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**Beyond Colors:
Communication and Social Identity
in Natural Groups**

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Abstract. This paper examines whether communication can mitigate in-group favoritism when group membership is based on a real-life trait (Italian vs. non-Italian citizenship among university students) rather than artificially induced, as in the minimal group paradigm. In our natural group setting, the identity effect is presumably stronger, making bias harder to counter. We do not find that communication significantly increases cooperation. Moreover, it does not reduce favoritism. However, the exchange of mutual promises increases cooperation and reduce in-group bias. A notable finding not found in previous studies is that gender differences also emerge: Italian males exhibit stronger in-group bias than females, whereas the opposite holds true among non-Italians. Overall, our findings confirm that not all groups are alike and that results from minimal group experiments may not always generalize to natural groups.

JEL codes: A13, C91, D03, D64, D90.

Keywords: In-group bias, promises, exogenous variation, natural groups, gender effect.

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1. Introduction

Society is made of relationships among individuals, but these relationships shape individuals from themselves: from Aristotle's view of humans as political beings to Durkheim's emphasis on the role of collective norms in forming personal decisions as important as suicide, the social environment has long been seen as fundamental to individual identity.¹ In the second half of the twentieth century, Tajfel and Turner (1979) formalized this intuition with their Social Identity Theory, demonstrating that people's self-concept is significantly influenced by their membership in social groups. In the history of mankind these groups have evolved from small ones based on kinship to large ones based on language, religion, geographical location, nationality, and many other characteristics.

Social identity can drive both prosocial behavior and in-group bias, the tendency to favor members of one's own group. Building on these insights, later introduced into economics by Akerlof and Kranton (2000), a large body of experiments has examined how group identity affects cooperation. Experimental evidence confirms the robustness of in-group favoritism across various games and settings.² One important question is whether favoritism is driven by preferences concerning the welfare of the ingroup, or by beliefs about the behavior of the ingroup and outgroup members (Ockenfels & Werner, 2014, Everett *et al.*, 2015; Ciccarone *et al.*, 2020).

Experimental evidence also shows that communication is a powerful coordination device in games. Cooper *et al.* (1992) demonstrate that cheap talk significantly enhances efficient coordination, a finding later confirmed by many (e.g., Blume and Ortmann, 2007, Ellingsen and Johannesson, 2004, and Ellingsen and Östling, 2010). Of particular interest to this study is that Charness *et al.* (2024), from which we draw for our setting finds that communication increases cooperation even if it does not eliminate favoritism. However, favoritism is eliminated when communication leads to agreements to cooperate.

¹ See Durkheim (1897).

² See Fershtman & Gneezy (2001), Whitt & Wilson (2007), Hargreaves Heap & Zizzo (2009), Chen & Li, (2009), Liebe & Tütic (2010), Ioannou *et al.* (2015), Pan & Houser (2013, 2019), Kranton & Sanders (2017), Kranton *et al.* (2018, 2020), among many others.

Much of the literature on social identity relies on the minimal group paradigm, where group identity is artificially assigned or based on trivial preference distinctions. The minimal group design isolates identity effects cleanly, and it is easier to organize, but its external validity can be questioned. However, as several authors note, minimal group settings may not evoke the emotional or normative weight that real-world identities carry.³ Similarly, Akerlof & Kranton (2000) and Goette *et al.* (2006) argue that group-based behavior in natural contexts is shaped by deeper motivational structures—personal attachments, shared narratives, symbolic affiliations, and norm adherence—that minimal categories fail to capture. Cooper and Kagel (2016) emphasize that identity effects observed in stylized laboratory environments may not survive when behavior involves strategic interdependence, reputational consequences, or high-stakes cooperation. Moreover, a given identity may be expressed differently in different social environments.⁴ Finally Ben-Ner *et al.* (2009) show that not all real identity categories have the same impact on group favoritism, and Abbink & Harris (2019) show that a political identity, as opposed to an artificial identity, creates outgroup discrimination on top of ingroup favoritism.⁵

This widely shared awareness of the limitations of the minimal group paradigm has led to growing calls to test whether results found in studies adopting it generalize when looking at groups whose members share thicker identities.

Our study responds to this challenge: the specific issue we focus on is the interplay between group favoritism and communication. Charness *et al.* (2024) using a modified dictator game show that while communication promotes cooperation, only mutual promises significantly reduce ingroup favoritism when experimenting with artificial groups. Our study adopts their same approach but looks at the behavior of natural groups, where social identity is likely to be more impactful. Specifically, both our groups are formed by students from Tor Vergata University, but one group is formed by Italian citizens and the other by non-Italian citizens. The idea is that the Italian group

³ See Charness *et al.* (2007), Chen & Li (2009), Everett *et al.* (2015)

⁴ See Shi *et al.* (1999), Chang *et al.* (2024), Chen *et al.* (2014), Charness *et al.* (2014), Adnan *et al.* (2022).

⁵ Specifically, Ben Ner *et al.* (2009) find that differences in nationality are not a significant driver of favoritism, unlike political and religious views.

may be integrated by an attachment to their common national identity.⁶ The non-Italian group shares the experience of being expatriates in Italy, and we will give detailed reasons that show they are more likely to be personally acquainted.

In this paper, we ask: do the desirable effects of communication and promises found within a minimal group paradigm survive in our natural group setting? Or do those effects depend on the nature of group boundaries being artificial and therefore easy to neutralize?

The answers to the questions we ask are not obvious: communication might have weaker or even negative effects in inducing cooperation when groups are natural; it could either reduce social distance between subjects by fostering mutual understanding and familiarity, as it does with artificial groups, or reinforce favoritism by making group distinctions more salient. This could happen, for instance, if conversations dwell on group differences themselves. Mutual promises could be more difficult to reach, or even when reached, could be felt to be less binding by promisors. Knowing whether and how communication and promises attenuate in-group bias in these contexts is essential for assessing the scope and limitations of social identity interventions.

Our results show a tendency toward in-group favoritism, which appears to be more pronounced among international students. Participants demonstrate a greater willingness to sacrifice monetary payoffs for partners belonging to their own group. Communication, *per se*, does not enhance cooperation and fails to reduce in-group bias. Mutual promises increase cooperation and they help eliminate in-group bias. Promises strongly affect outcomes of out-group pairs.

A growing body of experimental research has examined why people keep their promises, focusing on two main explanations: the first focuses on the desire to avoid letting others expectations down, in this literature often referred to as “guilt aversion”, e.g. Charness &

⁶ The origin of the notions of nation and of national identity are highly debated among social scientists. According to one influential current of thought, developed by Anderson (1983), Gellner (1983) and Hobsbawm (1983) among others, nations are modern constructs emerged within industrial societies, while Smith (1986) gives more weight to their ethnic roots, according to Wimmer (2015) themselves often politically constructed. In the experimental literature, many papers have shown that common nationality or ethnicity may generate a social identity effect: for instance, Glaeser *et al.* (2000), Fershtman & Gneezy, (2001), Bornhorst *et al.* (2010), Dugar and Shahriar (2012) report group favoritism along ethnic or national lines. For a survey see Charness & Chen (2020) and reference therein.

Dufwenberg (2006),⁷ the second consists in a will to keep one's word, i.e., "moral commitment" (Ellingsen & Johannesson, 2004). Identifying the motivations behind promise-keeping is challenging, as both imply a correlation between actions and beliefs. Disentangling these requires experimental manipulations that introduce exogenous variations in both promises and group membership.⁸ In this paper, we adapt Vanberg's (2008) methodology to explore the transmission channels of promises, potentially heterogeneous among natural groups, to better interpret our experimental results.⁹

We find that the motivation for promise-keeping appears to differ depending on the group membership: in-group cooperation is driven by guilt aversion (avoiding disappointment in the partner's expectations). In contrast, out-group cooperation is motivated more by a sense of moral commitment (honoring one's word). The difference in motivation is stronger among national dictators.

We also observe striking gender asymmetries. In-group favoritism is significant among international female and Italian male dictators, but is absent among Italian female dictators, while it is not statistically significant among international male dictators. We interpret these results as reflecting the qualitative difference in the identities binding the two groups. In fact, we argue that international students are more likely to know each other personally. A vast psychological literature maintains that females tend to be more sensitive to the needs of proximate others. We refer to this literature to interpret our finding that female foreign students are in-group biased in our experiment. On the other hand, attachment to national identity, which ties Italian students, is confirmed by other sources to be stronger for males.

The remainder of the paper is structured as follows. Section 2 presents experimental design and procedures. Section 3 outlines the theoretical framework and hypotheses. Section 4 reports the main results, while Section 5 discusses gender differences. Section 6 concludes.

⁷ See also Battigalli & Dufwenberg (2007).

⁸ See, e.g., Di Bartolomeo *et al.* (2019) and Ciccarone *et al.* (2020), Charness *et al.* (2024) and Di Bartolomeo *et al.* (2024).

⁹ Recent studies offer different findings using distinct experimental designs (Ederer & Stremitzer, 2017; Di Bartolomeo *et al.*, 2019). This highlights how testing motivational mechanisms can yield context-dependent results, particularly when communication formats and social setting related is vary.

2. Experimental design, procedures, and communication protocols

2.1 Experimental design

As already said our experimental design closely follows that of Charness *et al.* (2024), with one crucial modification: we replace artificially assigned group identities with natural groups, defined by participants' nationality (international vs. domestic students), so as to test the robustness of their findings. The core structure of our design is a binary-choice random dictator game with pre-play communication.¹⁰ At the outset, each participant knows that there is a 50% probability of playing as the dictator. Before role assignments, participants are randomly matched into pairs and engage in a bilateral pre-play communication phase. During this phase, they can exchange up to four short, free-form, non-binding messages (cheap talk). Each participant can also observe their partner's group affiliation. After communication, roles are assigned. One player becomes the dictator, and the other the recipient. Then, in half of the cases, recipients are randomly reassigned to a new dictator. Only dictators are informed whether a switch has occurred. Each dictator also learns the group membership of the (possibly new) recipient and can read the prior communication between that recipient and their original partner.

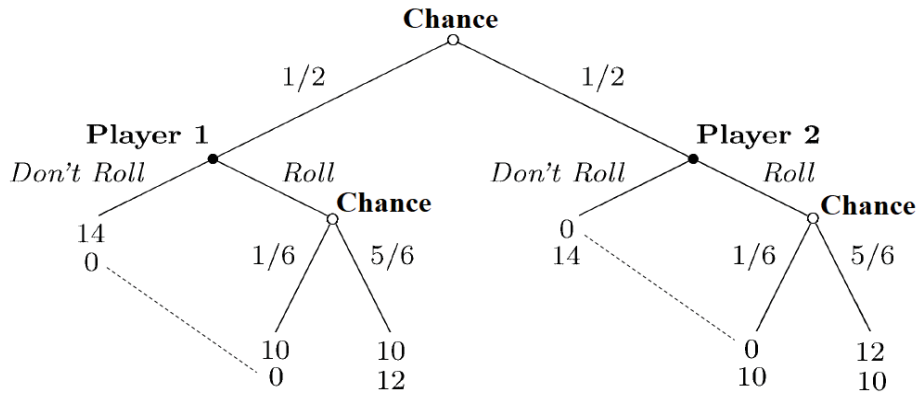


Figure 1. A binary-choice random-dictator game

¹⁰ Following prior research, we employ a dictator game for comparative analysis. This lets us assess social identity effects in a well-established experimental setting (e.g., Vanberg, 2008; Güth *et al.*, 2009; Ockenfels & Werner, 2014; Ciccarone *et al.*, 2020; Charness *et al.*, 2024).

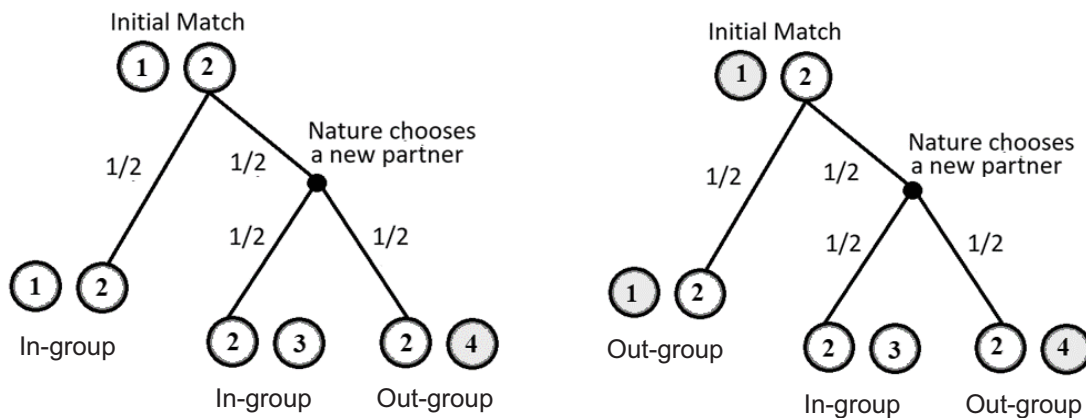
Figure 1 describes the binary-choice random dictator game. Players 1 and 2 may or may not have communicated beforehand. In either case, the player selected as the dictator will be informed of the recipient's group affiliation and access the messages that the recipient exchanged during the pre-play communication.

Dictators can choose *Roll* or *Don't Roll*. If they choose *Don't Roll*, they get 14 tokens and the recipient gets 0 tokens; if they choose *Roll*, they get 10 tokens, while the recipient gets 12 tokens with a probability of $5/6$ and 0 tokens with a probability of $1/6$. The payoffs are the same as in Charness *et al.* (2024) for easy comparability of results. Payoffs reflect monetary payments rather than utility, as this is unobserved and may not be purely self-regarding. At the end of the game, each recipient observes their payoffs, but not the associated dictator's choice, so they do not know whether they received zero tokens because the dictator chose "Do not Roll" or because the dictator chose "Roll," but they were unlucky with the die.

Our experimental design follows a 2×2 factorial structure, combining group identity (International vs. National) with pairing conditions (Non-switched vs. Switched), applied to the simple random dictator game illustrated in Figure 1.

Figure 2 depicts the switching mechanism adopted for ease of comprehension. The figure focuses, without loss of generality, on Player 2's selection as a national dictator.

Figure 2. The switching mechanism



Note: The figure is adapted from Charness *et al.* (2024). Numbers are shown to help the reader follow the matchings. The focus is on Player 2. The first line refers to the initial match. The switch occurs with $1/2$ probability. If the switch occurs, independently of the initial match, Player 2 can end up with an in-group or out-group match.

The figure illustrates that, before learning about their role in the game, Player 2 was initially involved in a chat with Player 1, and she could find herself either in an in-group match (left panel) or an out-group match (right panel). After getting to know their role, Player 2 is switched with a 50% probability. If there is no switch, Player 2 is still matched with Player 1 (their initial partner) in an in-group (left panel) or in an out-group match (right panel). By contrast, if switched, Player 2 can end up in an in-group or an out-group with the same probability, depending on whether they are re-matched with Player 3 or 4. In both cases, she is re-matched with someone who did not chat with them (i.e., a new partner).¹¹

In our design, non-switched participants remain matched with the partner they communicated with, while switched participants are paired with someone with whom they had no prior interaction. As a result, non-switched participants are, on average, more familiar with their counterparts than switched participants.

A key feature of the design is the asymmetric information introduced by the switching mechanism: dictators are informed whether a switch has occurred, while recipients are not. When a switch occurs, dictators also see the group identity of the new recipient and their prior partner, as well as the communication between them. Since recipients remain unaware of the switch or whether their messages were read, their first-order beliefs are unaffected. This implies that dictators' second-order beliefs (i.e., their beliefs about recipients' expectations) are exogenous to the switching conditions. This structure enables us to isolate the effects of group identity, communication, and expectations on prosocial behavior.¹²

Our structure enables us to address two core research questions.

1. Does direct communication reduce in-group favoritism? To answer this, we compare the behavior of non-switched participants, who communicated directly with their final partner, to switched participants, who did not. We implement a difference-in-

¹¹ Recall that, in both cases, Player 2 is informed whether the new partner was initially paired with someone from his own group or not.

¹² The setup ensures that dictators' second-order beliefs are independent of whether a switch occurs since recipients are unaware of the reassignment. Consequently, it allows for exogenous variation in group membership (in-group vs. out-group) and prior communication (switched vs. non-switched) while holding constant second-order beliefs.

differences strategy, contrasting the magnitude of in-group bias in the presence and absence of prior communication.

2. How does the content of communication affect in-group bias? Specifically, we examine whether mutual promises to cooperate affect group affiliation differently. Addressing this question requires coding the messages exchanged during the pre-play chat phase to identify cases where both players made reciprocal commitments.

Our hypotheses are illustrated in detail later. Preliminary, it is worth noting that we anticipate observing in-group favoritism but expect the effects may diverge from those found in weaker identity frameworks, such as the arbitrary *Red* vs. *Blue* group design in Charness *et al.* (2024). Their setup likely produced symmetrical effects due to random group assignments, but our use of natural group divisions (international and national students) may generate asymmetric behavioral patterns.

2.2 Procedures

The experiment was conducted at the Tor Vergata University of Rome. We used a within-subject design that involved 384 students (12 sessions, 10 rounds each, 32 subjects each) recruited using an online system.¹³ Upon arrival, subjects were randomly assigned to 32 isolated computer terminals.¹⁴ Two assistants handed out instructions and checked that participants correctly followed the procedures. The experiment took place between September and November of 2022.

The experimental design was carried out in three phases: 1) group assignment (pre-session), in which we divided the students into two groups: national and international, and formed pairs of subjects who observed their group of belonging (national and international depending on their citizenship); 2) experimental session; 3) final payment (post-session). In each of the 10 rounds, subjects interact with a different partner chosen randomly by the computer. No subject interacts with the same partner twice (perfect stranger matching). In each round, the following sequence of five stages was implemented.

¹³ We recruit students. We create two lists: international (students born and educated abroad) and national (domestic students), and we randomly select students from each list.

¹⁴ The experiment was designed using z-Tree (Fischbacher, 2007).

1. *Communication.* Subjects were randomly matched to form 16 pairs, with a random determination of who would start the conversation. Each conversation consisted of four one-way posts in sequence, each of which could be no longer than 90 characters.
2. *Role assignment.* At the beginning of the round, roles (dictator or recipient) were randomly assigned for each pair, and subjects were so informed.
3. *Switching.* Half of the pairs were switched. Only dictators were informed if a switch had occurred. Each switched dictator with a new recipient learned about a) the nationality of the new recipient, b) the nationality of the previous match of the new recipient, and c) the prior conversation between the new recipient and their previous match.
4. *Belief elicitation.* This stage has two parts: a) first-order beliefs: each recipient was asked to guess if the paired dictator would choose to *Roll*, and b) second-order beliefs: dictators, when there was a switch, were asked to guess the guess of the current recipient, for both those who were switched and those who were not.¹⁵
5. *Dictator's action.* The dictator chose either *Roll* or *Do not Roll*. All subjects were informed of their payoff for the round. As previously explained, when the received payoff was zero, a recipient could not be sure of the dictator's choice.¹⁶

At the end of the session, subjects were paid. All subjects received a fixed show-up fee of 2.50 tokens. One of the rounds was randomly chosen for payment. The payoffs shown in Figure 1 were computed in tokens (where one token=0.5 euro). Subjects were told truthfully that incentives for belief elicitation were provided for all rounds except the one chosen for payment; this removed any incentive to misreport beliefs.

2.3 Communication protocol

¹⁵ Belief elicitation is described in detail in the next section. We stress that we elicit beliefs after dictators know whether they have been switched but before dictators make their choice. Hence, we elicit switched and non-switched dictators' beliefs regarding their recipients' beliefs, as in Di Bartolomeo *et al.* (2019b, 2023b) and Vanberg (2008). Since switched dictators should infer the beliefs of the new recipients, elicitation is done after they read the chat of their partners. Again, dictators know that recipients do not know if they were switched.

¹⁶ Recipients could obtain a zero payoff in one of two ways: (i) their dictator had chosen *Don't Roll* or (ii) their dictator had chosen *Roll* and the outcome of the die-roll was "1."

Communication between participants is at the heart of our experiment. Research assistants, blind to our hypotheses, cataloged the messages.¹⁷ Recall that communication is bilateral and has back-and-forth messages. Moreover, participants chat before learning their role, knowing that one in each pair will be a dictator and the other will be a recipient.

We used the same protocol as Charness *et al.* (2024). Charness & Dufwenberg (2006) used free-form chat with a one-page limit and only one message sent from the second to the first mover. While we might expect more effective communication with more communication rounds, the 90-character restriction on each message could limit this. In our communication protocol, participants may be inclined and able to strike a deal of conditional cooperation: “I’ll pledge to choose *Roll*, if you also pledge to choose *Roll*.” When both players do so, we find it natural to define a “promise” as a promise to keep this agreement.¹⁸

Operationally, we asked the research assistants to classify each subject’s message according to whether it conveyed a pledge.¹⁹ We observe a “mutual promise (agreement)” if both sides state their intention to roll. By contrast, we classify all other communication outcomes as non-promises (without a mutual agreement). Our assumption is substantial since communication dynamics may also affect behavior. A dictator can infer some information about the kind of recipient they face from reading their communication. In principle, that information can be relevant. For instance, she could be more likely to support a recipient who claimed to be willing to roll.²⁰

3. Theoretical framework and hypotheses

We employ a simple utility-based model to structure our analysis and clarify some of the mechanisms underlying the choices observed in our experiment. The aim is not to capture the full

¹⁷ The research assistant was not involved in the design and execution of the experiment.

¹⁸ This random-dictator design is, therefore, different from Charness & Dufwenberg (2006). On this point, see Di Bartolomeo *et al.* (2023b), where unilateral one-shot communication is instead considered.

¹⁹ As in Vanberg’s protocol, each pair of chat messages sent by the same subject in a round was treated as a unit.

²⁰ In an online appendix, we consider a more detailed classification of messages classified as not leading to an agreement: “dictator’s unilateral promise,” “recipient’s unilateral promise,” and “no promise at all.” However, controlling for all taxonomies reduces the power of the tests since the number of observations in each resulting case drops correspondingly.

complexity of strategic reasoning, belief formation, or psychological motivations behind these choices, but just to anchor our exploration on how social identity, communication, and mutual promises interact in shaping behavior.

In the random dictator game used in our experiment, dictators choose between two actions: Not Roll, which yields a payoff of 14 tokens, or Roll, which results in 10 tokens and gives the recipient a 5/6 chance of receiving 12 tokens (on average).

Purely self-interested agents never choose to roll. However, substantial experimental evidence has shown that individuals often act because of altruistic, social, or norm-driven motivations. If we indicate by α_i , the effect on individual welfare (measured in tokens) from behaving altruistically, i.e., rolling, the dictator i chooses *Roll* when

$$(1) \quad 10 + \alpha_i > 14,$$

i.e., if $\alpha_i > 4$.

In what follows, we will introduce one at a time some key motivations related to social identity and communication that may explain altruism. In doing so, we will use the following notation.

3.1 Notation

We will use the indices $h \in \{NS, S\}$, $j \in \{I, O\}$, $k \in \{N, IN\}$, and $z \in \{M, A\}$ to classify dictators. Specifically, *NS* and *S* stand for Non-Switched and Switched, *I* and *O* stand for In-group and Out-group, and *N* and *IN* stand for National and International dictators. Finally, *M* and *A* stand for the dictator playing with a recipient who reached a Mutual agreement or an Absence of a mutual agreement during the pre-play communication phase. Given a characteristic $X \in \{h, j, z\}$, we will sometimes talk of an X -dictator if they have this characteristic. So, for example, a *N*-dictator is a national.

We will use R to indicate (average) roll rates. It will depend on the different attributes of dictators. To give an example, $R(NS, I, N, M)$ is the roll rate of a dictator who has directly communicated with the recipient (*NS*) who is matched with a recipient of the same group (*I*) who is national (*N*), and who has exchanged with the recipient a mutual promise to cooperate in the communication phase (*M*). We will omit one index when we are not focusing on the attribute the index refers to, so that the sample is made of agents that may or may not have the given attribute.

So, $R(NS)$ is the average roll rate of agents who have not been switched, irrespective of all other attributes these agents have.

3.2 Group favoritism

The first motivation we focus on is group favoritism. Akerlof and Kranton (2000) suggest that belonging to a certain group (in our case, being a national or an international student) may concur with the formation of the social identity of an individual and therefore condition their choices, for instance, leading them to behave more altruistically towards co-members. In particular, we would like to understand whether belonging to the same group as the recipient leads the dictator to roll more.

Formally, augmenting (1) with a social identity/group membership factor, indicated as in Akerlof and Kranton (2000) by m_i . If group membership produces social-identity favoritism, then $m_i(I)$ should be greater than $m_i(O)$, both for national and for international dictators. Dictator i will then roll if:

$$(2) \quad 10 + m_i(j, k) + \alpha_i > 14$$

We also investigate whether there is an asymmetry in this “in-group bias” between our two natural groups. Of course, testing for such an asymmetry makes sense only when the groups are natural.²¹ Summing up, when social identity is triggered by group membership, the likelihood of dictators rolling may be higher if they interact with in-group members, for both natural and artificial groups. However, the impact may differ depending on the nationality of the dictator.

To examine the possibility of in-group favoritism, we analyze the roll rates of switched dictators who are unaffected by direct communication by construction. Communication may affect social distance and, therefore, represent a confounding factor. By examining switched dictators alone, we isolate this communication effect. We then compare the average roll rate of switched dictators paired with in-group recipients to that of switched dictators paired with out-group recipients.

We are now ready to express the following hypothesis.

H1 (In-group favoritism). An in-group bias is observed if $R(S, I, k) > R(S, O, k)$.

²¹ In the sense that in artificial groups based on, e.g., color assignments, we do not expect any differences among groups. As previous experiments of this kind report (Ciccarone *et al.*, 2020; Charness *et al.*, 2024).

The above hypothesis provides us with three tests. We investigate the in-group bias in national and international dictators by fixing $k \in \{N, IN\}$, as well as the differences between the two.

In a similar context, a group favoritism effect is found by Ciccarone *et al.* (2020) and Charness *et al.* (2024), who work with artificial groups. This effect is likely to be even stronger with our natural groups, regardless of whether we deal with N -dictators or IN -dictators.

3.2 Communication

We now investigate the effects of communication *per se*, regardless of whether an agreement was reached through communication or not, by comparing the behavior of non-switched and switched dictators. There is considerable evidence from previous studies that this communication effect is positive with artificial groups (e.g., Charness *et al.*, 2024).

Let $s_i(NS)$ represent the effect (again measured in tokens) of the dictator's utility from rolling when matched with a recipient they have chatted with. The dictator i will then roll if:

$$(3) \quad 10 + m_i(j, k) + s_i(NS) + \alpha_i > 14$$

Clearly, if $s_i(NS) > 0$ the dictator i is more likely to roll when he has directly communicated with the recipient—all else equal. Our next hypothesis tests this straight communication effect.

In our design, communication can have different effects not only for in-group and out-group dictators, and for national and international dictators. We then formulate our next hypothesis as follows:

H2 (Communication and altruism). Direct communication increases the roll rate if $R(NS, j, k) > R(S, j, k)$.

H2 leads us to four tests by fixing $j \in \{I, O\}$ or $k \in \{N, IN\}$: two for in-group dictators (national and international) and another two for out-group dictators (national and international).

Charness *et al.* (2024) find that communication increases altruistic behavior both in-group and out-group dictators, but it does not eliminate in-group favoritism. This elimination would require the effect of communication to be stronger for the out-group than for the in-group. We study this issue within our framework, as it appears likely that the specific nature of the groups considered may influence the results. Indeed, the in-group bias is likely to be more challenging to

counteract through communication in a setting with natural groups, as in our case, the strength of social identity motivations and hence biases can be stronger and therefore more challenging to overcome through countervailing social practices.

Formally, we test the following hypothesis, which leads to two tests by fixing $k \in \{N, IN\}$ (i.e., focusing on either national or international students).

H3 (Communication and the in-group bias). Direct communication reduces the in-group bias if $R(NS, I, k) - R(NS, O, k) < R(S, I, k) - R(S, O, k)$.

In other words, to capture the effect of communication on in-group bias we use a difference-in-difference comparison. We look at whether the bias is stronger for non-switched than for switched dictators, for both national and international dictators. Since the national and international groups have different characteristics and different ways of communicating, the effect of communication on social identity can be different as well inside each group.

3.3 Promises

Charness *et al.* (2024) find that agreements (mutual promises of rolling) reached in the communication phase eliminate the in-group bias. This implies that promises are more effective in inducing altruistic behavior in out-group than in-group dictators.

To examine the effect of promises, we must inspect the content of communication. Deciding when communication has led to a mutual promise involves some subjectivity, making the investigation more challenging.²²

A mutual promise (agreement) is reached when each party tells the other of their intention to roll when acting as a dictator. We indicate the effect on utility of breaking a promise by c_i : breaking a promise might be costly $c_i(M) > 0$, while there cannot be any such cost when no promise has been made, $c_i(A) = 0$. Inequality (4) is then expanded to factor in this effect.

The dictator will roll if:

$$(5) \quad 10 + m_i(j, k) + s_i(h) + a_i > 14 - c_i(j, z)$$

Our hypothesis focuses on the possibility that promises are more effective for out-groups than in-groups, i.e., that promises may not only lead to higher rolling rates for all dictators but that the increase is higher for O -dictators than for I -dictators. In testing this hypothesis, we limit

²² In section 2.3 we already presented our classification strategy.

ourselves to the case in which dictators are not switched, because they cannot break a promise made to the partner they communicated with during the initial stage. In other words, we only consider cases where $h=NS$, even if we drop the index NS for ease of notation.

H4: *Mutual Promises override in-group bias.* Agreements are more effective in out-group than in in-group matches if $R(O, k, M) - R(O, k, A) > R(I, k, M) - R(I, k, A)$.

Again, we consider national and international dictators separately by fixing $k \in \{N, IN\}$.

3.2. 4 Guilt Aversion vs. Moral Commitment

In a final step to better understand the mechanisms through which promises influence behavior, we test whether actions are driven by a “moral commitment” purpose—honoring one’s own word—or a “guilt aversion” purpose—avoiding the disappointment of others, where we are using the terminology introduced by Vanberg (2008).

Our switching design enables us to capture this distinction because it creates an exogenous variation in promises. Specifically, non-switched dictators make the promise themselves, while switched dictators inherit a promise made by someone else. As recipients do not know whether their dictator has been switched, switched and non-switched dictators should have the same second-order beliefs on their partners’ expectations. Hence, any observed difference in their promise-keeping attitude cannot be explained by guilt aversion but only by moral commitment. That is evidence of moral commitment is supported by the observation of a higher average roll rate among non-switched dictators compared to the average observed among the switched ones (Vanberg, 2008).

Formally, we test the following hypothesis (H5), which provides us with two tests (one for in-group and one for out-group dictators, $j \in \{I, O\}$) for each natural group considered, $k \in \{N, IN\}$.

H5: *Promise keeping motivation (Vanberg’s) test.* People are less likely to respect agreements made by others than by themselves if $R(j, k, NS, M) > R(j, k, S, M)$.

The structured set of hypotheses presented in this section guides our empirical analysis. While stylized, the framework enables us to parse the relative weight of altruism, identity, communication, and moral commitment in determining prosocial behavior across naturally occurring social groups.

4. Experimental results

Our sample consists of 1920 dictator choices. The experiment involves 384 subjects, conducted over 12 sessions of 10 rounds each. Two distinct dynamics of interactions may emerge and combine. We may have ingroup bias, whereby individuals identifying with a group tend to support members of their own group (H1). Mere communication might impact cooperation (H2) and favoritism (H3). Mutual promises can generate cooperative effects that might counteract favoritism (H4), potentially motivated through different channels (H5). These effects can vary between the two natural groups (national and international students).

4.1 Favoritism

Table 1 tests the presence of ingroup bias where no direct communication occurs for both natural groups (H1), i.e., by looking at how switched dictators decide how to behave towards a partner (with whom they have not communicated before). Column (a) reports the average roll rate of switched international students when they interact with other international students (ingroup) versus when they interact with national students (outgroup), respectively. Column (b) reports the roll rates for switched national students when they interact with national students (ingroup) or with international students (outgroup), respectively.

Table 1 – Roll rates of switched dictators (no direct communication)

	International (a)	National (b)
Ingroup	0.60 (192)	0.52 (132)
Outgroup	0.46 (156)	0.45 (168)
In-group Bias	0.14**	0.07*

Note. Table reports the average roll rates for dictators who were switched and therefore did not communicate directly with their final partner. For the sake of fair comparisons, we consider switched pairs previously in the same situation, e.g., a switched ingroup (outgroup) pair is formed by two individuals who were both in the same ingroup (outgroup) prior to the switch that occurred. Column (a) refers to international dictators; column (b) to national dictators. The in-group and out-group conditions are based on the group identity of the recipient. The in-group bias is calculated as the difference in roll rates between in-group and out-group conditions. Asterisks denote significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$) based on Z-tests.

The table highlights a consistent pattern of in-group favoritism among international and national students (H1).

1. International students exhibit a marked preference for peers from their own group, with an in-group roll rate of 0.60, significantly higher than the 0.46 observed when interacting with national students (outgroup).
2. A similar, though slightly less pronounced, pattern is found among national students (ingroup), whose in-group roll rate reaches 0.52, compared to 0.45 when engaging with international students (outgroup).²³

Consequently, the in-group bias—calculated as the difference between the in-group and out-group roll rates—is more pronounced among international students, amounting to 14 percentage points and statistically significant at the 5% level ($Z=2.04$, $p=0.020$). The bias is comparatively smaller among national students at seven percentage points, yet it remains statistically significant at the 10% level ($Z=1.53$, $p=0.062$).

A further consideration concerns the emergence of in-group bias in our setting. Unlike in the minimal group paradigm, where effects tend to be symmetric across groups, our results reveal an asymmetry that is both meaningful and theoretically consistent. The bias is more pronounced among international students, who may have developed a stronger sense of group belonging through shared experiences and a heightened awareness of group membership. By contrast, the effect is weaker among national students, suggesting that natural groups—such as those based on nationality—can exhibit uneven patterns of favoritism, depending on the salience and cohesion of the underlying identity.

4.2 Communication

We now examine whether direct communication with a partner affects the behavior of dictators. Specifically, we investigate whether this effect depends on group membership, whether the partner is from the same or a different group, and whether it varies between international and national students (H2). To address these questions, we compare the roll rates of dictators who communicated directly with their partner to those who did not.

²³ Throughout the paper, all tests are Wilcoxon signed-rank tests on session-level data.

Table 1, previously introduced, reports the average roll rates of dictators who did not communicate directly. Instead, Table 2 presents the average roll rates of dictators engaged in direct communication (non-switched).

TABLE 2 – ROLL RATES OF NON-SWITCHED DICTATORS (DIRECT COMMUNICATION)

	International (a)	National (b)
Ingroup	0.57 (240)	0.60 (300)
Outgroup	0.54 (216)	0.50 (204)
In-group Bias	0.03	0.10**

Note. Table reports the average roll rates for dictators who were not switched and thus communicated directly with their final partner. Column (a) shows results for international dictators; column (b) for national dictators. In-group and out-group classifications refer to the recipient's group identity. The in-group bias is calculated as the difference in roll rates between in-group and out-group conditions. Asterisks indicate significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$) based on Z-tests.

Table 2 shows an ingroup bias only for National students (0.10). By contrast, the bias is not statistically different from zero (0.03) for international students.²⁴ As a first approximation, communication thus appears to eliminate bias among international dictators, but not among national ones. Let us investigate the point.

We test H2 by comparing the average roll rates of switched dictators (Table 1) and non-switched dictators (Table 2). This comparison allows for four tests since dictators can be national or international and interact with in-group or out-group partners.

1. Comparing switched and non-switched national dictators (columns b and row 2 of Table 1 and 2) who interacted with international recipients increases the average roll rate from 45% to 50% ($Z=1.10$, $p=0.272$ and $Z=1.18$).
2. This rate increases from 52% to 60% when national dictators (columns b and row 1 of Table 1 and 2) interacting with national recipients are considered ($Z=1.18$, $p=0.239$).

²⁴ The average difference in the roll rates of national ingroup and outgroup dictators is significantly higher than zero (0.60-0.50: $Z = 2.04$, $p = 0.020$). In contrast, the average difference in the roll rates of international ingroup and outgroup dictators is not significantly higher than zero (0.57-0.54: $Z = 1.02$, $p = 0.153$).

3. Similarly, we test H2 by comparing switched and non-switched international dictators when interacting with national recipients (columns a and row 2 of Tables 1 and 2); the average roll rate increases from 46% to 54% ($Z=1.26, p=0.209$).
4. In comparison, the average roll rate decreases from 60% to 57% ($Z=-0.35, p=0.724$) when interacting with international recipients (columns a and row 2 of Tables 1 and 2).

Differences are not statistically significant, as determined by conservative tests, in all cases.

Therefore, a difference in our results with respect to those from the same experiment (Charness *et al.* 2024), but using minimal groups is that communication does not in itself increase altruistic behavior.

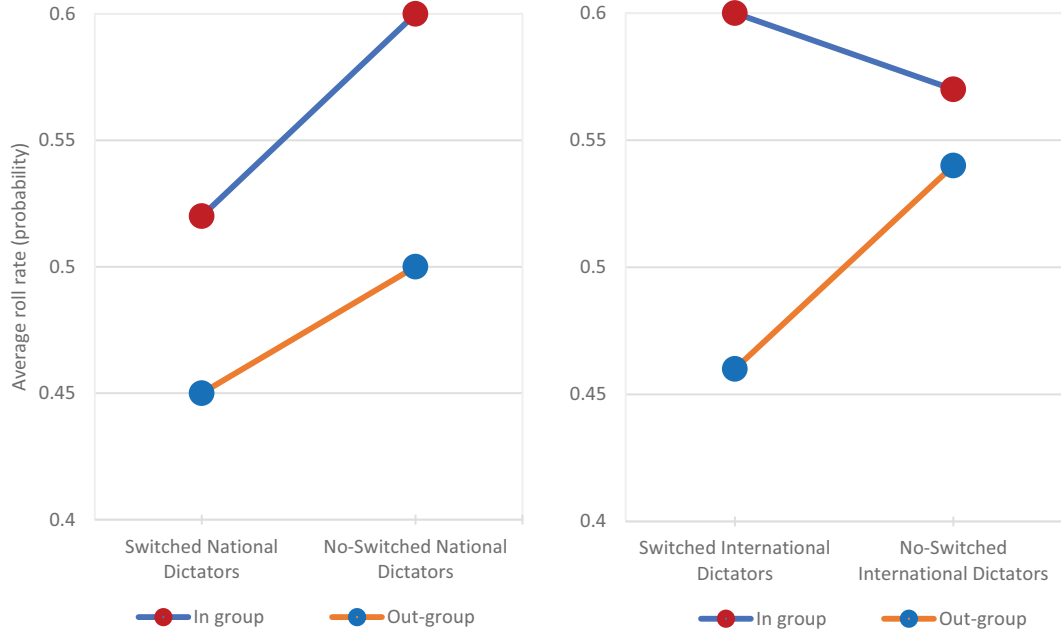
Now we look at H3 and ask: Does communication affect in-group bias? That is, does direct communication affect in-group dictators (national or international) more than out-group ones?

To answer this question (H3), we use our difference-in-difference approach. Figure 1 visualizes our experimental outcomes, illustrating the observed patterns. However, the answer is expected to be negative; in our case, we showed that communication does not appear to have any significant effect. Formally, we have two tests.

1. The average Roll rate for out-group national dictators increases from 0.45 to 0.52 with communication, while it increases from 0.50 to 0.60 for in-group national dictators. However, there is no significant difference between the differences (0.07 vs. 0.10: $Z=0.24, p=0.814$).
2. The average Roll rate for out-group international dictators increases from 0.46 to 0.60 with communication, while it increases from 0.54 to 0.57 for in-group international dictators. Again, there is no significant difference between the differences (0.14 vs. 0.03: $Z=1.26, p=0.209$).

We conclude that whether we examine national or international dictators, we find no evidence that direct communication affects in-group biases.

FIGURE 1 – THE EFFECT OF COMMUNICATION (H2 AND H3)



It is finally helpful to discuss our findings from H2 and H3 by comparing them with those of Charness *et al.* (2024), who investigate a minimal group paradigm. In their study, communication consistently increases roll rates—that is, dictators who did not switch partners show higher roll rates than those who did—even though it does not influence favoritism. By contrast, in our natural group setting, although the in-group bias appears to disappear in the table reporting results under communication for international students, a closer look at the underlying dynamics reveals a more nuanced picture. Communication *per se* does not affect behavior, whether among national or international dictators (H2), nor does it influence favoritism (H3).

Our results suggest that biases emerging in a natural group may be more persistent across contexts than those observed in minimal group settings, and therefore more challenging to eliminate in the real world.

4.3 Mutual Promises (agreements)

Next, we examine the contents of communication, specifically moving to H4 and H5. Since our experiment involves 384 subjects playing 10 rounds, our sample consists of 3840 messages (1920 chats). Notably, 53% of the chats were classified as leading to agreements, i.e., a mutual promise

to roll.²⁵ Chats among national (international) students led to an agreement in 52% (57%) of the cases. International students who chatted with a national one and later became dictators reached an agreement in 52% of the cases. National students who chatted with an international one and later became dictators reached an agreement in 53% of the cases. No subgroup of students formed agreements at a statistically significant rate different from that of any other.

Table 3 reports the roll rates of dictators (international and national) who did not reach a mutual support agreement during the communication phase (columns (a) and (c)) versus the roll rates of dictators who did it (columns (b) and (d)), distinguished by their interacting in-group (row (1)) or out-group (row (2)).

TABLE 3 – ROLL RATES OF DICTATORS WHO DID OR DID NOT MAKE PROMISES

	International		National	
	(a)	(b)	(c)	(d)
	No-switched	No-switched	No-Switched	No-Switched
	No Agree	Agree	No Agree	Agree
Ingroup	0.51 (102)	0.62 (138)	0.52 (140)	0.66 (160)
Outgroup	0.45 (113)	0.63 (103)	0.33 (98)	0.66 (106)
Bias	0.06	-0.01	0.19**	0.00

Note. The table reports roll rates for non-switched dictators, those who communicated directly with their final partner, broken down by whether an agreement was reached. Columns (a) and (b) present results for international dictators without and with an agreement, respectively; columns (c) and (d) do the same for national dictators. In-group and out-group classifications refer to the recipient's group identity. The in-group bias is the difference in roll rates between in-group and out-group recipients. Asterisks indicate statistical significance (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$) based on Z-tests.

Consistent with previous findings, the table reveals that dictators who form an agreement are, across all other conditions, more likely to choose prosocial action than those who do not. Among international dictators, being involved in a mutual promise increases the likelihood of rolling by an average of 11 percentage points in in-group settings and 18 percentage points in out-group

²⁵ Here, by promise, we intend a mutual declaration of rolling if chosen as dictator in the game. See Di Bartolomeo *et al.* (2023) for a discussion on the issue and methodology.

settings.²⁶ The effect is even more pronounced for national dictators, with prosocial behavior increasing by 14 percentage points in the in-group condition and by 33 percentage points in the out-group condition.²⁷ Hence, although communication does not increase altruistic behavior mutual promises do, in line with the previous literature on promise keeping.

Now we turn to the analysis of in-group bias. We do not observe any bias among international students. If an agreement is not reached (column a), the roll rate is 0.51 if the interaction is in-group, higher than when it is out-group, in which case it is 0.45, though this difference (0.06) is not statistically significant ($Z=1.18$, $p=0.119$). When an agreement is made (column b), the Roll rates in-group and out-group rise (to 0.62 and 0.63, respectively) and favoritism is not observed in either case ($Z=-0.67$, $p=0.747$). Of course, since we do not observe statistically significant ingroup bias, either with or without agreement, the difference-in-difference comparison (H4) provides no evidence of in-group bias (i.e., $0.62-0.63=-0.01$, while $0.51-0.45=0.06$, with $Z=1.41$, $p=0.158$).

Now, we turn to the case where the dictator is a national student. The in-group bias observed in Table 3 persists only when no mutual agreement has been made (column c). If no agreement is reached (column d), the in-group bias ($0.52-0.33$, a 19-percentage point difference) is significantly greater than zero ($Z=2.20$, $p=0.014$). However, once an agreement is in place, both in-group and out-group observed roll rates rise to 0.66. The difference-in-difference comparison (H4) supports the idea that agreements are more effective for out-groups. Promises significantly increase Roll rates and weakly eliminate in-group bias (i.e., $0.66-0.66=0.00$, while $0.52-0.33=0.19$, with $Z=1.65$, $p=0.099$).

Summarizing, Table 3 presents interesting findings. As anticipated from the existing literature, mutual promises significantly promote prosocial behaviors. However, in our natural

²⁶ The average roll rate of non-switched out-group international dictators when they agreed is 0.63 and when they did not is 0.45; the difference is significant $0.63-0.45$, $Z=2.00$, $p=0.022$; while the average roll rate of non-switched in-group international dictators when they agreed is 0.62 and when they did not is 0.51; the difference 0.62 vs 0.51 $Z=1.26$ $p=0.104$ is sizable but not statistically significant.

²⁷ Results show that having an agreement leads to higher levels of promise-keeping in both groups. In the national case, promise-keeping roll rates rise from 0.52 to 0.66 without and with agreement among the in-group, respectively, a statistically significant difference ($Z=2.51$, $p=0.006$). Among the out-group, promise-keeping significantly improves with an agreement from 0.66 to 0.33 without an agreement: $Z=2.82$, $p=0.002$.

group setting, interesting patterns seem to emerge as the behavior of national and international students is somewhat different. On the one hand, the effect of agreements between in-groups is only significant for national students; although similar in size, it is not significant for international students. On the other hand, the in-group bias without promises is stronger among national than among international students.

4.4 Motivations of promise keeping

Therefore, although the same result is observed following the agreements, the different starting points in their absence suggest that the mechanisms motivating national and international students to uphold agreements may differ. We explore this possibility in the following table, which helps us to test promise-keeping motivations (H5).

Table 4 allows us to examine the motivations for keeping promises. We employ the methodology Vanberg (2008) introduced to distinguish between moral commitment and guilt aversion as motivation for keeping promises. Table 4 reports the behavior of dictators who make promises to their partners versus those who interact with recipients who receive promises from others (H5).

Our experimental design ensures that the first-order beliefs of recipients are independent of whether they are switched or not, allowing us to assume and test the same for dictators' second-order beliefs.²⁸ This setup thus enables us to compare the behavior of dictators who are asked to keep their own promises with that of those who are asked to keep promises made by others. According to Vanberg (2008), dictators keeping only their own promises provide evidence supporting the hypothesis of moral commitment. Conversely, if they also honor promises made by others, the result aligns with guilt aversion, suggesting that dictators do not like disappointing recipients' expectations, regardless of who made the promise.

Table 4 presents the roll rates of international and national students who have entered into agreements and are paired with recipients who have also done so, distinguishing between in-group and out-group pairings.²⁹ It further differentiates between scenarios where the agreement was

²⁸ See appendix for details regarding all tests of first-and second-order beliefs.

²⁹ Note that we consider a switched dictator to be rematched with a recipient in the same condition as the original one, e.g., a dictator who made an agreement and is switched is considered only when rematched with a recipient who also agreed to roll with her/his partner.

established directly between the dictator and the recipient (no switch) and those where the agreement was made with another individual (switch). The numbers in parentheses indicate the sample sizes for each category.

TABLE 4 – PROMISE-KEEPING MOTIVATIONS (AGREEMENTS)

	International		National	
	Switched	Non-Switched	Switched	Non-Switched
Ingroup	0.62 (69)	0.62 (138)	0.62 (37)	0.66 (160)
Outgroup	0.51 (53)	0.63 (103)	0.40 (52)	0.66 (106)

Note. The table compares roll rates for dictators who were part of a mutual promise and distinguishes between switched and non-switched conditions. The goal is to assess whether promise-keeping is driven more by intrinsic commitment (non-switched dictators, who made the promise) or expectations (switched dictators, who inherited the promise). Columns report data separately for international and national dictators, with in-group and out-group referring to the recipient's group identity. Differences in roll rates help identify the underlying motivation behind promise-keeping.

Both international and national dictators consistently uphold promises when in-group, regardless of whether their recipients have switched partners. This behavior aligns with motivations based on guilt aversion for not keeping promises received from their partners, even when other people made these promises. The in-group average roll rate for international students remains steady at 0.62, irrespective of partner switching (0.62 vs. 0.62: $Z=0.62$, $p=0.534$). Similarly, national students exhibit an in-group average roll rate of 0.62 for those who switched partners and 0.66 for those who did not, indicating a negligible and statistically insignificant difference (0.66 vs. 0.62: $Z=0.66$, $p=0.508$). These findings suggest that both national and international students are motivated to a similar degree by guilt aversion towards in-group members.

In the out-group condition, we find something different. For international dictators interacting with an outgroup, the roll rates are 0.51 for those who switched and 0.63 for those who did not switch, indicating stronger effects of promises in no-switch situations. However, the statistical test (0.63 vs. 0.51: $Z=0.98$, $p=0.325$) shows this difference is insignificant. By contrast, in the case of out-group national dictators, the average roll rate significantly drops from 0.66 to 0.40 when participants switch partners (0.66 vs. 0.40: $Z=2.25$, $p=0.025$). This suggests that moral

commitment emerges among national students interacting with out-group members. In other words, only their own promises are kept by national dictators interacting with the out-group.

Overall, we can conclude that guilt aversion is as strong as moral commitment as a motivation when dictators interact with their own group, while moral commitment is a stronger motivation when they interact with an outgroup. However, the difference in strength between the two motivations is only significant for national dictators.

5. Gender effects

We now turn to the role of gender in shaping behavior, examining whether in-group favoritism varies systematically across gender lines. It should be emphasized from the outset that the analysis presented in this section is speculative, as it is not grounded in well-established hypotheses concerning gender effects. The existing literature on this issue remains inconclusive (Charness *et al.*, 2024).

5.1 In-group favoritism and gender

Table 5 reports in-group bias for switched dictators, broken down by gender and group membership. Table 5 is like Table 1, but the sample is split by gender.

TABLE 5 – INGROUP FAVORITISM BY GENDER (SWITCHED DICTATORS)

	International		National	
	Male (a)	Female (b)	Male (c)	Female (d)
Ingroup	0.57 (67)	0.62 (125)	0.55 (56)	0.49 (76)
Outgroup	0.44 (50)	0.47 (106)	0.40 (78)	0.49 (90)
Bias	0.13	0.15**	0.15***	0.00

Note. The table presents roll rates for switched dictators who did not communicate directly with their final partner, disaggregated by gender and group identity. Columns (a) and (b) show results for international male and female dictators, respectively; columns (c) and (d) show results for national male and female dictators. In-group and out-group classifications refer to the recipient's group identity. The in-group bias is calculated as the difference in roll rates between in-group and out-group recipients. Asterisks denote significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$) based on Z-tests.

Looking at favoritism, we see that among international students, the in-group bias is relatively uniform in size across genders. However, it is statistically significant only for females (0.62 vs. 0.47: $Z=1.92, p=0.027$), while for males it is not (0.57 vs. 0.44: $Z=1.18, p=0.119$). On the contrary, among national students, males exhibit in-group bias, as it is clear from column (c), where the bias is sizable and (weakly) significant (0.55 vs. 0.40: $Z=1.29, p=0.098$), while females do not at all (0.49 vs. 0.49: $Z=0.28, p=0.392$).

Summing up, among international students, the social identity effect is stronger in females than in males, while among national students, the opposite is true. How can we explain that favoritism is related to gender so differently among national and international students? Our answer to this question hinges on the different nature of the bond integrating our two groups.

The specificity of the connection among Italian students is represented by their citizenship, generally associated with a sense of national identity. Yuval-Davis and Anthias (1989), among many others, observe that gender and nationalism are deeply interconnected, as nationalist ideologies often rely on traditional gender roles with men idealized as protectors of the nation and women as its mothers.³⁰ However, this reliance is, for women, in obvious tension with their changed roles in our societies. This may help explain our finding of a lower impact of national identity on behavior among females.

We can also add that our findings align with other evidence. Specifically, using EVS data for Italy, we calculate that around 31.8% of men aged 18-29, versus 19% of women (survey-weighted proportions), say they are very proud of their country; the point difference is weakly statistically significant when tested with a survey-weighted logit regression. Looking at data for other countries from the same data source, we find qualitatively similar effects.³¹ Finally, we can mention that a gender divide has been found in the electoral behavior of Gen Z voters in advanced democratic countries, with males disproportionally voting for nationalistic right-wing parties.³²

³⁰ In the image of man (1998), historian George L. Mosse explores how modern masculinity—especially in Europe—was shaped alongside and in service of nationalist ideology.

³¹ Data: European Values Study Integrated Dataset 2017 (EVS 2017). GESIS Data Archive, Cologne, study no. ZA 7500, version 5.0.0 (2022-05-16). DOI: 10.4232/1.13897.

³² See among others the Milosav *et al.* (2025) study of 25,000 under 30 voters in 27 EU states.

We now try to figure out an interpretation of why international females are more ingroup-biased than their male counterparts and Italian females. Our interpretation is admittedly speculative, although it remains consistent with the experimental evidence.

Our proposed explanation is based on two premises, which we detail in what follows. The first is that the bond keeping together the international group is more likely than for the other group to be based on its members being personally acquainted with one another. The second premise is that a vast body of psychological and philosophical literature suggests that personal relationships play a prominent role in the moral orientations of women.

As to the first premise, the probability that international students have interacted with each other outside the laboratory is higher than for national students for three reasons:

- 1) The former tends to cluster in a narrower set of English-taught courses and are more likely to encounter one another in administrative settings (such as Erasmus or international student offices) or at events organized explicitly for them.
- 2) International students often face logistical and bureaucratic challenges, such as securing housing or navigating residency requirements, that affect domestic students less or not at all. These shared difficulties create opportunities for informal exchanges of information and assistance, strengthening interpersonal ties.
- 3) International students are more likely to seek new friendships and social connections locally as their friends and family are physically out of reach. Since domestic students already have established social networks, international students are more likely to form bonds among themselves.

We now come to our second premise on the female difference in moral attitudes. This was first theorized by Gilligan (1982), who argued that there are two moral voices: the “justice voice”, focused on abstract rules, seen more often in boys/men, and the “care voice”, focused on relationships and more prominent among girls/women. Neither voice is superior—they are different, but equally important in keeping societies together.³³ Of course, this does not imply that

³³ Philosophers such as Held (2006) and Tronto (2016) have extended care ethics from private morality to public life. Held argues that care should be a foundational element of moral theory, not subordinate to justice or rights. Tronto (2016) contends that care must be recognized as a political responsibility, rather than being relegated to females, minorities, or the marginalized, as is often the case in liberal societies that privilege autonomy over interdependence.

only women care for people they are emotionally related to and only men follow more abstract ideals. Rather, societal gender roles shape which voice people tend to develop: different life experiences shape different moral concerns. As a matter of fact, even in today's advanced societies, women's experiences push them to be more attentive to the needs of those close to them.

Summing up, since, for the reasons given above, international students are more likely to know each other, what is known from experimental psychology about the different moral orientations of women and men makes it hardly surprising that in our experiment international women show a stronger attachment to their group than international men and Italian women to theirs.

5.2 In-group favoritism, gender, and promise-keeping motivations

We look at the motivations for keeping promises by gender. Table 6 is like Table 4, but the sample is split by gender.³⁴

TABLE 6 – PROMISE-KEEPING MOTIVATIONS (AGREEMENTS) BY GENDER

	Females		Males	
	Switched	Non-Switched	Switched	Non-Switched
Ingroup	0.63 (64)	0.58 (177)	0.62 (42)	0.74 (121)
Outgroup	0.48 (60)	0.69 (123)	0.42 (45)	0.58 (86)
Bias	0.15*	-0.11*	0.20	0.16

Note. The table compares roll rates for dictators who were part of a mutual promise and distinguishes between switched and non-switched conditions. The goal is to assess whether promise-keeping is driven more by intrinsic commitment (non-switched dictators, who made the promise) or expectations (switched dictators, who inherited the promise). Columns report data separately for female and male dictators, with in-group and out-group referring to the recipient's group identity. Differences in roll rates help identify the underlying motivation behind promise-keeping. The in-group bias is calculated as the difference in roll rates between in-group and out-group recipients. Asterisks denote significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$) based on Z-tests.

Among female dictators who made an agreement with an outgroup member, the average roll rate significantly drops from 0.69 to 0.48 when they switch partners (0.69 vs. 0.48: $Z=2.12$, $p=0.034$). By contrast, the average roll rate increases from 0.58 to 0.63 when ingroup switch

³⁴ Remember that we consider a switched dictator to be re-matched with a recipient in the same condition as the original one.

members who made an agreement, although the increase is not statistically significant (0.58 vs. 0.63: $Z=-1.07$, $p=0.286$). This suggests that moral commitment emerges among females interacting with out-group members. In the other case, motivations seem related to the guilt-averse theory.

Among male dictators who made their own agreement with ingroup or outgroup members, the average roll rate drops from 0.74 to 0.62 ($Z=0.77$, $p=0.441$) and from 0.58 to 0.42 ($Z=1.13$, $p=0.259$) when they switch. In both cases, however, increases are not statistically significant. Hence, male motivations for promise keeping are consistent with guilt aversion theory. Our findings are consistent with those of Di Bartolomeo *et al.* (2024), who exploit gender differences by using exogenous variations in expectations and promises.

6. Conclusions

This study explores the role of communication in shaping cooperation and mitigating social identity biases within naturally occurring groups. Unlike settings based on arbitrary group assignments, our experiment involves two “natural groups”—Italian and international students from the same academic environment—where group identity is therefore socially grounded and potentially more salient.

We find that general communication alone does not reduce the bias in favor of in-group members: individuals continue to act more favorably toward those who share their group identity. However, when communication leads to mutual promises, cooperation increases significantly, particularly across group lines. In such cases, the social identity gap narrows or disappears, suggesting that mutual commitments can partially substitute for shared identity in fostering prosocial behavior.

Additionally, our data reveal a notable asymmetry in behavior across groups. While artificial groupings typically preclude such asymmetries by design, our natural group context allows them to emerge. Specifically, in-group favoritism is especially pronounced among male Italian participants and female international participants.

We interpret this pattern considering the different possible sources of social identity arising from the two groups: national identity shared by Italian students is cultural and abstract, whereas the bonds among international students are more relational and experience-based. These findings align

with broader insights and empirical evidence into how identity expression may vary by gender, with women more attuned to interpersonal dynamics while nationalism is stronger among men.

Our results suggest that while social identity remains a powerful driver of behavior, structured communication, particularly mutual promises, can play a critical role in bridging group divides, even when those identities are deeply embedded, and confirms that results from the minimal group paradigm may or may not survive in a natural group setting.

Appendix A – Beliefs (exogenous variations)

Elicitation of first-order beliefs: After communication, recipients were asked to guess what their (unknown) dictators would choose to do. Recipients could guess by ticking one of the five-point scale in Table A. This scale is the same as in Vanberg. Beliefs are then rescaled to 1, 0.75, 0.5, 0.25, and 0. Thus the numbers shown in Table 1A and (in Tables 2A and 3A below) represent the averages of recipients' (dictators') re-scaled responses. The payoffs correspond to a quadratic scoring rule for probability values 85%, 68%, 50%, 32%, and 15%, because due to the risk neutrality assumption, quadratic scoring yields flat payoffs as probabilities approach one (see Vanberg, p. 1472).³⁵

Table A – Incentives for first-order belief elicitation

The dictator will Please tick your guess	choose <i>Roll</i>			choose <i>Don't Roll</i>	
	Certainly	Probably	Unsure	Probably	Certainly
	☐	☐	☐	☐	☐
Your earnings if the dictator					
chooses <i>Roll</i>	0.65 tokens	0.60 tokens	0.50 tokens	0.35 tokens	0.15 tokens
chooses <i>Don't Roll</i>	0.15 tokens	0.35 tokens	0.50 tokens	0.60 tokens	0.65 tokens

Elicitation of second-order beliefs: After dictators were told whether their paired subject had been switched or not, they were asked to guess his guess. Specifically, they had to guess which of

³⁵ We also verify the robustness of our results to the quadratic scoring rule. Results are available upon request.

the five points of Table A had been ticked by their counterpart. Correct guesses were paid 0.50 tokens.

Table 1A – Ingroup favoritism (FOBs switch)

	International	National
Ingroup	0.67 (264)	0.61 (204)
Outgroup	0.65 (252)	0.63 (240)

Note. The table reports the first-order beliefs of dictators who were switched and therefore did not communicate directly with their final partner. Column (a) refers to international dictators; column (b) to national dictators. The in-group and out-group conditions are based on the group identity of the recipient. The in-group bias is calculated as the difference in roll rates between in-group and out-group conditions. Asterisks denote significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$) based on Z-tests.

The average difference in the first-order beliefs of switched national ingroup and outgroup dictators is not significantly higher than zero (0.61-0.63: $Z=-0.63$, $p=0.530$). The average difference in the second-order beliefs of switched international ingroup and outgroup dictators is not significantly higher than zero (0.67-0.65: $Z=0.47$, $p=0.638$).

The average difference in the first-order beliefs of switched international and national ingroup dictators is not significantly higher than zero (0.67-0.61: $Z=1.65$, $p=0.100$). The average difference in the first-order beliefs of switched international and national outgroup dictators is not significantly higher than zero (0.65-0.63: $Z=1.02$, $p=0.308$).

Table 2A – Ingroup favoritism (SOBs switch)

	International (a)	National (b)
Ingroup	0.69 (192)	0.65 (132)
Outgroup	0.67 (156)	0.65 (168)
Bias	0.01	0.00

Note. The table reports the second-order beliefs of dictators who were switched and therefore did not communicate directly with their final partner. Column (a) refers to international dictators; column (b) to national dictators. For the sake of fair comparisons, we consider switched pairs previously in the same situation, e.g., a switched ingroup pair is formed by two individuals that were both ingroups previous than the switch occurred. The in-group and out-group conditions are based on the group identity of the recipient. The in-group bias is calculated as the difference in roll rates between in-group and out-group conditions. Asterisks denote significance levels (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$) based on Z-tests.

The average difference in the second-order beliefs of switched national ingroup and outgroup dictators is not significantly higher than zero (0.65-0.65: $Z=0.16$, $p=0.888$. The average difference in the second-order beliefs of switched international ingroup and outgroup dictators is not significantly higher than zero (0.69-0.67: $Z=0.39$, $p=0.695$).

The average difference in the second-order beliefs of switched international and national ingroup dictators is not significantly higher than zero (0.69-0.65: $Z=0.94$, $p=0.346$. The average difference in the second-order beliefs of switched international and national outgroup dictators is not significantly higher than zero (0.67-0.65: $Z=0.31$, $p=0.754$).

Table 3A – The effects of communication (SOBs no-switch)

	International		National	
	No Agreement	Agreement	No Agreement	Agreement
Ingroup	0.61 (102)	0.82 (138)	0.61 (140)	0.78 (160)
Outgroup	0.60 (113)	0.79 (103)	0.55 (98)	0.81 (106)
Bias	0.01	0.03	0.06	-0.04

The average difference in the second-order beliefs of international ingroup and outgroup dictators who did not reach an agreement is not significantly higher than zero (0.61-0.60: $Z=0.78$, $p=0.433$. The average difference in the second-order beliefs of international ingroup and outgroup dictators who make an agreement is not significantly higher than zero (0.82-0.79: $Z=0.43$, $p=0.666$. The average difference in the second-order beliefs of national ingroup and outgroup dictators who did not reach an agreement is not significantly higher than zero (0.61-0.55: $Z=1.41$, $p=0.158$). The average difference in the second-order beliefs of national ingroup and outgroup dictators who make an agreement is not significantly higher than zero (0.78-0.81: $Z=-1.65$, $p=0.100$).

Table 4A – Promise keeping (SOBs no-switch and switch)

	International		National	
	Switch	No-Switch	Switch	No-Switch
Ingroup	0.74 (69)	0.82 (138)	0.83 (37)	0.78 (160)
Outgroup	0.77 (53)	0.79 (103)	0.77 (52)	0.81 (106)
Bias	-0.03	0.03	0.06	-0.03

The average difference in the second-order beliefs of international ingroup no-switched and switched dictators who reach and read an agreement is not significantly different from zero (0.82-0.74: $Z=0.98$, $p=0.328$). The average difference in the second-order beliefs of international outgroup no-switched and switched dictators who reach and read an agreement is not significantly different from zero (0.79-0.77: $Z=0.71$, $p=0.480$). The average difference in the second-order beliefs of national ingroup no-switched and switched dictators who reach and read an agreement is not significantly different from zero (0.78-0.83: $Z=-1.58$, $p=0.114$). The average difference in the second-order beliefs of national outgroup no-switched and switched dictators who reach and read an agreement is not significantly different from zero (0.81-0.77: $Z=1.17$, $p=0.241$).

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