Strangers Like Me: Does Group Affiliation Serve as a Noisy Signal of Agents' Types?

J. Braxton Gately*
School of Accounting, Finance, Economics, and Decision Sciences
Western Illinois University

Kushal Lamichhane
Department of Economics,
University of Arkansas

Ashley McCrea
Department of Economics,
University of Exeter

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Abstract

The assimilation of immigrants into new societies is a critical factor for the immigrants' success and integration. However, this process can often be hindered by the natives' concern over cultural differences between the immigrants and the host population. These cultural differences extend the concerns to potential challenges in coordination within and between different groups in society. In two pre-registered studies, we explore how group formation impacts the ability of people to coordinate effectively in a complex environment where people have very limited information about who they interact with and the beliefs that people have over how well groups aid coordination. In study 1, we use an extended version of the HFIG game in Gately et al. (2023), adapted to include a group formation mechanism, to investigate these dynamics. The data reveals a surprising result. In this complex coordination environment, groups do not significantly aid coordination. However, we do find that, when given the choice, participants choose to join groups that signal a type that is closer to their own. Given the fact that people joining groups similar to their type suggests that people do think groups help coordination, we use study 2 to investigate people's beliefs over the effectiveness of groups. Interestingly, while people generally overestimated the effectiveness of groups, their beliefs about the probability of coordination without groups were relatively accurate.

JEL: C71, C92, D90, J15
Keywords: Coordination, Immigration, Group Formation, Inaccurate Beliefs, Group Identity

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1 Introduction

The assimilation of immigrants into new societies is a critical factor for the immigrants' success and integration. Successful immigration is not only important for the immigrants themselves but also has a significant impact on the social and economic situation in the host country. Assimilation can lead to reduced societal conflict (Konya, 2007), improved economic outcomes for immigrants (Cai and Zimmermann, 2020; Carillo et al., 2023; Kuhn and Sweetman, 2002; Piracha et al., 2023), greater participation in cultural activities and civic participation (Aleksynska, 2011; Bertacchini et al., 2021) and improves the overall well-being of both immigrants and native populations (Akay et al., 2014, 2017; Howley and Waqas, 2024). A crucial aspect of this assimilation process is the readiness of the native population to accept newcomers. However, acceptance is often hindered by concerns amongst the native population, through fears of job competition, effect on crime and general societal impact (Hainmueller and Hopkins, 2014; Mayda, 2006).

A particularly strong fear is the concern over cultural differences between the immigrants and the host population (Card et al., 2012; Dustmann and Preston, 2007; Mayda, 2006; McLaren and Johnson, 2007). These cultural differences extend the concerns to potential challenges in coordination within and between different groups in society. The success of any society hinges on its ability to coordinate effectively internally and externally. In this context, across two pre-registered studies, we explore how group formation impacts the ability of people to coordinate effectively in a complex environment, where the different groups within a society function as signals of the characteristics of their members and influence the choices individuals make regarding group affiliation.

Our study utilizes an extended version of the HFIG game in Gately et al. (2023) to investigate the dynamics of group formation and its impact on coordination in complex environments. The HFIG game is a novel N-dimensional battle of the sexes coordination game. In HFIG, players are assigned a numerical type from 1 to N. They are then matched with a partner, who has also been assigned a type. They interact by simultaneously choose a number from 1 to N. If the players choose the same number, they successfully coordinate and receive a positive payoff. If they do not successfully coordinate on the same number, they both receive a payoff of 0. However, the potential payoff for a player is decreasing in the distance of the coordinated strategy from the player's type. This means that players want to coordinate but would prefer to coordinate on an action closer in type to themselves.

We deploy a 4-type version of this game. For each session, we recruit 8 participants. Each participant is assigned a type. Each round, players are randomly matched with one of the other players in the session and play the coordination game. However, in this setup, participants are not directly informed of the type of their partner, mirroring real-world uncertainties in social interactions, particularly when you meet someone for the first time.

In order to investigate how groups may facilitate coordination in this complex environment with a lack of information about the person a player interacts with, we used three treatments. The first was a baseline scenario where participants had no signals about others they were interacting with. The

second involved exogenously assigned groups every three rounds of play, where participants could see their partner's group affiliation and the average actions of that group. The third treatment allowed players to choose their groups, following the group formation mechanism outlined by Ahn et al. (2008).

The data reveals a surprising result. In this complex coordination environment, groups do not significantly aid coordination. However, we do find that, when given the choice, participants choose to join groups that signal a type that is closer to their own. Interestingly, we find that over time, the variation in actions chosen decreases, with a general trend towards convergence in the middle of the action space, suggesting that society finds a way to compromise as a whole, and forego the potential benefits of using the group signals as extra information.

This result that participants do choose to join groups similar themselves suggests that people perceive groups to be useful, when in fact they do not provide extra benefit. To investigate this further, we collected a second wave of data, using participants from Prolific, to explore beliefs about the usefulness of groups. In this phase, we explained the task that the first wave of participants undertook and then collected their beliefs about the likelihood of coordination. We use this data to explicitly examine their perceptions of the effectiveness of groups in aiding coordination. Interestingly, while people generally overestimated the effectiveness of groups, their beliefs about the probability of coordination without groups were relatively accurate.

This study has significant policy implications, particularly in addressing cultural frictions in the context of immigration and group dynamics. It suggests that while people believe in the utility of groups for better coordination and as such acting as a deterrent to assimilation, the actual effectiveness of these groups may be overvalued, leading to a misalignment between perception and reality in policy formulation and societal expectations.

Related Literature The first literature we contribute to is that of cultural assimilation. This literature has found a number of important determinants for successful assimilation, such as the immigrants' personal characteristics (Dustmann, 1996; Manning and Roy, 2010), the length of stay (Aleksynska, 2011; Manning and Roy, 2010), language proficiency and the linguistic distance of the host country's language from the origin country's language (Fouka, 2020; Hannafi and Marouani, 2023; Lochmann et al., 2019); the social network of the immigrant (Danzer and Yaman, 2013; Verdier and Zenou, 2017); and economic integration (Hannafi and Marouani, 2023).

The most important determinant that we relate to is the natives' attitudes towards immigrants and concerns about cultural differences (Aksoy et al., 2023; Bisin and Tura, 2019; Jaschke et al., 2022; Mayda, 2006; Schilling and Stillman, 2024). However, research has suggested that there may be a form cultural self-selection taking place, where migrants are more likely to immigrate to a country with similar values to themselves (Knudsen, 2021; Konya, 2007). Here, we find support for a similar idea in that people choose to join groups who signal a type more similar to themselves. Additionally, we find evidence that our participants perceive groups to be useful, which may be a reason for the concern for cultural differences. However, we do also find that the importance of these

cultural concerns may in fact be overstated as we find that groups do not necessarily aid coordination in this complex environment. Overall, our research contributes to this field by exploring how group affiliation, as a cultural factor, influences the assimilation process, particularly in the context of coordination within diverse societies.

The second literature we contribute to is the literature on group formation. Previous papers have considered the impact of endogenous group formation on public goods provision and charitable donations (Ahn et al., 2008; Aimone et al., 2013; Brekke et al., 2011, 2007; Charness and Yang, 2014). Generally, these papers find that groups tend to endogenously form across the type of strategy played or preference grouping. In our study, we build on this literature to examine how group formation affects and is affected by the process of coordination in a multicultural setting. In particular, we find limits to the effectiveness of groups in aiding coordination.

The remainder of the paper proceeds as follows. Section 2 introduces the experimental design for study 1. Section 3 presents the results of study 1. Section 4 introduces the experimental design for study 2. Section 5 presents the results of study 2. Section 6 concludes.

2 Experimental Design: Study 1

We conducted the experimental laboratory study at the Behavioral Business Research Lab (BBRL) at the University of Arkansas and one session at the Baylor Experimental Economics Lab (BEE Lab).[1] For each session, we recruited 8 participants. Our participants played through a number of rounds of a task, where each round of this task consisted of four sub-rounds. In three of these four sub-rounds, participants are randomly matched with another participant and they play a 4-Type Battle of the Sexes Coordination game, based on an N-Type Battle of the Sexes (Gately et al., 2023). In the fourth sub-round, participants form groups with the group formation mechanims of Ahn et al., (2008). Participants played a pre-drawn random number of rounds of this game, where the probability of continuation to another was 80%. As this was pre-drawn, all sessions had the same number of rounds, which was 20 rounds.

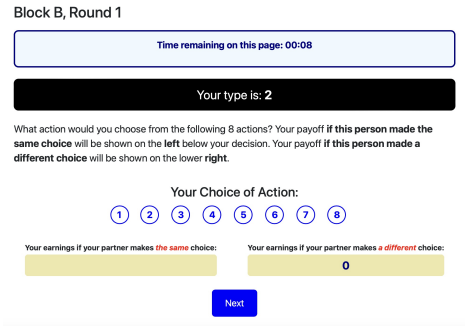
Participants were paid a show up fee of $10. The additional earnings for the main experimental task were based on the performance in one randomly selected sub-round from one randomly chosen round from each block. Across all tasks, participants earned experimental currency "points," which were convertible to dollars at a rate of 10 points = $1.00. On average, participants earned a bonus of $9.55 in addition to their show-up fee. We ran 20 sessions giving a sample of 160 participants. The experiment was coded using oTree (Chen, Schonger, & Wickens, 2016) and participant recruitment took place via BBRL's SONA systems.

At the start of all round, participants were given a number in the type space, $\Theta = 2, 4, 6, 8$[2]. This type remained fixed for the duration of the game. At the start of each sub-round, players

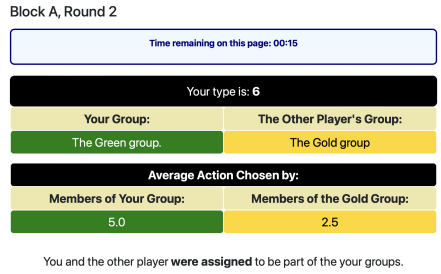
[1] We had several more sessions scheduled at Baylor, but these were cancelled due to insufficient sign-ups.

[2] It was possible that more than one player may have the same type and that some types may not be present in a session.

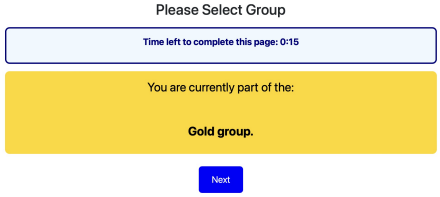
Figure 1: Experimental Screenshots



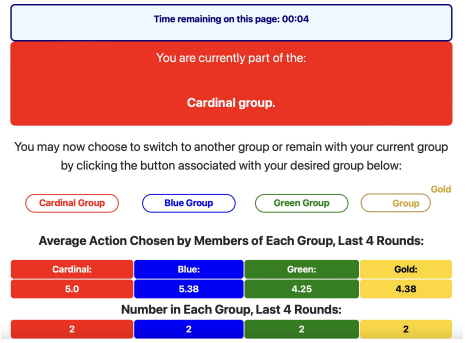
(a) Baseline Decision Screen



(b) Exogenous Decision Screen



(c) Exogenous Cue



(d) Endogenous Group Change Round

were randomly matched with one other player. These players then played the 4-Type version of the N-type Battle of the Sexes game (Gately et al., 2023). In this game, the two players each choose an integer from 1 to 8. A player's payoff depends on their choice and that of the other player. If the two players choose the same number, they both receive a positive payoff. However, if they choose different numbers, both players receive a payoff of 0. Assuming coordination, each player's payoff is a function of how far away the coordinated upon action is from their own type. If coordination is successful, individuals' payoffs are assigned as follows:

$$u_i(a_i, a_j, \theta_i) = \begin{cases} 64 - (\theta_i - a_i)^2 & \text{if } a_i = a_j \\ 0 & \text{otherwise} \end{cases}$$

We use this game because the payoff structure reflects the real-world intuition that one might prefer to interact with others who are most similar to ones' self, but that one would also generally prefer successful interactions to unsuccessful ones. Thus, as one moves away from their type, one's payoff decreases, though this is still preferred to a zero-payoff from an unsuccessful interaction.

In the version of the game we use, participants do not have direct information on their partner's type. The information that subjects have about their partner varies by treatment, and is dependent on their partner's group affiliation (if such an affiliation exists). In NO GROUPS, participants do not join groups. Participants are not provided any information when they are matched with a new partner, including their partner's type. Figure 1a shows the decision screen for NO GROUPS.

We also use two treatments that involve group formation: EXOGENOUS and ENDOGENOUS. In both treatments, subjects change groups every four rounds, and we denote group affiliation by color (Cardinal, Blue, Green, and Gold)[3]. In the EXOGENOUS treatment, participants are randomly assigned to groups of two participants at the start of the experiment. Unlike the type allocation mechanism, this is done without replacement to ensure all groups have the same number of initial members. After the first four rounds, subjects are randomly re-allocated to groups every four rounds, and have no control over the group to which they are assigned. In the ENDOGENOUS treatment, the first four rounds are played exactly the same as EXOGENOUS.

After the first 4 rounds, we allow subjects to choose which group to join every 4 rounds. The mechanism for endogenous group formation that we use is based on that of Ahn et al. (2008). After the three sub-rounds of the coordination game, the group reformation stage begins (see Figure 1d). At the start of the group reformation stage, the players are asked to choose a group from the four options presented (including their own prior group). They are shown the average choice of action for each group (or "N/A" if no players were in the group in the last 4 periods) as well as the number of players in each group. We chose this information set as it is the same as one of the main information

[3] "Cardinal" is one of the school colors of the University of Arkansas, where the majority of sessions were run. We wanted to avoid using the term "Red" to avoid any possible confusion with the historically racist term for Native Americans. We also avoided using "White" (University of Arkansas' other school color) to avoid any racial priming that might occur from the usage of that term, and substituted "Blue" instead. "Green" and "Gold" are the school colors of Baylor University.

sets a person has in the real-world: the idea that when deciding to join a group, one has information on what actions the group has taken as a whole, but not the actual type of each group member. When the group reformation sub-round has finished, the next round begins with participants playing with their new group labels as determined by the group formation mechanism. This process repeats every four rounds.

In both treatments, participants are provided with the partner's group color in each round as well as information about the average action chosen by the members of the partner's group in the previous round. In the first round after each group change round, this information about the average action of the members of the partner's group is not provided as it did not exist at that point. Figure 1b shows the decision screen for the EXOGENOUS treatment; the decision screen for ENDOGENOUS is identical except that the line at the bottom reads: "You and the other player chose your groups." Figure 1c gives the group assignment cue for EXOGENOUS, while Figure 1d shows the group change screen from ENDOGENOUS. We use random re-matching each round. When players are rematched at the start of subsequent sub-rounds, it is possible that players would be rematched with the same player that they had played with in the previous sub-round. This occurs for 3 sub-rounds before moving onto the group reformation sub-round.

In a session, participants completed this task twice, under two different treatments. All participants complete the task under the NO-GROUPS treatment ("Block A"). For the second completion of the task, the participants in a session are assigned to either EXOGENOUS or ENDOGENOUS ("Block B"), such that all participants in the same session have the same group formation treatment. We varied the order of the treatment blocks by experimental session. We preregistered the experimental design, hypotheses, and experimental code on the Open Science Foundation's website.[4]

2.1 Hypotheses

We test two preregistered hypotheses:

Hypothesis 1 *Groups improve coordination over no information.*

If our conjecture about groups serving as a signal of type is correct, then we would expect the coordination rates in both ENDOGENOUS and EXOGENOUS to be higher than those in NO GROUPS. Secondly:

Hypothesis 2 *Endogenous group formation improves coordination over exogenous group formation.*

If joining a particular group is a reflection of one's type, then we would expect subjects to join groups where the average action is closest to their type, lessening the noisiness of the signal; this option is not available to those in EXOGENOUS, so we would expect to see higher coordination rates in ENDOGENOUS than EXOGENOUS.

[4]The preregistration for Study 1 can be accessed at: https://osf.io/8tk9p

2.2 Experimental Procedures

At the start of the experiment, subjects were directed to the instructions, which were pre-recorded to minimize variation in the dissemination of the instruction materials. Subjects then completed a short comprehension quiz, and were required to re-take the quiz until they scored a 100% before moving on to the main experiment, which they completed in two blocks as described above; the ordering of these blocks was randomly counter-balanced across sessions to control for order effects. At the end of the experiment, subjects completed a brief survey consisting of questions from the Global Preferences Survey (Falk et al., 2016, 2018) to measure subjects' risk preferences, time preferences, preferences for fairness, and altruism, as well as demographic information and a strategy-method version of the HFIG game with known types (similar to Gately et al. (2023)) and the same type space as in the main experimental design, which was incentivized. In this version of the game, participants were assigned a type from the type space and asked to choose an action for each possible type present in the game.

Subjects were paid a show-up fee of $10.00 for active participation in the experiment. Additionally, subjects were paid an additional bonus payment in accordance with their performance in one randomly chosen sub round from each time they completed the task. That is, they were paid for one sub round when there were no groups, once for when they played in a group formation treatment, and once for the strategy-method task with known types at the end. The points they earned in the randomly chosen sub round was converted at an exchange rate of 10 points to $1.00.

3 Results - Study 1

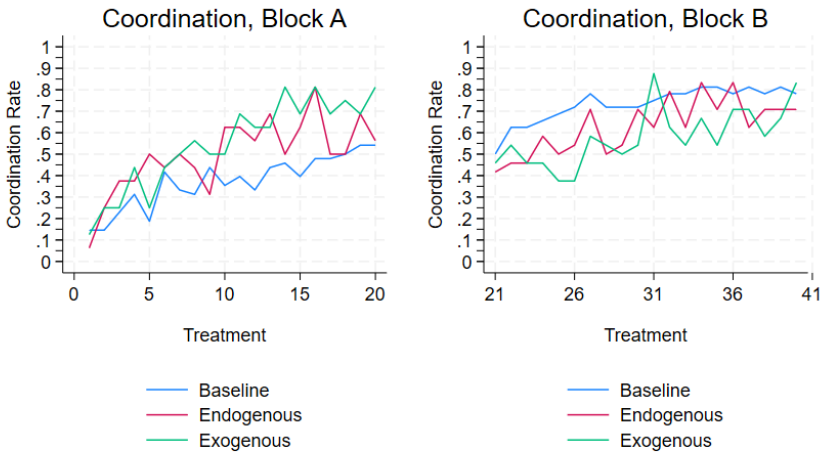
Table 1: Study 1 Descriptive Statistics

Variable	Mean	Standard Deviation
Choice of Action	4.604	1.208
Female	0.528	0.499
Asian	0.153	0.360
Black	0.047	0.211
Native or Indigenous	0.027	0.161
White	0.747	0.435
Number of Subjects	160	160

Note: this table gives the pooled descriptive statistics across all treatments for our demographic variables. Subjects chose roughly the midpoint of the distribution for their choice of action. Approximately 53% of our sample was female, and it was majority White (74.7%). We collected 19 sessions at the University of Arkansas and 1 at Baylor University.

Table 1 gives the descriptive statistics of our sample. We begin by conducting our preregistered

Figure 2: Coordination Rates by Treatment and Block



analyses, and then move on to our exploratory analyses. Figure 2 shows the coordination rate by treatment and block over time.

3.1 Preregistered Analyses

We begin our discussion of the results from Study 1 with a discussion of our preregistered specifications, starting with whether groups improve coordination or not (Hypothesis 1). Wilcoxon Signed-Rank tests of the equality of the treatment means of our coordination indicator variable show that coordination rates in ENDOGENOUS are marginally significantly greater than those in NO-GROUPS ($p = 0.024$), while there is no significant difference between EXOGENOUS and NO-GROUPS ($p = 0.463$).

We further investigate the richness of our within-subjects data using a panel logit model that takes the coordination indicator as its dependent variable, and an indicator for whether the subject is currently in either EXOGENOUS or ENDOGENOUS as the primary regressor of interest. Columns (1) and (2) of Table 2 gives the marginal effects of these models, with and without controls, respectively.

The marginal effects for our main variable of interest ("Groups") are not significant either with or without controls. We also check for significant differences between ENDOGENOUS and EXOGENOUS (Hypothesis 2). Mann-Whitney U-Tests find no difference in coordination rates between ENDOGENOUS and EXOGENOUS ($p = 0.8778$). We estimate a similar panel logit model to the one above, but over only those periods where the subject was in a groups treatment. The coordination indicator is again our dependent variable, while our primary explanatory variable of interest is a treatment indicator for ENDOGENOUS. Columns (3) and (4) of Table 2 give the marginal effects

of these panel logits, with and without controls, respectively.

Table 2: Treatment Effect of Groups

VARIABLES	(1) Coordinated	(2) Coordinated	(3) Coordinated	(4) Coordinated
Groups are On	0.056	-0.018		
	(0.061)	(0.049)		
Endogenous			0.012	0.015
			(0.080)	(0.078)
First Block		-0.244***		-0.086
		(0.046)		(0.075)
Female		-0.032		-0.039
		(0.045)		(0.041)
Black		-0.017		0.023
		(0.079)		(0.082)
Observations	6,400	5,960	3,200	2,980

Note: Robust standard errors clustered by session are reported below the coefficients in parentheses. ***, **, and * denote p<0.001, p<0.01, and p<0.05, respectively. Column (1) estimates the effects of both groups treatments (pooled) without controls; Column (2) includes controls for order effects ("First Block"), gender, and race. Column (3) tests the difference between EXOGENOUS and ENDOGENOUS, with observations in NO GROUPS omitted with no controls; Column (4) includes controls for order, gender, and race.

Again, the marginal effects on our main variable of interest are not statistically significant either with or without controls. Taken together, then, we do not find any support for either of our preregistered hypotheses. This gives:

Result 1: *There is no difference in treatments overall.*

3.2 Exploratory Analyses

Given that we do not find support for our preregistered hypotheses, we turn to a deeper exploratory analysis to examine why we do not find support for either hypothesis.

We begin by using a multinomial logit to examine why subjects join groups in ENDOGENOUS. We do this by regressing the probability of choosing to be in a particular group at the time the group switches occurred. Table 3 shows the marginal effects of this regression. Each row shows the marginal effects of choosing to be in that group conditional on the distance they are in type from

the signal of each of those groups. That is, the cell in "Type = Cardinal Average" and "Dist from Cardinal" shows the marginal effect of a participant's distance from the Cardinal group's average on choosing to be in the Cardinal group. We find significant negative effects on choosing to be in each of the groups conditional on the distance a participant's type is from that group's signal. We find positive effects on choosing to be in a group conditional on the distance between the participant's type and the signal of all other groups but that type, with 9 out of the 12 possible cells being significant at the 5% level.

Table 4: Marginal Effects for the Probabilities of Joining a Group with Distance in Type

	Dist from Cardinal	Dist from Blue	Dist from Green	Dist from Gold
Type — Cardinal Average	-0.101***	0.0382**	0.0413***	0.0219***
	(0.0151)	(0.0131)	(0.00903)	(0.00660)
Type — Blue Average	0.0369*	-0.115***	0.0493**	0.0289*
	(0.0143)	(0.0249)	(0.0166)	(0.0115)
Type — Green Average	0.0459*	0.0603***	-0.131***	0.0252
	(0.0192)	(0.0111)	(0.0351)	(0.0192)
Type — Gold Average	0.0370	0.0398***	0.0216	-0.0984***
	(0.0233)	(0.0115)	(0.0278)	(0.0264)

Note: Robust standard errors clustered by session are reported below the coefficients in parentheses. ***, **, and * denote $p<0.001$, $p<0.01$, and $p<0.05$, respectively.

The results from our multinomial logit analysis seem to indicate that subjects do seek to join groups that have an average that is more similar to their type. This gives:

Result 2: *Subjects are more likely to switch to a group that has an average most similar to their type.*

This result begs the question of whether our earlier, preregistered hypotheses did not adequately account for the possible effects of in-group bias. If subjects join groups that have an average action closer to their type, this means that subjects may believe that successful coordination is more likely with members of that group (even though pairings were randomly assigned and subjects were told this). To investigate this possibility, we use a random-effects logit where our coordination indicator is our main dependent variable. Our primary variable of interest is the indicator variable for whether the subject's partner is of their same group, "Same Group." We estimate the effect of this variable by itself, with a measure of the distance between the subject and their partner's type, and with an interaction between the "Same Group" variable and the distance between the subject's type and

their partner's type. Table 5 gives the marginal effects of these specifications.

Table 5: Ingroup Coordination

VARIABLES	(1) Coordinated	(2) Coordinated	(3) Coordinated
Same Group	0.007	0.010	0.100
	(0.058)	(0.059)	(0.114)
Type Distance		-0.011	-0.004
		(0.011)	(0.013)
Same Group X Type Distance			-0.032
			(0.024)
Observations	1,600	1,600	1,600

Note: Robust standard errors clustered by session are reported below the coefficients in parentheses. ***, **, and * denote p<0.001, p<0.01, and p<0.05, respectively. Column (1) examines the pure effect of the ingroup/outgroup indicator variable, while Column (2) controls for type distance. Column (3) checks for an interactive effect of distance and the ingroup/outgroup indicator.

As with our preregistered analyses, we fail to find a significant result for our main variable of interest. The marginal effects are not statistically different from zero (nor, interestingly, is the effect of type distance). This gives:

Result 3: *Subjects do not coordinate better with members of their same group compared to members of the other groups.*

Taken together with Result 1 and Result 2, Result 3 identifies a possible channel explaining why we fail to find result for our preregistered hypotheses: ingroup bias. Result 2 indicates that subjects join groups where the average choice of action best reflects their type, while Result 3 indicates that subjects do not coordinate any better with members of their same group than members of their outgroup; finally, Result 1 indicates that coordination rates are not better in ENDOGENOUS or EXOGENOUS than NO-GROUPS. However, we did not collect beliefs as part of our experiment, leaving us unable to say for certain whether subjects' beliefs about coordination with ingroup members is, in fact, the cause. To investigate this further, we ran a second study.

4 Experimental Design - Study 2

To investigate whether subjects' beliefs about the likelihood of successful coordination with ingroup members is a plausible explanation for our failure to find support for our preregistered hypotheses in Study 1, we ran a second study on the online platform- Prolific. In this study, we elicited subjects' beliefs about the likelihood that randomly-selected dyads from Study 1 successfully coordinated. We selected the dyads randomly *ex ante* to ensure comparability. We used three main treatments designed to mirror those in Study 1: ENDO (where the dyads were drawn from the ENDOGENOUS treatment in Study 1), EXO (where the dyads were drawn from the EXOGENOUS treatment from Study 1), and BASELINE (where the dyads were drawn from NO-GROUPS in Study 1). In ENDO and EXO, we showed subjects the group affiliations for both Study 1 participants, the group averages for each group affiliation, each participant's type, and a statement that indicated whether subjects were randomly assigned to groups (EXO) or chose to be part of their group (ENDO). In BASELINE, subjects were simply told each player in the dyad's type and a statement indicating that neither player had any information about the other.

We collected beliefs about each possible distance for BASELINE, while we collected beliefs about High (4 - 6) or Low (0-2) distance and varied whether the dyad was ingroup or outgroup for both ENDO and EXO. We asked subjects, "Based on the information above, How likely do you think it is that these two people chose the same number?" We used the Binarized Scoring Rule to incentivize subjects' answers, and we chose Wilson and Vespa (2018) methodology for explaining the Binarized Scoring Rule to subjects. After subjects submitted their guesses, the computer drew two random numbers. If the players in the original dyad chose the same number and the subject's guess was larger than either of the two draws, the subject earned 100 points (convertible to dollars at \$1.00 = 100 points). If the players in the original dyad did not choose the same number and the subject's guess was smaller than either of the two draws, the subject earned 100 points. Otherwise, the subject earned 0 points. Subjects were paid for one randomly-selected round.

The experiment proceeded as follows. Subjects completed an online consent form, and then input their Prolific ID. Subjects then proceeded to the instructions, where the original experiment was explained, and subjects were told their task and how they would be paid. Subjects then completed a comprehension quiz before proceeding to the main experimental task. As in Study 1, all subjects completed one block of BASELINE and one block of either ENDO or EXO; we varied the order of the blocks between sessions to control for order effects. Therefore, subjects completed 8 total rounds (one for each type distance in BASELINE, and low- and high-distance for ingroup and outgroup in ENDO or EXO). After completing the main experiment, subjects were told their earnings and directed to an end-of-experiment survey that contained demographic information and Global Preferences Survey measures of time preferences, risk preferences, altruism, and willingness to punish unfairness to ones' self and willingness to punish unfairness to others. Two hundred and fifty-eight subjects were paid \$3.50 for completion (We planned for the experiment to take 20 minutes). We paid subjects an additional \$1 bonus for correct beliefs in one randomly-selected round

for a maximum possible earnings of $4.50. The average earnings (in addition to the completion fee) was $0.36. We preregistered the basic experimental design and our main statistical hypothesis on the Open Science Framework's preregistration repository.[5]

4.1 Hypotheses - Study 2

We preregistered one hypothesis:

Hypothesis 3: *Subjects will overweight the likelihood of coordination when grouping is present relative to when grouping is not present, but there will not be a difference in weighting between randomly-assigned and endogenously-chosen groups.*

This hypothesis is rooted in our three results discussed above: if beliefs about likelihood of ingroup coordination is a possible channel that explains our lack of support for Hypothesis 1 and Hypothesis 2, then we would expect beliefs to be overweighted in the ENDO and EXO treatments relative to baseline.

5 Results - Study 2

Table 6 gives the descriptive statistics of our Prolific sample. In all, 258 subjects participated in our online study.

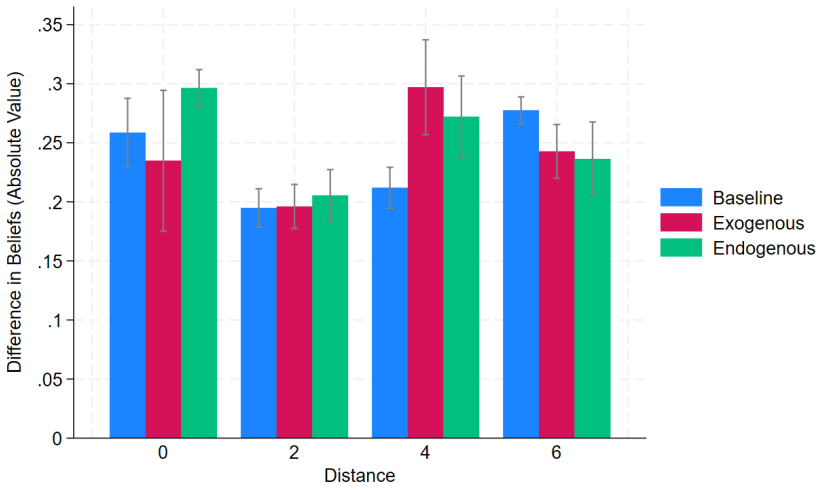
Table 6: Study 2 Descriptive Statistics

Variable	Mean	Standard Deviation
Reported Beliefs (Overall)	51.025	29.034
Reported Beliefs (BASELINE)	44.317	27.975
Reported Beliefs (ENDO)	55.783	29.323
Reported Beliefs (EXO)	59.682	27.609
Female	0.566	0.497
Asian	0.054	0.227
Black	0.136	0.343
Native or Indigenous	0.008	0.088
White	0.767	0.423
Number of Subjects	258	258

Note: this table gives the pooled descriptive statistics across all treatments for our demographic variables for Study 2. Subjects generally reported the likelihood of successful coordination as slightly better than a coin flip (the "Reported Beliefs" variable ran from 0 to 100 to avoid confusing subjcts). Approximately 57% of our sample was female, and it was majority White (76.7%).

[5]The preregistration can be accessed at: https://osf.io/xvbcr.

Figure 3: Belief Distance (Pure Accuracy)



5.1 Preregistered Results

We begin with our discussion of the results from our preregistered statistical analyses, before diving into a deeper exploratory analysis. Figure 3 shows the accuracy of subjects' guesses (given by the absolute value of the difference between the subject's reported beliefs and the actual coordination rate for the treatment - hereafter, "Beliefs Difference"). We begin with Wilcoxon Signed-Rank tests for our within-subjects differences. The "Beliefs Difference" variable is significantly different between BASELINE than EXO ($p < 0.001$), as well as between BASELINE and ENDO ($p > 0.001$). The difference between ENDO and EXO, measured using a Mann-Whitney U-Test, is not statistically significantly different ($p = 0.491$).

We also run a random-effects regression with our dependent variable being the Beliefs Difference variable, and our primary variables of interest being treatment indicators for ENDO and EXO (and, secondarily, an indicator for Ingroup). We note that a positive coefficient for either treatment indicator means that it is *less accurate* than the reference treatment (BASELINE), as a perfectly accurate guess would yield a value of zero for the Belief Difference variable (and, analogously, the same would be true for the coefficient on the Ingroup indicator, which we do expect to be positive and significant if ingroup bias is present). Table 7 gives the results. Column (1) gives the results without demographic controls, while Column (2) gives the results with demographic controls.

Contrary to what we had expected, the coefficient on the ENDO indicator is positive and significant with or without controls, meaning that subjects' beliefs are less accurate in ENDO than in BASELINE. The coefficient on the EXO treatment indicator is positive and marginally significant without controls included (Column (1)), but is not significant when controls are included. As we expected, the coefficient on the ingroup indicator variable is positive and significant, which implies

that subjects' beliefs about the likelihood of succesful coordination amongst dyads where the players are in the same group are less accurate than subjects' analogous beliefs about dyads where players are of different groups. This gives:

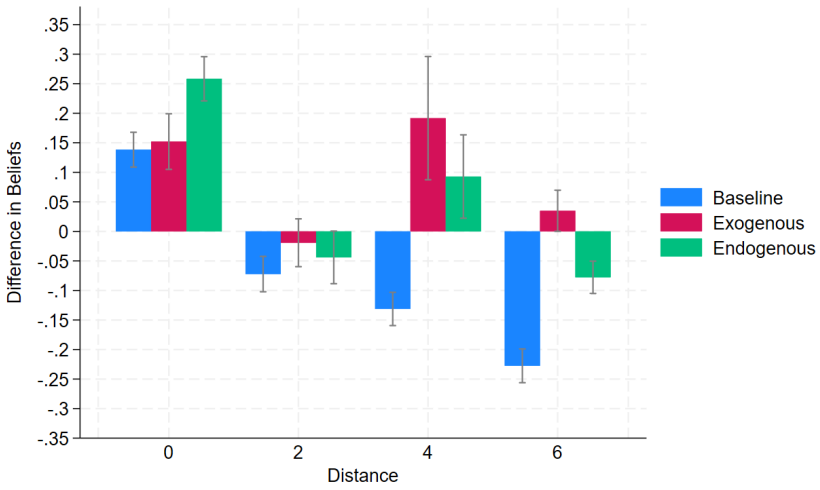
Result 4: *Beliefs about coordination in BASELINE are more accurate in absolute terms than in ENDO or EXO, and less accurate about coordination between members of the same group than members of different groups.*

Table 7: Belief Accuracy and Treatment

	(1)	(2)
VARIABLES	Belief Difference	Belief Difference
ENDO	0.031***	0.032***
	(0.009)	(0.009)
EXO	0.020*	0.020
	(0.010)	(0.011)
Ingroup	0.058***	0.058***
	(0.007)	(0.007)
Dyad Type Distance	0.005*	0.005*
	(0.002)	(0.002)
Female		-0.007
		(0.012)
Black		0.028
		(0.019)
Age		0.000
		(0.001)
Constant	0.163***	0.151***
	(0.016)	(0.017)
Observations	2,064	2,064
Number of Subjects	258	258

Note: Robust standard errors clustered by session are reported below the coefficients in parentheses. ***, **, and * denote p<0.001, p<0.01, and p<0.05, respectively. Column (1) estimates the belief difference variable as a function of treatment, ingroup/outgroup status, and the difference between the participants' types. Column (2) estimates a similar regression with demographic controls added.

Figure 4: Belief Weighting



5.2 Exploratory Results

We note that our preregistered specification does not allow for belief weighting, only belief accuracy.[6] Since our preregistered hypothesis was about weighting, we conduct an exploratory analysis where the Belief Difference variable is simply the difference between the subject's reported beliefs and the actual coordination rate (i.e., we do not take the absolute value as in our preregistered specification), and re-estimate the same specifications from Table 7 with this new variable. Unlike in our previous table, we now interpret the coefficients as a relative measure of weighting. A positive coefficient on the ENDO and EXO variables indicates that beliefs in these conditions are *overweighted*, while a negative coefficient means these coefficients are *underweighted*. Given our hypothesis, we expect both treatment indicators to be positive and significant. The results are given in Table 8.

As expected, the coefficient on the treatment indicators is positive and significant in both cases, indicating that beliefs are overweighted in both ENDO and EXO. Further, the coefficient on the ingroup indicator is positive and significant, implying that subjects overweight the likelihood of coordination amongst members of the same group. Finally, we also note that the coefficient on the Dyad Type Distance variable is negative and significance, implying that beliefs about the likelihood of successful coordination become more underweighted as the type distance increases. Taken together, this gives:

Result 5: *When allowing for over- or under-weighting, beliefs about coordination in ENDO and EXO are overweighted; further, beliefs about the likelihood of coordination amongst ingroup members is overweighted, and the beliefs about the likelihood of successful coordination become more under-*

[6]This specification was necessary, however, in order to check whether the accuracy of subjects' beliefs was significantly more or less accurate in either ENDO or EXO than BASELINE.

weighted as type distance increases.

Table 8: Belief Weighting and Treatment

VARIABLES	(1) Belief Difference	(2) Belief Difference
ENDO	0.203***	0.203***
	(0.017)	(0.017)
EXO	0.242***	0.242***
	(0.020)	(0.020)
Ingroup	0.215***	0.215***
	(0.014)	(0.014)
Dyad Type Distance	-0.033***	-0.033***
	(0.003)	(0.003)
Female		0.022
		(0.013)
Black		0.049
		(0.026)
Age		-0.001
		(0.001)
Constant	-0.189***	-0.167***
	(0.014)	(0.032)
Observations	2,064	2,064
Number of Subjects	258	258

Note: Robust standard errors clustered by session are reported below the coefficients in parentheses. ***, **, and * denote p<0.001, p<0.01, and p<0.05, respectively. Column (1) estimates the belief difference variable as a function of treatment, ingroup/outgroup status, and the difference between the participants' types. Column (2) estimates a similar regression with demographic controls added.

6 Discussion and Conclusion

Objections to immigration sometimes focus on the notion that immigrants of differing beliefs, religions, and social structures may struggle to assimilate in their new nation (Aksoy et al., 2023; Bisin and Tura, 2019; Jaschke et al., 2022; Mayda, 2006; Schilling and Stillman, 2024). This suggests that natives take immigrants' group affiliations as a noisy signal of the actual true type of agents within the group, and that belief weighting about the likelihood of being able to successfully interact with

others may also play a role. We examine whether and how group affiliation serves as a signal that aids or hinders coordination, and whether the methodology of group formation matters.

Using a controlled laboratory environment, we investigate whether the existence of groups improves coordination rates. This environment has the advantage of increased internal validity compared to statistical methods, allowing us to causally identify the effect of group affiliation on coordination. We further investigate whether groups that are formed beyond the control of a person (such as ethnicity) or by personal choice (such as religion) differentially help or harm coordination rates. We use the novel HFIG game from Gately et al. (2023) as a proxy for interactions amongst different types of people in conjunction with a group formation mechanism similar to that in Ahn et al. (2009), where the person's type is independent of their group affiliation. This allows us to reflect the real-world intuition that each group of people may have members with many differing preferences and characteristics. Some members of different groups may actually be quite similar in terms of their characteristics, preferences, and mannerisms, but this may be masked if agents are forming beliefs at the group level rather than the individual level.

Our analysis of subjects' behaviour in laboratory environment finds that both exogenously assigned and endogenously chosen groups do not significantly enhance coordination. While testing our initial hypothesis, we found that endogenously formed groups are marginally better at coordinating than no groups but exogenously formed groups are not. We cannot conclude that being part of a group is better than having no groups. Additionally, the way groups are formed does not seem to matter in terms of coordination rate. Subjects do not even coordinate better with members of their own group compared to members of outgroup. This suggests that the perceived identity within groups may not translate into effective cooperative strategies in coordination games. A behaviour we observe in our setting is the biased tendency of people to overweight the importance of joining groups that reflect their types.

While we were not able to elicit beliefs of subjects who participated in the first study, we ran an online study on Prolific to see if the subjects' beliefs are driving the results. We elicit beliefs over the interactions between participants in the laboratory experiments using the Binarized Scoring Rule methodology of Wilson and Vespa (2018); each subject reported beliefs over coordination rates in our baseline (no groups) treatment as well as in either the endogenous formation or exogenous formation treatment. We elicit beliefs for all distances in Baseline and for low and high distance in both groups treatments; we also collect beliefs for ingroup and outgroup interactions in the groups treatments. We find that beliefs about the likelihood of coordination in the groups treatments are less accurate than those in the baseline treatment. Generally (though not in every instance), beliefs about the likelihood of coordination in the groups treatments are over-weighted while beliefs about the likelihood of coordination in the baseline treatment are under-weighted. We also find evidence of ingroup biased beliefs about coordination between members of same group compared to different groups.

One possible concern is that there is no exclusion mechanism, that is to say people do not choose

who they interact with and they may be forced to interact with people they otherwise would not. This means that our results are conditional on an interaction occurring and we cannot determine where people would want to avoid interacting with people of different types. Future work could examine how exclusion mechanisms, in concert with the HFIG game, impacts our results. Another promising avenue could be the examination of specific policies, such as supportive or punitive interactions on coordination, impact beliefs about the likelihood of coordination as well as coordination outcomes.

An important area that our study addresses is in reference to natives' concerns about cultural differences between the immigrants and themselves, and particularly with how these cultural concerns may impact cohesion (Aksoy et al., 2023; Bisin and Tura, 2019; Jaschke et al., 2022; Mayda, 2006; Schilling and Stillman, 2024). Our results suggest that these cultural concerns may in fact be overstated as we find that groups do not necessarily aid coordination in this complex environment. Additionally, given that we find that people do choose to join groups when given a signal that is similar to their own type, people may perceive groups to be useful in this environment when, in fact, they are not. We provide evidence for the mechanism for this being based on inaccurate beliefs about the usefulness of groups in coordination. This potential misperceived usefulness of groups may itself be leading to cultural concerns.

Restrictions on immigration based on concerns about cultural differences may be unfounded. Allowing for continued group affiliation with one's prior identity may not harm one's ability to work with others in the hosting nation. Policies that encourage embracing immigration should emphasize individual similarities rather than group-level aggregations. The focus of any immigration policy should be to focus on encouraging individual interactions with immigrants, rather than top-down edicts. This is bolstered by evidence from McGee and Gately (in progress), which finds that supportive interventions are more effective at national identity building than punitive ones.

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A Appendix: Deviations from the Preregistration

We report two deviations from the preregistration for Study 1. First, we had preregistered that we would use dyadic standard errors for our statistical analyses (as in Gately et al., 2023); however, this was not possible since subjects played with a live partner instead of being matched in synthetic pairs with every other person in the experiment, preventing us from completing the matrix; we chose to use standard errors, clustered by session, since this would be as conservative (if not more so) than dyadic standard errors. Second, in our preregistered experimental instructions, we had stated the experiment would last for 90 minutes; however, after running the first two sessions, it became clear that subjects required only 60 minutes to complete the experiment, so we altered the instructions and consent form accordingly to avoid deceiving subjects about the length of the experiment.