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Determinants of Norm Compliance: Moral Similarity and Group Identification

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Abstract. What determines whether someone complies with a social norm? The social identity approach offers a mechanism for norm compliance: A person who feels similar to a group identifies more with that group and, in turn, complies with the group's norms. Using an experiment, we test whether similarity in values increases identification and adherence to a group rule. To do so, we varied the similarity/dissimilarity between the values of an individual and members of a social group and measured group identification and rule compliance. We find that moral similarity in progressivism increases group identification, and group identification can increase compliance with rules from the social group. However, our results also suggest that this relationship can be disrupted. Because of our design and empirically verified in our study, social norms do not differ between treatments. As a consequence, observed behavior changes are attributable to increased group norm sensitivity and not changes in group norms. We advance the study of social identity by testing a causal pathway between group identification, group norm sensitivity, and behavior change. Our contribution to the management literature is to extend the empirical rationale, and cautionary notes, for investing in value-alignment between individuals and organizations.

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Keywords: moral similarity • moral foundations theory • group identification • group norm sensitivity

1. Introduction

In today's workplaces, group work is ubiquitous (Lazear and Shaw 2007). However, for it to be effective, it often requires group members to follow their group's implicit and explicit rules even when those conflict with more self-serving incentives. The social identity approach has received considerable attention as a way to model the behavior of individuals in social groups (Tajfel and Turner 1979, Turner et al. 1987, Hornsey 2008, Turner and Reynolds 2012). The framework posits that people identify as members of social groups,[1] whereby identification with the group is defined as "the degree to which the ingroup is included in the self" (Tropp and Wright 2001, p. 586). Subsequently, they comply with behaviors that are seen as prototypical of the social groups. In economics, this is modeled such that individuals can derive utility directly from their identities (Akerlof and Kranton 2000). In particular, individuals may gain utility from complying with, or disutility from violating, group-specific social norms (Akerlof and Kranton 2000, Krupka et al. 2017, Chang et al. 2019). This framework provides explanations for many organizational behaviors that traditional economic theories cannot easily explain (Akerlof and Kranton 2000, 2002, 2005, 2008).[2] However, most empirical studies identify the average effect of social identities on behavior, even though many theories assume (Akerlof and Kranton 2000), and some empirical evidence shows (Kimbrough and Vostroknutov 2016, 2018; Gächter et al. 2025) that individuals vary in how strongly a particular social identity impacts their behavior. One source of this individual heterogeneity may be, as the social identity approach suggests, the extent to which an actor identifies with

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the social group (Tajfel and Turner 1979, Turner et al. 1987, Hornsey 2008, Turner and Reynolds 2012).

In this study, we experimentally test the causal effect of higher group identification on behavior *through* increased group norm sensitivity. The term "(group) norm sensitivity" is used here to refer to the weight individuals place on their (identity-specific) norm-dependent utility. We manipulate group identification by exogenously varying the similarity in moral preferences between an individual and a novel social group unknown to the participant prior to the study. To characterize the morality of individuals, we draw from moral foundations theory (Haidt and Joseph 2004; Graham et al. 2011, 2013) and use the "progressivism" scale (Clark et al. 2017). Progressivism measures the degree to which individuals endorse moral values that focus on the individual over moral values that bind people into larger groups. In one of our treatments, the subjects' and the social group's progressivism scores were similar and in another they were dissimilar. Subsequently, we measured participants' group identification and costly compliance with a rule created by the similar/dissimilar social group. We tested whether greater moral similarity between an individual and a social group increases group identification and subsequent compliance with that group's normative expectations.

We find that moral similarity in progressivism increases group identification. Furthermore, we find that group identification increases compliance with rules from the social group in our regression models. However, we only find a significant treatment effect on average rule-compliance in one out of three versions of the experiment. We return to this issue in Section 7. Because of our design, social norms do not differ between treatments. As a consequence, any observed behavior changes are attributable to increased group norm sensitivity and not changes in group norms.

Our contribution to the economics literature on social identity is to test a causal mechanism for the theoretical prediction that identification with a group leads to greater group norm adherence. Prior work in economics examined how group identification affects behavior (Goette et al. 2006, Chen and Li 2009, Chen and Chen 2011), but that work has not disentangled whether the behavioral effect was due to a change in group norm sensitivity or due to a change in the group norms themselves though both channels are theoretically possible and have been empirically surfaced (Pickup et al. 2022). We disentangle these mechanisms and test the causal effect of norm sensitivity by manipulating group identification without affecting the relevant group norms. Our finding offers an explanation for why there can be heterogeneity in group norm compliance among individuals who share a social identity and highlights the role that identification with the group plays in social identity theory.

Our contribution to the management literature is to extend the empirical rationale, and cautionary notes, for investing in value-alignment between individuals and organizations. We join a growing literature that investigates the benefits employees derive from working for organizations that share their values and vice versa (Akerlof and Kranton 2000, Besley and Ghatak 2005, Cassar 2019, Kranton and Thomas 2025). Although several have explored the role of mission (Cassar 2019, Aksoy et al. 2025), of corporate purpose (Gartenberg et al. 2019), faith in an organization's practices and leadership (Ghoshal and Bartlett 1994), and shared values (Burks and Krupka 2012, Kranton and Thomas 2025), the causal relationships remain less well documented. Across this body of literature, a consistently expressed theme is that management practices focused on strategy, structure, and systems are complimented by accompanying "softer," management approaches focused on building the development of shared purpose and values (Ghoshal and Bartlett 1994, Gartenberg et al. 2019) with Ghoshal and Bartlett (1994, p. 92) referring to such practices as "…the ultimate and invisible shaper of organizational performance." Nonetheless, a persistent challenge remains documenting the link to value alignment. We show that value alignment may be a fragile proposition but not without promise (Cassar 2019, Aksoy et al. 2025, Kranton and Thomas 2025).

2. Related Literature

Our work is closely related to the social-psychological research on self-categorization theory (Turner et al. 1987). The social-psychological social identity approach (Hornsey 2008, Turner and Reynolds 2012) consists of social identity theory (Tajfel and Turner 1979) and self-categorization theory (Turner et al. 1987, Turner and Reynolds 2012). Although social identity theory describes intergroup processes (e.g., why do individuals display ingroup favoritism or comply with norms), self-categorization theory focuses on intragroup processes (e.g., what are the processes that lead to categorization and why does categorization change behavior). According to self-categorization theory, individuals segment, classify and order their social environment in a process called categorization. In this process, they put themselves and others into different categories. Crucially, which categorization is chosen depends on the comparative fit of the categorization. Categories form in such a way that they maximize intraclass similarities and interclass differences (referred to as the meta-contrast principle). Subsequently, the chosen categorization influences self-perception and behavior. In particular, individuals who define themselves via a social identity engage in depersonalization and self-stereotypization. They no longer see themselves and others in the category as

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individuals but as exemplars of the group prototype. This perceptual shift leads to behavior that conforms with the normative expectations of a group. Expressed differently, Turner and Reynolds (2012, p. 406) write "… internalization [of the category] is critical to the emergent social norms having an impact on one's attitudes and behaviour … *and is affected by the degree to which individuals consider themselves psychologically to be members of the particular group* [emphasis added]." In our experiments, self-categorization theory predicts that individuals are more likely to self-categorize into similar rather than dissimilar social groups. This categorization creates group identification and causes individuals to comply with behaviors that are seen as prototypical of the social group (i.e., group norms).

Prior work in experimental economics demonstrates that group identification can influence behavior. Eckel and Grossman (2005) showed that group identification increases cooperation with groups in repeated public goods games. Goette et al. (2006) showed that individuals are more likely to cooperate with ingroup members in simultaneous prisoner's dilemmas and that third parties are more likely to punish individuals that harmed an ingroup member in a prisoner's dilemma with third-party punishment. Bernhard et al. (2006) examined how group identity in small tribes affects behavior in dictator games with third-party punishment. They found that third parties are more likely to punish dictators that harmed an ingroup recipient, that third parties are less likely to punish ingroup dictators that harmed an ingroup recipient, and that dictators transfer more to ingroup recipients (for more evidence on the effect of group identification on giving in dictator games, see also Fong and Luttmer (2009) and Müller (2019)). Shang et al. (2008) showed that peer effects are stronger if the peer is an ingroup member rather than an outgroup member. Chen and Li (2009) found that ingroup identification increased ingroup favoritism in other-other allocation tasks. Chen and Chen (2011) showed that ingroup identification influences equilibrium selection in minimum-effort games. Lastly, Jiang and Li (2019) found that principals in principle agent games make more generous offers to ingroup rather than outgroup agents, that ingroup rather than outgroup agents are less tolerant of low offers, and that ingroup rather than outgroup agents are more responsive to generous offers.

The dominant hypothesis tested in the literature is that when group identification is present, behavior differs between group members because group norms are different (Akerlof and Kranton 2000, Hogg and Smith 2007). Group identity–specific norms are defined as commonly believed standards of behavior among members of a social group in a given situation (Ostrom 2000, Bicchieri 2005, Krupka and Weber 2013, Nosenzo and Görges 2020). The definition references both common beliefs, which evoke an implied collective of individuals who hold those beliefs, and prescriptions, which evoke behavioral standards to which the collective adheres and expects others to adhere (Bicchieri 2005). To test whether norms and behavior differ between identities, one can make identity-norms salient. Chang et al. (2019) adopt this approach. Describing a series of dictator games to subjects whose political identity was, or was not, salient, they showed that differences in dictator game behavior can be explained by differences in identity-dependent norms. One can also look for patterns of behavior that are consistent with identity-dependent norms. For example, Harris et al. (2015) showed that third parties punish ingroup dictators less for displays of ingroup favoritism compared with outgroup dictators. They interpret the behavior differences to be consistent with norms that prescribe ingroup favoritism. More recently, Dimant (2024) focuses on measuring the norms themselves and testing for differences by group. Using an incentivized group norm elicitation, Dimant (2024) shows that ingroup favoring allocations in dictator and public good games are more socially appropriate. Critically, in all these approaches, group identification is assumed, and the behavioral heterogeneity is explained by differences in identity-dependent norms.

Yet, group identification may also affect behavior by increasing sensitivity to group norms. In particular, stronger identification could heighten how much people care about complying with those norms. There exists some direct empirical evidence of heterogeneity in norm sensitivity. Kimbrough and Vostroknutov (2016, 2018) introduce an incentivized experimental task, the rule-following task, that allows researchers to approximate an individual's norm sensitivity. They show that heterogeneity in norm sensitivity exists and that the measure predicts norm compliance in public goods, trust, dictator, and ultimatum games. Pickup et al. (2022) examined how group identification with political identities affects the willingness to incur a cost to prevent donations to identity inconsistent lobbying organizations. In their study, they observed a positive correlation between group identification and the willingness to incur a cost. They argue that this is (at least partly) due to an increase in group norm sensitivity. However, their study also showed that group identification changed norm knowledge and the perception of the (unincentivized) group norms, allowing for alternative explanations of why behavior changed. There is also evidence that norm sensitivity can be situational, because it is influenced by the decision-making context. Krysowski and Tremewan (2021) examine the effect of anonymity on giving behavior and distinguish between two possible channels: changes in perceived social norms and changes in the willingness to comply with those norms. They find limited evidence that

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anonymity changes the perceived norm and interpret behavioral effects to a reduction in the willingness to adhere to the norm under anonymity. This can be interpreted as evidence that the weight individuals place on norm compliance—what we call norm sensitivity—can vary across decision-making environments.

Taken together, prior research either did not engage with the question or was unable to clearly demonstrate that group identification affects behavior through norm sensitivity. Instead, most researchers focused on the effect of group identification on group norms. We argue that, although group identification might influence behavior by changing perceptions of group norms, the explanation is incomplete and/or possibly misidentified because it ignores the important psychological mechanism of identification itself (as theorized by Turner et al. (1987)). Therefore, we examine if group identification can influence behavior in an experimental task where the *group norms are held constant*.

In line with the social identity approach, there is laboratory and field evidence that similarity with a group increases identification. For example, Hett et al. (2020) used a laboratory experiment where they recruit supporters of two different German football clubs at two different universities. They found that identification increased with groups whose stereotypes were more similar to their own characteristics. Using naturally occurring data on food consumption in India, Atkin et al. (2021) show that similarity between self and the religious or ethnic group in the area is one driver of identification (see also Jia and Persson 2020). In experimental economics, the effect of similarity on group identification is often used to create groups that differ only in superficial characteristics. In particular, many researchers induce social identities through similarity of art preferences. Subjects are assigned to one of two groups based on their reported preference regarding pairs of Klee and Kandinsky paintings (see Chen and Li (2009) for an example of this protocol). The appeal of this approach is that one can create groups based on a preference that are (theoretically) orthogonal to the preferences or topic of study at hand. The method is inspired by the minimal group paradigm and has the benefit of achieving experimental control (see Chen and Li (2009), Chen and Chen (2011), Kranton and Sanders (2017), and Kranton et al. (2020) for reviews of the experimental approaches).

In our laboratory study, we create social groups based on naturally occurring moral values rather than naturally occurring or minimal groups without compromising identification of treatment effects. We create social groups that do not "exist" in any formal sense prior to subjects' participation in our experiment but the groupings, which are based on moral values, offer a more natural mapping between the individual and a dimension on which people are likely to sort into groups in their work and personal lives (Leach et al. 2007).

To conceptualize morality, we used moral foundations theory (Haidt and Joseph 2004; Graham et al. 2011, 2013). The theory draws on evolutionary psychology and anthropology to identify five dimensions or "foundations" of morality with which societies construct their moral values. For this reason, moral foundations theory is particularly suitable to measure and describe differences in moral concerns across individuals, social groups, and cultures. The five moral foundations are harm/care, fairness/reciprocity, ingroup/loyalty, authority/respect, and purity/sanctity (in the following, we will only call them harm, fairness, ingroup, authority, and purity).[3] The first two foundations (harm and fairness) are often called the "individualizing" foundations because they describe moral values concerned with protecting the rights and liberties of individuals. In contrast, the last three foundations (authority, ingroup, and purity) are often called the "binding" foundations because they describe moral values that bind individuals into larger groups and institutions. In our study, we primarily use the progressivism index. Progressivism describes the degree to which individuals endorse the individualizing moral foundations over the binding moral foundations (Clark et al. 2017).

Moral foundations theory has been used, among others, by Clark et al. (2017) to show that participants' measured progressivism is positively correlated with cooperative behavior in prisoner's dilemmas and trust games. However, other studies examined how moral similarity in moral foundations affects preferences and behavior more directly. Dehghani et al. (2016) showed that higher moral similarity, in particular in the domain of purity, leads to less social distancing in social networks and laboratory experiments. Winterich et al. (2012) discovered that moral similarity between an individual and a charity increases donations. Other researchers showed that similarity in moral foundations between voters and political parties predicts the intention to vote (Johnson et al. 2014) and voting decision (Franks and Scherr 2015, Milesi 2017, Enke 2020).

3. Theoretic Framework

In the following, we provide a theoretical framework that motivates our experimental design and empirical strategy. The framework is based on models articulated by Akerlof and Kranton (2000), Krupka and Weber (2013), and Kimbrough and Vostroknutov (2016). In our framework, an individual i is introduced to a new social group g. Subsequently, the individual must choose an action $x \in \mathbb{R}^+$ in a nonstrategic decision environment. In our experiment, the action describes the number of balls placed in the norm-violating yellow

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bucket rather than the norm-compliant blue bucket of a modified rule-following task (Kimbrough and Vostroknutov 2016, 2018). Crucially, while making this decision, the individual is aware of the normative expectation η of the social group which is to place all the balls in the blue bucket and must decide whether to comply with that expectation. We define the individual's utility function as follows:

$$U_i(x) = x + \phi_i \eta(x), \quad (1)$$

$$\phi_i = \phi(I_i), \quad (2)$$

$$I_i = I(D(m_i, m_g)). \quad (3)$$

We can write the utility function U_i (see Equation (1)) as consisting of two additively separable[4] parts: the standard and the norm-dependent utility component. In our model, the standard utility component contains only the valuation that the individual places on their monetary gain. In doing so, we assume that the individual's payoff equals the selected action x, and the individual gets linear consumption utility from money. We make the same simplifying assumptions that Kimbrough and Vostroknutov (2018) make to model behavior in the rule-following task. First, we assume linear consumption utility. Second, we assume that monetary gains are zero if the action is zero and increase proportionally to the action.[5]

The norm-dependent utility component potentially creates (disutility) utility if the individual (violates) follows the group norm of the social group. In particular, the group norm function $\eta : \mathbb{R}^+ \to [-1, 1]$ assigns to each action x a social appropriateness rating. In our experiment, the group norms prescribed placing zero balls in the norm-violating bucket. Thus, the norm-prescribed action is $x^* = 0$. This implies that $\eta(x)$ is decreasing in x: placing more balls in the norm-violating bucket increases monetary payoff but decreases norm compliance. In Equation (1), the group norm function is weighted by the individual's sensitivity to the norm, $\phi_i \in \mathbb{R}^+$. Sometimes this concept is referred to as the weight someone places on norm compliance or as the concern for norm adherence. We follow Kimbrough and Vostroknutov (2016) and refer to it as "sensitivity."

Crucially, informed by Turner's self-categorization theory (Turner 1991), we model norm sensitivity as a function of the identification an individual has with the respective social group. In particular, we assume that group identification $I_i \in \mathbb{R}^+$ is mapped via the group norm sensitivity function $\phi : \mathbb{R}^+ \to \mathbb{R}^+$ into group norm sensitivity ϕ_i (see Equation (2)). In turn, the distance between one's own moral position, $m_i \in \mathbb{R}$, and the average moral position of the social group $m_g \in \mathbb{R}$, $D(m_i, m_g)$ maps into the group identification function $I : \mathbb{R}^+ \to \mathbb{R}^+$ to group identification I_i (see Equation (3)).[6] Thus, our theory states that identification is a function of perceived value alignment between one's self and a social group. Identification, in turn, affects the weight an individual places on complying with the group norm. Lastly, the group norm itself is exogenous.

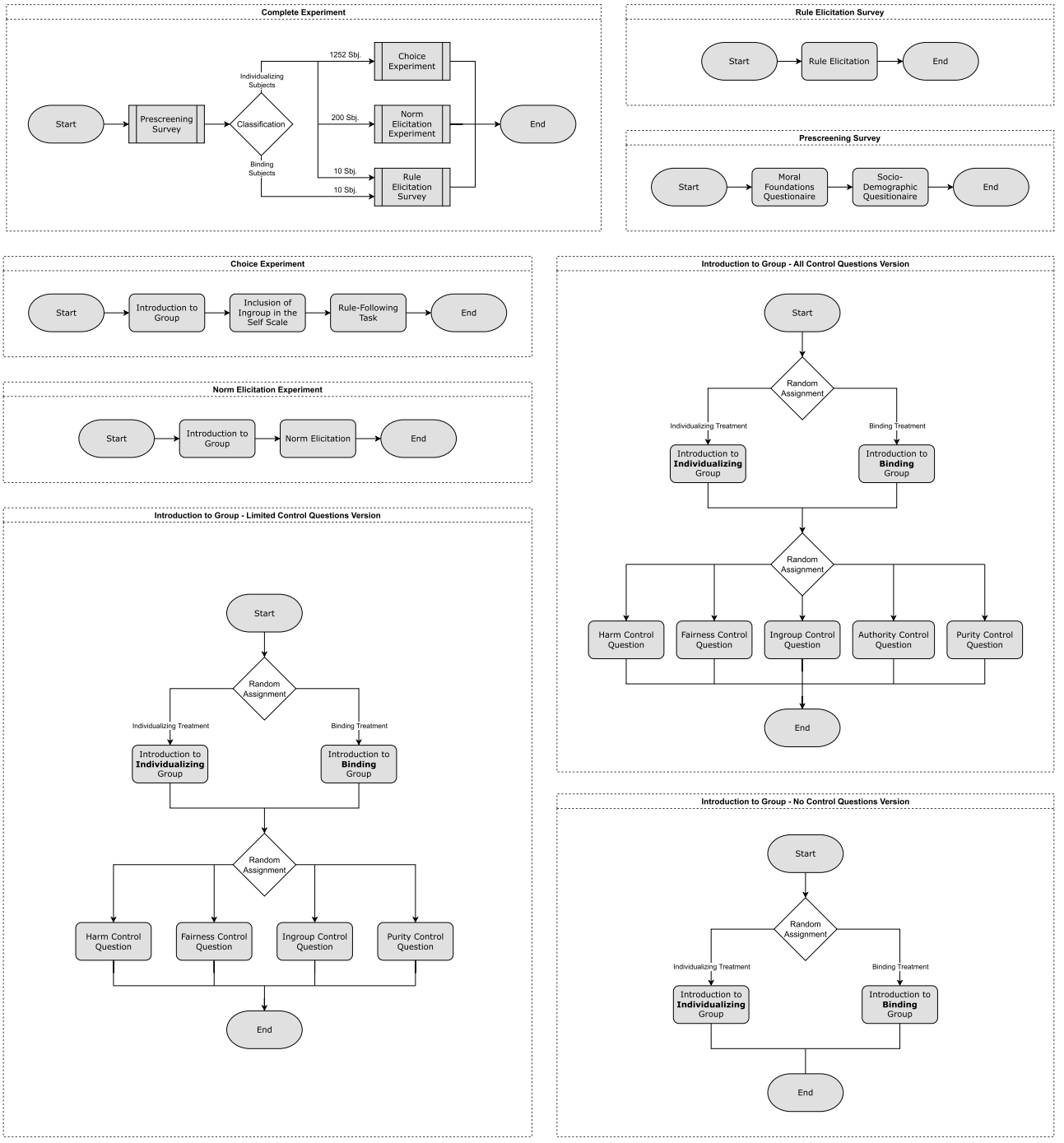
In our experiments, we measured the moral position of the individual m_i, the average moral position of the social group m_g, the identification of the individual with the social group I_i, and the selected action of the individual x. We also followed Krupka and Weber (2013) to obtain an empirical proxy of the norm, $\eta(x)$.

The goal of the experimental design is to distinguish whether group identification (I_i) influences behavior (x) via group norm sensitivity (ϕ_i) while eliminating the possibility that behavior is influenced by changes in the norm function ($\eta(x)$). We follow Kimbrough and Vostroknutov (2016, 2018) in using the rule-following task to approximate individual sensitivity to the group norm, ϕ_i. According to the authors, the observed action x of a utility-maximizing individual has a positive monotonic relationship with the group norm sensitivity ϕ_i. However, for this to be true, the norm function η must be a unique norm shared by all members of both treatments. In other words, our treatment manipulation must not have affected the group norms (articulated in Hypothesis 1). To test this pathway from similarity to increased identification and norm compliance, our experimental design keeps the moral position of the individual $m_i \in \mathbb{R}$ fixed while changing the average moral position of the reference social group $m_g \in \mathbb{R}$. We predict that in the treatment where there is greater moral value alignment between the individual and the assigned group, there will be greater identification with that group (articulated in Hypothesis 2). We predict that in the treatment where there is greater identification with the group, subjects will be more likely to adhere to the group norm (articulated in Hypothesis 3). Lastly, we predict that an increase in group identification will lead to increased group norm sensitivity (articulated in Hypotheses 4). We now turn to the design.

4. Experimental Design

We prescreened all participants for their moral type with the prescreening survey. From these responses we identified the progressivism score for each person. We then recruited from this subject pool into our study. They were either recruited into the rule elicitation survey, one of three versions of the choice experiments (randomized within each version to one of two treatments), or the norms elicitation experiments (randomized to one of two treatments). The choice experiments had three versions: the "all control questions," "limited control questions," and "no control questions" versions whereby the latter two versions were suggested by different referees. An overview of the experimental design can be found in Figure 1. All instructions can be found

Figure 1. Overview of the Experimental Design



Notes. The choice experiment had three versions that altered what control questions participants could receive. In the all control questions version participants were asked to recall one of the five moral foundations of Group A. In the limited control questions version participants were asked to recall one of four moral foundations of Group A (we excluded the authority control question). In the no control questions version participants were asked no control questions. In the table, "Sbj" refers to Subjects.

in Online Appendix C. We now describe each part of the design in more detail.

4.1. Prescreening Survey

In the *prescreening survey*, our goal is to identify the moral type of all participants and, with this, to create a subject pool where we know the progressivism score for each subject; we used this pool to recruit participants with distinct moral types into our study. In the prescreening survey, participants answered the moral foundations questionnaire (Graham et al. 2011). The moral foundations questionnaire measures the harm,

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fairness, ingroup, authority, and purity foundations described in moral foundations theory (Haidt and Joseph 2004; Graham et al. 2011, 2013). Each foundation was measured with six items which were scored on a six-point scale ranging from zero to five (see Section C.1 in Online Appendix C). Subsequently, we calculated the individualizing and binding indices by averaging the scores of the first two foundations and the last three foundations, respectively. Lastly, we calculated the progressivism index by subtracting the individualizing from the binding index (Clark et al. 2017). This index can take on values ranging from −5 (not at all individualizing and extremely binding) to +5 (extremely individualizing and not at all binding). Moving forward, we refer to individuals with a positive progressivism index as *individualizing subjects* and individuals with a negative progressivism index as *binding subjects*.

4.2. Rule Elicitation Survey

In the *rule elicitation survey*, our goal is to obtain a statement about the rule in the rule-following task that comes from a social group comprised of either individualizing subjects or binding subjects. Other subjects learned about "the rule" when we used the statement in our choice and norms elicitation experiments.

For the survey, we recruited 10 individualizing subjects and 10 binding subjects from our subject pool. Participants *read* a description of the Kimbrough and Vostroknutov (2018) rule-following task. In the rule-following task, a person must allocate 20 balls into either of two buckets: the yellow bucket or the blue bucket. At the end of the task, the person earns £0.05 for each ball that they place into the blue bucket and £0.10 for each ball that they place into the yellow bucket. Although allocating balls into the yellow bucket is payoff dominant, our subjects were told that "The rule is to put the balls in the blue bucket." Our subjects read about this task and were asked to describe "the rule." They could choose between the following two options: "The rule is to put the balls in the blue bucket" and "The rule is to put the balls in the yellow bucket."

This survey may seem odd to administer, but it gives us statements about the rule that we can communicate to other subjects without deception. After collecting the responses, we sorted the participants into individualizing and binding subjects. This allowed us to describe the rule statements to other subjects in our choice and norms elicitation experiments as coming from individualizing or binding subjects. A comparison of the statements between groups found no difference in the modal rule, which always was "The rule is to put the balls in the blue bucket." From here on, will call the group that consists of only individualizing subjects the *individualizing group* and its rule the *individualizing rule*. Analogously, we will call the group that consists of only binding subjects the *binding group* and its rule the *binding rule*. However, we did not use these terms for our subjects; rather the group was simply referred to as "Group A" regardless of whether it was comprised of individualizing or binding subjects.

4.3. Choice Experiment

For the choice experiment, we only recruited individualizing subjects from our subject pool. We limited recruitment to individualizing subjects because we only needed one moral type to test our hypotheses. In the choice experiment, there were three parts. In Part 1, we introduced participants to one of two social groups ("the individualizing group" or "the binding group"); in Part 2, we measured their level of identification with that group; and in Part 3, we told them the group's "rule" was to put balls into the blue bucket and then measured the number of balls they put in the blue bucket.

We ran three versions of the choice experiment at different points in time (see Section 5 for the dates). The versions only differed in how part one was executed; specifically, after they were introduced to the social group, they were sometimes asked control questions about the social group. In the all control questions version, participants were asked one of five control questions. In the limited control questions version, they were asked one of four control questions (but a question about authority was omitted). In the no control questions version, they were asked no control questions. We now turn to describing each part of the choice experiment in more detail. The versions are described further below.

4.3.1. Choice Experiment Part 1: Getting to Know Members from Group A.

The purpose of Part 1 was to familiarize subjects with the social group from whom they were going to receive a rule to follow in Part 3. Subjects were randomly assigned to either the individualizing or the binding treatment. Depending on the treatment, participants got to familiarize themselves with either the individualizing or the binding group. In both cases, the group was just referred to as Group A. Subjects learned about members of Group A by seeing five questions from the moral foundations questionnaire and the corresponding rounded average response to those questions by members of Group A. Each of the five questions represented one of the five dimensions of moral foundations theory. We selected the five questions (one from each of the five moral foundations), by selecting the item with the highest factor loading on the particular moral foundation according to the confirmatory factor analysis of Graham et al. (2011). Table 1 shows the displayed rounded average responses of

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Table 1. What Subjects Read to Learn About the Moral Position of Group A

Foundation	Question	Individualizing treatment	Binding treatment	Used as CQ in which version?
Harm	Whether someone cared for someone weak or vulnerable?	Very relevant	Somewhat relevant	All, limited
Fairness	Whether some people were treated differently from others?	Very relevant	Somewhat relevant	All, limited
Ingroup	Whether someone's action showed love for his or her country?	Not very relevant	Somewhat relevant	All, limited
Authority	Whether someone showed a lack of respect for authority?	Slightly relevant	Somewhat relevant	All
Purity	Whether someone violated standards of purity and decency?	Slightly relevant	Somewhat relevant	All, limited

Notes. All participants in the choice treatment saw the five questions and the corresponding rounded average responses (if they were randomized to the individualizing treatment, they saw the responses listed in column 3, and if they were randomized to the binding treatment, they saw responses listed in column 4). Subsequently, we asked participants in the all control questions and limited control questions versions to reproduce the answer to one randomly selected question. In the all control questions version, the question was selected from all five questions, whereas the question pool in the limited control questions version did not include the authority control question. The no control questions version did not include a control question. CQ, control question.

subjects in the individualizing group (column 3) and the binding group (column 4).

In the all control questions and in the limited control questions version, subjects were asked to memorize the responses to all five questions. However, in the all control questions version, after reviewing the responses, they were shown one randomly chosen (control) question selected from *all five* and asked to recall the group's rounded average response for that question. If they selected the correct answer, they received £0.50 and otherwise nothing. Participants were informed about whether their recollection was accurate (and they received payment) at the very end, when we reported their total earnings. In the limited control questions version, they were shown all five responses but, when we randomly selected a control question to show them for the attention check, we removed the authority control question from the pool of possible control questions. Otherwise, the protocol and the payment were identical to those in the all control questions version. In the no control questions version, we removed the incentivized control question entirely, and there was no attention check. Table 1 lists which version used which control question (column 5).

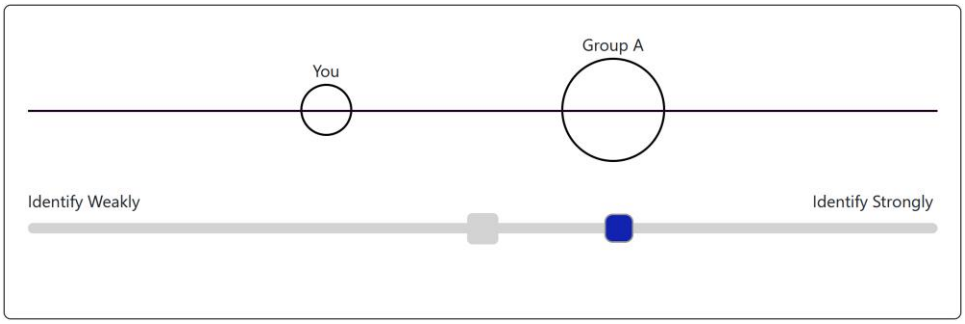
We sequentially added the different versions, because our data analysis on the all control questions version revealed that the results are influenced by the presence of the specific control question on authority (see Section 7). We conducted two more versions of the choice experiment that eliminate the authority control question: Remove all control questions and remove only the authority questions. Thus, the no control questions version differed in that we did not ask subjects to recall anything about Group A's responses to the moral foundations questions while the limited control questions version differed in that we did not include a potentially problematic control question about the moral obligation to adhere to authority.[7]

4.3.2. Choice Experiment Part 2: Measuring Identification with Group A. In Part 2, we used a continuous version of the "Inclusion of Ingroup in the Self Scale"[8] (Tropp and Wright 2001) to measure the degree to which subjects identify with Group A.[9] Subjects saw a small circle labeled "You" and a big circle labeled Group A (Figure 2). Participants could move a slider below the circles to the left (moving the circles further apart) or to the right (moving the circles closer to each other).[10] Consistent with prior work, we interpret distant circles as weak, and adjacent circles as strong, identification with Group A. The distance identification measure can take values from zero (weak identification/the circles are at opposites ends of the scale) to one (strong identification/the circles' centers are at the scale's midpoint).

4.3.3. Choice Experiment Part 3: Following Rules from Members of Group A. In Part 3, participants played a modified version of the rule-following task (Kimbrough and Vostroknutov 2018), which we call the group rule-following task (Figure 3; the location of the blue and yellow bucket was left right randomized, the figure shows the yellow bucket on the left and the blue bucket on the right). Although the rule from Kimbrough and Vostroknutov had no obvious source except the experimenter, our rule originates from members of Group A.[11] In our study, the rule read: "According to the members of Group A, the rule is to put the balls in the blue bucket." The number of balls in the rule-compliant bucket is our dependent variable of interest and our behavioral measure. The experiment concluded with a questionnaire that included an alternative group identification measure used by Chen and Li (2009).

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Figure 2. (Color online) Screenshot of the "Inclusion of Ingroup in the Self Scale"



4.4. Norm Elicitation Experiment

In the *norm elicitation experiment*, our main goal was to obtain data to test whether there were different norms to follow a rule that was issued by a similar or dissimilar group (as our identification depends on these norms *not differing*). As in the choice experiment, we randomized these subjects to either be introduced to Group A comprised of either individualizing or binding members. As with the choice experiments, we only recruited individualizing subjects from our subject pool. The experiment consisted of two parts.

Part 1 followed the same process as Part 1 in the all control questions version of the choice experiment. In short, we randomly assigned participants to learn of the moral position of the individualizing or the binding Group A.

In Part 2, we used the coordination task designed by Krupka and Weber (2013) to elicit the group norms of individualizing subjects for the group rule-following task when the rule comes from a group that has similar or dissimilar moral values. To do this, we informed participants that they were going to evaluate the potential behavior of participants in a different study. We described the choice experiment and that the other participants were introduced to Group A, we told them that Group A was the very same group they just learned about in part one and that the other subjects then had to play the rule-following task after learning about the rule statement from Group A. For example, if in Part 1 of the norm elicitation experiment, subjects learned about Group A members who had binding values, then they also read about other participants in the choice experiment who learned about the same binding Group A members. Thus, our treatment varied whether participants read about a situation where others learned about the "rule" for the group rule-following task from similar or dissimilar others.

Then we asked them to evaluate the actions those other participants could have taken. That is, our norm elicitation participants played a coordination game with other participants in the same treatment over appropriateness ratings for each action ("put 0," "5," "10," "15," and "20" balls into the blue bucket while placing the remaining balls into the yellow bucket). The appropriateness ratings ranged from, "Very socially appropriate," "Socially appropriate," "Somewhat socially appropriate," "Somewhat socially inappropriate," "Socially inappropriate," and "Very socially inappropriate." Recall, that we only recruit individualizing participants to the norm elicitation experiment. To establish common knowledge that they are coordinating with other individualizing subjects (without explicitly stating this), we tell them how the participants they will be playing with responded to the moral foundations questions in the prescreening survey. Specifically, we say that the questions belonged to one of two categories ("category 1" and "category 2"). While doing this, we displayed all questions included in either category graphically separated. We designed it so that category 1 contained the questions of the individualizing index, and category 2 contained the questions of the binding index. Participants then learned that they would try to coordinate ratings in the coordination game with others who stated that questions in category 1 were more important to them than those in category 2.

Figure 3. (Color online) Screenshot of the Group Rule-Following Task



Note. The position of the norm-violating bucket (the left bucket in this image) and the norm-compliant bucket (the right bucket in this image) were left/right randomized for each subject.

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After all participants completed their ratings, we randomly chose one action (corresponding to an allocation of blue and yellow balls such as "0 in the blue bucket and 20 in the yellow bucket") and determined the modal social appropriateness rating for that allocation. If a subject selected the same social appropriateness rating as the modal social appropriateness rating of other raters in their treatment, then they received an additional payment of £2. Otherwise, they received nothing.

Krupka and Weber (2013) argue that in these coordination games the social norm creates a salient focal point. Consequently, in our experiment individuals have a monetary incentive to reveal their perception of the individualizing group norm about compliance with the rule when it comes from group members who are similar or dissimilar. This allows us to test whether norms to follow the rule differ depending on the composition of the group stating the rule.

5. Experimental Procedure and Payoffs

We conducted all our surveys and experiments online using the experimental software oTree (Chen et al. 2016) and the online recruitment platform Prolific.com (Palan and Schitter 2018).[12] For our study, we only invited subjects to the prescreening survey who fulfilled the following requirements. They were United States nationals who were at that time located in the United States,[13] used a desktop or laptop, had between 10 and 1,000 previous submissions, and had an approval rate of at least 90%. Furthermore, we only reinvited individualizing participants to the choice and norm elicitation experiment. In doing so, each subject either participated in the rule elicitation survey, one version of the choice experiment, or the norm elicitation experiment.

We conducted the experiments across three distinct time periods. In each period, we created a new pool of prescreened participants by recruiting new participants for the prescreening surveys. The first data collection, which included the rule-elicitation survey, the all control questions version of the choice experiment, and the norm elicitation experiment, took place between March 3, 2020, and March 20, 2020. The second data collection, which included the no control questions version of the choice experiment, took place between October 26, 2023, and January 4, 2024. Lastly, the third data collection, which included the limited control questions version of the choice experiment, took place between May 30, 2024, and June 18, 2024.

Table 2 reports details about the number of subjects, the paid participation fee, and the average bonus payment in each study. We had a total of 2,925 participants complete our prescreening survey. Of these, 10.50% had a progressivism index of 0 or below (from −5 to 0), and 89.5% had an index above 0 (from 0 to +5). In the choice experiment, we collected data until we obtained 160 observations in each treatment of the all control questions version (320 total), and 233 observations in each treatment of the limited control questions and no control questions version (466 total). We had 100 observations in each treatment of the norm elicitation experiment (200 total).[14] Within the choice and norm elicitation experiments, participants were randomly assigned to either the individualizing or binding treatment.[15] Participants received a participation fee for their participation, and in some cases, a bonus based on their decisions.

6. Results

We begin by examining the group norms from the norm elicitation experiment. Subsequently, we investigate the effect of moral similarity on group identification. Finally, we test the effect of group identification on group norm sensitivity.[16]

6.1. Norm Elicitation Experiment: Does the Norm to Follow the Rule Differ by Treatment?

We tested whether receiving the individualizing or binding rule statement in the group rule-following task changed the social appropriateness of allocations. We predicted no significant difference in the group norms, which should prescribe rule following and putting all the balls in the rule-compliant (blue) bucket irrespective of treatment.

Hypothesis 1 (Comparison of the Group Norms by Treatment). *The group norms in the group rule-following task will not differ significantly between the individualizing and the binding treatment.*

We followed Krupka and Weber (2013) in converting the categorical social appropriateness ratings obtained from the norms elicitation experiment into numerical ratings. We coded the rating "Very socially inappropriate" as −1, the rating "Socially inappropriate" as −0.6, the rating "Somewhat socially inappropriate" as −0.2, the rating "Somewhat socially appropriate" as 0.2, the rating "Socially appropriate" as 0.6, and finally the rating "Very socially appropriate" as 1 (Gächter et al. 2013, 2017; Erkut et al. 2015; Veselý 2015; D'Adda et al. 2016; Kimbrough and Vostroknutov 2016; Chang et al. 2019).

In Table 3, we report for each action of the group rule-following task the average social appropriateness rating and the frequency distribution of social appropriateness ratings separated by treatment. To test our hypothesis, we used the same approach as Chang et al. (2019). For each of the five actions that we considered in the norm elicitation experiment (place 0, 5, 10, 15, or 20 balls in the rule-compliant bucket), we used a two-sided Wilcoxon-Mann-Whitney test and a two-sided *t*-test to compare the norm ratings between the

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Table 2. Number of Subjects, Participation Fee, and Average Bonus in Each Study

Study	Individualizing treatment (n)	Binding treatment (n)	Total (n)	Participation fee (£)	Average bonus (£)
Prescreening survey	—	—	2,925	1.20	0.00
Rule elicitation survey	—	—	20	0.70	0.00
Choice experiment					
All control questions	160	160	320	1.60	2.01
Limited control questions	233	233	466	1.60	1.98
No control questions	233	233	466	1.60	1.63
Norm elicitation experiment	100	100	200	1.60	0.96

Notes. We invited 10 prescreened individualizing and 10 prescreened binding subjects to the rule elicitation survey. The choice experiment had three versions: the all control questions, the limited control questions, and the no control questions version.

individualizing and the binding treatment. Because of the inflated likelihood of type 1 errors, we applied the Bonferroni correction to our statistical significance threshold of $p = 0.05$. The new threshold became $p = 0.01$. As in Chang et al., we considered two group norms to be significantly different from one another when the majority of their norm ratings (in our case, three) were significantly different from each other. The p-values of the five t-tests and the five Wilcoxon-Mann-Whitney tests are displayed in Table 3. None of the 10 comparisons reached the significance threshold (in all cases $p > 0.134$).

Result 1 (Comparison of the Group Norms by Treatment). The group norms in the group rule-following task did not differ significantly between the individualizing and the binding treatment.

Result 1 is important for the interpretation of our remaining findings because it allows us to attribute behavior variance between treatments to our group identification manipulation without concern that norms are changing. In addition, in Section B.2 of Online Appendix B, we test and find that the social appropriateness of an action increased with the number of balls placed in the rule-compliant (blue) bucket.

6.2. Choice Experiments

6.2.1. Does Greater Moral Similarity Increase Group Identification? Our treatments in the choice experiment were designed so that subjects would be more similar to Group A in the individualizing treatment than in the binding treatment. Table 4 reports the average progressivism score for our subjects by choice experiment version and by treatment and the average absolute difference between their progressivism index and the index for Group A.

We can see from Table 4 that in each of the three versions, the average participant progressivism index ranged from 1.36 to 1.72 in the individualizing treatments and from 1.39 to 1.59 in the binding treatments (on a scale from −5 to 5). Recall that subjects in the individualizing treatment learned about a Group A with members whose progressivism index average was 2.33 and subjects in the binding treatment learned about a Group A with members whose index average was 0. We construct a similarity measure between the individual subject and their assigned Group A by using the absolute difference between the individual's progressivism index and Group A's average progressivism index. The average absolute differences ranged from 0.97 to 1.12 in the individualizing treatment and from 1.39 to 1.59 in the binding treatment (on a scale from 0 to 10). To summarize, our treatment manipulation was successful, and participants were on average morally more similar to the individualizing than the binding group.

In order to test whether our treatment manipulation successfully affected *self-reported* group identification, we compared subjects' group identification in the

Table 3. Elicited Social Appropriateness Ratings for the Group Rule-Following Task in the Individualizing and Binding Treatment

Action	Individualizing treatment ($n = 100$)							Binding treatment ($n = 100$)							p-values[a,b]	
In blue	Mean[a]	− − −	− −	−	+	++	+ + +	Mean[a]	− − −	− −	−	+	++	+ + +	WMW	T
0 balls	−0.35	44	18	8	8	6	16	−0.22	32	20	8	15	10	15	0.134	0.200
5 balls	−0.18	7	42	18	13	13	7	−0.10	9	30	19	16	20	6	0.249	0.279
10 balls	0.23	5	5	23	30	19	18	0.21	3	9	23	29	20	16	0.741	0.790
15 balls	0.26	5	9	12	23	41	10	0.20	0	13	19	30	32	6	0.143	0.329
20 balls	0.36	15	11	6	7	10	51	0.35	12	11	6	12	16	43	0.648	0.970

Notes. − − −, very socially inappropriate; − −, socially inappropriate; −, somewhat socially inappropriate; +, somewhat socially appropriate; ++, socially appropriate; + + +, very socially appropriate. WMW, Wilcoxon-Mann-Whitney test; T, t-test.

[a]We converted the categorical ratings in the same order as described above into the following numerical scores: −1, −0.6, −0.2, 0.2, 0.6, 1.

[b]The p-values are based on a two-sided Wilcoxon-Mann-Whitney test and t-test that compared the ratings between treatments.

Table 4. Average and Average Absolute Difference Between Subject and Group A Progressivism Index by Treatment in the Choice Experiment Versions

	Progressivism					
	Individualizing treatment			Binding treatment		
Choice experiment version	Average subject	Average Group A	Absolute difference	Average subject	Average Group A	Absolute difference
All control questions	1.72	2.33	0.97	1.59	0	1.59
Limited control questions	1.36	2.33	1.12	1.39	0	1.39
No control questions	1.51	2.33	1.10	1.47	0	1.47

Note. The table displays the average progressivism of participants according to their answers to the moral foundations questionnaire, the implied progressivism according to the five questions displayed when participants are introduced to Group A, and the average absolute difference between participants' progressivism and Group A's implied progressivism.

individualizing and the binding treatment. In line with the psychological social identity approach (Hornsey 2008, Turner and Reynolds 2012), we hypothesize that moral similarity increases group identification. As a consequence, self-reported group identification should be higher in the individualizing treatment than in the binding treatment.

Hypothesis 2 (Comparison of Self-Reported Group Identification by Treatment). *Self-reported group identification will be significantly higher in the individualizing treatment than the binding treatment.*

Table 5 lists the distance measure of the "Inclusion of Ingroup in the Self Scale" by choice experiment version and treatment. Column 2 reports the average responses for subjects in each version of the choice experiment who were randomized to the individualizing treatment (values range from 0.80 to 0.84) and column 3 reports those values for subjects randomized to the binding treatment (values range from 0.62 to 0.71). Column 4 reports the one-sided t-test values.

In all versions of the choice experiment, self-reported group identification was significantly higher in the individualizing treatment than in the binding treatment using one-sided t-tests ($p < 0.001$). In Section B.5 in Online Appendix B, we also considered other identification measures. We did not observe any noticeable differences due to changes in how we measure group identification.

Result 2 (Comparison of Self-Reported Group Identification by Treatment). Group identification was significantly higher in the individualizing treatment than in the binding treatment.

Although Hypothesis 2 tests whether the averages significantly differ between treatments, we can also use a Tobit regression to explore this relationship between moral similarity and self-reported group identification further. The regression has two advantages. First, we can use the individual measure of similarity for each subject using the difference between their responses and the progressivism index for their Group A. Second, the Tobit regression will address censoring at zero and one. We estimate a separate Tobit regression for each choice experiment version, which handles the censoring at zero and one (the minimum and maximum group identification a subject could indicate). In all models, we regressed the distance group identification measure of the "Inclusion of Ingroup in the Self Scale" against the subject's progressivism and the absolute difference between the individual's progressivism and the social group's implied progressivism index.

The results are reported in Table 6. As can be seen, distance progressivism was significant and negative in all specifications ($p < 0.001$). This result suggests that an increase in distance progressivism decreased group identification. In particular, if distance progressivism increased from 0 to 10 (on a scale from 0 to 10), group

Table 5. Comparison of Responses to "Inclusion of Ingroup in the Self Scale" Between Individualizing and Binding Treatments in the Choice Experiment Versions

	Identification (distance)		
Choice experiment version	Individualizing treatment	Binding treatment	p-value[a]
All control questions	0.80	0.62	0.001
Limited control questions	0.84	0.71	0.001
No control questions	0.80	0.67	0.001

Note. The table displays the average self-reported group identification according to the distance measure from the "Inclusion of Ingroup in the Self Scale."

[a]The p-values are based on the preregistered one-sided t-tests that compared answers between treatments.

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Table 6. Tobit Regression Estimating the Effect of Moral Similarity on Self-Reported Group Identification

	Identification (distance)		
	All control questions	Limited control questions	No control questions
Progressivism	0.019	−0.011	−0.001
	(0.016)	(0.013)	(0.013)
Distance progressivism	−0.118***	−0.085***	−0.079***
	(0.017)	(0.014)	(0.015)
Constant	0.836***	0.907***	0.843***
	(0.033)	(0.025)	(0.026)
Log-likelihood	−41	−58	−57
BIC	104	140	138
Number of cases	320	466	466

Notes. We used Tobit regressions. The standard errors are displayed in parentheses.
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

identification, ceteris paribus, decreased between 0.79 and 1.18 (on a scale from 0 to 1).[17]

6.2.2. Does Compliance with Group Rules Differ by Treatment? We now test whether our treatment manipulation affected participants' willingness to put more balls in the rule-compliant bucket. Because we find no differences in the norm to follow the rule (Result 1), we interpret the number of balls placed in the rule compliant bucket as an approximation of group norm sensitivity. In line with the psychological social identity approach (Hornsey 2008, Turner and Reynolds 2012), we hypothesized that moral similarity would increase compliance with group rules.

Hypothesis 3 (Comparison of the Number of Balls in the Rule-Compliant Bucket). *The number of balls in the rule-compliant bucket will be significantly higher in the individualizing treatment than the binding treatment.*

Table 7 reports the average number of balls placed in the (blue) rule compliant bucket by choice experiment version and treatment. Column 2 reports the average number of balls placed in the blue bucket for subjects randomized to the individualizing treatment by version; the average number of balls ranges from 7.74 to 9.41. Column 3 reports the average number of balls placed in the blue bucket for subjects randomized to the binding treatment by version; the average number of balls ranges from 7.08 to 7.79.

Although the direction is consistent with our hypothesis, only in the limited control questions version is the number of balls in the rule-compliant bucket significantly higher in the individualizing treatment than the binding treatment (column 3, $p = 0.012$). In the all control questions and the no control questions versions, there were no significant differences (column 3, $p = 0.212$ and $p = 0.172$, respectively).

Result 3 (Comparison of the Number of Balls in the Rule-Compliant Bucket). The number of balls in the rule-compliant bucket was significantly higher in the individualizing treatment than the binding treatment in the limited control questions version. In the all control questions and the no control questions versions, there were no significant differences.

Although comparison of average balls in the blue bucket by treatment suggests that the result could be unstable (an observation we discuss further in Section 7), we can also use the self-reported individual identification responses to explore the relationship between group identification and norm compliance using a Tobit regressions. As noted earlier, the psychological social identity approach (Hornsey 2008, Turner and Reynolds 2012) suggests that group identification

Table 7. Comparison of Average Balls Placed in Rule-Compliant Bucket Between Individualizing and Binding Treatments in the Choice Experiment Versions

	Average balls in the blue bucket		
Choice experiment version	Individualizing treatment	Binding treatment	*p*-value[a]
All control questions	8.15	7.44	0.212
Limited control questions	9.41	7.79	0.012
No control questions	7.74	7.08	0.172

Note. The table reports the average number of balls in the rule compliant (blue) bucket in the rule-following task.
[a]The *p*-values are based on the preregistered one-sided *t*-tests that compared choices between treatments.

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should increase the likelihood of prototypical group behavior. As such, we predicted that increased group identification increases group norm sensitivity and, with this, the number of balls placed in the rule-compliant bucket.

Hypothesis 4 (Effect of Group Identification on Group Norm Sensitivity). *Higher self-reported group identification will increase the number of balls placed in the rule-compliant bucket.*

Table 8 reports the estimates from Tobit regression models run for each choice experiment version, which handle the censoring at 0 and 20 (the minimum and maximum number of balls someone could place in the blue bucket). In all models, we regressed the number of balls in the rule-compliant bucket of the group rule-following task against the distance measure of the "Inclusion of Ingroup in the Self Scale."

In Table 8, the coefficients of group identification are positive and significant in all three versions ($p < 0.01$). For example, if group identification increases from zero to one (on a scale from zero to one), the number of balls in the rule-compliant bucket, ceteris paribus, increases between 8.68 and 14.13 balls (out of 20 balls).[18]

Result 4 (Effect of Group Identification on Group Norm Sensitivity). Higher group identification increased the number of balls placed in the rule-compliant bucket.

Taken together, Results 3 and 4 suggest that the effect of identification on following the rule may be present but can be easily disrupted in our laboratory setting. When comparing the average number of balls placed in the blue bucket across versions and treatments, we find that there are more balls placed in the blue bucket in the individualizing treatment, but the only difference that is significant is the limited control questions version. Using Tobit regressions, the relationship between identification and rule compliance is positive and significant in all three data sets we collected. We now turn to using the data we have to further explore what may be driving the patterns we observe in Tables 7 and 8.

7. Discussion

In our study, we set out to test whether identification with a rule-setting group can increase compliance with the group's rule *through* increased group norm sensitivity. Using an experiment, where decision makers were randomized to receive a rule from a group that was more or less similar to the decision maker's moral values, we were able to show that assignment to morally similar groups increased identification with the group (see Result 2). However, evidence on the effect of group identification on rule-compliance was less clear. Although average rule compliance was higher when the rule came from the more similar group, it only reached significance in one of three versions (see Result 3). When we examined the relation between rule-compliance and group identification via Tobit regression models, we found that the coefficient of identification was always significant and positive (see Result 4). Because group norms did not differ between treatments (see Result 1), we argue that whatever behavior variation we observe is due to changes in group norm sensitivity. Exploring the data a bit further, we now offer a few insights into why we might not have observed significant differences in the average rule compliance in two of the three versions.

The first reason, which would have been relevant in all three choice experiment versions, might be that censoring affected our mean comparisons of rule compliance. In all three specifications of the Tobit regressions (presented in Table 8), the coefficient on group identification is positive and significant. This raises the possibility that the insignificant comparison of means presented in Result 3 could stem from censoring at 0 and 20 (the minimum and maximum number of balls one could place in the blue bucket). To examine this further, we present the frequency of participants who placed 0 and 20 balls in each treatment of each choice experiment version in Table 9. Table 9 indicates a clustering of responses at 0 and 20. For instance, when examining the proportion of subjects who deposited 0 balls in the blue bucket across different groups and treatments, we observe that this percentage ranges

Table 8. Effect of Group Identification on Rule Compliance

	Balls in the blue bucket		
	All control questions	Limited control questions	No control questions
Identification (distance)	14.131***	8.689**	9.039**
	(4.034)	(3.076)	(2.881)
Constant	−4.636	0.923	−1.440
	(3.099)	(2.494)	(2.259)
Log-likelihood	−738	−1,157	−1,125
BIC	1,493	2,332	2,269
Number of cases	320	466	466

Notes. We used Tobit regressions. The standard errors are displayed in parentheses.
*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

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Table 9. Frequency of 0 or 20 Balls in the Rule-Compliant Bucket by Treatment and Choice Experiment Version

Number of balls in the blue bucket	Percentage					
	All control questions		Limited control questions		No control questions	
	Individualizing treatment	Binding treatment	Individualizing treatment	Binding treatment	Individualizing treatment	Binding treatment
0	33.75%	40.00%	24.89%	33.91%	34.33%	36.91%
20	20.00%	20.63%	25.75%	17.60%	18.45%	15.02%
Sum	53.75%	60.63%	50.64%	51.51%	52.78%	51.93%

Note. The table displays the frequency with which participants placed 0 or 20 balls in the rule-compliant blue bucket by treatment and choice experiment version.

from 24.89% to 40.00%. Similarly, the proportion of subjects who deposited 20 balls in the blue bucket ranged between 15.02% and 25.75%. Thus, for any column, between 50.64% and 60.63% of the data fall at the edges of action space and suggest that censoring could be an issue which a Tobit regression would be more suited for addressing.

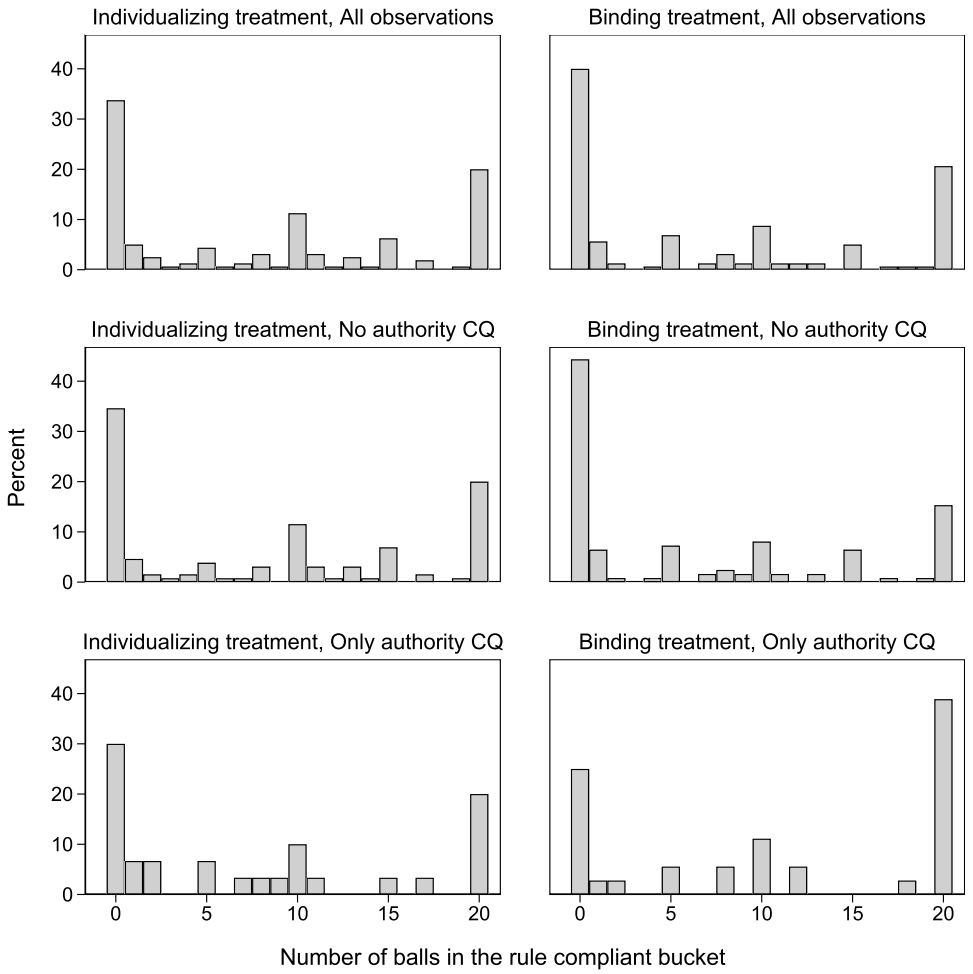
The second reason, which would have been only relevant in the all control questions choice experiment version, might be that priming from the authority control question affected our mean comparisons of rule compliance. The authority control question asked the participants to state how the social group answered the following question: "When you decide whether something is right or wrong, to what extent are the following considerations relevant to your thinking: Whether someone showed a lack of respect for authority?" In the individualizing treatment, the correct answer was "Slightly relevant," and in the binding treatment, "Somewhat relevant." Based on this variation, the authority control question may have primed participants to follow rules in the binding treatment (i.e., the treatment where they were *less* similar to the moral values of Group A).

To explore this possibility, Figure 4 plots six histograms using data from the all control questions choice version. The figure shows the number of balls in the rule-compliant bucket separated by whether subjects were in the individualizing or binding treatment (left and right columns, respectively) and by whether the participant was randomly assigned any control question (Row 1), any control question *other than the authority control question* (Row 2), or whether they were assigned the authority control question (Row 3). The histograms show that in the binding treatment assigning the authority control question increased the number of participants who fully complied with the rule (placing all 20 balls in the blue bucket) and reduced the number of participants who ignored the rule (placing 0 balls in the blue bucket). This resulted in a significant difference in the average number of balls placed in the blue bucket when the authority control question was used as compared with when *any other control question* was used (10.86 compared with 6.44, two-sided t-test $p = 0.003$). No such effect was observed for subjects in the individualizing treatment (where they were most similar to Group A and, per our hypothesis, inclined to comply with the rule as a result). We can see this by comparing the number of balls placed in the blue bucket by participants assigned to the individualizing treatment who received *any control question other than the authority control question*; they placed an average of 8.24 balls in the rule-compliant bucket. Individuals assigned to the individualizing treatment and the authority control question placed an average of 7.77 balls in the rule-compliant bucket (two-sided t-test, $p = 0.768$). Thus, one explanation for why our mean comparisons in Result 3 were fragile is that the authority control question primed subjects who would otherwise be less inclined to follow the rule (because their identification with Group A was lower) to be *more* rule compliant.[19] This had the unintended effect that the priming ran counter to the group identification effect we expected to see.

The third reason, which would have been only relevant in the no control questions choice experiment version, might be that inattention affected our mean comparisons of rule compliance. It is possible that omitting the control question in the no control questions version reduced subjects' attention on Group A's moral position and thus reduced the impact of our treatment variation. Empirical evidence on the effect of monetary incentives on performance in experimental tasks is generally mixed (Camerer and Hogarth 1999, Bonner et al. 2000, Hertwig and Ortmann 2001). However, the meta-studies by Bonner et al. (2000) and Camerer and Hogarth (1999) highlight that the type of task matters. In particular, both studies suggest that monetary incentives may be particularly helpful in improving performance in memorization tasks. As a consequence, we suspect that omitting control questions, and therefore lack of monetary incentives to be attentive to the moral position of Group A in our task, reduced memory performance and thus reduced the salience of the treatment stimulus.

Our discussion explores three possible explanations for why we observe fragility in our mean comparisons

Figure 4. Number of Balls Placed in the Rule-Compliant Bucket in the All Control Questions Version of the Choice Experiment by Treatment and Control Question



Notes. The histograms above report the balls placed in the norm compliant bucket in the all control questions version of the choice experiment. The top row displays the histograms for all observations. The middle row displays the balls placed in the norm compliant bucket for subjects in the all control questions version who received a control question other than the authority control question. The bottom row displays the balls placed in the norm compliant bucket for subjects in the all control questions version who received the authority control question. For reference, 130 of 160 observations in the individualizing treatment and 124 of 160 observations in the binding treatment were not assigned the authority control question. In the table, "CQ" refers to control question.

in Table 7: Censoring of responses could have affected behavior in all versions, priming to authority could have affected behavior in the all control question version, and lack of attention could have affected behavior in the no control question version. When combined with the Tobit analysis (Table 8), where higher self-reported identification significantly increased rule compliance across all versions, the results suggest a delicate relationship.

With regard to the external validity of our results, it should be noted that the limitations we have identified may partly reflect features of our experimental design. First, in real-world settings, norm compliance may involve costs or requirements that differ substantially from those in the group rule-following task. We use this task because it allows comparability across our studies. However, in many real-world settings, full norm compliance may be more costly or even unattainable. In such cases, measures of norm sensitivity may not have a binding upper bound, and analogous treatment comparisons could yield different results. Second, individuals in real-world settings may be less uncertain about whether and how to follow social norms because they have experience with the relevant group, institution, or community. In contrast, to improve experimental control, we induced social norms in the laboratory. Participants may therefore have been uncertain about whether they should follow these previously unfamiliar norms, making them more responsive to signals about appropriate behavior, such as the authority control question. Third, in many real-world settings, individuals have richer information about the moral

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positions of social groups through repeated interaction. By contrast, our use of minimal groups with previously unknown moral positions improves internal control but may also make behavior more sensitive to how attention is allocated during the experiment. These considerations suggest caution in extrapolating directly from our results to naturally occurring norm compliance; what we can conclude is that norm compliance is easily disrupted in our setting.

Nonetheless, our research connects with a nascent literature regarding the stability of norm sensitivity (Kimbrough and Vostroknutov 2016, 2018; Kimbrough et al. 2024). Although (group) norm sensitivity might be stable in the short term, the moral position of individuals (and, consequently, of social groups) can change in the medium term (Van de Vyver et al. 2016). As a consequence, our results suggest that group identification and (group) norm sensitivity may have limited stability or be easy to disrupt.

8. Conclusion

Our study finds that moral similarity in progressivism increases group identification, and group identification can increase compliance with rules from the social group. However, our results suggest that this relationship can be unstable and easily disrupted in our setting. Finally, we show that in our setting the behavior changes that are observed can be attributed to increased group norm sensitivity rather than differences in the group norms.

Our work advances several important strands of literature. We advance the study of social identity by testing a causal pathway between group identification, group norm sensitivity, and behavior change. Our study identifies one situational variable that determines when social identities are likely to matter (when there is value alignment). Moreover, we show that similarity in moral preferences, just like similarity in other preferences (Chen and Li 2009), increases group identification. Lastly, our results suggest that the relationship between identification and behavior can be disrupted by inattention or priming of other norms such as adherence to authority. This may be critical to understanding how and when social identity impacts behavior. The sources of fragility for social identity effects are underexplored in research to date.

Our contribution to the management literature is to extend the empirical rationale and offer cautionary notes for investing in value alignment between individuals and organizations. In our view, understanding the causal relation is important for predicting when increased group identification can be helpful. For example, if the effect of group identification stems from changes in group norm sensitivity but the group norms dictate inefficient behavior, increased group identification (for example from team building exercises) will not improve work performance and might even decrease it. In such a case, norm nudges that influence group norms might be a more promising intervention.

On a methodological note, we show how one can create groups in the laboratory based on moral values and how the rule-following task (Kimbrough and Vostroknutov 2018) can be modified to measure group norm sensitivity rather than general norm sensitivity. In principle, this method can measure the group norm sensitivity toward any communicable social group.

Our work identifies several promising avenues for future research. Our results suggest that it might be possible to describe and predict how an individual's set of acquired social identities changes over time. At any point in time, an individual's set of acquired social identities is (partly) determined by the individual's and the available social groups' moral positions. Hereby, previously acquired social identities determine an individual's moral position, and its members' moral position determines a social group's moral position. These connections might prove useful in the creation of dynamic social identity models. Second, it would be fruitful to study how extreme de-identification (despising another group) affects group norm sensitivity. It might be that individuals' group norm sensitivity becomes negative, and individuals are willing to incur a cost to violate group norms. Finally, it would be valuable to explore whether similarity in different concepts of morality or specific constructs related to social preferences affects group identification similarly.

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Endnotes

[1] According to Tajfel and Turner (1979, p. 59), "We can conceptualize a group … as a collection of individuals who perceive themselves to be members of the same social category, share some emotional involvement in this common definition of themselves, and achieve some degree of social consensus about the evaluation of their group and of their membership of it."

[2] Others have also suggested that social identity and the identity-associated norms might help to better understand social preferences (Krupka and Weber 2013, Kimbrough and Vostroknutov 2016), peer effects (Gächter et al. 2017), and framing (Chang et al. 2019).

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[3] Iyer et al. (2012) have argued for an additional liberty/oppression foundation. For a discussion on the inclusion of this and other foundations, see Graham et al. (2013).

[4] Several authors have used similar additively separable utility functions to model social identity, including Akerlof and Kranton (2002, 2005), Benjamin et al. (2010), Chang et al. (2019), Krupka and Weber (2013), Krupka et al. (2023), and Kimbrough and Vostroknutov (2016).

[5] In our experiments, participants must allocate 20 balls between a norm-compliant bucket that earns them £0.05 and a norm-violating bucket that earns them £0.10. As a consequence, the actual payoff function is $\Pi(x) = 0.10x + 0.05(20 - x) = 1 + 0.05x$ for $x \in [0, 20]$. In our theoretical framework, we follow Kimbrough and Vostroknutov (2018) and model a closely related simplified rule-following task in which placing balls in the norm-compliant bucket earns them £0, whereas placing them in the norm-violating bucket earns them £1. This changes the payoff function to $\Pi(x) = 1x + 0(20 - x) = x$ for $x \in [0, 20]$. Thus, we normalize the payoff structure while preserving the central tradeoff between monetary payoff and norm compliance.

[6] One explanation for why individuals identify with social groups that are (morally) similar to them might be that it is rational to do so. Other researchers have used similar arguments in the past. For example, Bernard et al. (2016) and Shayo (2009) assumed in their models that individuals choose their group identification to maximize utility. A rational individual identifies with a social group if identification increases utility. If an individual does not identify with the social group ($I = 0$ and therefore $\phi_i = 0$), the utility function only includes the standard utility component. However, if the individual identifies with the social group ($I > 0$ and therefore $\phi_i > 0$), the utility function includes both the standard and norm-dependent utility components. This change impacts the utility in two ways: First, identification potentially reduces the utility from the standard utility component since it might change the optimal action. Second, identification potentially increases or decreases the utility from the norm-dependent utility component. A rational individual will identify with a social group if there is at least one action in which the utility gain from the norm-dependent utility component is higher than a potential utility loss from the standard utility component. In other words, the standard and the norm-dependent utility component must be somewhat aligned. Now assume that the individual encounters a social group but is not aware of its normative prescriptions. The similarity between an individual and a social group might signal an alignment of the standard and norm-dependent utility components. Hereby, moral similarity might be perceived as a particularly informative signal. However, other characteristics might also inform the individual. For example, the general similarity in gender, ethnicity, and even art preferences suggests a somewhat related past and might also be used to create expectations about a social group's normative expectations.

[7] We thank anonymous referees for suggesting these additional versions.

[8] The "Inclusion of Ingroup in the Self Scale" is a version of the "Inclusion of Other in Self Scale" (Aron et al. 1992). However, instead of indicating the perceived distance to another person, one sets the perceived distance to a social group. The "Inclusion of Other in Self Scale" was validated by Gächter et al. (2015) and has been used in economics by Bicchieri et al. (2022) and Gächter et al. (2017). We use a continuous version of the scale to allow for higher precision. The change is identical to the modification of the discrete to the continuous "Inclusion of Other in Self Scale" (Aron et al. 1992, Le et al. 2007).

[9] We only measured the identification with the ingroup (Tropp and Wright 2001) and not the identification with the ingroup and the outgroup (Schubert and Otten 2002) because we only had a clearly defined ingroup: Group A.

[10] We asked participants to "… move the slider to a position in which the distance between the two circles most accurately describes your identification with Group A".

[11] In Kimbrough and Vostroknutov, the rule read: "The rule is to put the balls in the blue bucket." Related research demonstrated that individuals respond to such rule statements. Kimbrough and Vostroknutov (2016, 2018) show that individuals respond to the arbitrary rule of the experimenter, whereas Pryor et al. (2019) show that individuals comply with arbitrary descriptive norms even if they do not reflect the preferences of the group.

[12] Peer et al. (2017) examined various crowd sourcing platforms and discovered that the data quality on Prolific.com was comparable with Amazon Mechanical Turk. They also found that participants were more naive, less dishonest, and more diverse. Similarly, Douglas et al. (2023) found that participants on Prolific.com were more likely to pass attention checks, provide meaningful answers, follow instructions, remember information, have unique IP addresses and geolocation, and spend enough time on pages to read all items than on various other platforms like Amazon Mechanical Turk.

[13] We pay our subjects, who are U.S. nationals, in pounds because, at that time, Prolific paid all participants, including U.S. nationals, in pounds.

[14] The sample size in the all control questions choice experiment was based on the following power calculation: We used data from Kimbrough and Vostroknutov (2018) to calculate an expected effect size of 0.3. Subsequently, we used G-Power (Faul et al. 2007) to calculate the minimum required sample size for a one-tailed Wilcoxon-Mann-Whitney test with a parent distribution min ARE, an effect size of 0.3, an α-error probability of 0.05, a power of 0.80, and an even allocation between treatments. Because the limited control questions and no control questions data collection occurred after data collection for the all control questions version, we were able to estimate an effect size of 0.23 from those who were not randomly assigned the authority control question in the all control questions version. We used G-Power (Faul et al. 2007) to calculate the minimum required sample size for a one-tailed t-test, an effect size of 0.23, an α-error probability of 0.05, a power of 0.80, and an even allocation between treatments. The sample size of the norm elicitation experiment was based on samples used in related studies (Chang et al. 2019).

[15] We did not observe any significant differences in the measured moral foundations and socio-demographic characteristics between the treatments in any version of the choice experiment or in the norm elicitation experiment (see Section B.1 in Online Appendix B).

[16] Data collections were preregistered at the American Economic Association's registry for randomized controlled trials (https://doi.org/g5qw, https://doi.org/mcw3, and https://doi.org/m675). In Online Appendix A, we note any deviations from the preregistration.

[17] In Section B.3 in Online Appendix B, we examined which moral foundations drive the effect of moral similarity on group identification. In Section B.4 in Online Appendix B, we examined how robust our results are to changes in the conceptualization of moral similarity. We found that the distance in the individualizing and the binding index both influenced group identification. Furthermore, this effect was mainly driven by the distance in the fairness foundation. We also found that the effect of moral similarity on group identification was robust to changes in the conceptualization of moral similarity.

[18] In Section B.6 in Online Appendix B, we also consider other identification measures. The replications suggest that our results are largely unaffected by the conceptualization of group identification. The overlap identification measure is positive in all three versions

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and significant in two out of three versions. The question identification measure is significant and positive in all three versions. Furthermore, we can also run these regression using OLS models and find that the coefficients of "Identification (distance)" are also significant and positive ($p < 0.002$).

[19] A review of priming in economics can be found in Cohn and Maréchal (2016). This finding contrasts the results of Kimbrough and Vostroknutov (2016), who did not observe any effect of the five moral foundations of moral foundations theory on rule-following in the traffic-light rule-following task (see appendix E.1 of their study). However, as one referee points out, the norms in novel laboratory environments may be too new to subjects and could lead to instability in the results.

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