however, our predictions regarding the moderating role of justification were not supported, with disadvantaged participants displaying the highest ingroup bias when the inequality was ambiguously justified. We discuss implications of these results for understanding the causal factors underlying ingroup bias.

1. Stage 1

Large wealth disparities exist between socioeconomic groups in all modern societies. Understanding the psychological dynamics that exist between groups marked by asymmetrical economic relationships has thus been a major interest within the social sciences for the last 70 years (e.g., Allport, 1954; Blumer, 1958; Bobo & Hutchings, 1996; Massey & Denton, 1993), and this area of study has gained further momentum more recently in the context of the meteoric rise in wealth inequality in the United States and globally (Jetten et al., 2017; Kteily, Sheehy-Skeffington, & Ho, 2017; Richeson & Sommers, 2016; Smith, Pettigrew, Pippin, & Bialosiewicz, 2012) . However, significant questions remain. Though it is clear that members of groups often behave in ways that privilege their own group over others, the conditions under which groups relatively high versus low in wealth display increased ingroup bias—specifically, how perceptions of the underlying reasons for wealth disparities factor into these processes—remain poorly understood.

Drawing from research on Social Identity Theory (Tajfel & Turner, 1986), relative deprivation and gratification (e.g., Guimond & Dambrun, 2002; Smith et al., 2012; Walker & Smith, 2002), and inequity aversion (e.g., Dawes, Fowler, Johnson, McElreath, & Smirnov, 2007; Fehr & Schmidt, 1999), we aim to conduct a well-powered, pre-registered experiment utilizing the Minimal Group Paradigm (MGP; Tajfel, Billig, Bundy, & Flament, 1971) to examine the causal effect of relative resource wealth on ingroup bias, as well as the moderating role of perceived justification of the wealth discrepancy. In this registered report, we detail our theoretical rationale, hypotheses, methodology, and analysis plan for this proposed study.

1.1. Relative group wealth and ingroup bias

How, if at all, does relative advantage or disadvantage affect ingroup bias? One line of thinking, informed by the literature on *inequity aversion*, suggests that groups with lower relative wealth will display greater ingroup bias than those with higher relative wealth, as people tend to show resistance to incidental inequality and act in ways that promote fairness (Dawes et al., 2007; Fehr & Schmidt, 1999). This hypothesis is borne out by research highlighting how relative deprivation—a group's perception that it is economically deprived in comparison to a particular standard or relevant outgroup (Walker & Smith, 2002) can promote ingroup bias and other pernicious intergroup behaviors due to feelings of resentment and hostility regarding one's

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diminished status, both in the context of real and minimal groups (Durrheim, Quayle, Tredoux, Titlestad, & Tooke, 2016; Harvey & Bourhis, 2012; Rubin, Badea, & Jetten, 2014; Smith et al., 2012).

However, a counterpoint is provided by the *V-Curve* hypothesis (Guimond & Dambrun, 2002), which postulates that both relatively disadvantaged *and* advantaged groups may exhibit discriminatory tendencies (e.g., Dambrun, Taylor, McDonald, Crush, & Méot, 2006; Jetten, Mols, & Postmes, 2015; Moscatelli, Albarello, Prati, & Rubini, 2014). This theorizing echoes the reasoning described above regarding why groups lower in wealth may display elevated ingroup bias, but additionally suggests that groups higher in wealth should exhibit similar levels of bias, as a result of feelings of entitlement and deservingness (e.g., Côté, House, & Willer, 2015; Gee, Migueis, & Parsa, 2017). This latter notion is corroborated by research documenting that wealthy groups are typically more supportive of unequal economic distributions and thus engage in behaviors designed to further the ingroup's dominant status (e.g., Brown-Iannuzzi, Lundberg, Kay, & Payne, 2014; Dawtry, Sutton, & Sibley, 2015).

1.2. The moderating effect of justification for wealth disparities

Whether the poor or the wealthy are more likely to show ingroup bias in response to their relative inequality is likely to depend on the extent to which the income distribution is perceived as justified. Consistent with the principles of inequity aversion, past research has found that while relatively disadvantaged groups tend to exhibit greater ingroup bias when inequality between groups is weakly justified (e.g., when inequality between groups is not provided any justification, said to be based on chance, or is inversely proportional to groups' task performance; Harvey & Bourhis, 2012; Rubin et al., 2014; Sachdev & Bourhis, 1991), advantaged groups tend to exhibit greater ingroup bias when the inequality between groups is more strongly justified (e.g., when the inequality is tied to some ostensibly relevant between-group difference in abilities or task performance; Bettencourt, Charlton, Dorr, & Hume, 2001; Harvey & Bourhis, 2013; Rubini, Moscatelli, Albarello, & Palmonari, 2007). This suggests that people may be more likely to believe the poor deserve to stay poor and the rich deserve to stay rich when such outcomes are perceived to result from equitable processes, a prediction supported by research demonstrating that perceiving the economic system to be more just is associated with a reduced likelihood of acting to reduce inequality (e.g., Chambers, Swan, & Heesacker, 2015; Day & Fiske, 2017; Starmans, Sheskin, & Bloom, 2017).

However, while this research suggests some broad conclusions about the interactive influence of group inequality and distributive fairness in determining ingroup bias, it also possesses a glaring limitation—in all of the relevant studies, group wealth disparities were framed as either unambiguously justified (e.g., based on performance differences) or unambiguously unjustified (e.g., based on chance). Yet real-world wealth disparities between individuals and groups are seldom accompanied by such explicit justifications, or lack thereof. Most people would agree that their relative economic success is the result of some combination of internal factors (e.g., hard work, sacrifice, intellect) and external factors (e.g., structural barriers, un-earned privileges, luck), and most people also see economic attainment based on internal characteristics as being relatively more justified, and attainment based on external characteristics as less justified. However, few people have a way of precisely quantifying the respective contribution of each factor.

This creates ambiguously justified inequality within societies. Individuals are assigned at birth into groups with unequal access to resources, but are blind to the precise influence of the various factors contributing to those inequalities. Nonetheless, people do form beliefs about these relative contributions, and come to very different conclusions on this question (e.g., Chambers et al., 2015; Jost, Banaji, & Nosek, 2004; Kluegel & Smith, 1986; Kraus, Piff, & Keltner, 2009; Newman, Johnston, & Lown, 2015). Evidence suggests that these conclusions are often likely to be self-serving—wealthier individuals are more likely to attribute economic outcomes to internal factors, thereby maximizing emphasis on personal responsibility for their position, whereas poorer individuals are more likely to accentuate external influences outside of personal control (Kraus et al., 2009). When participants played an investment game in which they were led to believe that outcomes depended on some combination of sound decision-making and luck, and were randomly assigned to either succeed or fail, those who succeeded perceived the game to be fairer than those who failed (Brown-Iannuzzi et al., 2014).

This suggests that when faced with ambiguously justified inequality, individuals in relatively advantaged groups may respond with heightened ingroup bias due to perceiving the inequality as completely justified, and individuals in relatively disadvantaged groups may also respond with heightened ingroup bias due to perceiving the inequality as completely unjustified. To date, this hypothesis has not been tested.

In the present experiment, we will take steps to fill this gap in the extant literature. In addition to measuring ingroup bias in the context of highly justified (ostensibly performance-based) and unjustified (ostensibly random) between-group inequality, we will include a condition in which the level of justification is ambiguous. This feature can hopefully help address the critical empirical question of how economically asymmetrical groups respond to uncertain information regarding the cause of the income distribution.

1.3. The present research

We will test how relative wealth between groups impacts ingroup bias under minimal conditions, and how the justification provided for the inequality moderates this effect, in the context of a 2 (initial group wealth allocation: low or high) × 3 (justification: weak, ambiguous, or strong) between-subjects experiment, measuring ingroup bias as the proportion of wealth participants award to ingroup members compared to outgroup members. We will test between various hypotheses predicting distinct patterns of results within this design. With regard to the main effect of initial group wealth allocation, we will test between two competing predictions:

The Inequity Aversion hypothesis: The low wealth group will display greater ingroup bias than the high wealth group.

The V-Curve hypothesis: The low and high wealth groups will display relatively equal levels of ingroup bias.

In exploring the moderating role of perceived justification of the initial wealth allocations, we will also test between competing hypotheses:

The Linear hypothesis: An extension of the Inequity Aversion hypothesis, this predicts that, among the low wealth group, ingroup bias will increase in a linear fashion as the inequality is less justified, and will be intermediate under the partially justified inequality of the ambiguous condition. By contrast, among the high wealth group, ingroup bias will increase in a linear fashion as the inequality is more justified, and will again will be intermediate under the partially justified inequality of the ambiguous condition.

The Motivated Interpretation of Ambiguity hypothesis (MIA): Similar to the Linear hypothesis, this hypothesis predicts that ingroup bias will increase among the low-wealth group when inequality is weakly justified and among the high-wealth group when inequality is strongly justified. However, based on evidence that individuals respond to ambiguously justified inequalities in self-serving ways, this hypothesis predicts that individuals in the high wealth group will respond to ambiguously justified inequality as if the inequality were strongly justified, and will therefore show similar levels of ingroup bias compared with the strong justification condition, while the low wealth group will respond to ambiguously justified inequality as if the inequality were weakly justified, and will therefore show similar levels of ingroup bias compared with the weak justification condition.

1.4. The minimal group paradigm

In line with previous studies exploring the impact of resource inequalities on egalitarian and discriminatory behaviors between groups (e.g., Dambrun et al., 2006; Harvey & Bourhis, 2012), the present study will use the Minimal Group Paradigm (MGP). In MGP studies, members of two groups, artificially created based on some arbitrary variable (e.g., over- versus under-estimators in an estimation task), allocate resources to anonymous ingroup and outgroup members (Tajfel et al., 1971). By examining ingroup bias in the context of such groups who have no actual social interaction within or between themselves, the MGP affords a tightly-controlled, validated experimental protocol in which to observe group-level behavior detached from the historical and cultural complications of real intergroup relations. Studies using the MGP have shown that arbitrary group divisions are sufficient to trigger discriminatory behavior relevant to realistic group conflict, most pertinently in the form of favoring one's own group in resource allocations (Diehl, 1990; Hewstone, Rubin, & Willis, 2002; Hogg & Abrams, 1988), but also in trait inferences of ingroup and outgroup members (Otten & Moskowitz, 2000; Rubin, Paolini, & Crisp, 2010) and punishment for wrongdoing (Chen & Li, 2009).

Importantly, the MGP has proven advantageous for examining the influence of socio-structural variables believed to play significant roles in real group conflicts in society, including people's perceptions of where they lie in the status hierarchy, and their impressions of the permeability, stability, and legitimacy of that hierarchy (see (Hornsey, 2008) for review). Furthermore, patterns of responses are typically moderated by the extent to which group members identify with that group, precisely in the way assumed by Social Identity Theory and in line with findings from real groups (e.g., Castano, Yzerbyt, Bourguignon, & Seron, 2002; Leach, 2008). This intimates that, though the MGP is unlikely to fully capture the cultural and historical complexities of real intergroup relations, it does afford us a highly controlled way of testing a set of causal predictions drawn from particular theoretical explanations of ingroup bias that are nonetheless relevant to structural (i.e., group wealth asymmetries) and psychological (i.e., perceptions of fairness) factors present in real-life group dynamics. Of course, we do not wish to intimate that the factors we are attempting to investigate are the only factors, or even the most important factors, underlying the ingroup bias of social groups, which is undoubtedly a complex and multiply-determined construct, and sensitive to many of the historical and socio-cultural variables the MGP explicitly attempts to remove (e.g., Allport, 1954; Bobo & Hutchings, 1996). Rather, the guiding premise of the present study is only that these factors are likely to play a role in shaping individuals' ingroup bias, and that by stripping away the other factors via the MGP, we can better understand that role.

2. Method

2.1. Participants

Participants will be recruited from Amazon's Mechanical Turk (MTurk), and required to be US Citizens, over the age of 18, and fluent in English. We will recruit participants until we obtain 1500 valid responses, defined by correctly answering three comprehension checks within the experiment.

2.2. Procedure

All planned measures, manipulations and exclusions are disclosed below. We will use Breadboard—a software platform for conducting online human interaction studies with real, anonymous participants (McKnight & Christakis, 2016) to facilitate the experiment. Participants will be recruited in groups of six and split randomly into a low-wealth and a high-wealth group. Participants will also be randomly assigned to one of three justification conditions involving different explanations for the between-group inequality.

Upon beginning the experiment, participants will be told that they will complete two tasks: an Investment Task designed to measure their financial decision-making abilities, and an Allocation Task in which they will make decisions about allocating rewards between pairs of fellow participants.

2.2.1. Investment task

Participants will complete an Investment Task modified from past research (Brown-Iannuzzi et al., 2014). In the task, participants will make three investment decisions regarding how much out of $1000 in seed money to invest between two companies. Paragraphs describing the companies, as well as information about each company's past stock performance and price-earnings ratio, will be provided. This task has two main features that make it suitable for the current study. First, there has been little evidence of any skepticism on the part of participants toward either positive or negative feedback regarding task performance when the task was used in past research (Brown-Iannuzzi et al., 2014). Second, the task ostensibly measures a skill relevant to real-world socioeconomic outcomes. Just as socioeconomically privileged individuals may tend to ascribe their relative status to their own ability and choices (e.g., Kraus et al., 2009), individuals within our experiment will also be able to rationalize their relative advantage in terms of individual differences in decision-making abilities perceived as relevant to real-world inequality. In this respect, the task is similar to that used by Harvey and Bourhis (2013), who ostensibly measured participants' inclination to "make more personal effort to reach their occupational goals" as a means of dividing participants into low- and high-status groups. After completing the Investment Task, participants will be told that 6 months of stock-market activity will be "simulated on the basis of real-world performance."

2.2.2. Justification manipulation

Participants in the strong justification condition will be told that their assignment to the low- or high-wealth group is completely determined by their performance in the Investment Task. Participants in the weak justification condition will not receive any feedback on their task performance and will be told that their assignment to the low- or high-wealth group is determined completely randomly. Participants in the ambiguous justification condition will be told that their assignment to the high or low wealth group was partially the result of their performance in the Investment Task, and partially decided randomly.

2.2.3. Allocation task

During the study, participants will be represented by a circular node on the left of their monitors containing a number from 1 to 6. Nodes will initially appear as grey, but following the Investment Task, participants will be placed in the 'Red Group' or the 'Blue Group', and nodes will become the color of participants' groups. Colors will be randomly assigned such that either red or blue may represent the high or low wealth groups. At this point participants will also see the initial wealth of their ingroup and outgroup and reminded of the justification for the inequality (see the top panel of Fig. 1). The low wealth group will begin with 500 points each (equal to $1), and the high wealth group will begin with 2000 points each (equal to $4).

Following this, participants will perform 20 rounds of the Allocation Task. In each round, participants will choose between 13 options representing different allocations of points to distribute to each player (see the bottom panel of Fig. 1). These options will be taken from the four original matrices used in Minimal Groups research (Tajfel, 1970). Participants will be able to view their point total, as well as the scores of all other players, at all stages of the allocation task.

2.2.4. Quantifying ingroup bias

Traditionally in Minimal Group experiments, six 'pull scores' were computed for each participant to measure the relative strength of

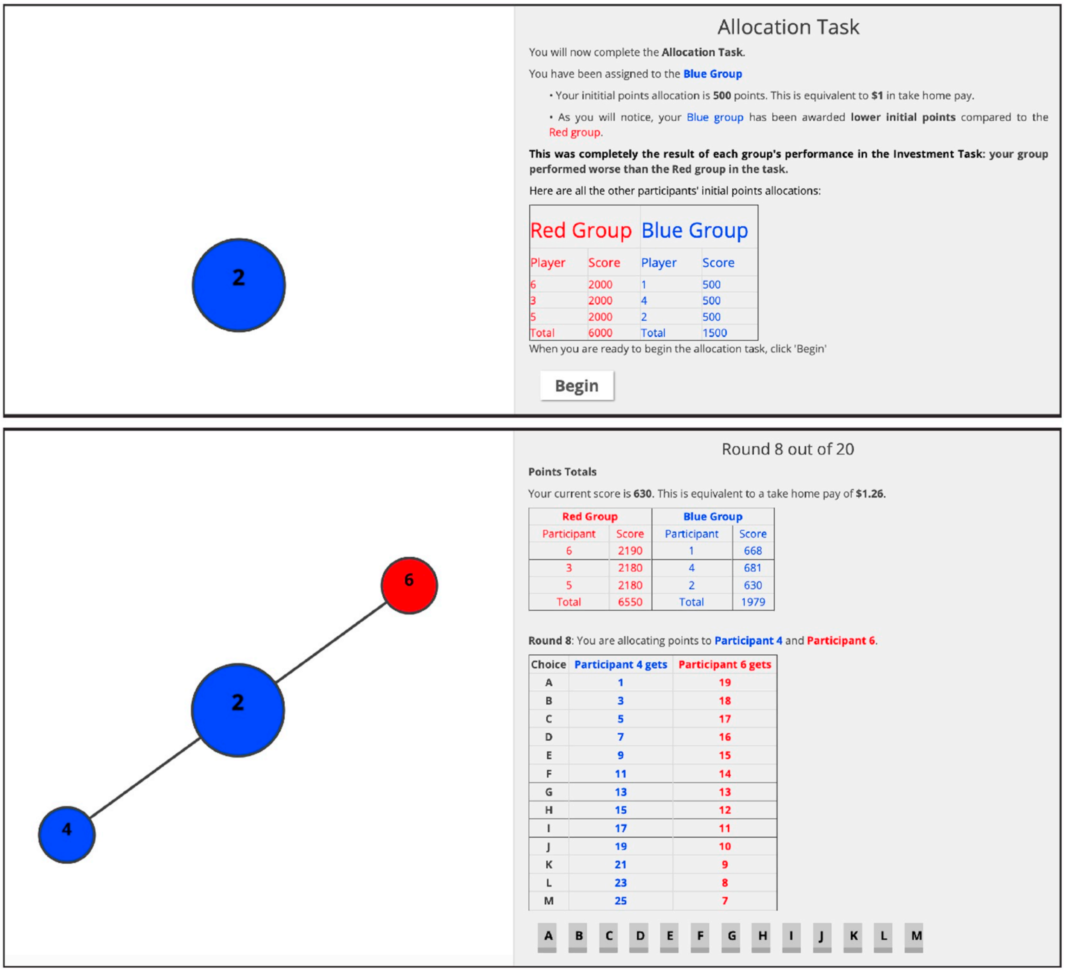

Fig. 1. The screen showing participants' initial wealth assignment and reminding them of the justification for this assignment (top), and an example allocation round (bottom).

specific intergroup motivations (e.g., the motivation to maximize ingroup profit vs. the motivation to maximize the difference between the ingroup and the outgroup; Turner, 1983). However, recent investigations have sought to derive single overall scores representing ingroup bias (e.g., Amiot & Bourhis, 2005). In line with this trend, our measure of ingroup bias will be the total proportion of points awarded to ingroup members in allocation choices between an ingroup and an outgroup member. This measure is simple, intuitive, and meaningful—it is a direct measure of how much more participants choose to help their ingroup compared to the outgroup—and correlates strongly with both the 'Maximum Dominance' pull score traditionally seen as the key component of ingroup bias in Minimal Group tasks (Turner, 1983), as well as more recent measures (Amiot & Bourhis, 2005).

2.2.5. Attrition

If participants drop out of the experiment, or if less than six participants are recruited to fill the task, the missing participants will be replaced by bots who will make allocation choices randomly. Participants are unlikely to discern a difference between participating with humans or bots, because they will not be aware of who is allocating points to them; even if noticeably large or small gains occur in any particular round, this should be rationalizable as potentially resulting from the choices of ingroup or outgroup members.

2.2.6. Manipulation checks

After completing 20 rounds of allocations, we will administer two manipulation checks to assess participants' subjective feelings of relative resources within the experiment and how unfair they considered the initial wealth inequality to be. First, depending on condition, participants will be asked "compared to the Red/Blue Group, did you feel like your resources were relatively scarce or relatively abundant?" with responses measured via a slider from 1 = "Relatively scarce" to 100 = "Relatively abundant". We will then ask "when you think about how points were distributed between the Blue group and the Red group, how fair do you think that it was?" with responses measured via a slider from 1 = "Completely unfair" to 100 = "Completely fair".

2.2.7. Comprehension checks/Exclusion Criteria

Participants will complete three comprehension checks. First, after being told their group-specific justification for the between-group wealth inequality, they will be asked to recall the justification. Second, after receiving instructions regarding the Allocation Task, they will be asked to demonstrate comprehension of the task by selecting the implications of a particular choice ("Fill the blanks: In the example below, if you choose option E, Participant 5 will receive __ points, Participant 4 will receive _ points"). Finally, following the Allocation Task, participants will be asked to recall if their group had greater, less, or equal resources compared to the opposing group. Each comprehension check will be posed as a multiple-choice question. Participants who fail any of these checks will be excluded from analyses. Participants will also complete an open-ended suspicion probe in which they are asked if there was anything that they did not believe about the experiment. Any participant who explicitly states that they did not believe either (a) that groups were assigned on the basis of justification instructions or (b) that they were participating with other real participants will also be excluded from analyses. Two independent coders will rate these responses to determine if there is agreement as to whether or not participants should be excluded.

2.3. Analysis plan

2.3.1. Manipulation checks

First, we will use three independent groups *t*-tests to ensure that, (a) participants in the high wealth group perceive their resources to be more abundant than participants in the low wealth group, (b) participants in the strong justification condition perceive the wealth inequality to be more equitable than participants in the ambiguous justification condition, who in turn (c) perceive the wealth inequality to be more equitable than participants in the weak justification condition. We consider these results likely to hold, but in the event that they do not, we will substantially qualify our interpretations of any results we obtain. Specifically, if test (a) fails, we will not interpret any set of results as conclusively supporting or failing to support either the *Inequity Aversion* or *V-Curve* hypotheses, as these hypotheses rely upon participants perceiving relative wealth differences. And if tests (b) or (c) fail, we will not interpret any set of results as conclusively supporting or failing to support either the *Linear* or *MIA* hypotheses, as these hypotheses rely upon participants perceiving the inequality justification to be relatively weak, ambiguous, and strong, across the corresponding conditions.

2.3.2. Primary analyses

Our primary analyses will use a set of planned contrasts to test whether results align significantly better (a) with the *Inequity Aversion* or *V-Curve* hypotheses regarding the effect of relative wealth, and (b) with the *Linear* or *MIA* hypotheses regarding the moderating effect of justification. Planned contrasts are ideal for the present project due to the complexity of our design and our goal of comparing multiple hypotheses. Within a 2 × 3 design, any predicted pattern of results implies specification of 15 distinct pairwise relationships. While it is possible to run all 15 pairwise comparisons and ensure that each is as predicted, doing so is inefficient, and tends to inflate either Type 1 errors (if multiple comparisons are not corrected for) or Type 2 errors (if multiple comparisons are corrected for, and power is reduced). Planned contrasts provide a means of testing whether data are significantly more compatible with one specific predicted pattern of results compared to another, and enable researchers to make and test specific and complex predictions while maintaining high power and controlling Type-1 error rates (see Furr & Rosenthal, 2003; Rosenthal & Rosnow, 1985).

To test between competing hypotheses using planned contrasts, researchers must specify at least two potential patterns of results. In our case, we have specified four potential patterns based on the theoretical discussion above. These patterns, depicted in Fig. 2, comprise each combination of the *Inequity Aversion* and *V-Curve* effects of relative wealth, and the *Linear* or *MIA* interaction patterns. For example, pattern A shows results consistent with the *Inequity Aversion* and *Linear* hypotheses: the low wealth group is generally more biased than the high wealth group, but this difference is especially pronounced when the justification is weaker, with the gap reducing in a linear fashion as justification increases in the ambiguous and then the strong justification conditions. Pattern D shows results consistent with the *V-Curve* and *MIA* hypotheses: the low and high wealth groups are equally biased, and responses show evidence of motivated interpretations of ambiguity, with levels of ingroup bias identical under weak and ambiguous justification for the low wealth group, and identical under strong and ambiguous justification for the high wealth group.

These patterns do not represent an exhaustive account of all possible results within our experiment: a 2 × 3 design can literally produce thousands of patterns of six means, each pair of which can be equal, or differ in either direction. Instead, they are an attempt to formalize what we consider the central predictions suggested by past theory and data, while keeping the number of predicted patterns sufficiently low to maintain Type 1 error control and adequate statistical power.

Once predicted patterns are specified, contrast weights must be chosen that (a) match predicted patterns of results and (b) sum to zero (Furr & Rosenthal, 2003; Rosenthal & Rosnow, 1985). The absolute magnitude of weights is arbitrary, but for simplicity, we chose for each pattern the smallest integers that would meet criteria (a) and (b). We also took the simplest possible approach to arranging relative cell means within each pattern. For example, pattern B (*Inequity Aversion + MIA*) predicts the low wealth group to show greater bias under ambiguous justification than strong justification, and the high wealth group to show greater bias under ambiguous justification than under weak justification. In the absence of theories strong enough to specify exactly what these differences will be, we have projected them to be equal (an absolute difference of 6, though this number, as mentioned above, is arbitrary). This is the case for all directional shifts between cell means in all the patterns. The contrast weights for patterns A, B, C, and D are depicted above each bar in Fig. 2 and reported in Table 1.

The contrast weights for patterns A, B, C, and D represent directional predictions, so will be evaluated via one-tailed *t*-tests, according to the procedure outlined in Furr and Rosenthal (2003). However, these contrasts are insufficient to decide between competing hypotheses. To determine if one pattern is supported significantly more than another, it is necessary to compute a new set of weights representing the difference between the two patterns. This new set of weights is evaluated via a two-tailed t-test, as the difference predictions are non-directional. We will test each of the six pairwise comparisons between patterns A through D; these difference weights are listed to two decimal places in Table 1. Thus, in total we will perform 10 t-tests. To control Type-1 error rate, we will adjust *p* values for 10 comparisons using the Benjamini-Hochberg procedure (Benjamini & Hochberg, 1995).

It should also be noted that planned contrasts are agnostic to the absolute values of cell means, so will not provide a test of whether specific conditions allocated more or < 50% of wealth to ingroup members. This is because our experiment is aimed at understanding causal factors that influence the ingroup bias of real groups, not at understanding the true absolute levels of ingroup bias of real groups affected by complex socio-historical contexts, a task we consider much better suited to observational studies. If, for example, our high wealth groups were to show significant outgroup bias by allocating < 50% of wealth to their ingroup, we would not consider this evidence that members of high wealth groups in societies affected by complex socio-cultural and historical factors will similarly tend to show outgroup bias. However, if we find the high wealth group to show relatively greater ingroup bias under ambiguous and strong justifications, we will consider this to be evidence that, ceteris paribus, the perceived justification of wealth inequalities is likely to moderate real-world ingroup bias among high wealth groups.

2.3.3. Inference decision rules

To decide if our data supports either the *Inequity Aversion* or *V-Curve* hypothesis and either the *Linear* or the *MIA* hypothesis, we will use the following decision rules (also outlined in Table 2). First, whichever pattern of A, B, C, and D returns the highest *t* value will be deemed to be best supported by the data. If the contrast for this pattern is significant at an alpha level of 0.05, we will examine the difference contrasts between it and the two patterns representing the alternate hypothesis about the effect of relative wealth. For example, if pattern A is best supported (an *Inequity Aversion* pattern), we will examine the A-C and A-D differences (the differences between pattern A and the two *V-Curve* patterns), and if both difference contrasts are significant will consider

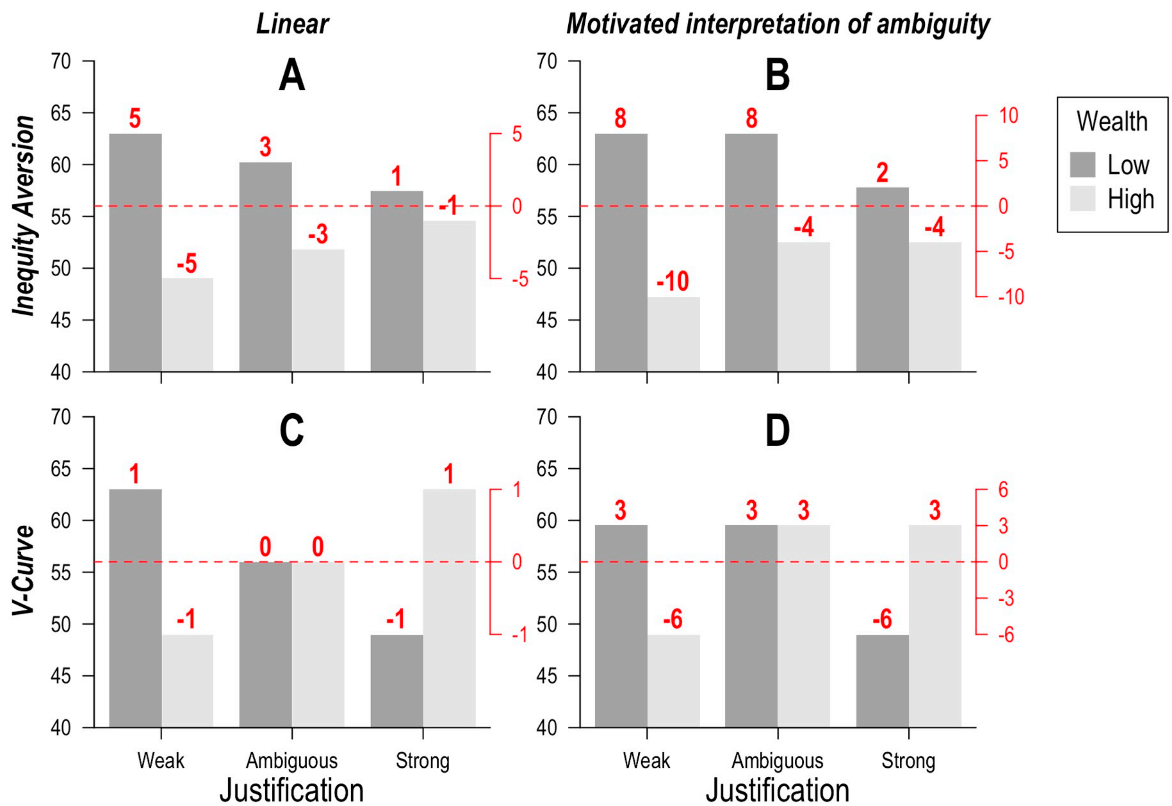

Fig. 2. Predicted patterns of results, representing different combinations of *Inequity Aversion* or *V-Curve* hypotheses and *Linear* or *Motivated Interpretation of Ambiguity* hypotheses. Y-axes on the left display theoretical scores on the primary outcome (percentage of points awarded to the ingroup). Y-axes on the right and numbers above bars (in red) represent the contrast codes matching each data pattern. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Table 1
Planned contrast weights for data patterns A through D and the pairwise differences between them.

Justification	Wealth	Contrast									
		A[a]	B[a]	C[a]	D[a]	A-B[b]	A-C[b]	A-D[b]	B-C[b]	B-D[b]	C-D[b]
Weak	Low	5	8	1	3	0.26	0.24	0.76	0.02	0.52	0.5
	High	−5	−10	−1	−6	0.04	−0.24	−0.05	0.28	0.19	−0.09
Ambiguous	Low	3	8	0	3	−0.33	0.88	0.17	−1.21	−0.71	0.5
	High	−3	−4	0	3	−0.28	−0.88	−1.59	0.6	−0.71	−1.31
Strong	Low	1	2	−1	−6	−0.01	1.52	1.71	−1.53	0.19	1.72
	High	−1	−4	1	3	0.31	−1.52	−1	1.83	0.52	−1.31

[a] A = Inequity Aversion + Linear; B = V-Curve + Linear; C = Inequity Aversion + Motivated Interpretation of Ambiguity; D = V-Curve + Motivated Interpretation of Ambiguity.
[b] Following Furr and Rosenthal (2003), difference weights are calculated by standardizing the contrast weights (the weights divided by their *SD*).and computing the differences between the pairs of standardized weights.

that the data supports the *Inequity Aversion* hypothesis. Following this, we will examine the difference contrast between the best supported pattern and the pattern representing the alternate hypothesis regarding the interaction pattern. In this case, we will examine the A-B difference (between A: *Inequity Aversion* + *Linear* and B: *Inequity Aversion* + *MIA*), and if this difference is significant will consider that the data supports the *Linear* hypothesis significantly more than the *MIA* hypothesis.

2.4. Pilot study

We conducted a pilot study to test and refine our methods.[2] We recruited 91 adult participants from MTurk, but here exclude 19 participants assigned to conditions removed from the experimental design following Stage 1 review, and a further 36[3] for failing to demonstrate comprehension of either their group's relative wealth status or the justification provided for the inequality, leaving a sample of 38 (18–24 years = 3, 25–34 years = 18, 35–44 years = 8, 45–54 years = 7, 55–64 years = 2; 10 female, 32 USA, 6 from other nations). The procedure was as described above, except that, (a)

[2] All data and code for the Pilot and power analysis are available at https://osf.io/3n2ad/?view_only=d6a22743306b46e7a48c58be422d12d9.

[3] While this is a high exclusion rate, it should be noted that this seems to be largely a result of Breadboard previously not allowing researchers to specify conditions on recruited mTurk workers. Altogether we recruited 29 Indian participants, 27 of whom failed the comprehension checks, perhaps due to language issues. By contrast, 38 of 54 American respondents passed the comprehension checks. In the current version of Breadboard we will be able to limit the sample to US participants.

Table 2
Planned contrast weights for data patterns A through D and the pairwise differences between them.

If:	and:	then:
A[a] is significant and has the highest t value…	…contrasts A-C + A-D are significant…	…the data supports *Inequity Aversion* over *V-Curve*
	…contrast A-B is significant…	…the data supports *Linear* over *MIA*
B[a] is significant and has the highest t value…	…contrasts B-C + B-D are significant…	…the data supports *Inequity Aversion* over *V-Curve*
	…contrast A-B is significant…	…the data supports *MIA* over *Linear*
C[a] is significant and has the highest t value…	…contrasts A-C + B-C are significant…	…the data supports *V-Curve* over *Inequity Aversion*
	…contrast C-D is significant…	…the data supports *Linear* over *MIA*
D[a] is significant and has the highest t value…	…contrasts A-D + B-D are significant…	…the data supports *V-Curve* over *Inequity Aversion*
	…contrast C-D is significant…	…the data supports *MIA* over *Linear*

[a] A = Inequity Aversion + Linear; B = V-Curve + Linear; C = Inequity Aversion + Motivated Interpretation of Ambiguity; D = V-Curve + Motivated Interpretation of Ambiguity.

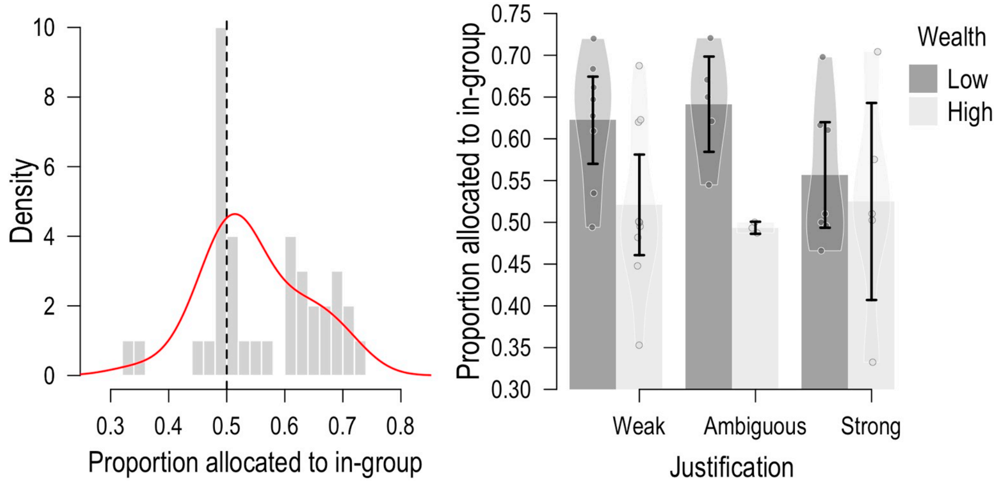

Fig. 3. Distribution of the outcome in the Pilot (left panel), and results by condition (right panel). Bars indicate 95% CIs.

participants made 30 rounds of allocations rather than 20, (b) performed a 'Mind in The Eyes' (Baron-Cohen, Jolliffe, Mortimore, & Robertson, 1997) task instead of an Investment Task,[4] (c) made allocation choices from an additional two matrices (the 'F vs. MIP & MD' matrices described in Turner, 1983).

For each participant we calculated the total proportion of points allocated to the ingroup in allocations involving an ingroup member and outgroup member. Allocations displayed evidence of ingroup bias (Fig. 3). Participants allocated an average proportion of 0.56 points to ingroup members (SD = 0.10), a number significantly > 0.5, $t(37) = 3.96$, $p < .001$, Cohen's $d = 0.64$. Proportions of points awarded to the ingroup correlated strongly with the traditional 'Maximum Dominance' pull score ($r = 0.60$) and the principal component-based measure used by Amiot and Bourhis (2005, $r = 0.93$).

Following the analysis plan outlined above, we evaluated our 10 sets of contrast weights (Table 1), and adjusted p values for multiple comparisons using the Benjamini-Hochberg adjustment. As shown in Table 3, contrasts A (*Inequity Aversion + Linear*) and B (*Inequity Aversion + MIA*) were significant, but no difference contrasts were significant. This suggests that while the data provided more support for the *Inequity Aversion* hypothesis than an absolute null of all cell means being equal, they did not provide significantly more support for any of the four predicted patterns of results compared to the others. The omnibus ANOVA main effect of wealth explained 20% of outcome variance ($\eta^2 = 0.204$), the omnibus wealth × justification interaction explained

[4] The 'Mind in the Eyes' task was replaced by the Investment Task after the pilot stage to create a procedure that is more germane to real socioeconomic outcomes. The pilot data still represents a valid source by which to determine our power sensitivity analysis and overall clarity and usefulness of the procedure.

Table 3
Planned contrast results from pilot study.

Contrast	Estimate	*SE*	*df*	*t*	*p*	$r_{contrast}$[b]
A[c]	0.98	0.29	32	3.33	0.01[a]	0.51
B[c]	1.94	0.57	32	3.42	0.01[a]	0.52
C[c]	0.07	0.07	32	1.03	0.47[a]	0.18
D[c]	0.38	0.36	32	1.05	0.47[a]	0.18
A-B	0	0.02	32	−0.2	0.93	0.04
A-C	0.2	0.1	32	2.05	0.39	0.34
A-D	0.2	0.11	32	1.78	0.42	0.30
B-C	−0.21	0.11	32	−1.94	0.40	0.32
B-D	0.2	0.11	32	1.90	0.40	0.32
C-D	0	0.05	32	−0.08	0.93	0.01

[a] One-tailed p value.
[b] Following Furr and Rosenthal[61], $r_{contrast} = \sqrt{\frac{t^2}{t^2 + df}}$.
[c] A = Inequity Aversion + Linear; B = V-Curve + Linear; C = Inequity Aversion + Motivated Interpretation of Ambiguity; D = V-Curve + Motivated Interpretation of Ambiguity.

5% ($\eta^2 = 0.047$). Results are visualized in Fig. 3.

2.5. Power sensitivity analysis

We performed a power sensitivity analysis using data gathered in the pilot study to approximate the expected distribution of our key outcome. By sampling with replacement from the pilot data, we created large populations for each cell of the design (N = 10,000 each). Because we consider data pattern B (*Inequity Aversion + MIA*) the most likely pattern of results, and because it was also best supported in our pilot data, we then added or subtracted values from each cell in order to create populations with effects matching pattern B. We varied the size

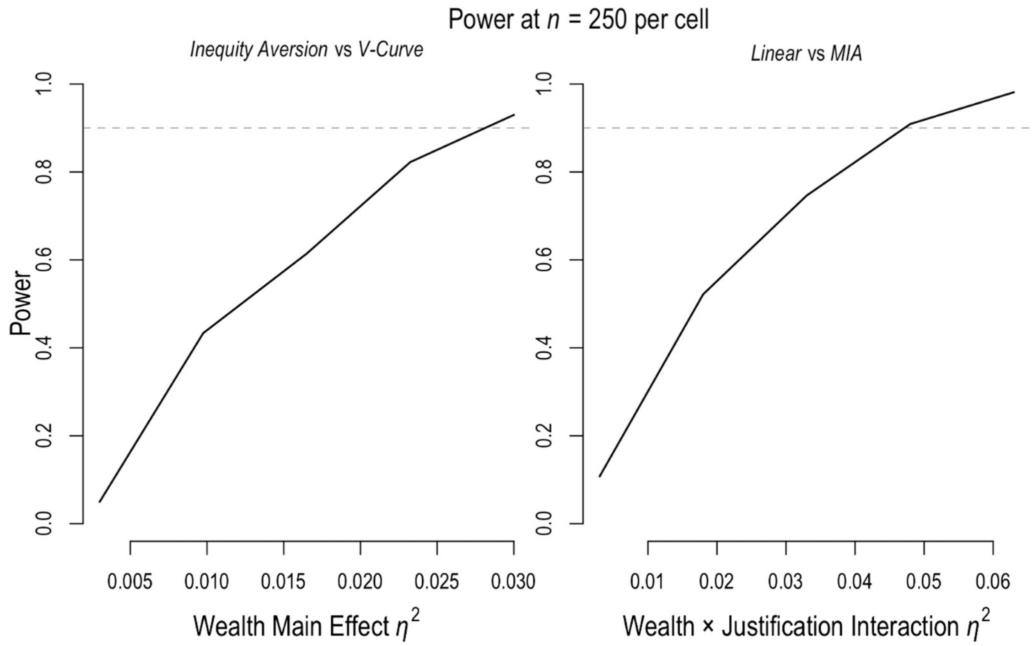

Fig. 4. Power curves for varying effect sizes with sample size set to 1500.

of effects in simulated populations, as indexed by η^2 values for the wealth main effect and the wealth × justification interaction effect returned from omnibus ANOVAs on the full simulated populations.

Due to financial constraints, we nominated a sample size near the upper limit of what we can feasibly afford, which is an *N* of 1500, or 250 participants per cell. Across 10,000 iterations we drew samples of this size from the simulated populations, and following the analysis plan and inference decisions outlined above for each iteration, we tested our power at each effect size to: (a) detect significantly more support for *Inequity Aversion* or *V-Curve* and (b) detect significantly more support for *Linear* or *MIA*.

The results (depicted in Fig. 4) suggested that, at an *N* of 1500, we will have extremely high power to differentiate between the *Inequity Aversion* and *V-Curve* hypotheses. When the population wealth main effect was set to $\eta^2 = 0.03$, less than one sixth as large as the effect observed in our pilot study, we observed over 90% power to decide between these competing hypotheses. Results suggested power will be lower to test between the *Linear* and *MIA* hypotheses, but was nonetheless above 90% when the interaction effect size was set at $\eta^2 = 0.05$, which was the magnitude of the effect observed in the pilot study.

3. Timeline

If our project achieves Stage 1 acceptance, we will embark on data collection as soon as possible, and will aim to complete data collection, analysis, and write up our results by the end of December 2019.

4. Stage 2

4.1. Participants

As planned, we recruited 1893 US-based participants from MTurk from November to December 2019, 393 of whom did not meet inclusion criteria, leaving our total sample size at 1500 (M_{age} = 36.32, SD_{age} = 10.87, 634 female, 1138 White, 117 Black, 100 Asian, 90 Hispanic/Latino, 45 other or not reported).

4.2. Results

4.2.1. Manipulation checks

As expected, participants assigned high wealth perceived their resources as more abundant (M = 82.44, SD = 18.76) than participants assigned low wealth (M = 18.68, SD = 23.6), t(1373.5) = 57.58, p < .001. Participants assigned to the strong justification condition perceived the initial wealth allocations as being more justified (M = 47.68, SD = 34.14) than participants in the ambiguous condition (M = 34.96, SD = 30.80), t(981.77) = 6.14, p < .001, who in turn perceived the initial wealth allocations as being more justified than participants in the weak justification condition (M = 29.84, SD = 29.67), t(955.72) = 2.64, p = .008.

However, while these overall differences were as expected, it is worth noting that there was little consensus regarding the fairness of the wealth allocations. Similar to past work suggesting that individuals' perceptions of the fairness of a system are coloured by their relative outcomes (e.g., Brown-Iannuzzi et al., 2014), cell means suggested that the high wealth group perceived the wealth distribution as substantially more justified than the low wealth group in each of the justification conditions (see Fig. 5). Also notable was that the low wealth group appeared to perceive the wealth inequality to be equally unjustified across the weak and ambiguous justification conditions. We discuss these observations more below.[5]

4.2.2. Confirmatory analyses

Fig. 5 displays proportions of points allocated to the ingroup overall (left panel), and proportions of points allocated to the ingroup by experimental condition (right panel). Similar to the pilot, participants displayed evidence of ingroup bias, allocating an average of 56% of points to the ingroup in ingroup/outgroup allocations (M = 0.56,

[5] Due to an internal miscommunication, we did not include any open-ended suspicion probe as pre-registered. However, this is unlikely to have influenced results. We examined responses to the two open-ended suspicion probe questions in our pilot ("What do you think the main hypothesis of this study is?" and "Was anything strange or appeared not to work properly in the study?") and found that of the 91 participants, only 1 displayed insight that the justification of the wealth disparity was related to our hypotheses, and only 1 expressed skepticism that they were participating with other real people.

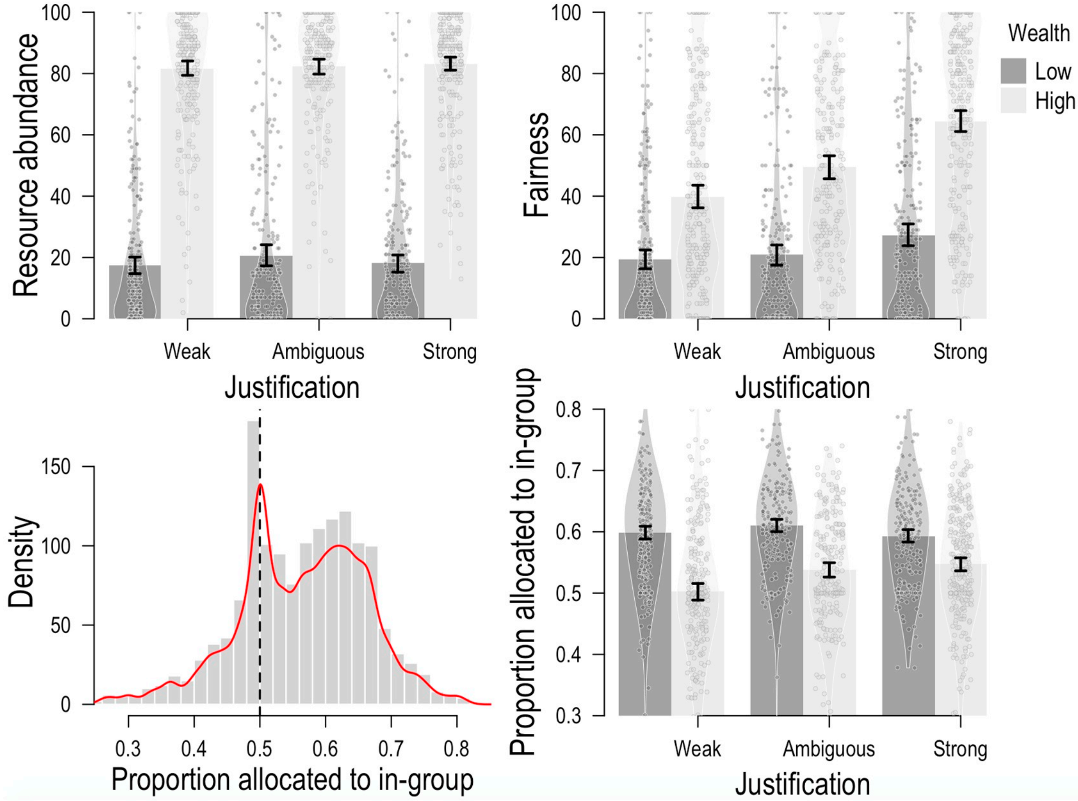

Fig. 5. Perceptions of resource abundance (top left) and fairness (top right) by condition, and points allocated to the ingroup in ingroup/outgroup allocations overall (bottom left) and by experimental condition (bottom right). Bars indicate 95% CIs.

Table 4
Planned contrast results.

Contrast	Estimate	*SE*	*df*	*t*	*P*[a]	$r_{contrast}$[b]
A[c]	0.75	0.05	1494	15.61	< 0.001[d]	0.37
B[c]	1.5	0.09	1494	16.16	< 0.001[d]	0.39
C[c]	0.05	0.01	1494	4.44	< 0.001[d]	0.11
D[c]	0.31	0.06	1494	5.16	< 0.001[d]	0.13
A-B	−0.01	0.003	1494	−2.16	0.06	0.06
A-C	0.16	0.01	1494	11.03	< 0.001	0.27
A-D	0.15	0.02	1494	9.46	< 0.001	0.24
B-C	−0.16	0.02	1494	−10.57	< 0.001	0.26
B-D	0.15	0.02	1494	10.13	< 0.001	0.25
C-D	−0.01	0.01	1494	−1.53	0.13	0.04

[a] *p* values are adjusted according to the Benjamini-Hochberg procedure (Benjamini & Hochberg, 1995).
[b] Following Furr and Rosenthal[61], $r_{contrast} = \sqrt{\frac{t^2}{t^2 + df}}$.
[c] A = Inequity Aversion + Linear; B = V-Curve + Linear; C = Inequity Aversion + Motivated Interpretation of Ambiguity; D = V-Curve + Motivated Interpretation of Ambiguity.
[d] One-tailed *p* value.

SD = 0.1), a figure significantly above 50%, t(1499) = 25.1, $p < .001$, $d = 0.65$.

The results of our confirmatory analyses following the plan outlined above are presented in Table 4. As expected, pattern B—*Inequity Aversion* + *MIA*—was best supported by the data and statistically significant (see the second row in Table 4). Both difference contrasts between pattern B and the *V-Curve* patterns (the B-C and B-D contrasts) were also significant, suggesting the data provides significant support for the *Inequity Aversion* hypothesis over the *V-Curve* hypothesis. However, the difference contrast between pattern B—*Inequity Aversion* + *MIA*—and pattern A—*Inequity Aversion* + *Linear*—(the A-B contrast) yielded a *p* value of 0.06 after adjustment for multiple comparisons. Thus, while pattern B was closer to the observed pattern of results than pattern A, it was not significantly closer. The data therefore did not provide significant support for the *MIA* over the *Linear* interaction hypothesis.

4.2.3. Exploratory analyses

We explored multiple other aspects of our data, including (a) whether ingroup bias increased or decreased over the 20 allocation rounds and whether this differed by experimental condition; (b) whether ingroup bias was affected by individuals' relative wealth compared to other ingroup members and whether this differed by experimental condition; (c) whether ingroup bias was affected by the number of AI bots present within allocation tasks; (d) how our manipulations affected participants on the 'pull scores' used in traditional Minimal Groups research; and (e) whether ingroup bias, or the effect of our experimental manipulations, was moderated by participants' subjective SES, age, gender, or race. For concision, the majority of these analyses (b–e) are relegated to Supplementary materials.

However, the results of the first of these explorations—how implicit bias evolved over time in each experimental group—are worthy of mention here. To investigate this, we analysed data at the individual allocation level, and fit the following series of hierarchical linear models:

$$y_{ij} = \beta_0 + \beta_1 wealth_j + \beta_2 ambiguous_j + \beta_3 strong_j + \beta_4 wealth_j ambiguous_j + \beta_5 wealth_j strong_j + \eta_j + \varepsilon_{ij} \quad (1)$$

$$y_{ij} = \beta_0 + \beta_1 wealth_j + \beta_2 ambiguous_j + \beta_3 strong_j + \beta_4 wealth_j ambiguous_j + \beta_5 wealth_j strong_j + \beta_6 round_i + \eta_j + \varepsilon_{ij} \quad (2)$$

$$y_{ij} = \beta_0 + \beta_1 wealth_j + \beta_2 ambiguous_j + \beta_3 strong_j + \beta_4 wealth_j ambiguous_j + \beta_5 wealth_j strong_j + \beta_6 round_i + \beta_7 round_i wealth_j + \eta_j + \varepsilon_{ij} \quad (3)$$

$$y_{ij} = \beta_0 + \beta_1 wealth_j + \beta_2 ambiguous_j + \beta_3 strong_j + \beta_4 wealth_j ambiguous_j + \beta_5 wealth_j strong_j + \beta_6 round_i + \beta_7 round_i wealth_j + \beta_8 round_i ambiguous_j + \beta_9 round_i strong_j + \eta_j + \varepsilon_{ij} \quad (4)$$

$$y_{ij} = \beta_0 + \beta_1 wealth_j + \beta_2 ambiguous_j + \beta_3 strong_j + \beta_4 wealth_j ambiguous_j + \beta_5 wealth_j strong_j + \beta_6 round_i + \beta_7 round_i wealth_j + \beta_8 round_i ambiguous_j + \beta_9 round_i strong_j + \beta_{10} round_i wealth_j strong_j + \beta_{11} round_i wealth_j ambiguous_j + \eta_j + \varepsilon_{ij} \quad (5)$$

where y_{ij} is the proportion of points awarded to the ingroup in the ingroup/outgroup allocation made by participant j in round i, $wealth_j$is a dummy indicating assignment to the high wealth group, $ambiguous_j$and $strong_j$are dummies indicating assignment to the ambiguous and strong justification conditions, $round_i$is the round number (from 1 to 20), $\eta_j$is a random intercept adjustment for participant j, and $\varepsilon_{ij}$is the residual term. Thus, Model 1 includes fixed effect of our experimental manipulations as well as a random effect of participants, Model 2 adds a main effect of round, Model 3 allows the effect of round to vary by wealth condition, Model 4 allows the effect of round to vary by justification condition, and Model 5 allows for a three-way interaction between wealth condition, justification condition, and round. We compared the fit of each successive model and computed χ^2 statistics and p values for each model comparison. Results are presented in Table 5. Due to their exploratory nature, we denote only if (un-adjusted) p values fell below $p = .001$, and urge that results be interpreted with caution.

As shown in Table 5, results were suggestive of potential 2-way interactions between allocation round and wealth condition (Model 3 displayed improved fit compared to Model 2), with ingroup bias increasing over time in the low wealth group and decreasing over time in the high wealth group, and between allocation round and justification condition (Model 4 displayed improved fit compared to Model 3), with bias more likely to increase over time in the ambiguous condition compared to the wealth or strong justification conditions. This meant that in addition to displaying the highest overall ingroup bias of all cells, the low wealth group in the ambiguous justification condition also showed the greatest increase in ingroup bias across the 20 trials (see Fig. 6). However, these effects were extremely small (in part due to the increased outcome variation at the individual allocation level), with both interactions increasing Nakagawa's R^2 (Nakagawa, Johnson, & Schielzeth, 2017) by < 1/20th of 1%.

5. Discussion

The present study provides insight into the way ingroup bias is affected by relative wealth disparities and their justification. As expected, participants randomly assigned lower wealth showed greater ingroup bias than participants randomly assigned higher wealth. This finding has previously been observed within similar experiments (e.g., Durrheim et al., 2016; Harvey & Bourhis, 2012; Rubin et al., 2014), but the present study is arguably its most robust demonstration to date, insofar as it is based on a considerably greater sample size compared with previous studies, and our use of the registered report format. This provides clear, unambiguous support for the *Inequity Aversion* hypothesis (e.g., Dawes et al., 2007; Fehr & Schmidt, 1999) over the *V-Curve* hypothesis (Guimond & Dambrun, 2002), within the context of the Minimal Group Paradigm.

More unexpected, however, were the effects of the justification manipulation. While we were able to predict quite accurately how the high wealth group would respond to the different justification conditions (compare the original *Inequity Aversion + MIA* predicted pattern of results for high wealth groups in Fig. 2 to the observed results in Fig. 5), we did not accurately anticipate responses in the low wealth group, who presented a puzzling set of observations. The low wealth group demonstrated the most ingroup bias when inequality was ambiguously justified, but did not appear to rate the ambiguous justification condition as being less fair than the weak justification condition. Moreover, subsequent explorations suggested that the unique combination of low wealth and ambiguous justification may have produced a progressive increase in ingroup bias, which did not occur in any other cell of our design (see Fig. 6). We think this was a fascinating result, as it suggests that there may be something unique about the effect of

Table 5
Model results exploring effect of round on ingroup bias. Wealth is a dummy indicating assignment to high wealth, Ambiguous and Strong are dummies indicating assignment to ambiguous or strong justification conditions, respectively.

	Model 1	Model 2	Model 3	Model 4	Model 5
Fixed					
Intercept	0.623 (0.007)	0.626 (0.007)	0.617 (0.007)	0.619 (0.007)	0.623 (0.007)
Wealth	−0.118 (0.009)	−0.118 (0.009)	−0.1 (0.01)	−0.1 (0.01)	−0.108 (0.01)
Ambiguous	0.017 (0.01)	0.017 (0.01)	0.017 (0.01)	0.004 (0.01)	−0.001 (0.011)
Strong	−0.007 (0.01)	−0.007 (0.01)	−0.007 (0.01)	−0.002 (0.01)	−0.008 (0.011)
Wealth × Ambiguous	0.027 (0.014)	0.027 (0.014)	0.027 (0.014)	0.027 (0.014)	0.038 (0.015)
Wealth × Strong	0.062 (0.013)	0.062 (0.013)	0.062 (0.013)	0.062 (0.013)	0.074 (0.015)
Round		−0.0003 (0.0001)	0.001 (0.0002)	0.0003 (0.0002)	−0.00004 (0.0003)
Wealth × Round			−0.002 (0.0002)	−0.002 (0.0002)	−0.001 (0.0004)
Ambiguous × Round				0.001 (0.0003)	0.002 (0.0004)
Strong × Round				−0.0005 (0.0003)	0.0001 (0.0004)
Wealth × Ambiguous × Round					−0.001 (0.001)
Wealth × Strong × Round					−0.001 (0.001)
Random					
Participant	0.104	0.104	0.104	0.104	0.104
Residual	0.2	0.2	0.2	0.2	0.2
Model comparison					
χ^2 (*df*)		8.367 (1)	52.908 (1)*	34.91 (2)*	5.201 (2)
R^2[a]	0.043	0.043	0.043	0.043	0.043

* $p < .001$.
[a] R^2 = Nakagawa's R^2.

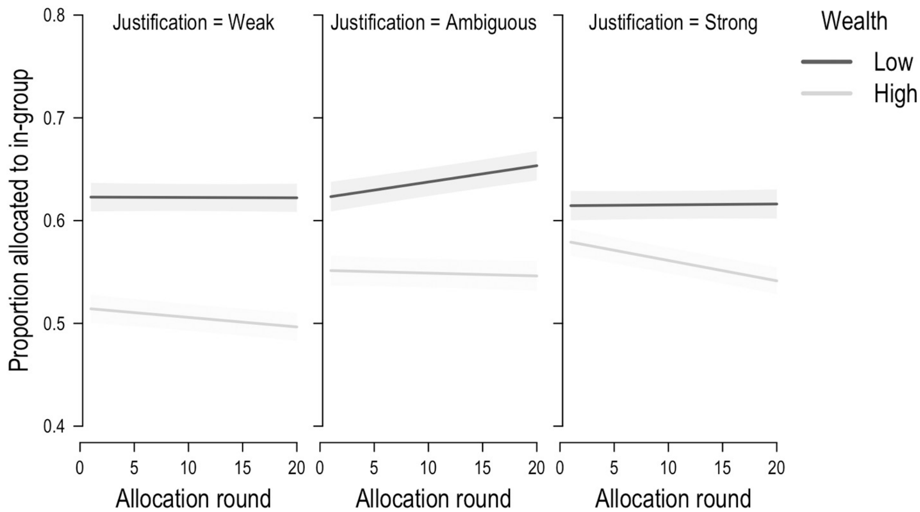

Fig. 6. Trends in ingroup bias by experimental conditions. Lines and shaded regions indicate predictions and 95% confidence intervals from Model 5 (see Table 5).

ambiguously justified inequality on relatively disadvantaged groups' inclination to exhibit ingroup bias. Why might this be?

Additional research is clearly needed to address this question, particularly given the unanticipated nature of this finding, but we offer a few speculations. First, individuals may possess separate and distinct aversions to both inequity *and* ambiguity. Much research in social psychology has examined individual differences in ambiguity tolerance, and arrived at a general consensus that ambiguity itself is aversive to many people, is capable of producing negative affective states, and incites efforts to perceive patterns and structure in the environment as a means of regaining the control that it undermines (e.g., Kay, Whitson, Gaucher, & Galinsky, 2009; for a review, see Furnham & Ribchester, 1995). Some research has even focused on the notion that cross-race interactions come with interpersonal ambiguity that elicits discomfort between interaction partners (e.g., Shelton & Richeson, 2006). Thus, for the low wealth group, or perhaps just a subset of ambiguity-intolerant group members, this may have translated into frustration and resentment toward more advantaged others, resulting in heightened ingroup bias.

Alternatively, additional feelings of intergroup resentment among low wealth group members may have been fuelled by assumptions about how their high wealth outgroup counterparts interpreted the inequality. In this vein, research suggests that individuals readily perceive biased reasoning in others, despite being relatively unwilling or unable to recognize it within themselves (e.g., Brown-Iannuzzi et al., 2014; Hansen, Gerbasi, Todorov, Kruse, & Pronin, 2014; Hastorf & Cantril, 1954; Lord, Ross, & Lepper, 1979). Applied to the present context, when inequality is ambiguously justified, individuals in disadvantaged groups may have assumed that individuals in advantaged groups were employing biased reasoning in their interpretations of the inequality, and so produced perceptions of a kind of double injustice: not only did members of disadvantaged groups perceive the inequality as unfair, they may also have perceived this unfairness as likely being misconstrued or outright unrecognized by their high wealth counterparts. This could potentially help explain why ingroup bias appeared to increase over time among the in the low wealth/ambiguous justification condition. Because this 'double injustice' is relatively complex, requiring consideration of the perspective and possibly biased reasoning of other participants, it may simply require time to become salient. This phenomenon may have occurred specifically in the ambiguous justification condition—where there was room for varied perceptions and interpretations—in contrast to the other two justification conditions wherein the justifications were, by design, unambiguous, constraining participants from biased reasoning and conclusions. This line of reasoning is supported by research suggesting that members of disadvantaged groups make more attributions of bias or discrimination when situational prejudice cues are ambiguous relative to when they are explicit or non-existent (e.g., Crocker & Major, 1989; Major, Quinton, & Schmader, 2003).

These are just speculations, but we believe the potential for ambiguity to ratchet up feelings of intergroup resentment via any of the above routes may have important real-world applications, and warrants further study. As noted above, to our knowledge this is the first study to have investigated the effect of ambiguously justified inequality between groups on ingroup bias. Based on our results, we think there is much work to be done in this area, which could profitably attend to (a) the mediating role of negative affective states (b) perceptions of the beliefs of outgroup members, and associated resentments.

Finally, some important limitations of the present research should be noted. As discussed above, there are major limitations on how applicable results from minimal group studies are to real-world intergroup processes marked by group memberships that are vastly more complex, meaningful, and enduring, so the extent to which our results are useful in predicting real-world behaviors and outcomes remains to be seen. Additionally, our reliance on a 'WEIRD' sample of U.S. MTurk workers limits generalizability to different samples (Henrich, Heine, & Norenzayan, 2010), especially in light of evidence that there may be cultural differences in how individuals interpret and respond to inequality (Cheung, 2016), and stark differences between countries in baseline levels of economic inequality, which may inform reactions to between-group wealth differences. Finally, while we find robust evidence regarding the effect of relative wealth, and potentially generative results regarding the moderating role of justification, we cannot make inferences about the effect of changes in absolute levels of inequality. Given recent increases in income inequality within many societies (Piketty, Saez, & Zucman, 2017) and recent correlational evidence suggesting an association between income inequality and some measures of intergroup prejudice (Connor, Sarafidis, Zyphur, Keltner, & Chen, 2019), we believe it will be important for future work to examine the effect of the absolute level of inequality between groups, and/or changes in the absolute level of inequality between groups, on ingroup bias.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jesp.2020.103967.

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