Group identity and peer effects in rule-following

Bicchieri, C.; Gächter, S.; Molleman, L. ; Nosenzo, D.

DOI
10.1016/j.jebo.2025.107264
Publication date
2025
Document Version
Final published version
Published in
Journal of Economic Behavior & Organization

Journal of Economic Behavior and Organization 239 (2025) 107264

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Research paper

Group identity and peer effects in rule-following ☆

Cristina Bicchieri [a], Simon Gächter [b,c,d,*], Lucas Molleman [e], Daniele Nosenzo [f]

[a] Center for Social Norms and Behavioral Dynamics, University of Pennsylvania, United States of America
[b] School of Economics, University of Nottingham, United Kingdom
[c] CESifo, Germany
[d] IZA, Germany
[e] Faculty of Social and Behavioural Sciences, University of Amsterdam, The Netherlands
[f] Department of Economics and Business Economics, Aarhus University, Denmark

ARTICLE INFO

Dataset link: https://doi.org/10.17605/OSF.IO/ETF9M

JEL classification:
C91
D03

Keywords:
Norms
Rule following
Y task
Peer effects
Group identity
Social proximity

ABSTRACT

Social life is governed by a myriad of rules but the behavioral logic of why people follow rules is only incompletely understood. Here, we investigate how rule following is influenced by other people and social proximity to them. In particular, we are interested in the identity composition of an individual's observed peer group: does it matter whether a rule breaker is an ingroup or an outgroup member? To investigate this question, we use a novel abstract rule-following task with strong incentives to break the rule. We call our rule-following task the "Y task" because the rule requested participants (n=7,033 Prolific workers) to take one of the two diverging paths of a Y-shaped maze. Consistent with previous research, we show that examples of rule violations trigger further rule violations, even though overall rule compliance remains high. Contrary to our hypotheses, and research on honesty and cooperation, we do not find that group identity moderates the influence of peer compliance on people's willingness to follow rules. We conclude that rule breaking is contagious regardless of the ingroup or outgroup status of the rule breaker.

1. Introduction

Respect for rules and norms lies at the heart of human social behavior, ensuring the establishment and maintenance of relationships based on cooperation, trust, reciprocity, and honesty (e.g., Bicchieri, 2006, 2016; Krupka and Weber, 2013; Kimbrough and Vostroknutov, 2016; Fehr and Schurtenberger, 2018; Gächter et al., 2025). In recent years, economists and behavioral scientists have become increasingly interested in understanding the forces that shape this fundamental human trait, exploring the factors that strengthen or weaken the willingness to comply with rules and norms.

An important behavioral force behind rule following — and the focus of this paper — is conformism. Conformity with social expectations (what other people do and think one should do) plays a central role in the social norms framework of Bicchieri (2006) and Bicchieri (2016), and the CRISP rule-following framework of Gächter et al. (2025), who provide evidence from rule-following experiments that conditional conformity with social expectations plays a central role for rule compliance. Consistent with the importance of conditional rule compliance, information about other individuals' norm-following propensity systematically affects compliance (e.g., Cialdini et al., 1990; Keizer et al., 2008; Bicchieri and Xiao, 2009; Gächter et al., 2013; Goerg et al.,

☆ We are grateful to Jack Goldberg, Karl Meyer and Alex Shpenev for excellent research assistance. This work was supported by the European Research Council[grant number ERC-AdG 101020453 PRINCIPLES].

* Corresponding author at: School of Economics, University of Nottingham, United Kingdom.
E-mail addresses: cb36@sas.upenn.edu (C. Bicchieri), Simon.Gaechter@nottingham.ac.uk (S. Gächter), l.s.molleman@uva.nl (L. Molleman), daniele.nosenzo@econ.au.dk (D. Nosenzo).

https://doi.org/10.1016/j.jebo.2025.107264
Received 18 April 2025; Received in revised form 23 August 2025; Accepted 24 September 2025
Available online 1 October 2025

2024). However, these so-called "peer effects" are asymmetric, as observing examples of rule violations exert a stronger effect than observing examples of rule compliance (Gächter et al., 2025). The extent of social proximity people perceive between themselves and the peers whose behavior they observe has also been found to regulate compliance with norms (e.g., Gino et al., 2009; Barr et al., 2018; Dimant, 2019; Bicchieri et al., 2022; Lane et al., 2024).

In this paper, we sharply focus on how observing rule compliance by other people and the social proximity to them — whether they are "ingroup" or "outgroup" — influences people's compliance with rules. Recent work by Bicchieri et al. (2022) shows that in the context of a take or give donation game, social proximity moderates the relative strengths of "bad" (taking) and "good" (giving) examples: both types of examples affect behavior when they are set by socially proximate peers, whereas only bad examples are influential in the case of socially distant peers. Moreover, Bicchieri et al. (2022) present suggestive evidence that the effect is moderated by the degree of heterogeneity in the perceived proximity of one's reference group: the influence of good and bad examples in give or take behavior is stronger in groups with a majority of socially proximate members.

This latter finding deserves further scrutiny because the relation between social heterogeneity in groups and peer influence may be complex. Specifically, the relative share of socially proximate ("ingroup") peers in one's reference group may affect whether individuals engage in comparisons that result in behavioral assimilation with or differentiation from peers' behavior. Behavioral assimilation occurs when an individual's behavior becomes more similar to their peer's behavior, while behavioral differentiation implies a tendency to differentiate one's behavior from that of a peer.

Which effect is likely to prevail depends on the direction of the "contrast effect", a cognitive bias that occurs when something stands out significantly from its surroundings (e.g., Brewer and Weber, 1994; Mussweiler et al., 2004; Bordalo et al., 2022). Because ingroup-outgroup distinctions ("us vs. them") abound in the real world, our conjecture is that observing examples of rule violations by a socially distant minority (an "outgroup" minority) may make the rule violation stand out more and in a more negative light, resulting in *more* compliance - relative to a situation where the same violation is instead set by a socially proximate ("ingroup") peer. For example, the failure of an ethnic minority member to offer up their seat on public transport to a person who needs it more might be perceived more negatively than the same failure by an ethnic majority member, who might therefore become more likely to offer their seat. The flip side of this logic is that a violation by an ingroup minority member may carry a disproportionate weight in triggering rule violations compared to violations by outgroup individuals.

In this paper, we set out to examine these conjectures, using an incentivized experiment based on a novel rule-following task, called the "Y task" (because the decision interface is Y-shaped). We developed the Y task based on previous rule-following tasks by Kimbrough and Vostroknutov (2016, 2018) and Gächter et al. (2025). In the Y task, participants face a trade-off between monetary payoff maximization and compliance with an arbitrary costly rule set by the experimenter where rule breaking affects nobody. In terms of the CRISP framework by Gächter et al. (2025) — which explains rule compliance (C) as driven by an intrinsic respect for rules (R), extrinsic incentives (I), conformity with social expectations (S), and social preferences (P) — only S and R can account for rule following in our setup. This is because the setting is anonymous, extrinsic incentives favor breaking the rule, and social preferences are irrelevant here, as breaking the rule has no impact on others.

Prior to making a decision in the Y task, we let participants observe the behavior of a reference group of peers who have previously completed the task. Across treatments, we exogenously vary the extent to which the peer group complies with the rule. Further, we manipulate the social proximity between the participants and the members of the reference group (that is, the ingroup-outgroup composition of the peer group), using a minimal group identity manipulation. While previous research has studied norm violations by ingroup and outgroup members in the context of honesty (e.g., Gino et al. (2009) and Gross and Dreu (2020)) or cooperation (e.g., Kerr et al. (2009)), we are not aware of studies that investigate how rule violations in a rule following setup depend on ingroup or outgroup distinctions, and whether there is a contrast effect.

Our design is based on ten between-subjects treatments with 7,033 Prolific workers, allowing us to explore a rich set of configurations where rule violations are committed by socially proximate or socially distant individuals within reference groups that are socially homogeneous or heterogeneous. We find evidence that rule violations by peers are contagious, although, overall, rule compliance remains high. However, in contrast to our conjectures, these contagion effects are not moderated by the peers' social proximity, that is, whether the rule violator is ingroup or outgroup, nor by the share of ingroup or outgroup members in the observed peer reference group.

2. Experimental design and hypotheses

Our experiment consisted of two tasks, described in the following, which were administered sequentially to the participants.

2.1. Part 1: Group identity manipulation

The first task was a minimal group identity manipulation to classify the participants into two arbitrary categories, based on the response they gave to a well-known optical illusion ("The Dress", see e.g., Schlaffke et al., 2015): participants saw a picture of a striped dress that some people perceive as blue-and-black, while others as white-and-gold. After seeing the picture, participants reported the colors they perceived and their response was used to assign them to a group.

In a pilot experiment, we ran a *manipulation check* to assess the extent to which the task induced group identification. Using a third-party allocation game (split a dollar between an ingroup and an outgroup member), we compared the ingroup bias induced by our identity manipulation with that induced by a non-minimal identity manipulation based on partisan identities (Democrats vs. Republicans in the US). In both manipulations, we found evidence of a statistically significant ingroup bias ($p < 0.001$). Furthermore,

Table 1
Overview of treatments.

	0 violations	1 ingroup violation	1 outgroup violation
7 ingroup, 0 outgroup	7In-0Out-0V (n = 715)	7In-0Out-1InV (n = 697)	–
6 ingroup, 1 outgroup	6In-1Out-0V (n = 705)	6In-1Out-1InV (n = 697)	6In-1Out-1OutV (n = 714)
1 ingroup, 6 outgroup	1In-6Out-0V (n = 705)	1In-6Out-1InV (n = 697)	1In-6Out-1OutV (n = 703)
0 ingroup, 7 outgroup	0In-7Out-0V (n = 696)	–	0In-7Out-1OutV (n = 704)

we found that minimal and non-minimal manipulations induced similar and statistically indistinguishable levels of ingroup bias in the third-party allocation game ($p = 0.111$). These results indicate that we have successfully manipulated group identities.[1]

2.2. Part 2: The "Y task" and experimental treatments

In the second part, participants completed our novel rule-following task, called the "Y task". Participants had to move a circle figure along a Y-shaped path from the bottom to the top of the screen (see Panel A of Fig. 1). Participants had to decide whether to go left or right at the fork in the middle of the Y-path, with one option paying twice as much as the other (1.00 USD vs. 0.50 USD; the option that pays more was randomized across participants). The instructions, however, stated that the rule was to choose the option that pays the least ("The rule is to turn left (right) at the fork"). Thus, as in previous rule-following tasks (Kimbrough and Vostroknutov, 2016, 2018; Gächter et al., 2025; Suri et al., 2025), participants in the Y task faced a trade-off between rule compliance and maximizing material payoffs.

Unlike the traffic light tasks (Kimbrough and Vostroknutov, 2016; Gächter et al., 2025), the Y task is fully abstract and de-contextualized. Additionally, tasks like the traffic light scenarios, the abstract rule-following task of Gächter et al. (2025), and the "coins task" by Suri et al. (2025), all involve implicit waiting—e.g., waiting for a light to turn green, an "x" to disappear, or a "+" to become a "−". The Y task avoids this confound, as it involves no waiting. In this respect, it resembles the ball task (Kimbrough and Vostroknutov, 2018), which is also abstract and free of contextual or time-based elements. However, the Y task differs in offering a binary choice between compliance and violation, whereas the ball task permits a continuum of rule-following behavior.

After reading the instructions and completing a comprehension check, but before making a choice (left/right at the fork), participants viewed a brief animation showing how seven other participants ("peers") had behaved in the same task.[2] Our between-subjects treatments manipulate the group identity composition of the seven peers in the animation, as well as how they behaved in the task according to two design principles.

First, we varied the fraction of the seven peers who shared the same minimal identity as the participant, to create peer groups that were either homogeneously proximate (i.e., all seven peers shared the same minimal identity: the "ingroup"; treatments names including "7In-0Out"), or homogeneously distant (all peers had a different minimal identity than the participant: the "outgroup"; "0In-7Out"), or heterogeneous with either one outgroup minority (six ingroup peers and one outgroup peer; "6In-1Out") or one ingroup minority (six outgroup peers and one ingroup peer; "1In-6Out").

Second, treatments also differed in whether peers violated or complied with the rule. In particular, either all peers complied with the rule (i.e., the participant observed zero violations; "0V"), or six peers complied and 1 violated the rule. In the latter case, we varied whether the violator shared the same identity as the participant ("ingroup violation"; "1InV"), or not ("outgroup violation"; "1OutV").

In summary, our design considerations resulted in ten between-subjects treatments, shown in Table 1 with their corresponding labels and the number of participants.

Note also that our design allows us to isolate the effect of the violator's identity without confounding it with overall group composition. This is because our design includes two variants of conditions with six ingroup members and one outgroup member—the only difference being whether the violator is ingroup or outgroup ("6In-1Out-1InV" versus "6In-1Out-1OutV"). Similarly, we can compare "1In-6Out-1InV" versus "1In-6Out-1OutV".

[1] We report further details of this pilot in Online Appendix A.

[2] The animation started with all seven peer circles in the bottom-left corner of the screen (the subject's own circle was in the bottom-right corner). One by one, the seven peer circles then moved, turning either left or right at the fork, depending on the actual decisions made by the previous participants (for a gif of the task, see https://cmstudies.net/media/Y-maze_task.gif). These participants were recruited from the same participant pool for two earlier pilot studies ($N = 71$, and $N = 204$) that we used to create the experimental conditions and to determine the sample size for the main experiment. The aim of the pilot studies was to obtain a sufficient number of in-group and out-group participants who either followed or violated the rule (at least 7 in each category). From these data, we manually selected individual responses to construct the experimental conditions in the main experiment. Thus, while the peer behavior shown was based on real decisions made in the pilot studies, the specific individuals were selected by us to match the treatment design. This procedure did not involve deception because all decisions shown to participants were based on actual rule-following decisions by previous participants. The percentages of participants who followed the rule in these pilots are reported in Online Appendix B.

The reader may ask why we only study one or no rule violation. Of course, one violation is the minimal case of how deviance in a group can look like, but given our focus on possible effects of ingroup and outgroup examples, this minimal setup already results in ten treatments (see Table 1). The case of more violations was studied by Gächter et al. (2025) although not in an ingroup-outgroup context. They studied group sizes where participants could see 1, 2, …, 6 peers who could either violate the rule or comply with it (for a total of 28 between-subjects treatments with systematically varying shares of violators and compliers). Having more than one violator and an ingroup-outgroup treatment would have exploded the design. Moreover, Gächter et al. (2025) found that the biggest relative effect already occurred with one violator. We therefore opted for one violator only, which allowed us to run a high-powered experiment with 7,033 participants (i.e., about 700 participants per treatment cell).

2.3. Hypotheses

The treatment variation was designed to test three main hypotheses and three auxiliary hypotheses to rule out alternative interpretations. In particular:

Main Hypothesis 1. *Observing one example of rule violation reduces rule compliance.*

Hypothesis 1 is derived from evidence that (1) based on strategy method data, people are conditional rule followers who only follow rules if others follow them (McBride and Ridinger, 2021; Desmet and Engel, 2021; Gächter et al., 2025) and (2) consistent with (1), observing others' rule-following decisions influences their own rule following decisions, as found in the direct response experiments of Gächter et al. (2025) (who only studied peer effects in rule following, not the contrast effect we study in the current paper). Thus, Hypothesis 1 aims to establish a benchmark for our subsequent conjectures.

Given that rule violations are materially advantageous, violations cannot be punished in our setting, and people are conditional rule followers, CRISP suggests that observing a rule violation should reduce compliance, holding constant the identity composition of a group. We test it by comparing treatments that vary whether subjects see zero or one example of a rule violation, keeping constant the identity composition of the group. This results in six comparisons in total:

- *Comparing rule breaking in homogeneous groups*
  - 7In-0Out-1InV vs. 7In-0Out-0V
  - 0In-7Out-1OutV vs. 0In-7Out-0V
- *Comparing minoritarian ingroup*
  - 1In-6Out-1InV vs. 1In-6Out-0V
  - 1In-6Out-1OutV vs. 1In-6Out-0V
- *Comparing minoritarian outgroup*
  - 6In-1Out-1OutV vs. 6In-1Out-0V
  - 6In-1Out-1InV vs. 6In-1Out-0V.

In all cases, we expect rule compliance to be reduced by peer violations.[3]

Main Hypothesis 2. *Ingroup rule violations reduce compliance more than outgroup rule violations.*

Hypothesis 2 tests our core conjecture about the interactions between social proximity and peer effects in rule following. We have three tests, each addressing a different facet of Hypothesis 2.

1. First, for *homogeneous* groups where all peers are either ingroup or outgroup, we expect observing a violation by ingroup members to reduce rule following more than observing a violation of outgroup members, in line with previous findings that social proximity moderates the influence of peer effects. Specifically, as per our Main Hypothesis 1, we expect that in both homogenous ingroups and homogenous outgroups rule compliance is reduced by observing a rule violation (that is, (7In-0Out-1InV - 7In-0Out-0V) < 0 and (0In-7Out-1OutV - 0In-7Out-0V) < 0). However, we expect the reduction in rule compliance to be larger in ingroups than in outgroups, that is, we expect (7In-0Out-1InV - 7In-0Out-0V) - (0In-7Out-1OutV - 0In-7Out-0V) to be negative.
2. Second, in *heterogeneous* groups where the *outgroup is minoritarian*, we expect a "contrast effect": observing a rule violation by a minority outgroup member has less of a negative effect on rule compliance, compared to a case where the same violation is committed by an ingroup member, that is, (6In-1Out-1OutV - 6In-1Out-0V) is less negative than (7In-0Out-1InV - 7In-0Out-0V). We test this by computing the difference-in-differences: (7In-0Out-1InV - 7In-0Out-0V) - (6In-1Out-1OutV - 6In-1Out-0V), which we also expect to be negative.

[3] Below we report the results of all six tests but in our pre-registration we mistakenly specified only the first three comparisons. Our inferences are robust to the set of comparisons we use to test this hypothesis.

3. Finally, in *heterogeneous* groups where the *ingroup is minoritarian*, we expect a "reverse contrast effect": observing a rule violation by a minority ingroup member reduces rule compliance more than when the same violation is committed by an outgroup member. We test this by computing the difference-in-differences: (1In-6Out-1InV - 1In-6Out-0V) - (0In-7Out-1OutV - 0In-7Out-0V), which we expect to be positive.

Main Hypothesis 3. *Rule compliance reduces more strongly when the rule violator is a minority ingroup member than an ingroup member in a homogeneous group.*

Hypothesis 3 zooms in on our novel conjecture that there is an interaction between minority status and the influence of ingroup peer effects. Violations by an ingroup member have a stronger negative effect on compliance when the ingroup identity is minoritarian. We test this by computing the difference-in-differences: (1In-6Out-1InV - 1In-6Out-0V) - (7In-0Out-1InV - 7In-0Out-0V), which we expect to be negative.

We also pre-registered three additional hypotheses that allow us to rule out alternative interpretations of the tests of our main hypotheses. First, we need to exclude that the "contrast" and "reverse contrast" effects of Main Hypothesis 2 are due to a mere change in the identity composition of the peer group observed by the subject, which is heterogeneous in the treatments with minority violations (6In-1Out and 1In-6Out) and homogeneous in the others (7In-0Out and 0In-7Out). If the effects are due to a change in the peer identity composition and not to the specific fact that is it a minority outgroup/ingroup violating the rule, it should also be the case that the effect of observing a violation should be the same regardless of whether it is committed by a minority ingroup/outgroup or by a member of the majority identity group. We therefore compare violations in 6In-1Out-1OutV with 6In-1Out-1InV (**Auxiliary Hypothesis 1**) and violations in 1In-6Out-1InV with 1In-6Out-1OutV (**Auxiliary Hypothesis 2**), and expect both differences (6In-1Out-1OutV minus 6In-1Out-1InV) and (1In-6Out-1InV minus 1In-6Out-1OutV) to be positive.

Moreover, our conjecture about the contrast effect is that rule compliance is *strengthened* by observing a rule violation by a minority outgroup member, while our Main Hypothesis 2 merely tests that minority outgroup violations have a smaller effect than majority ingroup violations. To test the more specific prediction of our conjecture, we also test that rule compliance is greater in 6In-1Out-1OutV than 6In-1Out-0V (**Auxiliary Hypothesis 3**).

2.4. Experimental procedures

The experiment and hypotheses were pre-registered in the AEA RCT Registry (AEARCTR-0010950).[4] The experiment was run between April and May 2023 via the software LIONESS (Giamattei et al., 2020) with 7,033 US-based participants recruited from Prolific (mean age = 39.0, s.d. = 13.7; 48% male, 50% female, 2% other/did not want to say). The screenshots of the experiment are documented in Online Appendix C. Subjects were paid a flat fee of 0.50 USD, and they could earn a bonus of 0.50 or 1.00 USD depending on their decision in the task, resulting in an average total payment of 1.22 USD per subject (average duration was 4 min). Based on our sample size in each treatment cell (Table 1), our *ex ante* power analysis suggested that we would have 80% power (with alpha = 0.05) to detect differences in compliance rates of the order of between 5 and 7 percentage points (the latter assuming the use of corrections for multiple hypothesis testing).

3. Results

Fig. 1, Panel B displays the fraction of subjects who complied with the rule across the ten treatments (i.e., turned right (left) when the rule was to turn right (left)). Rule compliance rates vary between 51.2% and 62.6%, which is in line with previous results with similar rule-following tasks (see, e.g., Kimbrough and Vostroknutov (2016, 2018), Gächter et al. (2025) and Suri et al. (2025)). Rule compliance decreases with age and is higher for females than males (see Table 2).

To test our hypotheses, we conduct logistic regressions where the dependent variable takes value 1 if a participant complied with the rule, and 0 otherwise. Results are reported in Table 2. As independent variables, we use treatment dummies and, in column 2 of Table 2, we add controls for gender and age.

The analysis confirms Main Hypothesis 1: compliance is significantly lower (all $p < 0.028$) when subjects observe one violation in their peer group, relative to zero violations. This is the case in all comparisons that we specified to test this hypothesis (see bottom panel of Table 2) and the conclusion is robust to correcting for multiple hypotheses tests using the Benjamini–Hochberg False Discovery Rate procedure. A (non-pre-registered) joint test across all six comparisons yields a similar conclusion (60.2% compliance with zero peer violations versus 54.5% with one violation; $p < 0.001$). Overall, this result echoes previous findings that (i) examples of peer violations are detrimental for rule compliance and (ii) rule compliance remains high even if rule violations are observed (Gächter et al., 2025). In terms of CRISP, these rule-following rates are consistent with conformity (C) with social expectations (S) and respect for rules (R) as important determinants of rule-following behavior even in as minimal a setup as our Y task.

Although the direction of the effects is consistent with Main Hypothesis 2, we find no statistical support for it: ingroup violations do not reduce rule compliance significantly more than outgroup violations. The effect of a rule violation by a peer is slightly stronger when the violator is an ingroup member (7In-0Out-1InV - 7In-0Out-0V = 51.2%–59.2% = −8 percentage points) than an outgroup

[4] https://www.socialscienceregistry.org/trials/10950.

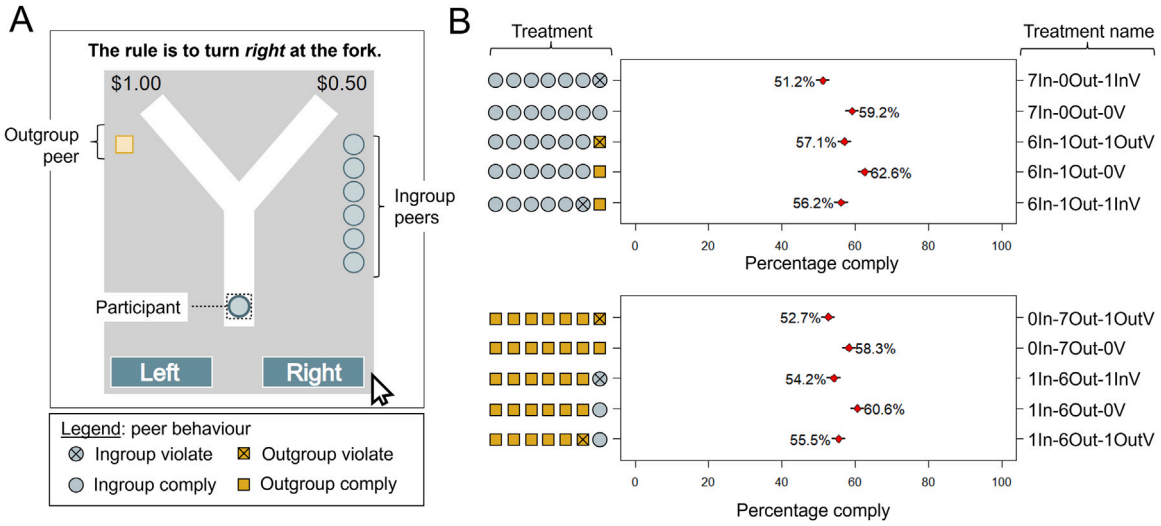

Fig. 1. Rule-following task and rule-compliance rates across treatments.
Panel A illustrates the rule-following task (shown is the example case of 6In-1Out-1OutV). Participants had to move their circle to the left or the right through the Y-shape. In this example, choosing left yielded more earnings, but participants were told that "the rule is to turn right at the fork" (this was randomized). Before making their decisions, participants observed the movements of 7 peers. Panel B plots average compliance rates across the ten treatments.

member (0In-7Out-1OutV - 0In-7Out-0V = 52.7%–58.3% = −5.6 percentage points (pp)), but the difference of −2.4 pp is insignificant (p = 0.539 in column 1 of Table 2; p = 0.452 in column 2).

We also do not find evidence of a "contrast" or "reverse contrast" effect, that is, minority or majority status of an outgroup rule violator does not matter for ingroup rule compliance. Observing a minority outgroup member violating the rule reduces compliance by 5.5 pp (from 62.6% in 6In-1Out-0V to 57.1% in 6In-1Out-1OutV), compared to an 8 pp reduction when the violation is by an ingroup member (7In-0Out-1InV vs. 7In-0Out-0V), but the difference of 2.5 percentage points is insignificant (p = 0.528 in column 1,p = 0.547 in column 2). Violations by minority ingroup members reduce compliance by 6.4 pp (from 60.6% in 1In-6Out-0V to 54.2% in 1In-6Out-1InV), which is insignificantly larger than the 5.6 pp reduction between 0In-7Out-1OutV and 0In-7Out-0V (p = 0.839 in column 1; p = 0.762 in column 2).[5]

Turning to Main Hypothesis 3, the effect of minority ingroup violations on compliance is not statistically different from the effect of an ingroup violation without minority status (−6.4pp vs. −8pp), which contradicts our hypothesis (p = 0.683 in column 1; p = 0.655 in column 2).

One may argue that with varying the identity of the rule violator we are also implicitly varying the identity of the rule followers. For instance, comparing 6In-1Out-1OutV with 0In-7Out-1OutV keeps constant the 1 outgroup rule violator and varies whether the 6 rule followers are from the ingroup or the outgroup, yields rule compliance rates of 57.1% and 52.7% (see Fig. 1), suggesting that the identity composition of the rule followers matters for rule compliance. However, comparing rule compliance in 6In-1Out-1OutV with 1In-6Out-1OutV (57.1% vs. 55.5%) shows that the identity composition effect is not systematic. Interestingly, however, the one ingroup rule follower in 1In-6Out-1OutV compared to no ingroup rule follower in 0In-7Out-1OutV seems to have a compliance-enhancing effect (rule following rates of 55.5% vs. 52.7%, resp.).[6]

4. Discussion and conclusions

Our pre-registered, well-powered study documents further evidence of the role of peers in shaping individual willingness to comply with costly rules of behavior. As in previous research (e.g., Gächter et al., 2025), we find that compliance is systematically reduced when individuals can observe past violations by other individuals.

The key conjecture that we tested in this paper was that these peer effects are moderated in subtle ways by the degree of social similarity (ingroup vs. outgroup) between the decision-maker and their peers. Inspired by research in social psychology, we set out

[5] Given the null results for Main Hypothesis 2, our auxiliary hypotheses become redundant. We report the results here for completeness. We cannot reject the null hypothesis of no difference in rule-compliance rates between 6In-1Out-1OutV and 6In-1Out-1InV (Auxiliary Hypothesis 1; p = 0.732 in column 1; p = 0.529 in column 2) or between 1In-6Out-1InV and 1In-6Out-1OutV (Auxiliary Hypothesis 2; p = 0.640 in column 1; p = 0.753 in column 2). We also cannot reject the null that rule compliance is the same in 6In-1Out-1OutV and 6In-1Out-0V (Auxiliary Hypothesis 3; p = 0.962 in column 1; p = 0.960 in column 2).

[6] Similarly, keeping ingroup violators constant, that is, comparing 1In-6Out-1InV with 7In-0Out-1InV results in rule compliance rates of 54.2% and 51.2%, respectively. Comparing 1In-6Out-1InV with 67In-10Out-1InV reveals rule compliance rates of 54.2% and 56.2%, respectively (for all comparisons see Fig. 1). We add the caveat that none of these comparisons was pre-registered and are therefore exploratory.

Table 2
Logistic regressions of rule-following behavior.

	(1)	(2)
7In-0Out-1InV	-0.322***	-0.348***
	(0.107)	(0.110)
6In-1Out-0V	0.142	0.154
	(0.109)	(0.111)
6In-1Out-1OutV	-0.083	-0.099
	(0.107)	(0.109)
6In-1Out-1InV	-0.120	-0.169
	(0.108)	(0.110)
1In-6Out-0V	0.059	0.065
	(0.108)	(0.111)
1In-6Out-1OutV	-0.201	-0.213
	(0.108)	(0.110)
1In-6Out-1InV	-0.151	-0.178
	(0.107)	(0.110)
0In-7Out-0V	-0.034	-0.039
	(0.108)	(0.111)
0In-7Out-1OutV	-0.263**	-0.269**
	(0.107)	(0.110)
1 if Female		0.560***
		(0.050)
Age		-0.013***
		(0.002)
Constant	0.371***	0.622***
	(0.076)	(0.107)
N.	7,033	6,887
Linear restriction tests, p-values – Main Hypothesis 1		
7In-0Out-1InV vs 7In-0Out-0V	0.001	0.001
0In-7Out-1OutV vs 0In-7Out-0V	0.017	0.019
1In-6Out-1InV 1In-6Out-0V	0.008	0.006
1In-6Out-1OutV vs 1In-6Out-0V	0.027	0.014
6In-1Out-1OutV vs 6In-1Out-0V	0.008	0.002
6In-1Out-1InV vs 6In-1Out-0V	0.019	0.011
Linear restriction tests, p-values – Main Hypothesis 2		
(7In-0Out-1InV - 7In-0Out-0V) - (0In-7Out-1OutV - 0In-7Out-0V)	0.264	0.274
(7In-0Out-1InV - 7In-0Out-0V) - (6In-1Out-1OutV - 6In-1Out-0V)	0.270	0.226
(1In-6Out-1InV - 1In-6Out-0V) - (0In-7Out-1OutV - 0In-7Out-0V)	0.420	0.381
Linear restriction tests, p-values – Main Hypothesis 3		
(1In-6Out-1InV - 1In-6Out-0V) - (7In-0Out-1InV - 7In-0Out-0V)	0.342	0.328

The top panel shows coefficient estimates (with standard errors in parentheses) from logistic regressions where the dependent variable takes value 1 if the subject complied with the rule, and 0 otherwise. Column 1 only includes treatment dummies; Column 2 also includes controls for gender and age (the sample size drops slightly due to missing variables). The reference category in the models is the treatment 7In-0Out-0V. Levels of significance: *** < 0.001; ** < 0.01. The bottom panel reports one-tailed p-values from linear restriction (Tukey) tests for the treatment comparisons testing our three main directional hypotheses. Results are unchanged when using the Benjamini–Hochberg False Discovery Rate procedure to correct for multiple hypotheses tests.

to examine whether the strength of the influence of rule breaking on compliance depends on the interaction between the relative status (minoritarian/majoritarian) and the social proximity (ingroup/outgroup) of the peers who broke the rule. Specifically, we expected to observe contrast effects, whereby deviant behavior by minoritarian outgroups strengthens compliance, while deviant behavior by minoritarian ingroups is most detrimental to it.

However, this conjecture was not supported by our data. We find no evidence that the strength of peer effects in rule following depends on the social proximity between the decision-maker and their peers, nor on the relative share of ingroup/outgroup in the decision-maker's reference group.

It is tempting to speculate that the use of minimal groups to manipulate social proximity may explain this null result. However, results from an additional pilot experiment suggest that this is not the case. As we discuss in detail in the Online Appendix A, we pre-tested the strength of our group identity manipulation by measuring its effect in a third-party allocation game, which is a common decision setting to study ingroup/outgroup effects (see, e.g., Lane, 2016). We find evidence of ingroup bias: on average decision-makers allocate significantly more money to those with whom they share a common minimal identity. Moreover, we compared the ingroup bias induced by our minimal manipulation with the bias induced by a non-minimal identity manipulation based on partisan identities (Democrats vs. Republicans in the US). Although the non-minimal manipulation induced a slightly stronger ingroup bias, the difference was not statistically significant. This suggests that our minimal group identity has the potential to induce group categorization and ingroup bias, which casts doubt on an explanation of our null result based on weak manipulation arguments.

Our results can instead be reconciled with previous findings by considering the asymmetry between examples of peer deviance and compliance documented in the existing literature. In our paper, we focused on examples of deviance and found that social proximity does not moderate the strength of their influence on behavior. Previous studies have shown that deviant examples exert a stronger influence on behavior than examples of compliance (Bicchieri et al., 2022; Gächter et al., 2025), but only when these examples are set by socially distant peers; in the case of socially proximate peers, examples of both compliance and deviance exert the same influence on behavior (Bicchieri et al., 2022).

One way to rationalize the evidence emerging from this paper and previous studies is that the core mediating influence of social proximity may operate through the positive effects of examples of compliance, and not through the detrimental effects of examples of deviance. Specifically, Bicchieri et al. (2022) and Gächter et al. (2025) found that with socially neutral/distant peers observing rule violations has a stronger impact on compliance than observing rule following. Here, we find that rule violations have the same weight on compliance regardless of whether the violation was committed by an ingroup or an outgroup peer.

A further reason for the lack of strong contrast effects despite successful ingroup-outgroup manipulations could be that the Y task is morally neutral. A rule in a decontextualized task may not evoke strong feelings toward rule violators because – unlike in context-rich real-world settings – the rule does not support or contradict one's moral worldview. Similarly, the rule is also not a group identity marker where a violation could undermine group cohesion (for vivid examples of gang members' rule-following behavior, see Skarbek (2014)). Future research should also investigate whether contrast effects may emerge when ingroups and outgroups differ in the worldview associated with the rule and when rule compliance is fundamental to maintaining group identity.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgments

This work was supported by the European Research Council grant ERC-AdG 101020453 PRINCIPLES awarded to Simon Gächter.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jebo.2025.107264.

Data availability

Data is available for download at OSF: https://doi.org/10.17605/OSF.IO/ETF9M.

References

Barr, A., Lane, T., Nosenzo, D., 2018. On the social inappropriateness of discrimination. J. Public Econ. 164, 153–164.
Bicchieri, C., 2006. The Grammar of Society: The Nature and Dynamics of Social Norms. Cambridge University Press.
Bicchieri, C., 2016. Norms in the Wild: How to Diagnose, Measure, and Change Social Norms. Oxford University Press.
Bicchieri, C., Dimant, E., Gächter, S., Nosenzo, D., 2022. Social proximity and the erosion of norm compliance. Games Econom. Behav. 132, 59–72.
Bicchieri, C., Xiao, E., 2009. Do the right thing: but only if others do so. J. Behav. Decis. Mak. 22 (2), 191–208.
Bordalo, P., Gennaioli, N., Shleifer, A., 2022. Salience. Annu. Rev. Econ. 14, 521–544.
Brewer, M.B., Weber, J.G., 1994. Self-evaluation effects of interpersonal versus intergroup social comparison. J. Pers. Soc. Psychol. 66, 268–275.
Cialdini, R.B., Reno, R.R., Kallgren, C.A., 1990. A focus theory of normative conduct: Recycling the concept of norms to reduce littering in public places. J. Pers. Soc. Psychol. 58, 1015–1026.

Desmet, P.T., Engel, C., 2021. People are conditional rule followers. J. Econ. Psychol. 85, 102384.
Dimant, E., 2019. Contagion of pro-and anti-social behavior among peers and the role of social proximity. J. Econ. Psychol. 73, 66–88.
Fehr, E., Schurtenberger, I., 2018. Normative foundations of human cooperation. Nat. Hum. Behav. 2 (7), 458–468.
Gächter, S., Molleman, L., Nosenzo, D., 2025. Why people follow rules. Nat. Hum. Behav. 9 (7), 1342–1354.
Gächter, S., Nosenzo, D., Sefton, M., 2013. Peer effects in pro-social behavior: Social norms or social preferences? J. Eur. Econ. Assoc. 11 (3), 548–573.
Giamattei, M., Yahosseini, K.S., Gächter, S., Molleman, L., 2020. LIONESS lab: a free web-based platform for conducting interactive experiments online. J. Econ. Sci. Assoc. 6, 95–111.
Gino, F., Ayal, S., Ariely, D., 2009. Contagion and differentiation in unethical behavior: The effect of one bad apple on the Barrel. Psychol. Sci. 20 (3), 393–398.
Goerg, S.J., Himmler, O., König, T., 2024. Norm violations and behavioral spillovers—Evidence from the lab and the field. Eur. Econ. Rev. 166, 104776.
Gross, J., Dreu, C.K.W.D., 2020. Rule following mitigates collaborative cheating and facilitates the spreading of honesty within groups. Pers. Soc. Psychol. Bull. 13 (4), 395–409.
Keizer, K., Lindenberg, S., Steg, L., 2008. The spreading of disorder. Science 322 (5908), 1681–1685.
Kerr, N.L., Rumble, A.C., Park, E.S., Ouwerkerk, J.W., Parks, C.D., Gallucci, M., Lange, P.A.M.V., 2009. How many bad apples does it take to spoil the whole barrel? Social exclusion and toleration for bad apples. J. Exp. Soc. Psychol. 45 (4), 603–613.
Kimbrough, E.O., Vostroknutov, A., 2016. Norms make preferences social. J. Eur. Econ. Assoc. 14 (3), 608–638.
Kimbrough, E.O., Vostroknutov, A., 2018. A portable method of eliciting respect for social norms. Econom. Lett. 168, 147–150.
Krupka, E.L., Weber, R.A., 2013. Identifying social norms using coordination games: Why does dictator game sharing vary? J. Eur. Econ. Assoc. 11 (3), 495–524.
Lane, T., 2016. Discrimination in the laboratory: A meta-analysis of economics experiments. Eur. Econ. Rev. 90, 375–402.
Lane, T., Miller, L., Rodriguez, I., 2024. The normative permissiveness of political partyism. Eur. Econ. Rev. 162, 104661.
McBride, M., Ridinger, G., 2021. Beliefs also make social-norm preferences social. J. Econ. Behav. Organ. 191, 765–784.
Mussweiler, T., Rüter, K., Epstude, K., 2004. The ups and downs of social comparison: mechanisms of assimilation and contrast. J. Pers. Soc. Psychol. 87, 832–844.
Schlaffke, L., Golisch, A., Haag, L.M., Lenz, M., Heba, S., Lissek, S., Schmidt-Wilcke, T., Eysel, U.T., Tegenthoff, M., 2015. The brain's dress code: How the dress allows to decode the neuronal pathway of an optical illusion. Cortex 73, 271–275.
Skarbek, D., 2014. The Social Order of the Underworld: How Prison Gangs Govern the American Penal System. Oxford University Press.
Suri, D., Kube, S., Schultz, J., 2025. Evaluating online data collection platforms using a simple rule-following task. Econom. Lett. 255, 112509.