

Large-scale cross-societal examination of real- and minimal-group biases

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Large-scale cross-societal examination of real- and minimal-group biases

Stage 1 Registered Report

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Abstract

Biases in favor of culturally prevalent social ingroups are ubiquitous, but random assignment to arbitrary experimentally created social groups is also sufficient to create ingroup biases (i.e., the minimal group effect; MGE). The extent to which ingroup bias arises from specific social contexts versus more general psychological tendencies remains unclear. This registered report focuses on three questions. First, how culturally prevalent is the MGE? Second, how do critical cultural and individual factors moderate its strength? Third, does the MGE meaningfully relate to culturally salient real-world ingroup biases? We compare the MGE to bias in favor of a family member (first cousin) and a national ingroup member. We propose to recruit a sample of > 200 participants in each of > 50 nations to examine these questions and advance our understanding of the psychological foundations and cultural prevalence of ingroup bias.

Introduction

Over 50 years ago, researchers seeking to systematically examine the origins of intergroup bias discovered something striking: ingroup bias, i.e., the tendency to prefer ingroup members over outgroup members, emerges even when participants are assigned to previously unfamiliar and largely arbitrary social identities¹⁻³. For example, forming groups on the basis of putative tendencies to over- or under-estimate dots, preferences for different abstract artists, or even explicit random assignment is generally sufficient to induce positive evaluations of and increased generosity towards ingroup members as compared to outgroup members^{4,5}. This phenomenon is now commonly referred to as the “minimal group effect” (henceforth the MGE, arising from the minimal group paradigm, MGP) due to the minimal level of social meaning the groups have outside the experimental context. The MGE has since been supported by several meta-analytic investigations⁶⁻¹⁰. If the MGE is a valid index of the generalized tendency to prefer ingroups (i.e., of the effects of “mere membership”⁴) it is a valuable lens through which to understand how intergroup biases play out in the real world.

Despite the large body of empirical work on the MGE, several important questions remain. The first concerns the cultural ubiquity of the MGE. While meta-analytic evidence suggests the effect is robust^{6-8,10}, evidence for the MGE is fairly narrow, built primarily on Western, Educated, Industrialized, Rich, and Democratic participants (WEIRD¹¹), which may well not be representative of broader world populations¹². This becomes particularly important given scattered evidence suggesting cultural variation in the magnitude of the MGE both when comparing participants in North America to those in Japan¹³ and when comparing participants in the United States to Australians¹⁴. However, the extent and underlying cause of such variation remain unknown due to the scarcity and small-scale nature of past investigations.

A second and perhaps even more critical question centers on the fact that theorists from different traditions have provided different and sometimes conflicting explanations for ingroup bias in the minimal group context⁴. More specifically, some authors have implicated an evolved coalitional psychology, perhaps constituted by a set of heuristics or norms concerning within-group reciprocity¹⁵⁻¹⁷. Other scholars have suggested that it emerges due to the close connection between the group and the self, and in particular as a cause or consequence of positive self-evaluation¹⁸⁻²¹. Others have pointed to psychological or personality processes, perhaps embedded in cultural practices, that could motivate ingroup favoritism (e.g., uncertainty reduction²²). Adding to the complexity, ingroup bias has also been linked to deeply rooted cultural and bio-ecological factors, including pathogen prevalence, intergroup conflict, individualism-collectivism (particularly as it relates to group permeability), stability and trust of impartial institutions, market integration, and kinship dynamics²³⁻²⁷. To advance our understanding of these myriad factors we propose a large-scale and culturally broad investigation of these questions. By capturing natural variation in many of these putative causes, including self-esteem, social status, and a selection of cultural and bio-ecological factors, our study will provide substantive evidence regarding the underlying mechanism(s) driving ingroup favoritism in the MGE.

A third question concerns the relationship between the MGE and biases involving real-world groups. Real-world group biases vary cross-culturally^{7,28,29} (cf³⁰); if the MGE reflects an individual's or culture's default tendency towards ingroup preference, then the dynamics of the MGE should at least generally parallel the dynamics of real-world group biases. On this view, the magnitude of an individual's bias towards minimal groups should be positively correlated with the magnitude of that individual's bias towards real-world groups. While intuitive, several findings undermine this possibility. For example, individual difference moderators that consistently relate to real-world group biases often fail to predict the MGE (e.g., Right-Wing Authoritarianism and ethnocentrism^{31,32}), and the neural signatures of different forms of intergroup bias may have little overlap³³. These findings

suggest that a distinct set of causal processes may underlie each form of bias. Thus, the extent to which real and minimal group effects are meaningfully linked remains unknown, and may be subject to cultural moderation. Our study will explore these relationships and so contribute to our understanding of the origins of ingroup bias and discrimination.

To summarize (see also Table 1, Design Table), Research Question 1 (RQ1) asks whether the MGE holds across diverse cultures and so addresses the possibility that a basic tendency towards ingroup preference could be a human universal³⁴. Correspondingly, Hypothesis 1 (H1) predicts that the MGE is culturally ubiquitous, i.e., that the effect of ingroup bias will be statistically significant and positive in all societies tested (or, at a minimum, that the pattern of observed results is consistent with a universal presence of the MGE coupled with sampling error). However, we anticipate that even against the backdrop of a culturally ubiquitous MGE, we will nonetheless observe cultural variation.

Research Question 2 (RQ2) asks whether the magnitude of the MGE varies as a function of several moderators. Because we conceptualize group biases as culturally situated, where possible, we examine societal-level and individual-level operationalizations of each moderator. We focus on several of the most critical potential moderators identified via past work.

First, societies vary in the ease with which people conceive of themselves as able to form and dissolve their memberships in real-world groups. This phenomenon has been referred to as group permeability in some areas of psychology³⁵ and is closely related to the construct of relational mobility³⁶; we use the term permeability going forward. In societies such as Morocco or Egypt in which the extended family or a small network of close relationships are the central reference groups, permeability and relational mobility tend to be lower because the most critical social groups are enduring and difficult to enter and exit. By contrast, in societies such as the USA or France in which the extended family plays a smaller role and where groups are formed and dissolved more frequently, permeability and relational mobility are high. This factor has been highlighted as an important potential contributor to the MGE going as far back as the early minimal groups work³, but has not been explored extensively in this context (for an exception see⁷). That said, conceptually related constructs including Hofstede's³⁷ individualism (which positively correlates with permeability) and the strength of family ties (which negatively correlates with permeability) have been central predictors in many studies of other forms of cross-societal variation^{25,26,36,38,39}. In choosing measures we sought to go beyond a simple Western / non-Western or WEIRD / non-WEIRD dichotomy by employing several continuous measures of cultural variation, which we detail in our methods section, below.

Given that minimal groups seem to be better analogs for rapidly formed and dissolved groups than stable elements of social life such as the family, Hypothesis 2.1 (H2.1) predicts a positive relationship between permeability and the MGE. In other words, the MGE will be stronger in societies and individuals who show more flexibility moving between new and various social groups. Conversely, in contexts in which long-standing social groups that are difficult to depart from are paramount (e.g., intensive kinship networks), individuals will be less given to affiliate with newly formed transient social identities such as minimal groups. Thus, we predict a positive relationship between permeability/relational mobility and the MGE. Note that this prediction holds only for minimal group preferences; we do not predict this pattern for real-world groups. Indeed, some past theorizing suggests the opposite direction of relationship for some social groups, grounded in the notion that one must be more committed to groups from which one is less able to depart^{21,23,40,41}.

Newly formed social identifications (as appear to be generated by the MGP) entail a degree of risk due to the possibility of forming a link with an untrustworthy partner¹⁷ and/or not having recourse available if exploited. The extent to which one is open to such new (and potentially risky) links is plausibly related to societal-level trust and the strength of institutions that enforce social contracts

and mediate between individuals engaged in disputes⁴². This motivates Hypothesis 2.2 (H2.2): the MGE will be stronger in societies and individuals with a higher baseline level of trustworthiness towards unfamiliar others and that exhibit greater trust in the institutional structures that regulate cooperative behavior. Further support for this possibility comes from evidence that the MGE depends, at least in part, on the expectation that individuals will behave prosocially towards other ingroup members^{16,17}, an important form of ingroup favoritism⁶. Importantly, this prediction again holds for the MGE but not necessarily for affiliation with salient real-world groups. Indeed, past work^{23,43} provides evidence that in societies with less secure institutions and reduced trust in the rule of law, some forms of ingroup favoritism, including that directed towards the family and the nation, tend to be stronger. This may occur because, in the absence of strong institutions, one is incentivized to favor closer social ties with highly familiar others. On the other hand, it is possible to motivate a competing hypothesis, namely that in societies and institutions high in generalized trust, trust towards outgroup members will be higher and thus the MGE will be lower. Our data will allow us to adjudicate between these possibilities.

Our final moderator is self-esteem. There is a long history of debate about the relationship between self-esteem and ingroup favoritism, both within and beyond the context of the MGE. Early work in the social identity tradition conceptualized ingroup favoritism as a response to low self-esteem, but this relationship was not consistently found⁴⁴. In the context of the MGE, past work suggests a positive association between the MGE and self-esteem^{19,45–47}, a finding generally interpreted as suggesting that self-related positivity is automatically extended to the ingroup. The most direct cross-cultural inquiry also provides support for this link, finding that Japanese participants show a reduced MGE as compared to North American participants, and that this difference is mediated by self-esteem¹³. Building on this finding, Hypothesis 2.3 (H2.3) predicts that the MGE will be stronger in individuals with higher self-esteem. While there are many conceptualizations of self-esteem, some, in particular collective self-esteem⁴⁸, overlap considerably with notions of group identity. In the present work, we focus on a much simpler conceptualization grounded in the basic question of how positively one evaluates oneself in a relatively neutral context (i.e., personal self-esteem) and the possibility that this evaluation is extended to the minimal ingroup.

Research Question 3 (RQ3) asks whether the magnitude of the MGE is systematically related to real-world intergroup biases. Understanding an individual's orientation towards more familiar and locally relevant real-world groups will always be more complex than that towards an artificial group because it is affected by many factors that do not contribute to the MGE (such as social status, cultural stereotypes, etc.). This is why the MGP has frequently been used as a simplified proxy reflecting the consequences of “mere membership”⁴, but it is unclear whether it can play that role successfully. In other words, does the MGE relate to, or might it even be, the initial foundation of an individual's tendency to prefer culturally salient real-world groups? If so, the first-order prediction is that real-world group biases and the MGE should be positively correlated (i.e., the extent of an individual's MGE should positively correlate with their bias towards real-world groups); Furthermore, this perspective implies the prediction that at least some individual difference or cultural level moderators should similarly relate to *both* real and minimal group bias.

Alternatively, real-world groups and minimal groups might have a trade-off or compensatory relationship, such that individuals who have stronger biases in one have weaker biases in the other. This relationship could occur if cultural emphasis on cooperation with transient groups serves to fill a gap that arises as more traditional social ties break down, a hypothesis derived from the literature on the role of strong institutions and kinship intensity^{23,25}. A hint of this emerges above when we suggest that some moderators might relate to real and minimal group bias in different ways; for example, permeability might positively relate to minimal groups while negatively relating to salient real-world groups.

In other words, in low-trust and low-permeability societies, people might be so strongly tied to existing real groups (such as kinship networks) that they are less likely to favor groups that are based on minimal cues. If this contrastive pattern is ubiquitous, ingroup bias towards those real groups might be negatively related to the MGE. We will explore these possibilities with respect to the three sets of moderators described above in RQ2.1–RQ2.3, as well as with a few additional moderators that have been actively discussed in the context of real-world groups, as described in our methods section below (e.g., beliefs about the similarity of group members and the status of the groups involved⁴⁹). Thus, by assessing attitudes and behaviors towards real groups as well as towards a minimal group, we can explore a range of questions concerning how real and minimal group biases relate. Evidence concerning this research question remains sparse; therefore, we do not advance a specific hypothesis here but specify our analysis plan to be able to weigh in on a range of possible patterns of results.

To investigate these relationships in a way that is maximally comparable across cultures, the real-world groups we focus on will be the family and the nation; we describe the rationale for these choices in more detail below. Here, we merely stress that, while these groups will be subject to cultural variation in their meaning and construal, we believe they reflect more consistent and comparable aspects of social identity than, for instance, racial, ethnic, or religious identities, since specific instantiations of the latter identities are much less consistently present across countries and regions.

We now briefly review relevant past work. The literature on minimal groups is vast, thus our summary here must remain brief; we point interested readers to recent narrative reviews^{4,5}. Most relevant to the present inquiry are several meta-analyses that have tackled part of this literature, all of which included real and artificial groups (a category somewhat broader than minimal groups because it can include, for example, lab-created groups about which substantial information is then provided). The oldest of these meta-analyses¹⁰ focused on ingroup bias in real and lab-created artificial groups and revealed a small-to-medium sized average effect for artificial groups, $r = .26$, which was significantly smaller than the effect for real-world groups, $r = .40$; the authors attributed these findings to the idea that real groups are more psychologically salient to participants. By contrast, in two analyses focusing on cooperation tasks, one with adults⁶ and one with children⁹, both real and lab-created groups produced highly similar mean levels of ingroup favoritism. Adding further complexity, in an analysis focusing specifically on incentivized group discrimination tasks⁸ bias was actually stronger in artificial as compared to real-world groups, which the author suggested can be attributed to the higher salience of the former groups, given how little other information or expectations one has to go on (e.g., in a minimal group context in which all other patterns of similarity and difference are obscured). Taken together, these papers paint an inconclusive picture and highlight the value of a large-scale investigation with standardized procedures.

A further meta-analysis focused on the extent of ingroup favoritism across 18 societies⁷. The authors did not find substantial cultural variation in ingroup favoritism with respect to artificial groups (though they did find variation with respect to real-world groups), a finding that can be taken as suggestive of the universality hypothesis we explore here. However, despite including a reasonable number of societies, just over 5% of their samples can be considered non-WEIRD, limiting the conclusions that can be drawn.

We also note that meta-analyses can suffer from a variety of important limitations including file drawer and publication bias and difficulty aggregating across research designs, measures, and settings⁵⁰. A registered report alleviates these concerns by combining large sample sizes with standardized measures and preregistered analysis plans.

In summary, our proposed research investigates several open issues concerning the minimal group effect (MGE), one of the most replicated and consistent effects in the social sciences, but one that is still subject to many unanswered questions. By harnessing the power of a large-scale collaboration with standardized procedures⁵¹, we will assess the prevalence and/or cultural variability of the MGE as assessed via both attitudinal and incentivized economic dependent measures, tie it to several plausible moderators, and assess its relationship with two culturally ubiquitous forms of real-world bias.

Methods

Ethics information

This study has been reviewed by the Yale University Institutional Review Board (FWA00002571; IRB Protocol #2000031444), which determined that it has Exempt Status. The research team will ensure IRB review and all necessary approvals at all other institutions as needed when they join the project. Participants will be given basic information about the study and will agree to participate before the study begins. Please see the Recruitment and Sampling section, below, for further details. The study will take approximately 20 minutes to complete. Participants will be paid a show-up fee of an equivalent of approximately US \$2.50 in their local currency (or course credit, depending on the local labs) and every 10th participant will be randomly selected to obtain a bonus amount determined by the results of the incentivized economic games (with an expected average payout of US \$10 per selected participant, with the exact amount depending on actual decisions made by participants in the incentivized allocation task, see below). The average expected bonus amount across all participants is therefore US \$1 per person. These amounts will be adjusted for international sites to equate for local purchasing power following established methods^{52–54}.

Pilot data

We conducted two pilot experiments to confirm the feasibility of our design. Data and code related to both projects are provided on the project Github page (<https://github.com/adamrkenny/psa008>) as well as the project OSF page (https://osf.io/b5dqf/?view_only=bd0d6b35b9f54455a531fcff53c62fde).

Pilot Study 1: Feasibility of an online minimal group procedure. First, we conducted a study (pre-registration link: <http://aspredicted.org/blind.php?x=in3rd6>) to confirm that we can elicit the MGE when implemented as a survey delivered via Qualtrics to online participants. A sample of 207 primarily US adults recruited through Amazon's Mechanical Turk were randomly assigned to a minimal group defined via a noun label serving as a group name (the "Flurps" vs. the "Zazzes"). Participants then provided ratings of their views of the two groups and completed a hypothetical monetary allocation task. Both dependent measures showed robust effects of group assignment at magnitudes similar to those described in past work; we found a moderate ingroup advantage in both liking ($M = 0.78$, $SD = 1.31$, on a 7-point scale; $t(206) = 8.60$, $p < .001$, $d = 0.60$, 95% CI = .32, .88) and allocation preferences ($M = 0.97$, $SD = 1.39$, on a 5-point scale; $t(206) = 10.02$, $p < .001$, $d = 0.70$, 95% CI = .41, .98). This study provided a workable template for a minimal group paradigm that can reliably elicit the MGE and is suitable for computer-based implementation.

This study also allowed us the opportunity to examine the group ratings items, which constitute our evaluative dependent measure. Participants answered a total of eight items, four targeting the ingroup and four the outgroup. For each group one item directly asked about liking, one about desire to help, one about extent of identification, and one about extent of attachment. These items reliably cohered, $\alpha = .84$, and a Factor Analysis over these items further suggested that they loaded on a

single factor, supporting our decision to aggregate. However, interested future analysts can explore these items in other ways.

Pilot Study 2: Pilot of full experimental procedure. The second pilot experiment implemented the full experimental procedure that we proposed for the initial submission of this registered report, conducted in two countries, the US and the UK, to means-test the full procedure including our bonus payment procedure. We emphasize here that because this pilot was conducted prior to our initial submission, it does not implement changes made in this revised submission in terms of measures or final analysis plan. It nonetheless provides valuable information about feasibility and length, and also provides some preliminary evidence in support of several of our hypotheses, at least in this limited context. Full implementation of the analysis pipeline with the Pilot Study 2 data is provided in the code file on OSF.

We enrolled 200 US (modal age category: 35–44 years; gender: 47.5% women, 52.5% men) and 200 UK (modal age category: 25–34 years; gender: 50% women, 50% men) participants via Prolific (www.prolific.co). A complete analysis is provided in supplemental materials; here we focus on the following critical points (Figure 1). First, we observed ingroup bias on all minimal group measures (attitudes: $M = 0.63$ scale points, $SD = 0.96$, $t(399) = 13.10$, $p < .001$, $d = 0.66$, 95% CI = 0.45, 0.86; first-party allocation task: $M = 0.91$ units, $SD = 3.22$, $t(399) = 5.67$, $p < .001$, $d = 0.28$, 95% CI = 0.09, 0.48; third-party allocation task: $M = 1.81$ units, $SD = 3.29$, $t(399) = 11.00$, $p < .001$, $d = 0.55$, 95% CI = 0.35, 0.75), as well as with respect to the family (attitudes: $M = 0.62$ scale points, $SD = 1.44$, $t(399) = 8.60$, $p < .001$, $d = 0.43$, 95% CI = 0.23, 0.63; first-party allocation task: $M = 1.64$ units, $SD = 4.32$, $t(399) = 7.60$, $p < .001$, $d = 0.38$, 95% CI = 0.18, 0.58; third-party allocation task: $M = 2.74$ units, $SD = 5.15$, $t(399) = 10.60$, $p < .001$, $d = 0.53$, 95% CI = 0.33, 0.73). Bias with respect to nationality was weaker and only appeared on two of three measures, the attitudinal and third-party allocation task (attitudes: $M = 0.34$ scale points, $SD = 1.14$, $t(399) = 5.92$, $p < .001$, $d = 0.30$, 95% CI = 0.10, 0.49; first-party allocation task: $M = 0.04$ units, $SD = 3.05$, $t(399) = 0.23$, $p = .81$, $d = 0.01$, 95% CI = -0.19, 0.21; third-party allocation task: $M = 0.81$ units, $SD = 3.18$, $t(399) = 5.06$, $p < .001$, $d = 0.25$, 95% CI = 0.056, 0.45).

Figure 1: Summary of data from Pilot Study 2 (full implementation). Results for (a) attitudinal bias, (b) first-party bias, and (c) third-party bias across the three groups types/paradigms (minimal, family, and national). Results are similar across participants from the United Kingdom (GBR, $n = 200$) and the United States of America (USA, $n = 200$). Error bars show 95% CI around the mean. Scale ranges from -6 to 6 for (a), and -20 to 20 for (b,c). Bias is not standardized.

Second, we observed preliminary relationships between some proposed moderators and minimal group bias. A model predicting the MGE from self-esteem improved model fit as compared to a reduced model that did not include self-esteem (via a likelihood ratio test, $\chi^2(1) = 5.17$, $p = .023$), as did a model predicting the MGE from trust in institutions (as compared to a model without trust in institutions, $\chi^2(1) = 11.83$, $p < .001$), suggesting positive relationships between both of these variables and the MGE. Third, we found preliminary evidence of relationships between the strength of minimal group bias and the strength of real-world bias. In a model predicting real-world bias (collapsed across bias type) from the strength of the MGE and the type of real-world bias, the inclusion of the term for MGE significantly improved model fit, $\chi^2(1) = 32.25$, $p < .001$, as compared to a model with the MGE term. Although these results should be considered preliminary at this point, they do provide a strong validation of our basic procedure and the primary hypotheses advanced above.

Design

To aid in evaluation, the entire procedure is included in the supplemental materials and available to be reviewed on Qualtrics at (example version) https://yalesurvey.ca1.qualtrics.com/jfe/form/SV_07F0ZaTGI9vIMH4?CountryLan=US_EN&Q_Language=EN. Further, the design is described in our design table (Table 1). Participants will take part in three group paradigms (within-subjects): one minimal group paradigm and two that involve real groups (family and nation). We will measure ingroup bias using both attitudinal measures and behavioral measures. Thereafter, participants will be presented with the individual-level measures in a randomized order (permeability, trust, and self-esteem). At the end of the study, we will gather survey and demographic items and debrief participants.

Operationalization of minimal groups paradigm. The MGP has been operationalized in dozens of ways, including putative estimation tendencies of dot arrays³, putative artistic preferences¹ and random assignment⁴⁶. We are not interested in an exact replication of any particular MGP, including the classic MGP from Tajfel and colleagues but rather in extracting the core properties of the MGP in a way that maximizes cultural comparability. With this in mind, we will use an explicitly random assignment procedure to assign individuals to groups indexed only by a group label (e.g., “Group M” or “Group K”). This approach provides a strict test of the MGE compared to versions based on more meaningful shared properties because it rules out the possibility that participants expect others who are similar to them in one area (e.g., art preferences) to also be similar to them in other areas⁵⁵. We consider this methodological decision to be particularly important in the cross-cultural context in which the same dimension of similarity (e.g., artistic preferences) might carry different weight across cultures, and elicit different responses or different expectations about other patterns of expected similarity. We do not claim that the meaning of a randomly assigned group will be identical across cultures, but we argue that comparability is higher for this “more minimal” form of social identity. Importantly, there is also experimental evidence suggesting that different group assignment strategies do not produce large differences in resulting preference patterns^{56,57}.

Group labels will be randomly assigned as one of four letter pairs (i.e., H vs. J, K vs. M, L vs. N, or P vs. S). The ingroup assignment will be counterbalanced within each pair so approximately half of participants will be assigned the first of the two letters in the alphabet as their ingroup label and approximately half will be assigned the second. We chose letters toward the center of the Latin alphabet and randomized their assignment to minimize any effects of local cultural associations with the letters. When the task is translated for languages that use a different alphabet (e.g., Greek), native speakers across cultural contexts will be asked to confirm the absence of meaningful semantic associations with particular letters or letter pairs.

Table 1. Design Table

Question	Hypothesis	Sampling plan (e.g., power analysis)	Analysis Plan	Interpretation given to different outcomes
RQ1: Does the MGE hold across diverse societies?	H1: the MGE is culturally ubiquitous in tested cultures	Sample at least 30 and ideally 50+ countries (cultural units) from the PSA network, with the number of countries based on Figure S1 and the number of participants per country > 200; see Table S1 and supplemental materials on OSF/Github for power analysis output. We currently have labs in over 50 countries that have signaled intent to participate.	Analysis 1: Linear mixed-effects regressions for each outcome variable with random intercepts for individual and country and a single dummy-coded predictor indicating ingroup vs outgroup. Analysis 2: Random effect meta-analyses for each outcome variable (attitude, first-party allocations, third-party allocations), with effect size as outcome.	Analysis 1: A significant (positive) estimate of the ingroup parameter would indicate the overall (cross-cultural) presence of the MGE in our sample; a significant (negative) estimate would indicate the overall (cross-cultural) presence of the opposite of the MGE (i.e. outgroup favoritism [6]) in our sample; if there is no statistically significant effect of the ingroup parameter then we will conduct an equivalence test, following the two one-sided tests (TOST) rule (see analysis plan). Analysis 2: Report the percentage of countries in which (i) the null hypothesis of no MGE can be rejected and there is an effect in the expected direction, (ii) the null

				hypothesis can be rejected and there is an effect in the unexpected direction, and (iii) the null hypothesis cannot be rejected.
RQ2: Does the MGE systematically vary as a function of individual or cultural moderators?	Individual-level measures of permeability (H2.1a), trust (H2.2a), and self-esteem (H2.3a) predict the minimal group effect across three measures. Country-level measures of permeability (H2.1b), and trust (H2.2b) predict the minimal group effect.	Sample at least 200 participants per country; see Table S2 and supplemental materials on OSF/Github for power analysis output.	For H2.1–3a: Run a series of mixed-effects models, each one with a single moderator. For H2.1–3b: Correlate measure with the countries' means of our MGE measures.	For H2.1–3a: Report results from each; if a predictor has a significant, positive association with bias in the minimal condition, then we have evidence of a positive relationship between the predictor and the MGE; if there is no statistically significant effect of the predictor then we will conduct an equivalence test, following the two one-sided tests (TOST) rule (see analysis plan). For H2.1-3b: If the (positive) correlation is significant, then there is an association between the measure and the MGE; if there is no statistically significant effect of the predictor then we will conduct an equivalence test, following the two one-sided tests (TOST) rule (see analysis plan).

RQ3: Is there a relationship between the MGE and bias towards real-world groups?	We do not examine a specific hypothesis here but structure our analysis to test for a positive, negative, or no relationship between real and minimal group bias.	Sample at least 200 participants per country; see Table S3 and supplemental materials on OSF/Github for power analysis output.	Run three linear mixed effects models with real bias as outcome, with minimal group bias, real-world group, and their interaction as predictors.	If the MGE has an overall significant, positive association with real bias, then we have evidence of a positive relationship between the MGE and real bias; if the interaction of the MGE and group type is significant and positive, then we have evidence of a differential relationship between the MGE and different real-world groups; if there is no statistically significant effect of the predictor then we will conduct an equivalence test, following the two one-sided tests (TOST) rule (see analysis plan).
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Real-world group specification. We focus on two real-world groups: the family and the nation. As with minimal groups, we do not mean to imply that cultural conceptions of these groups are invariant across cultures. However, we do anticipate that these groups will be familiar and relatively salient to all participants in a way that maximizes comparability across societies. By contrast, many of the most studied groups in the social sciences, such as racial, ethnic, political, and religious groups, are considerably more variable across cultural contexts. Of course, with only two groups to compare minimal groups to, results from this study cannot be assumed to generalize to groups in general; thus, the answer that this study will provide to RQ3, concerning the relationship between real and minimal groups, will by necessity be only provisional. Given the specific opportunity of collecting data across many societies, we favored cultural comparability over other factors that could have influenced selection (such as choosing groups on the basis of status, similarity, immediate relevance, etc.).

The family is one of the most important real-world groups from birth through death, as well as a central place of lifelong socialization experiences. The nuclear family (parents and children) is widely recognized around the world as an important institution even when it is embedded in larger extended family systems⁵⁸, and orientation towards it is widespread⁵⁹. For this reason, we might not observe sufficient variation in favoritism towards the nuclear family to capture potential associations with minimal group favoritism. We therefore focus on the contrast between first cousins and an unrelated other. First cousins are a more distant family tie which we expect to vary more considerably in cultural importance²⁵.

Our second real-world group is nationality. In a cross-country context, the nation is a natural candidate to study because membership is mostly acquired by birth and most people have at least one nationality. Our primary analysis will focus on citizens of the country in which we collect data, tested in that country. We will elicit attitudinal and behavioral measures towards a participant's fellow citizen vs. a foreigner. Identification with the nation can have beneficial, welfare-enhancing consequences (e.g., by increasing tax compliance or supporting fellow citizens); on the other hand, it can lead to nationalism, which may foster wars or suppression of immigrants⁶⁰. We anticipate data from participants in approximately 50 nations around the globe, allowing us to provide generalizable estimates of effects.

Our inquiry into nationality should be set against the stage of several recent large-scale investigations of this topic. The first of these⁶¹ conducted trust games in 17 countries, finding ingroup favoritism in most of them despite some heterogeneity across nations, although that variation was not explained by the cultural moderators included in the study (including, relevant for the present study, a material security measure that included some factors overlapping with our institutional trust measures). A more recent investigation³⁰ conducted a prisoner's dilemma game in 42 nations that included a measure relating to our institutional trust items, and found ingroup favoritism in the large majority of nations; however, investigated moderators, again, did not reliably relate to ingroup favoritism. Despite that, there remained substantial heterogeneity in effect sizes for ingroup preference, ranging from $d = .05$ to $d = .44$. Interestingly, both of these papers did identify some moderators of overall levels of trust and cooperativeness but not of trust or cooperativeness towards the national ingroup. Placed against the backdrop of these findings, our proposed study will produce another large dataset with new dependent measures, explore some potential moderators not included in that past work, and allow us to directly compare minimal group, national, and familial biases.

Dependent Measures. For each of the three group conditions (minimal, nationality, family), we will employ both attitudinal and incentivized behavioral measures of ingroup bias. For the family condition, we will use a hypothetical version of the incentivized task because of the infeasibility of its implementation (i.e., having family members participate and matched for payments; see below). We also include an embedded experimental test of the potential difference between hypothetical and incentivized tasks, described in detail below. We briefly describe each measure below; further details are provided in the supplemental materials.

Attitudinal task. Individuals will make eight judgments of statements describing liking for and identifications with the groups (four items relating to each group) using 7-point standard rating scales. Although past work conceptually and empirically distinguishes evaluations and identifications, these items reliably cohered in our pilot study ($\alpha = .84$) and all loaded on a single component when submitted to a Factor Analysis (see Pilot 1 sub-section, below, for further details). Our primary analysis will thus focus on a single aggregate based on these four items, though they can be treated separately in supplemental analyses. All items will be coded such that positive scores indicate greater identification with or positive evaluation of the group. Our primary analysis will involve a single ingroup bias score constructed for each target group comparison (minimal group, nationality, and family) by subtracting the mean of the outgroup items from the mean of the ingroup items. The full set of items is included in the supplemental materials.

Allocation task. In the behavioral allocation task, based on the Dictator Game (DG⁶²), individuals will make three allocation decisions for each of the target groups (minimal group, nationality group, and in hypothetical form the family). They allocate a sum of money between (i) themselves and an ingroup member, (ii) themselves and an outgroup member, and (iii) an ingroup and an outgroup member. The first two allocation decisions (potentially) involve a personal cost, and the difference between them will be used as a composite measure of first-party ingroup bias⁶³ for each target group. The third allocation decision represents a measure of third-party ingroup bias, again for each

target group; the value of the third-party measure is that it removes self-interest from the decision such that it can still detect ingroup favoritism even in contexts in which self-interest dominates in first-party tasks, a potentially important consideration given variability in average rates of DG giving across cultures⁶⁴.

One aspect of these design choices is that we do not have a baseline in which participants allocate between themselves and an unaffiliated other. Although in theory useful, we note that both the large-scale meta-analysis from Balliet and colleagues⁶ and a recent large-scale investigation of national ingroup favoritism³⁰ found that behavior towards unaffiliated strangers was qualitatively similar to behavior towards outgroup members, and most past evidence similarly suggests that the MGP is primarily driven by ingroup positivity and outgroup neutrality^{19,65,66}. The notion of an unaffiliated stranger is also somewhat strained in the context of several of the groups we employ. For example, in the case of the family, an “unidentified stranger” is, at least to a straightforward intuitive reading, by definition an outgroup member. Most centrally, our focus here is squarely on a relative matter, the tendency to prefer ingroups to outgroups. This focus is reflected in our analytic strategy described below, and is in our estimation best served by having measures that also focus there.

Task Incentivization and Payment Procedure. In the minimal group and nationality conditions, participants are (accurately) informed that their decisions may have real financial consequences for their own and other participants’ earnings. Following established methods in experimental economics to calculate and adjust for local purchasing power⁵⁴, stake sizes will reflect purchasing power of local participants, so that, in real terms, the possible real earnings will be similar across subject pools. To facilitate this, each allocation task will be done in terms of 20 points with point values fixed to local currency to equate the purchasing power to approximately US\$1.00 per point (i.e., participants can allocate an amount ranging from US\$0–20). Because we will only pay out every tenth participant (see below) in the role of either an allocator or receiver, the average bonus payout per participant will be ~US\$1, with the average bonus payout for the randomly selected participant at ~US\$10.

It is not feasible to involve real incentives in the allocation tasks involving family members due to the challenges associated with locating and paying out those individuals (i.e., non-participant family members). Given both the implausibility of any claim to involve real allocations in the family task, and our desire to avoid deception following standard practices in economics⁶⁷, the family condition will be (again accurately) described as hypothetical. However, because this deviation from the procedure in the other conditions could affect results, following the lead of a recent paper using incentivized tasks in the context of national ingroup bias³⁰, we build a test of the effect of real versus hypothetical incentives into our design. In this embedded experiment, we will randomly allocate individuals either to the standard (incentivized) version or to a version in which all economic games are explicitly and accurately described as hypothetical, with the rest of the design unchanged. Both arms of this intervention will be conducted in two societies that we will select based on providing substantial differences in GDP per capita as well as in permeability, and that are well-represented in our participating labs so that we can implement this experiment without compromising our main data collection targets. We currently propose to do this in the USA and either Brazil or India. These countries were chosen for having a high (USA: ~\$69.3K) and relatively low GDP per capita (Brazil: ~\$7.5K, India: ~\$2.3K), and because they differ along other metrics of interest (e.g., group permeability) such that they provide a reasonably broad test of the role of incentivization. We will ensure a minimum sample size of 200 in each arm of this intervention (in addition to our normal data collection targets for these countries). This method allows us to test for behavioral differences while still being able to use the incentivized allocation task as part of our main sample.

In addition, our standard design will likewise inform the question of incentivized vs. non-incentivized tasks given that we will examine how the non-incentivized attitudinal measure and the incentivized

allocation tasks (in the MGE and national condition) relate to each other; if the pattern of relationships is similar for the non-incentivized attitudinal measure and the non-incentivized allocation tasks (in the family condition), it will provide additional evidence that the family task is patterning similarly despite the lack of incentivization.

Thus, our study contains six incentivized allocation decisions in a 3 (contrast: self vs. ingroup, self vs. outgroup, ingroup vs. outgroup) $\times$ 2 (group: minimal or nationality) within participants design. To determine the payment a participant is eligible for, each participant will be randomly assigned the role of *either* an “Allocator” *or* a “Recipient.” One of their six allocation decisions will be randomly selected to determine the potential payout. Allocator payouts will be determined according to the money they kept for themselves in one of their randomly selected first-party allocation decisions. Recipient payouts will be determined according to a matched decision of a randomly matched Allocator (i.e., they will be matched with another participant who completed the study, and will be eligible for the amount given by that Allocator based on their group membership information). This procedure – only paying one out of several incentivized tasks – follows a common practice in experimental economics, which aims to mitigate confounding due to income effects (i.e., that the financial consequences of earlier allocation decisions spill over to subsequent decisions) and appears to be at least as effective in incentivizing participant behavior as “all-pay” approaches⁶⁸. Participants will be fully informed as to this procedure. In addition, to simplify the implementation of our task in a broad range of settings around the world, we will pay out a randomly selected subset of participants, in particular every tenth participant. This approach allows us to offer high financial incentivization (conditional on being selected for payment) compared to an “all-pay” design without increasing study cost.

This has two additional advantages compared to the all-pay design. First, due to data protection regulations particularly in European countries, participants being tested online have to complete an unlinked survey after the experiment and provide personal information to facilitate the payment. Low anticipated earnings paired with a burdensome payment procedure may potentially weaken financial incentives in an all-pay design. Second, in an in-person lab setting it will shorten the time necessary for payment because only a subset will need to be paid.

Task Order. We have three group conditions with each condition containing both attitudinal and allocation tasks. Because the primary focus is the minimal group paradigm, it will always come first. Subsequently, we will randomize the order of the family and nationality conditions across participants. The order of the attitudinal task and allocation task will be counterbalanced across participants but kept constant within participants. The order of items will be randomized within each task. After the tasks for all three groups have been completed, participants will fill out the additional explanatory measures (e.g., permeability) and demographic items.

Explanatory Measures. All measures are presented in full in Supplementary Materials.

Moderator 1: Permeability. Our aim is to demonstrate the broad applicability of the idea that permeability is an important determinant of the MGE. Given that there are several reasonable ways to capture this construct, rather than zooming in on one specific operationalization, we instead draw on several established indicators. These indicators range from anthropological measures of kinship intensity, survey measures of attitudes towards nuclear family, relational mobility, and measures of individualism-collectivism (see table SI.1 in the supplementary materials for an overview). The common theme is that all indicators tap into some aspect of permeability, i.e., the embeddedness of people in existing groups and the ease of forming new ties with people. The kinship aspect of permeability on the country level will be measured by the Kinship Intensity Indicator (KII²⁵), which captures the strength of kinship as proxied by the presence of kin-marriages, polygyny, unilineal descent, communal residence rules, and co-residence of extended families. This

measure is based on ethnographic records and aims to capture the strength of kinship before industrialization or European contact; thus, it is not influenced by contemporary factors such as current GDP or quality of formal institutions. Countries high in KII are low in permeability because kin networks predominate over other forms of social classification. We predict that those with comparably weaker KII will show a stronger MGE due to the potential trade off between minimal and well established social ties. To gain insight into the role of the family at the individual-level, we'll use the survey-based attitudinal Family Ties measure of Alesina and Giuliano³⁸ that captures the strength of nuclear family ties.

A second country-level indicator of permeability that we use in our analysis is the updated version of Hofstede's³⁷ measure of individualism-collectivism provided by Minkov and Kaasa⁶⁹. Individualism refers to the extent to which people feel independent, as opposed to being interdependent members of larger wholes. Because members of individualist cultures can more freely move between groups and social identifications, it is closely linked to permeability. As the individual-level implementation, we will use a portion of the individualism-collectivism scale proposed by Sivadas and colleagues⁷⁰. We will focus on the "vertical collectivism" and "horizontal individualism" dimensions, and calculate the scores by subtracting the mean of the three horizontal individualism items from the mean of the four vertical collectivism items (and subsequently standardizing them). The so-constructed individualism-collectivism item reflects the dichotomous nature of permeability and mirrors the country-level individual-collectivism dimension described above. We do not include "Horizontal collectivism" and "vertical individualism" because, rather than permeability, they emphasize well-being of others and competition, respectively. However, as we will collect the entire scale, our dataset will allow other conceptualizations of this or related components of individualism-collectivism⁷¹. As a third individual-level measure we will employ the Relational Mobility Scale³⁶, which correlates with a wide range of socially relevant outcomes including interpersonal trust.

We predict that higher permeability captured by these indicators will be associated with a stronger MGE. The rationale for this prediction is that in societies with high permeability it is more common for people to flexibly move in and out of new groups, thereby shaping psychology towards subtle cues of group membership. This contrasts with low permeability societies in which people are embedded in more stable natural groups such as extended kinship networks. In such cases we propose that individual psychology is less geared toward picking up minimal cues of potentially transient group membership and that people are more reluctant to form new social ties. Of course, the several indicators we describe above are not fully overlapping and may not show the same relationship with the MGE; however, if they do it would lend increased support to the broader framework proposed here.

Moderator 2: Measures of trust. We employ two distinct measures relating to trust. First, our measure of *institutional trust* will be a composite based on three items from the World Value Survey⁷² relating to trust in government, courts, and police. We will use the country means as our societal-level variable (based on the joint dataset of the European and World Value Survey). The individual-level measures will be constructed in the same way except that we center it at the individual rather than national level based on our participants' responses. We predict that higher institutional trust will be associated with a stronger MGE. The reason is that well-functioning institutions serve as arbiters that mitigate risks associated with the rapid formation of new and short-term group ties such as those implied by the MGE.

We also include a measure that we term trust in strangers based on a widely-used question from the WVS focusing on trust in people one meets for the first time. The country mean of this item, as well as the individual response to this item, will serve as our country and individual-level indicator respectively. We hypothesize that this trust in strangers measure is positively associated with the

MGE because the perceived risk of forming new groups with unfamiliar people is lower, and, consequently, individuals who trust unfamiliar people are more willing to form cooperative ties with newly encountered others such as those in minimal groups.

Moderator 3: Self-esteem. Self-esteem will be measured using the 10-item Rosenberg Self-Esteem Scale (RSE⁷³), the most widely used measure of personal self-esteem and one that has also been used extensively across cultures⁷⁴. We predict that higher self-esteem will be associated with a stronger MGE¹³. Whether such a relationship also holds for real-world groups is an important additional question we will examine.

Moderator 4: Beliefs and status indicators. A powerful recent approach to understanding the origins and magnitudes of intergroup bias towards different groups argues that we gain clarity by distinguishing between status-indicative and belief-indicative groups⁴⁹. Status-indicative groups are those that are well-differentiated by positions in a vertical status hierarchy; belief-indicative groups are those that are well-differentiated by patterns of shared customs and beliefs. These authors argue that preference for status-indicative groups is a manifestation of a more general preference for higher status, while preference for belief-indicative groups is a manifestation of assumptions about meaningful patterns of similarity such as shared values. Of course, logically speaking, one should not make any substantive inferences about the status or beliefs of a randomly assigned social identity such as a minimal group, because random assignment should ensure that ingroups and outgroups are approximately equivalent in these dimensions. That said, people clearly make a large number of at least implicit inferences about minimal ingroups and outgroups (e.g., that they have more positive traits) that are implausible for a similar reason¹³. Thus, it is possible that participants project a belief in higher status or greater interpersonal similarity onto ingroups and that this in turn drives ingroup favoritism. These factors are also particularly likely to be relevant for nationality, as it is plausible that people have assumptions about their national ingroup's status as well as about the extent to which they share beliefs with national ingroup members. Thus, these factors could moderate national ingroup bias. To assess these constructs, we will employ two strategies. First, we will assess how informative participants believe each group membership in our study (i.e., minimal, national, and family) is concerning the social status and beliefs and values of a member of that group. This item concerns the inferential richness of the category itself. Second, we will assess how the respondent considers themselves in relation to the group in question along the dimension of beliefs and status. That is, we assess whether they consider themselves to share values and beliefs with ingroup members and outgroup members, and do they consider themselves higher or lower in status than a randomly selected member of their ingroup and outgroup. We will ask these questions with respect to all target groups, and then construct a difference score reflecting the relative ingroup advantage in beliefs and status for each kind of group, which can then be used as predictors of ingroup bias, as detailed in supplemental materials.

Demographic questions. While we will strive to collect a full set of demographics in all research sites, local norms and regulations will result in some differences in what we are able to elicit. In any case, our goal is to elicit basic demographics including age, gender, political orientation (in a subset of countries for which local ethics rules allow this information to be solicited), work situation (e.g., full-time employee, unemployed, and student), highest education level, number of siblings, marital status, region of residence and the place where they grew up, relative income at age 16, religion, household size, whether they are a member of a cultural majority and/or minority, and whether they are familiar with the MGP. In countries in which ethics protections make it feasible, we may also collect data on race/ethnicity. We will also ask all participants whether they have a first cousin (since attitudes and behavior towards first cousins is one of our primary DVs); this question will allow us to ensure that all results hold when excluding participants without first cousins. We consider these as exploratory variables and do not present primary hypotheses with respect to them,

though we do note that one recent multinational study found positive relationships between conservatism and national ingroup bias³⁰.

Translation. Our approach to translation is based on the Psychological Sciences Accelerator guidelines for a translation and back-translation procedure⁵¹ that was implemented in other studies in PSA^{75,76}. A similar procedure was implemented in another study by this team⁷⁷. First, two bilingual translators in English and the target language will independently translate the material from English to that language. Then, together with the project translation coordinator, they will create the first version of the translated material (Version A). In the second step, two bilingual translators will back-translate the material independently from the target language to English. Similar to the first step, they will integrate the material into a single back-translated version (Version B). The team will then verify that the content has been preserved across this procedure by comparing A and B versions. After this discussion, at least two non-academics fluent in that language will review the target language version to determine if any additional areas are unclear; if so, final adjustments including integration of any necessary cultural adjustments will be made by the translation team. In some cases, this procedure will be used to produce a language version that will be used in multiple countries (e.g. Spanish), though it will still be reviewed by native informants of that country to ensure that it is fully appropriate.

Sampling plan

Power analysis

We conducted a series of power analyses to determine an appropriate number of both countries ($K = 30, 40, 50$) and participants per country ($N = 100, 150, 200, 250$). To assess the expected power, we set $\alpha = .05/3$ (as each hypothesis is tested three times, once for each dependent variable). We used the *simr* package in R to run 1,000 simulations of the planned mixed-models specified in the “Analysis Plan” across a range of parameters. The model parameters varied across RQ1–3 and were estimated from the literature and/or Pilot Study 2. See power analysis in Supplementary Information for power curves for all three research questions across these parameter spaces. The code to reproduce the power analysis is available at

<https://github.com/adamrkenny/psa008/tree/main/power> and https://osf.io/b5dqf/?view_only=bd0d6b35b9f54455a531fcff53c62fde.

For RQ1, we considered the effect of minimal group (in-group vs out-group) on each of the three outcomes. Based on the past literature we identified an expected minimal group effect size of $d = 0.19$. The effect size was based on that reported for experiments lacking mutual independence in Balliet et al⁶ ($d = 0.19$), which is about half the effect size of bias in allocation tasks for artificial groups reported in Lane⁸ ($d = 0.37$). Thus, we tested a beta of 0.10 (variance = 1) which approximates the smaller effect size to ensure we are adequately powered, though we also tested smaller (beta = 0.05) and larger (beta = 0.15) values. The minimal group effect was assumed to vary across countries, so we allowed random slopes for the minimal group effect, estimated at several levels of slope variance (0.01, 0.02, 0.03). At $N = 200$, we achieved power > 95% for beta = 0.10 when the slope variance is ≤ 0.02 and $K \geq 40$.

For RQ2, we considered the effect of a continuous moderator (e.g., self-esteem, one of our six moderators) on the minimal group effect. We conducted a sensitivity analysis, in which we varied the beta for the moderator (beta = 0.05, 0.10, 0.15) and the random slope for the moderator within country (slope variance = 0.01, 0.02, 0.03). The betas for the moderator were based on the observed correlation of .11 between self-esteem and the minimal group effect in our Pilot Study 2 data. Again, at $N = 200$, we achieved power > 95% for beta = 0.10 when the slope variance is ≤ 0.02 and $K \geq 40$.

For RQ3, the primary effect of interest is the strength of the relationship between the minimal group effect (MGE, as predictor) and bias towards real-world groups. There is little past work upon which to estimate the strength of this relationship, so we used our Pilot Study 2 as a guide ($\beta = 0.13$). To estimate the effect of MGE in a model that also included a predictor for group type (family vs. national) as well as the interaction between group type and MGE, we contrast coded group type such that the β for MGE reflects its marginal effect collapsed across group type. In a model with contrast codes of -1 and 1, the estimated β for MGE would be approximately half of its simple main effect; we therefore tested values for the β of MGE of 0.025, 0.05, and 0.075. We included random slopes for MGE within country (slope variance = 0.01, 0.02, 0.03). At $N = 200$, we achieved power > 95% for $\beta = 0.075$ when the slope variance is ≤ 0.02 and $K \geq 30$.

In sum, these simulations suggest that 200 participants per country across 40 countries gives us sufficient power (> 95%) to meet research goals in most cases. Considering the desire to power for various supplemental analyses as well as future analysts, we settled on a target minimum of 200 participants per country. Given the number of countries enrolled already as well as logistical discussions held with various recruitment sites to date we should be able to meet and even exceed these values (see Table 2).

As a supplemental approach we also adopted a meta-analytic strategy in which we employed a random effects meta-analysis of the country-level effects. As above, we varied K and N to explore power across a range of reasonable assumptions. The simulations also suggest that with $N \geq 200$ and $K \geq 30$ we achieve > 95% power under expected levels of heterogeneity.

Recruitment procedures. Our project has been approved to be run with the already existing network of labs that constitutes the Psychological Science Accelerator⁵¹ (PSA; visit <https://psysciacc.org/> for details of the network and general approach). We will recruit from any willing lab in the PSA and will also make targeted efforts to recruit labs that capture variation in the predictors at the country level that we are most interested in (e.g., variation in permeability; see above). As indicated in the power analysis above, we anticipate asking participating labs to enroll at least 100 participants each (we will have multiple labs from a given country, to ensure we reach the required sample of 200 participants per country following expected exclusions). We completed an initial interest survey that identified over 250 interested data collection teams planning to provide samples from over 60 countries across the globe, who all indicated their intention to join and to contribute and all who indicated their ability to contribute at least 100 participants (see Table 2). For comparison purposes we note that the first PSA project⁷⁵ enrolled 11,570 participants from 45 countries. Several more recent PSA projects have had similar success, such as studies with 27,502 participants from 45 countries⁷⁸ and 27,989 participants from 87 countries⁷⁶.

Table 2. Interested collaborator sample characteristics. Labs included in this table have indicated their interest in joining the project.

Continent	UN Geo Subregion	N samples	N Nations / Regions
Africa	Northern Africa	2	1
	Sub-Saharan Africa	4	2
America	Latin America / Caribbean	11	5
	North America	69	3
	Eastern Asia	16	4

Asia			
	South-Eastern Asia	9	5
	Southern Asia	16	4
	Western Asia	38	8
Europe			
	Eastern Europe	30	8
	Northern Europe	40	5
	Southern Europe	30	7
	Western Europe	38	5
Oceania	Australia and New Zealand	18	2
Total		321	59

With a project like this, ensuring sampling diversity and addressing popular sample imbalances are important. In particular, given the global distribution of labs as well as the composition of the PSA network, we anticipate enrolling a larger number of labs from WEIRD societies than non-WEIRD societies. We can address this potential issue in several ways. First, we note that the labs planning to provide data already reflect a wide range of world regions and cultures, including approximately 30 nations from outside the WEIRD context, amounting to approximately 20% of our current intended enrollments. Second, prior to data collection, we will make targeted efforts to recruit, and if necessary fund, additional labs to ensure that we have representation of all major world regions. These efforts already suggest that this will be not just the largest but the most diverse and world-representative study of intergroup biases and especially minimal group biases yet conducted. To support this claim we have examined the distribution of expected labs along several dimensions of cultural and economic variation and found that our current plans should allow us to include a degree of diversity that begins to mirror that found in the population of countries for which data is available (details and accompanying figures in Supplemental Materials). Beyond these recruitment and sampling considerations, we will also take several analytic steps to ensure that a possible sampling imbalance is taken into account when we report conclusions and display primary results (see below).

Labs will focus their efforts on recruiting participants who are citizens of the country the lab is based in, though we will verify via our demographics questions and exclude non-nationals. Labs will be able to implement the study either in-lab or via online delivery, but in both cases will draw on local participant pools rather than any national-level participant pool or crowdworker service such as Mechanical Turk or Prolific. We made this decision to ensure that our approach respects the regional nature of our sampling plan with labs based within countries that may vary in interesting ways; the use of national panels would reduce this dimension of variability. The exception to this is the use of ZPID samples described above, which will be used in a targeted manner to increase representativeness in specific countries or regions of interest.

Exclusion Criteria. For the main analyses, we will exclude data from participants for four reasons. First, we will exclude the data from those who fail more than one of the three attention check

questions (“Please select ‘Somewhat Disagree’ on this question”; “Do you remember which group you were assigned to in the previous section?”; “What is the sum of 10 and 8?”), as this might indicate the participant was providing careless responses throughout the questionnaire. During the pilot, 30 (7.5%) participants answered at least one attention check incorrectly (see “Pilot Data” for more details of the pilot), but previous large-scale projects have had considerably higher exclusion rates due to failed attention checks⁷⁸, so we consider this reasonable. Our sampling plan will be able to account for potentially higher rates of exclusions, as indicated above.

Second, we will exclude the data of those who reveal familiarity with the MGE, as previous knowledge of minimal groups might influence responses to our focal dependent measure. Specifically, we will exclude those who respond “Yes” to the question “Are you familiar with the Minimal Group Effect (MGE)?” and whose open response to the follow-up question asking for more information (“What do you know about the Minimal Group Effect (MGE)? Please describe in 1–2 sentences”) demonstrates a basic understanding of the effect. In the pilot, 4 (1%) participants indicated “yes” to the familiarity question, 3 of which subsequently demonstrated understanding (e.g., “People will show ingroup preferences even if the ‘ingroup’ is based on a minimal piece of information/affiliation.”), whereas one did not (“I have only heard the name”). Two members of the core project team (the authors of this submission), assisted by data collection labs with the relevant language expertise, independently assessed whether the open response demonstrates understanding, with a third adjudicating instances of disagreement between the first two.

Third, we will exclude participants who are not a citizen of the country in which they are tested. Effort will be taken to avoid sampling non-citizens, but our demographics items will allow us to confirm this and exclude based on this criterion. Fourth and finally, we will exclude the data of those who experience any technical problems (e.g., the survey website crashes), as interruptions might affect respondents behavior.

Analysis Plan

Overview. The core of our analysis will be to test our three main research questions. In addition, data collected for this study will allow us to conduct exploratory analyses shedding further light on the minimal group paradigm and real-world groups, as well as the link between attitudinal and behavioral/incentivized dependent measures of intergroup bias. Our project will ultimately produce a rich openly available dataset that will also allow other investigations to test additional questions. We detail our proposed statistical models below. All analyses will be conducted in R ⁷⁹. Formulas provided below adopt the *lme4* package notation for mixed (multilevel) models⁸⁰. As each hypothesis is tested three times, alpha is adjusted to .017 (i.e., .05/3). We focus on the main analyses here; additional analyses including robustness checks are reported in the SI.

When analyses call for a mixed model approach, we will initially fit models with random intercepts and random slopes for each predictor. If convergence errors arise, we will first employ the alternative optimizers from the *optimx* package for R ⁸¹. If this does not resolve the issue, we will trim random slopes starting with higher-order terms until convergence is achieved.

Interpretation of null results: Where an analysis returns a non-significant result for a fixed effect (i.e., p -value > our specified alpha, generally .05/3), we will conduct an equivalence test following the two one-sided tests (TOST) rule⁸², using the parameters package⁸³. This allows us to determine whether our observed result is statistically significantly lower than a smallest effect size of interest. We set this value, which becomes the lower and upper equivalence bounds for the equivalence test, to $\Delta L/M \pm 0.10$. This corresponds to the standardized effect size of the MGE in RQ1 and the standardized relationship between predictor and an outcome in RQ2 and RQ3. We will conclude statistical equivalence if both p -values are smaller than $\alpha = .05/3$. Based on power analysis using the

above equivalence bounds and alpha in the package TOSTER⁸⁴, in all cases we achieve 95% power with a total sample size of 1674, which we anticipate exceeding by a substantial margin (see Sampling Plan, above).

RQ1: Does the MGE hold across diverse societies? First, we will perform three linear mixed-effects analyses, using the package lme4⁸⁰. The outcomes will be (i) attitude, (ii) decision in the first-party allocation tasks, and (iii) decision in the third-party allocation task. Group (with two levels, in-group and out-group) will be the fixed effect. The model will include the random intercepts of individuals and countries. The model specification is: attitude/decision ~ group + (1 | id) + (group | country). This specification provides an alternative way of assessing whether there is a difference between ingroup and out-group treatments, taking into account individual variation in either attitudes or allocation. We will then compare these models with ones excluding the country component, to see whether the MGE differs by country. We will also run further robustness checks (see section “Robustness checks” below).

As another means of addressing this question, we will assess whether or not the MGE is present across countries via a meta-analytic approach. We will calculate three effect sizes for each country, representing the difference between in-group and out-group (i) attitudes and (ii) decisions in the first-party allocation tasks, as well as (iii) the decision in the third-party allocation task. The first two are represented by Cohen’s d , the third by Cohen’s d_z . We will report the weighted mean of the effect size, using a random-effects meta-analysis with country as random effect (using the *metafor* package⁸⁵). We will then report the percentage of countries in which we can reject the null hypothesis of no MGE (and find an effect in the expected direction and in the unexpected direction), as well as the percentage of countries in which we cannot reject the null hypothesis; we will also report a subgroup analysis based on world region, including a forest plot to aid in visualizing primary results. We do not state, a priori, the percentage of countries in which we find an effect that would suggest the MGE is ubiquitous but we expect to find it in all or nearly all countries.

Additionally, we will then check variability in the country-level effect sizes by reporting three heterogeneity estimates from the meta-analysis: Q , I^2 , and τ . As the study design is kept constant across countries (except for translations), heterogeneity captures how much of the variation is due to an aspect of the country vs. measurement error. We will also check for heterogeneity across settings (online vs. lab) by including it as a moderator, as well as across laboratories by including it in the random structure and reporting the within-cluster (laboratory within country) heterogeneity.

Note that the approaches described above will allow us to examine the MGE in each country independently as well as across regions and across the study in its entirety. They also will provide estimates of cross-society heterogeneity and overall prevalence rates of the MGE across societies. These analyses, therefore, will allow us to avoid over-reliance on a single pooled effect, which could be biased if, as anticipated, our sample is unbalanced towards WEIRD participants.

RQ2: Does the MGE vary as a function of cultural or individual moderators? For RQ2 there will be three dependent measures of bias towards each target group: the difference between ingroup and outgroup attitudes (“attitudinal bias”; range: -6 to 6), the difference between ingroup–self and outgroup–self decisions in the allocation task (“first-party bias”; range: -20 to 20), and the allocation decision in the third-party ingroup–outgroup allocation task (“third-party bias”; range: 0 to 20 points). As noted above, the attitudinal and first-party allocation measures will involve subtracting ratings of the outgroup from ratings of the ingroup to create an index of ingroup bias. The third-party allocation task is a direct measure of ingroup preference as it involves a relative allocation of ingroup versus outgroup and so can be used directly.

First, we will analyze the MGE at the individual level, followed by the country level. For the individual-level analysis, we will run a series of linear mixed-effects models. Each model will include one of the following individual-level moderators as a fixed effect: for permeability (Hypothesis H2.1), these are family ties, Sivadas et al.'s⁷⁰ individualism–collectivism, and relational mobility; for trust (Hypothesis H2.2), these are trust in strangers and institutional trust; for self-esteem (Hypothesis H2.3) it is the Rosenberg Self-Esteem Scale⁷³. See Table SI.1 in the supplemental materials for an overview of moderators. The model will include the random intercept of countries. The three measures of bias will be analyzed separately. For each measure, the model specification is: $MGE \sim \text{predictor} + (\text{predictor} \mid \text{country})$.

Our cross-country analysis will consist of testing whether the ex-ante hypothesized country-level predictors are significantly correlated with the countries' means of our MGE measures as well as providing visualizations via plots of each core predictor against the MGE per country. Further, to investigate the robustness of these relationships, we will conduct OLS regressions that will control for a set of biogeographic covariates and address spatial and cultural autocorrelation (see supplementary materials). The country-level predictors are listed in Table SI.1. To test Hypothesis H2.1 (permeability), the core predictors are the Kinship Intensity Index, and Minkov-Hofstede individualism. In addition – to check for consistency – we will report on the individual-level moderators aggregated to the country-level (family ties, Sivadas et al.'s individualism collectivism, and relational mobility). For Hypothesis H2.2 (trust) we will use country aggregates of trust in strangers and institutional trust (based on the representative data of the World and European Value Survey). We will not conduct a country-level analysis for H2.3 (self esteem), because we are unconvinced that the concept of nation-level self-esteem is useful. In addition, closely following a past approach³⁰, we will conduct a supplementary analysis which tests whether people in culturally more similar countries are also more likely to behave similarly in the MGE. This analysis will draw on previously developed and validated cultural indices, in particular genetic distance⁸⁶ and cultural distance⁸⁷; this allows us to examine cultural concordance in ways that go beyond a WEIRD / non-WEIRD dichotomy (see the supplementary material for details).

RQ3: Is there a relationship between the MGE and bias with real-world groups? For RQ3 the dependent measures will be our indices of ingroup favoritism with respect to the nation and the family. We will assess whether the magnitude of bias towards the minimal group predicts bias towards the nation and/or family. The response-level model will regress those real-world bias scores on MGE, with MGE and real-world group (with two levels, nation and family) as fixed effects. The model will include the random intercepts of individuals and countries, as well as random slopes of countries for each fixed effect but not their interactions⁸⁸. The three measures of bias will be analyzed separately. For each measure, the model specification is: $\text{real_bias} \sim MGE * \text{group_type} + (1 \mid \text{id}) + (MGE + \text{group_type} \mid \text{country})$. In an exploratory analysis described in full in Supplemental Materials, we will also incorporate our measures of beliefs and status, by examining whether these items predict the strength of ingroup bias in a similar way with respect to the family, the nation, and the minimal group.

Hypothetical vs. incentivized decisions. While the MGE and the national conditions use financially incentivized choices, as discussed above this is not feasible in the family conditions. To gain insight into how financially incentivized choices relate to hypothetical choices we also implement an embedded experiment examining the effect of hypothetical vs incentivized decisions in the MGE and national conditions, deployed in two countries (we anticipate that this will be the USA and either Brazil or India). To test for the effects of incentivization we run a model for each allocation task with condition (minimal, national), payment (hypothetical, incentivized), and their interaction predicting bias. If neither the effect of payment nor the interaction is significant, it suggests that the presence or absence of incentives did not appreciably change bias and lends assurance that the hypothetical family condition is a valid measure. However, if we find evidence that hypothetical vs incentivized

differ then we will carefully discuss the implications and limitations for the non-incentivized family condition. These limitations are most relevant to RQ3, as it looks at the relationship in bias across conditions; the validity of such comparisons will be threatened if incentivization appreciably affects measured bias.

Robustness checks. We will perform a series of robustness checks. For all RQs, we include the following demographic characteristics as predictors as they constitute standard controls: age (continuous), gender (Man, Women, Non-Identifying/Undisclosed), income (ordinal scale), and in the subset of countries where data is available, political orientation (ordinal scale). Additional checks apply to RQ1 only. First, we check for an effect of the experimental setting as online or in-lab, by including setting as a predictor. There is the possibility that an effect of setting might be confounded by country, depending on the distribution of settings within and between countries. Second, we check for an effect of familiarity by: (i) running the main analysis on the sample that does not apply our second exclusion criterion and (ii) running another model with “familiarity” (“yes”, “no”) as predictor. We include this model because familiarity with the MGE could reasonably have an effect on decisions, an effect we wish to test. Third, we check for an effect of attention by (i) running the main analysis on the sample that does not apply our first exclusion criterion and (ii) running another model with “attention” (“correct”, “fail”) as predictor (note, this sample will include those who demonstrate familiarity if the previous robustness check shows no effect of familiarity; the sample will exclude those who demonstrate familiarity if the previous robustness check shows an effect of familiarity). We do this as a fail-safe in case the exclusion criteria remove too many participants. Further, as indicated in our supplemental materials, we incorporate several additional supplementary analyses as further robustness checks. These involve the incorporation of recently developed measures of cultural and genetic distances between countries.

Data availability

All data, including that related to the first and second pilot study, are openly available at <https://github.com/adamrkenny/psa008> and https://osf.io/b5dqf/?view_only=bd0d6b35b9f54455a531fcff53c62fde. Upon acceptance of a Stage 2 manuscript all study data will be made publicly available at those same repositories.

Code availability

All code, including that related to the first and second pilot study, are openly available at <https://github.com/adamrkenny/psa008> and https://osf.io/b5dqf/?view_only=bd0d6b35b9f54455a531fcff53c62fde. Upon acceptance of a Stage 2 manuscript all study code will be made publicly available at those same repositories.

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Author contributions

XY, JS, and YD designed the initial study. KS has served as the project manager with help from SCL and AE. ARK has served as the lead data analyst. GP and BG have served as ethics coordinators. ID has served as the translation coordinator. Author order is not final. All named authors of this submission approved the manuscript and have or will contribute to translation and data collection.

Competing interests

Katharina Fellnhofer is the owner of the Research and Innovation Management group GmbH. The authors declare no other competing interests.