The Ethical Perils of Personal, Communal Relations: A Language Perspective

Maryam Kouchaki[1] iD, Francesca Gino[2], and Yuval Feldman[3]

[1]Kellogg School of Management, Northwestern University; [2]Negotiation, Organizations and Markets Unit, Harvard Business School, Harvard University; and [3]Faculty of Law, Bar-Ilan University

Abstract

Most companies use codes of conduct, ethics training, and regular communication to ensure that employees know about rules to follow to avoid misconduct. In the present research, we focused on the type of language used in codes of conduct and showed that impersonal language (e.g., "employees" or "members") and personal, communal language (e.g., "we") lead to different behaviors because they change how people perceive the group or organization of which they are a part. Using multiple methods, including lab- and field-based experiments (total $N = 1,443$), and a large data set of S&P 500 firms (i.e., publicly traded, large U.S. companies that are part of the S&P 500 stock market index), we robustly demonstrated that personal, communal language (compared with impersonal language) influences perceptions of a group's warmth, which, in turn, increases levels of dishonesty among its members.

Over the past three decades, numerous cases of corporate corruption and misconduct have had a deep impact on the economic and political landscape and have led organizational members and the public at large to become more distrusting of and cynical about corporate leaders and practices. In the wake of a large number of corporate scandals, from the fall of Enron in the 1990s to the more recent scandals at Wells Fargo and Volkswagen, policymakers have responded with tighter regulations, and leaders in companies across the globe have vowed to act more ethically (Feldman, 2018).

To make ethics salient within organizations and curb unethical behavior, regulators and companies have primarily focused on codes of conduct based on a set of moral standards to clarify appropriate and inappropriate behaviors—standards that are often similar across companies (Paine, Deshpande, Margolis, & Bettcher, 2005). Codes of conduct, as well as other forms of corporate communication, reflect the company culture and shape what employees believe is and is not important (Weick, 1979). Yet past empirical evidence on the effectiveness of codes of conduct in promoting ethical behavior in organizations is mixed (e.g., Kish-Gephart,

Harrison, & Treviño, 2010). However, research suggests that the degree to which an organization enforces its code of conduct—that is, whether the organization conveys to employees that it punishes code violations—does make a difference (Kish-Gephart et al., 2010). A salient component of enforcement is the type of language used in corporate communications and, more specifically, corporate codes of conduct.

At the extremes, language can be either personal and communal (e.g., using words such as "we") or rather impersonal (e.g., using words such as "employees"). For example, Wells Fargo's code of conduct, entitled "Our Code of Ethics and Business Conduct: Living Our Vision, Values & Goals," (Wells Fargo, n.d.) uses the communal language "we" (e.g., "We must never lose sight of putting our customers first and helping them succeed financially," p. 3). By contrast, 3M's code of conduct, "Be 3M: Code of Conduct Global Handbook,"

Corresponding Author:
Maryam Kouchaki, Northwestern University, Kellogg School of Management, 2211 Campus Dr., Evanston, IL 60208
E-mail: m-kouchaki@kellogg.northwestern.edu

(3M, 2016) uses the more impersonal term "3M People" (e.g., "3M has a longstanding and well-deserved reputation for doing business with uncompromising honesty and integrity because 3M People obey the law and 3M's Code of Conduct," p. 10).

We argue that the different types of language used in codes of conduct give members different perceptions of the company of which they are a part, as the language signals the type of relationship one has with others in one's group or organization. Personal language (e.g., "we") highlights a communal relationship, whereas impersonal language (e.g., "employees") refers to an exchange relationship (Clark & Mills, 1979; Mills & Clark, 1982). Communal relationships are often exemplified by close and family relationships. Exchange relationships, instead, are often exemplified by business or work-related relations and relations between acquaintances. Members of a communal relationship assume that "each [individual] is concerned about the welfare of the other" (Clark & Mills, 1979, p. 13), whereas more distance exists between members in exchange relations. According to previous research, individuals in a communal relationship, relative to those in an exchange relationship, for example, tend to follow communal norms (Clark, 1984; Clark, Mills, & Powell, 1986), help others more (Clark, Ouellette, Powell, & Milberg, 1987), and do not keep track of individual inputs for rewards or punishments (Clark, 1984).

Personal language (e.g., "we"), then, signals a more communal environment in which members are likely perceived to be easygoing, helpful, cooperative, and forgiving of one another. Impersonal language (e.g., "employees") signals a more exchange-based environment in which members are more formal and distant. Perceptions such as these are captured by "warmth"—a fundamental dimension, along with competence, of judgments we make when forming impressions of others (Fiske, Cuddy, Glick, & Xu, 2002), including groups and organizations (Aaker, Vohs, & Mogilner, 2010). Though some prior work has conceptualized warmth as interchangeable with moral character traits, here we rely on the work by Goodwin, Piazza, and Rozin (2014), which shows that the two are separable. In this conceptualization, warmth traits that are high in moral relevance, such as kindness, lovingness, and generosity, are considered moral traits that are distinct from pure warmth traits. These latter traits are less relevant to morality and include being easygoing, tolerant, and forgiving. We argue that companies that use "we" language in their corporate communications, compared with those that use impersonal language, signal a communal rather than exchange relationship to their members and thus signal that the group is warmer on pure warmth traits. These warmth traits capture how members perceive the group in terms of potential punishment resulting from behavior the group does not support. A more tolerant and forgiving group, in fact, is likely to be perceived as less likely to punish its members for misconduct or other forms of unacceptable behaviors.

We also argue that these perceptions are consequential, as they influence dishonest behavior of organizational members. In many situations, whether at work or in life, external pressures or our own selfish desires conflict with our moral values (Mazar, Amir, & Ariely, 2008). When balancing these competing motivations as we face the choice of whether to act dishonestly or not, one aspect we consider is the cost of being caught, including the potential punishment. In fact, according to standard economics and legal theorizing, the probability of being caught and the magnitude of punishment are key determinants of dishonesty (Becker, 1968). Prior research in economics has mainly focused on explicit punishment expectations, that is, punishment that can be enforced. For instance, enforceable punishment has been found to enhance a form of ethical behavior, namely cooperation, in public-goods games (Gächter, Renner, & Sefton, 2008), though the presence of punishment does not always lead to less unethical behavior (Sánchez-Pagés & Vorsatz, 2007). Related research by Bernhard, Fehr, and Fischbacher (2006) found that participants' expectations of being punished by a third party affected how much money they offered to recipients in a dictator game and that those expectations varied depending on whether the third party was an in-group or an out-group member. Specifically, dictators expected to be punished much less by in-group rather than out-group members for their violations (i.e., transferring less than 50% of their endowment to the recipient, thus violating an egalitarian norm), and the dictators who expected harsher punishment gave significantly more money to the recipient.

Though studies in the economics literature generally focus on punishment that is enforceable, there is ample evidence from social psychology that even implicit perceptions of punishment, with no actual perceived threat of being caught or punished, can affect behavior. For instance, Mazar et al. (2008) found that participants in an experiment in which they could cheat acted less dishonestly when they read an honor code beforehand rather than when they had not read an honor code, even though, objectively, the honor code made no implications about the potential for being punished: Participants played in an anonymous context so none of their personal information was identifiable. In another study, Shariff and Norenzayan (2011) found that people who believed in a comforting and forgiving God (i.e., a God who is less likely to punish) were more likely to cheat than those who believed in a punitive

and angry God. Together, these lines of research suggest that a greater expected severity of punishment if caught reduces the likelihood of cheating.

In the present research, we proposed that perceptions of pure warmth—capturing a person's or group's willingness to forgive and be easygoing—influence moral behavior by altering such expectations.[1] Specifically, we expected that perceptions of warmth, by indirectly influencing the expected magnitude of punishment, would increase dishonest behavior. Thus, in situations in which employees face opportunities to behave unethically, we suggest that those in a communal relationship, relative to an exchange relationship, perceive misconduct as less costly because they see their group as more forgiving and tolerant, which would not impose the potential negative consequences of rule breaking.

Though in our research we mainly focus on how different types of language in codes of conduct influence perceptions of warmth and, through them, dishonest behavior, we also consider the role of group identification. Personal language may in fact result in higher group identification compared with impersonal language, and such higher group identification may influence the choice of whether to act dishonestly, though prior research has often not found evidence for this link (e.g., Hildreth, Gino, & Bazerman, 2016).

The Present Research

To shed light on our hypotheses, we conducted nine studies that employed a variety of methods and sample populations. Importantly, except in Study 2b, participants completed tasks anonymously with no identifiable information and thus with no explicit expectation of being punished or potentially caught.

Study 1a

This study examined how the use of personal (i.e., "we") versus impersonal (i.e., "group members") language in group communications affects members' perceptions of the group. In addition, to test an alternative mechanism, we included a measure of group identification to examine whether the type of language used influences identification with the group.

Method

Participants and design. Ninety-five individuals (47.4% male; age: $M = 30.3$ years, $SD = 12.9$) at a university in the United States participated in this study, receiving $20 for completing an hour-long session. Study 1a had a two-conditions, between-subjects design (members code, we code). In this first study, we aimed for about 50

participants per condition, consistent with the recommendations of Simmons, Nelson, and Simonsohn (2013). The sample size was determined by the number of participants who showed up during the scheduled laboratory sessions. Before conducting the study, we planned to stop data collection after the scheduled sessions were over, hoping to recruit at least about 50 participants.

Procedure. To mirror the role of a corporation, we recruited participants to become members of the research laboratory of one of the authors. We told participants that researchers were currently looking to hire participants to help the research group pilot-test different surveys. We randomly assigned participants to two conditions and varied the content of the documents they read when joining the research group, including a code of conduct. We had 47 participants in the we-code condition (i.e., personal, communal language) and 48 in the members-code condition (i.e., impersonal language). We introduced the manipulation at the beginning of the study session. The instructions informed participants that the research group was interested in understanding the factors that influence task performance. Next, participants received the code-of-conduct manipulation and then completed a survey.

Code-of-conduct manipulation. The two different codes that participants received were similar, except for the language used to introduce them to the group. In the members-code version, the code of conduct was written in impersonal language from the group's perspective, referring to "[lab's name]" and "members," whereas the we-code condition had words such as "we" and "you." For instance, in the members-code version, participants read,

> [Name of group] emphasizes integrity, fairness, and respect. All staff including study participants are expected to exercise his or her best judgment and behave according to the highest ethical standards. [Name of group] values integrity in all aspects of the group's work. Honesty and quality must come from every person. Only in this manner can the group generate valid results which other organizations and teams can use effectively.

In the we-code version, this passage read,

> At [name of group], we emphasize integrity, fairness, and respect. We expect each other to exercise our best judgment and behave according to the highest ethical standards. We value integrity in all aspects of our work. Honesty and quality stem from each of us as members of this group. Only then can we generate valid results which other organizations and teams can use effectively.

Perceptions of the group. After the manipulation, participants responded to a number of questions about their group. As we explained earlier, even though perceptions of warmth and morality are closely linked, recent work has made a clear theoretical and empirical distinction between warmth perceptions (conceptualized as inclusive of highly morally relevant traits, such as sincerity, as well as less morally relevant traits, such as friendliness) and moral character traits that are low on warmth (e.g., principled, just, trustworthy; Goodwin et al., 2014). Though we predicted that the language used in codes of conduct would affect pure warmth traits, for completeness, we used an inclusive list of attributes capturing perceptions of both warmth and morality. Participants were provided with a list of 21 traits (adapted from Goodwin et al., 2014), including 6 from the high-warmth–high-morality category (forgiving, helpful, kind, empathetic, cooperative, tolerant; $\alpha = .91$) and 5 from the high-warmth–low-morality category (agreeable, warm, sociable, easygoing, playful; $\alpha = .91$). There were 4 traits that captured moral character traits (low-warmth–high-morality category; fair, principled, responsible, trustworthy; $\alpha = .89$) and 6 that measured competence (competent, effective, efficient, innovative, organized, logical; $\alpha = .89$). Participants rated their perceptions of each trait on a scale ranging from 1 (*not at all*) to 7 (*very much*).

We included three items ($\alpha = .94$) to measure group identification (adapted from Doosje, Ellemers, & Spears, 1995). Sample items were "I feel a strong sense of belonging to this group" and "I identify with this group."

At the end of the study, we asked participants a few questions to see whether the language in the code was comprehended differently depending on condition. Specifically, we asked, "How clear was the writing in the group's goals and values document that you read?" "How abstract would you rate the group's goals and values document that you read?" and "How well did you understand the group's goals and values document that you read?"

Results

Perceptions of the group's warmth. The language used ("we" vs. "members") increased perceptions of traits related to high warmth–low morality (we code: $M = 5.13$, $SD = 1.16$, 95% confidence interval, or CI = [4.79, 5.47]; members code: $M = 4.47$, $SD = 1.31$, 95% CI = [4.09, 4.85]), $t(93) = 2.62$, $p = .010$, and also those related to high warmth–high morality (we code: $M = 5.26$, $SD = 1.17$, 95% CI = [4.91, 5.60]; members code: $M = 4.44$, $SD = 1.28$, 95% CI = [4.07, 4.81]), $t(93) = 3.23$, $p = .002$. See Figure 1.

Perceptions of the group's morality and competence. However, the language used did not significantly influence perceptions of low warmth–high morality (we code: $M =$

5.84, $SD = 1.03$, 95% CI = [5.54, 6.14]; members code: $M = 5.58$, $SD = 1.28$, 95% CI = [5.21, 5.95]), $t(93) = 1.08$, $p = .284$, or perception of competence (we code: $M = 5.68$, $SD = 0.93$, 95% CI = [5.41, 5.96]; members code: $M = 5.54$, $SD = 1.02$, 95% CI = [5.24, 5.83]), $t(93) = 0.74$, $p = .462$.

Group identification. Group identification did not differ between conditions, either (we code: $M = 4.03$, $SD = 1.59$, 95% CI = [3.56, 4.49]; members code: $M = 3.50$, $SD = 1.78$, 95% CI = [2.98, 4.02]), $t(93) = 1.53$, $p = .130$.

Reactions to the code. We also found no significant differences between the two codes in terms of how clear, abstract, and understandable participants found them to be (ps > .323).

Discussion

In this study, we found initial evidence that the use of personal (i.e., "we") versus impersonal (i.e., "group members") language in group communications affects members' perceptions of the group, leading them to view the group as warmer. However, we did not find any difference in group identification between the two conditions (we code and members code). It is possible that our direct request to join the research team in a laboratory experiment was not a strong manipulation, and, therefore, the difference in identification based on the language in the code of conduct was not salient enough to influence participants' level of identification. Nevertheless, we included this measure in our next study to further investigate the role of group identification, because group affiliation has been shown to affect willingness to forgive egalitarian-norm violations and punishment expectations (Bernhard et al., 2006).

Study 1b

In this preregistered study, we aimed to replicate the effect of the use of personal versus impersonal language on perceptions of warmth. We also directly measured group members' perceptions of the magnitude of punishment if they were caught.

Method

Participants and design. Two hundred three individuals (56.7.4% male; age: $M = 36.1$ years, $SD = 11.5$) recruited from Amazon's Mechanical Turk participated in this study in exchange for $0.50. Study 1b had a two-conditions, between-subjects design (members code, we code). We aimed for approximately 100 participants per condition. This study was preregistered on the Open Science Framework (https://osf.io/epsva).
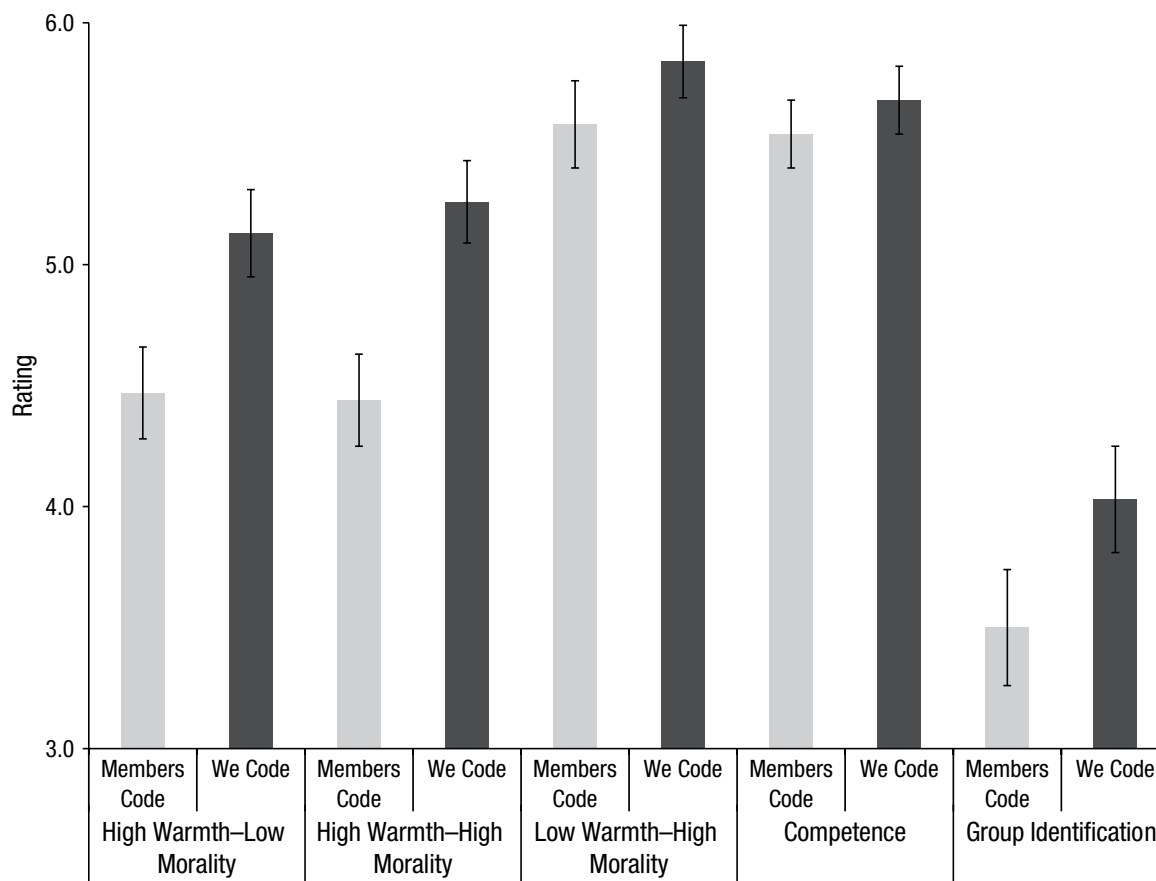
Fig. 1. Mean ratings of traits attributed to the group by participants in the two code conditions, separately for each of the five trait categories in Study 1a. Error bars indicate standard errors.

Procedure. We recruited participants to become members of the research laboratory of one of the authors and varied the content of the code of conduct they read when joining the research group. We used code-of-conduct instructions similar to those in Study 1a. We randomly assigned participants to two conditions: we code (*n* = 101) and members code (*n* = 102).

After the manipulation, participants responded to a number of questions about their group in random order. We measured expected magnitude of punishment if caught with two items ("To what extent do you think a member of this group would be punished if they are caught making a mistake?" and "How severe do you expect the punishment to be if a member of this group is caught making a mistake?"; α = .88). In addition, we included two items to assess the expected probability of being caught ("How probable do you think it is in this group that your actions would be scrutinized?" and "How probable do you think it is in this group to be detected if you make a mistake?"; α = .70). These four items were measured on a scale ranging from 1 (*not at all*) to 9 (*extremely*).

Participants were provided with the same list of 21 traits as in Study 1a, which they rated on a scale from 1 (*not at all*) to 7 (*very much*). These traits were from the high-warmth–high-morality category (α = .91), high-warmth–low-morality category (α = .87), low-warmth–high-morality category (α = .88), and competence category (α = .92). Finally, we included three items (α = .96) to measure group identification, as in Study 1a.

Results

Expected severity of punishment. Consistent with our theorizing, results showed that the language used ("we" vs. "members") lowered participants' expectation of the magnitude of punishment for violations (we code: *M* = 4.71, *SD* = 1.87, 95% CI = [4.34, 5.08]; members code: *M* = 5.73, *SD* = 1.74, 95% CI = [5.39, 6.08]), *t*(201) = 4.03, *p* < .001.

Probability of punishment. However, there were no differences between conditions on the perceived probability of
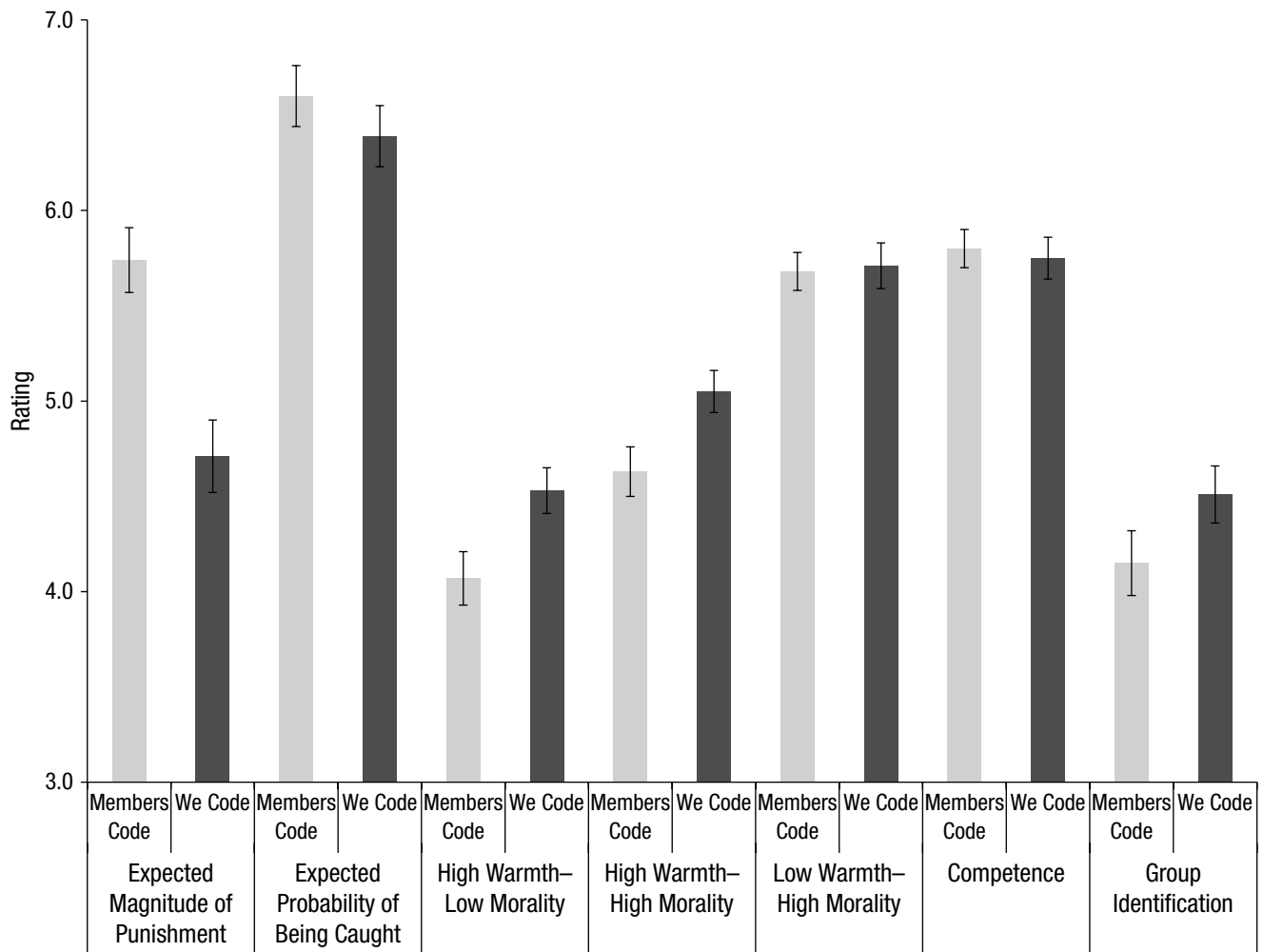
Fig. 2. Mean ratings of traits attributed to the group by participants in the two code conditions, separately for each of the seven trait categories in Study 1b.

being caught (we code: $M = 6.39$, $SD = 1.66$, 95% CI = [6.06, 6.72]; members code: $M = 6.60$, $SD = 1.62$, 95% CI = [6.28, 6.92]), $t(201) = 0.92$, $p = .358$.

Perceptions of the group's warmth. As shown in Figure 2, participants rated the research group higher on high-warmth–low-morality traits in the we-code condition ($M = 4.53$, $SD = 1.16$, 95% CI = [4.30, 4.76]) than they did in the members-code condition ($M = 4.07$, $SD = 1.43$, 95% CI = [3.79, 4.35]), $t(201) = 2.51$, $p = .013$. Perceptions of high warmth–high morality also differed between the two conditions (we code: $M = 5.05$, $SD = 1.13$, 95% CI = [4.82, 5.28]; members code: $M = 4.63$, $SD = 1.35$, 95% CI = [4.37, 4.90]), $t(201) = 2.41$, $p = .017$.

Perceptions of the group's morality and competence. We found no significant differences between conditions on perceptions of moral character (we code: $M = 5.70$, $SD = 1.21$, 95% CI = [5.47, 4.95]; members code: $M = 5.68$, $SD = 0.99$, 95% CI = [4.48, 4.87]), $t(201) < 1$, or on perceptions of competence (we code: $M = 5.75$, $SD = 1.09$, 95% CI = [5.54, 4.97]; members code: $M = 5.80$, $SD = 0.98$, 95% CI = [5.60, 5.99]), $t(201) < 1$.

Group identification. The language used did not produce differences on group identification (we code: $M = 4.51$, $SD = 1.51$, 95% CI = [4.21, 4.81]; members code: $M = 4.15$, $SD = 1.77$, 95% CI = [3.80, 4.49]), $t(201) = 1.58$, $p = .116$.

Discussion

This preregistered study provided further evidence for the effect of personal versus impersonal language on perceptions of warmth. Group identification, instead, was again not significantly affected by our manipulation.

Study 2a

Having demonstrated that the use of personal versus impersonal language in codes of conduct affects perceptions of warmth of the group with that code of conduct, we next examined how language also influences members' choices to act dishonestly.

Method

Participants and design. One hundred twenty individuals (66.7% male; age: $M = 31.9$ years, $SD = 10.0$) recruited from Amazon's Mechanical Turk participated in this study in exchange for $1 and had the opportunity to earn up to $1 extra during the study. Study 2a had a two-conditions, between-subjects design (members code, we code). We aimed for approximately 50 participants per condition, consistent with the recommendations of Simmons et al. (2013).

Procedure. We used similar code-of-conduct instructions as in our previous studies. There were 58 participants in the we-code condition and 62 in the members-code condition. In this study, participants completed a dishonesty measure, which consisted of the following performance task. They received 10 matrices (Kouchaki & Smith, 2014), each appearing for 15 s on their computer screen. Each matrix had 12 three-digit numbers with two decimal places (e.g., "2.56" and "7.44"), and participants were asked to find the two numbers that summed to exactly 10.00. Participants had an opportunity to cheat: They were told to indicate that they found the matching pair by clicking the "found it" box on the screen and were not asked to actually reveal the matching pair. To identify clear instances of cheating, we provided participants with five matrices that were unsolvable (i.e., they did not contain two numbers that summed to 10); the other five were solvable. Participants were told that for each pair they found, they would receive 10 cents.

Results

The number of all matrices reported as solved was significantly different between the two code conditions (we code: $M = 6.36$, $SD = 2.89$, 95% CI = [5.60, 7.12]; members code: $M = 5.06$, $SD = 2.76$, 95% CI = [4.36, 5.76]), $t(118) = 2.52$, $p = .013$. Participants in the we-code condition reported solving a higher number of both unsolvable matrices ($M = 2.83$, $SD = 1.85$, 95% CI = [2.34, 3.13]) and solvable ones ($M = 3.53$, $SD = 1.26$, 95% CI = [3.20, 3.87]) compared with participants in the members-code condition (unsolvable: $M = 2.06$, $SD = 1.62$, 95% CI = [1.65, 2.48]; solvable: $M = 3.00$, $SD = 1.38$, 95% CI = [2.65, 3.35]), $t(118) = 2.41$, $p = .017$, and $t(118) = 2.21$, $p = .029$, respectively.

Discussion

Overall, the results of this study show that the use of personal versus impersonal language impacts dishonest behavior.

Study 2b

In Study 2b, to provide further evidence of the relationship between the use of personal versus impersonal language and individual misconduct, we conducted a field experiment on Upwork (formerly oDesk), an online labor market of registered freelancers. Using this platform allowed us to hire people who would be working for a few weeks in a real setting. This was a natural field experiment: Although participants were informed they would be joining a research group, they did not know that their instructions were part of an experiment. The oDesk platform allowed us to conduct targeted hiring of workers with data-entry expertise at a specific base wage, which gave us the control over recruitment needed for a field experiment.

Method

Participants and design. We hired freelancers for a data-entry task. All identified themselves in their profiles as administrative support professionals and classified themselves as having a data-entry specialty. We restricted the recruitment to North American freelancers, a subcategory that lists requested hourly wages of less than $12, according to their profiles. To ensure that we included active workers, we further restricted our sample to those who had logged into the platform within the last 30 days. We ended up with a total of 438 active workers (28% male) in our subcategory at the time of data collection (late 2014).

We notified all of the workers that we had a job for them and invited them to accept the job offer within the next 5 days. We told them that we were looking to hire a group of people to help our research team with simple data entry or pilot tests over the course of 3 weeks. They were informed that each week they would receive a link through the platform that would direct them to an external website, where they were to complete the task within 5 days. Over the course of the 3 weeks, participants would complete 1.5 hr of work and earn at least $20 (not including potential bonuses). They were informed that the entire job consisted of an orientation survey (~15 min in duration), for which they could earn $6, and two tasks (~30 min each in duration), for which they could earn $14. Of the 438 we contacted, 157 workers (31.8% male) accepted our offer of employment within 5 days, an overall take-up rate of 36%.

Procedure. Each Monday, we sent workers a message through the platform with a link to an external website where they could complete a survey within 5 days. They were paid at the end of each week. Out of 157 workers who were hired, 151 completed the orientation survey, and each was randomly assigned to one code-of-conduct condition (51 to no code, 50 to members code, and 50 to we code).

In the no-code condition, participants were given only instructions about the tasks to be completed in the following weeks; they then filled out a short demographic survey. In the members-code and we-code conditions, participants started with an orientation activity. As part of this onboarding process, participants received similar code-of-conduct instructions as in our previous studies. Then they completed a short demographic questionnaire.

All participants were informed that the link for the following week's performance task would be sent to them on Monday and were told that they had 5 days to complete the task. On Monday of Week 1, we sent each participant who completed the orientation ($n = 151$) a message through the platform, with a link based on his or her assigned condition to make sure we controlled for the language used throughout the task. On the basis of the condition participants had been assigned to, they were briefly reminded of the code of conduct they had read a few days earlier.

Afterward, participants received instructions about the tasks they had to complete. They started with a filler typing task. Next, we gave them a captcha activity that involved typing the letters and numbers displayed in an ambiguous image. They were informed that the purpose of this task was to determine their average speed of visual processing and typing. Given that we recruited only workers who listed data entry as a specialty on their oDesk profile, we chose a data-entry task (entering captchas) that is fairly common in online labor markets. There was a reasonable demand on the platform for data entry, and specifically for entering captchas; thus, this was an ordinary request that would not arouse the suspicion of workers that they were part of an experiment. All participants completed a first round of 65 captchas.

Next, we measured cheating by giving participants the opportunity to overstate their performance on a round of data entry and thus earn undeserved money. Participants were informed that in a second round, they would have 3 min to complete a captcha task again. However, in this round, their goal would be to complete 35 captchas in 3 min, and they would be paid an additional $2 if they exceeded this goal. They were further told that given the complexity of the task, the online survey platform could not check their work; therefore, they would be the ones to provide the number of captchas they completed, and their bonuses would be paid on the basis of their self-reports. Thus, we gave workers an opportunity to lie about their performance and receive an additional $2.

Similarly, on Monday of Week 2, we sent a message through the platform with a link to another survey ($n = 141$). We used the same task from the previous week with a new set of captchas and once again gave workers the opportunity to lie during their second round about their performance on the captcha task and earn an additional $2.

Results

Out of 151 workers who were assigned to different code-of-conduct conditions, 141 of them completed the captcha task in Week 1 (49 in no code, 43 in members code, and 49 in we code). We compared the number of workers in each of the three conditions in Week 1 who lied to earn a $2 bonus (i.e., they indicated that they had completed 35 captchas when in reality they did not) and found a significant difference in unethical behavior, $\chi^2(2, N = 141) = 8.63$, $p = .013$ (see Fig. 3). More workers in the we-code condition lied about their performance (20/49, or 40.1%) compared with those in both the members-code condition (6/43, or 14%, $p = .004$) and the no-code condition (12/49, or 24.5%, $p = .085$), though the difference between the we-code condition and the no-code condition was nonsignificant. There was also no significant difference in lying between the members-code and the no-code conditions ($p = .20$).

During Week 2, out of 141 workers who participated in Week 1, 123 workers (42 in no code, 39 in members code, 42 in we code) returned and entered a new set of captchas. We compared the number of workers across conditions who lied to earn a $2 bonus and again found a significant difference, $\chi^2(2, N = 123) = 10.18$, $p = .006$. More workers in the we-code condition lied about their performance (8/42, or 19%) compared with both the members-code condition (1/39, or 2.6%, $p = .018$) and the no-code condition (1/42, or 2.4%, $p = .014$). There was no significant difference in lying between the members-code and the no-code conditions ($p = .96$).

We also performed repeated measures analyses to test the effects of the code-of-conduct manipulation on the level of dishonesty each week. Results showed a significant main effect of time, $F(1, 120) = 26.40$, $p < .001$, and no significant interaction of condition and time, $F(2, 120) = 1.55$, $p = .22$. As expected, we found a significant main effect of condition, $F(2, 120) = 5.82$, $p = .004$. The significant main effect of time demonstrates that
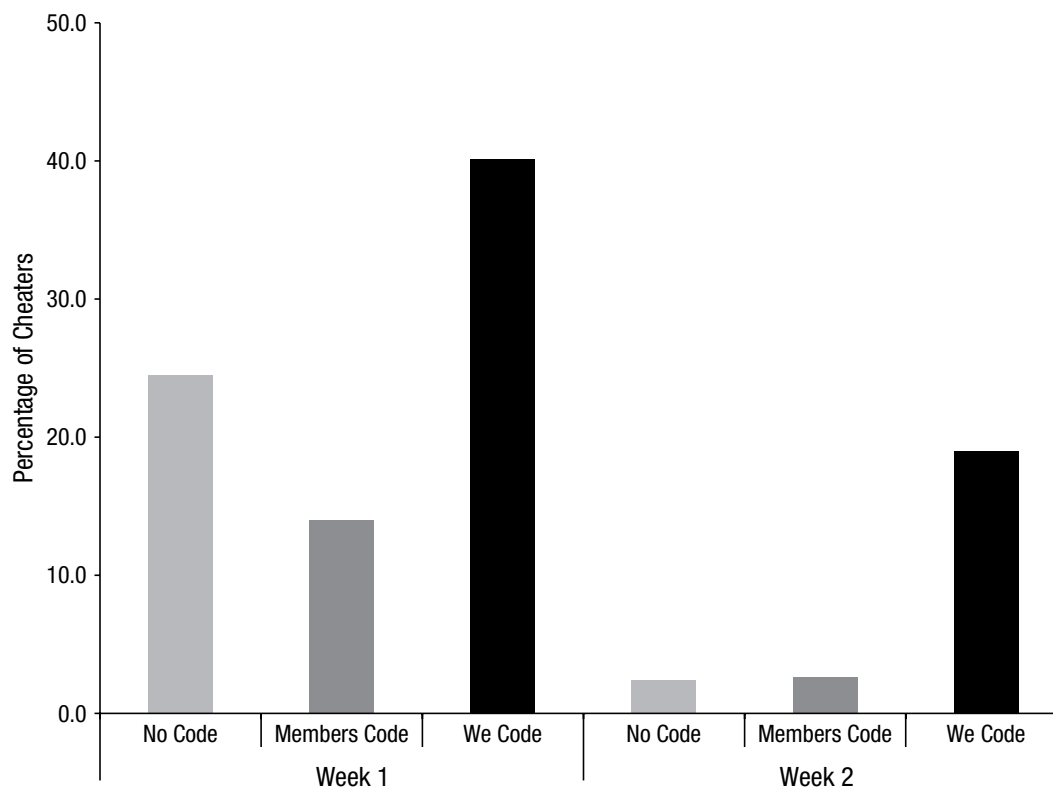
Fig. 3. Percentage of cheaters in each of the three code conditions, separately for each week of Study 2b.

participants in all conditions were less likely to cheat the second week than the first one. We had no a priori prediction about this, and thus we can only speculate post hoc as to why people cheated less over time. When looking more closely at the cheaters in Week 2, we found that most of them (90%) also cheated during Week 1.

The results of this study provide further evidence for an effect of the use of personal versus impersonal language on dishonest behavior. In this field experiment, participants were not anonymous, and therefore there was some threat of being caught or punished if caught. However, we did not explicitly manipulate their expectations of being caught or punished.

Study 2c

In Study 2c, we attempted to replicate our findings in a controlled laboratory setting. Although we did not find a difference in identification in Studies 1a and 1b, we tested for group identification once more as a possible mechanism explaining why personal, communal language results in higher levels of dishonesty.

Method

Participants and design. One hundred thirty-four students (33.8% male; age: $M = 20.5$ years, $SD = 1.8$) at a

university in the United States participated in this study for pay. We used the same recruiting strategy as in Study 1a. The experimental session lasted up to 60 min, and the students received $20 for participation with the possibility of earning another $10, depending on their performance. Study 2c had a three-conditions, between-subjects design (no code, members code, we code). The sample size was determined by the number of participants who showed up during the scheduled sessions. Before conducting the study, we planned to stop data collection after the scheduled sessions were over, hoping to recruit at least about 50 participants for each condition, similar to our earlier studies.

Procedure. We randomly assigned participants to the three conditions and ran each condition in a separate session. There were 47 participants in the we-code condition, 43 in the members-code condition, and 44 in the no-code condition. We ran a total of 18 sessions with a minimum of 3 participants in each session. The experimenter was blind to session condition.

The code manipulation was identical to that used in previous studies. In the no-code condition, participants did not receive a code of conduct and proceeded to complete the first task. In the other two conditions, we introduced this manipulation at the beginning of the session, informing participants that they would help
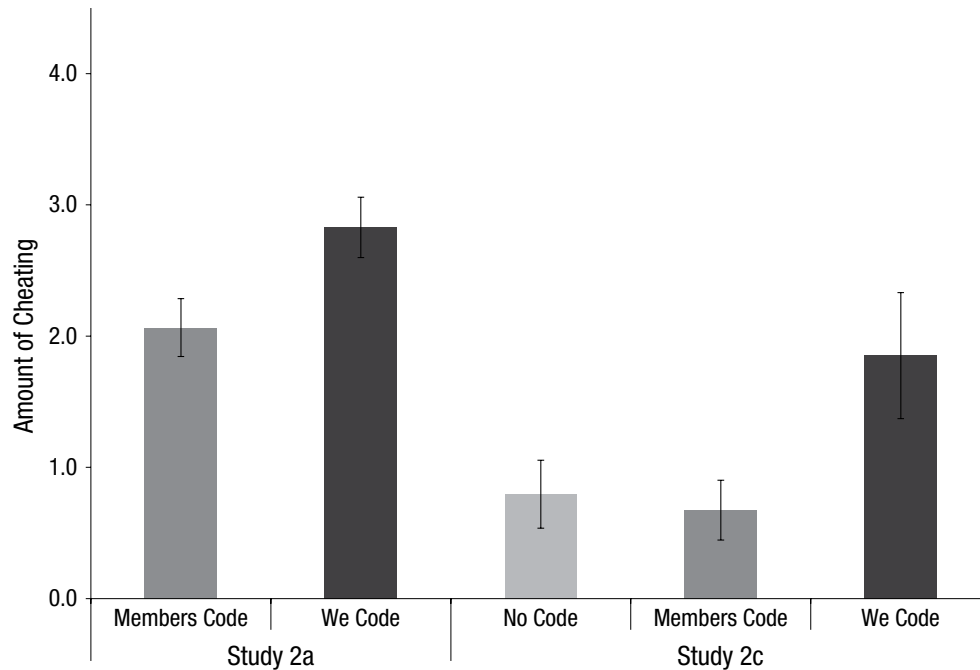
Fig. 4. Amount of cheating in each code condition in Studies 2a and 2c. Cheating was measured by the number of unsolvable word jumbles that participants claimed to have solved (Study 2a) and by the number of word jumbles, starting from the fifth (unsolvable) word jumble that participants reported to have correctly solved (Study 2c).

pilot-test a number of performance tasks. Each session started with two performance filler tasks, captchas, and typing. Each task was timed and lasted about 10 min. Afterward, we measured cheating by giving participants the opportunity to overstate their performance on a problem-solving task and thus earn undeserved money.

We gave participants an envelope that contained $10 (nine $1 bills and four quarters) along with a sheet of paper. The paper was a worksheet with 20 matrices, each consisting of 12 three-digit numbers with two decimal places (e.g., "2.56" and "7.44"; Mazar et al., 2008). Participants had 4 min to find two numbers in each matrix that added up to 10.00. For each correctly solved matrix, they would earn $0.50. After 4 min, participants were asked to correct their own answers, report the number of problems they correctly solved on an online survey, pay themselves, recycle their worksheet, and leave the rest of money in the envelope.

In the eyes of the participants, the procedure seemed anonymous because no identifying information (e.g., name, participant number) was apparent on any of the forms or the envelope. All participants received the same matrices to solve, except that a single number was unique to each participant, so we could calculate the difference between self-reported and actual performance and use this difference as our main dependent variable, dishonesty. Positive difference scores indicated that participants overreported their performance and

cheated on the task. At the end of the task, we assessed participants' perceived identification with the group they joined using the same three items ($\alpha = .94$) as in Studies 1a and 1b.

Results

Amount of dishonesty. A one-way analysis of variance on amount of dishonesty revealed a significant difference among the three conditions, $F(2, 131) = 3.49$, $p = .033$ (see Fig. 4). Participants in the we-code condition overreported their performance more ($M = 1.85$, $SD = 3.29$, 95% CI = [0.89, 2.82]) than did those in the members-code condition ($M = 0.67$, $SD = 1.49$, 95% CI = [0.22, 1.13], $p = .034$) and those in the no-code condition ($M = 0.80$, $SD = 1.72$, 95% CI = [0.27, 1.32], $p = .061$). The difference between the we-code and no-code conditions did not reach significance ($p = .061$) but was consistent with the expected trend. There was no significant difference in dishonesty between the members-code and the no-code conditions ($p = .84$). In addition, we found no significant differences in actual performance across conditions, $F(2, 131) = 0.73$, $p = .483$. None of the comparisons were significant.

Group identification. We also found a significant difference among the three conditions on group identification, $F(2, 131) = 7.73$, $p < .001$; participants in the we-code condition felt stronger identification ($M = 3.66$, $SD = 1.50$,

95% CI = [3.22, 4.10]) compared with both those in the members-code condition (M = 2.67, SD = 1.09, 95% CI = [2.33, 3.00], p = .001) and those in the no-code condition (M = 2.74, SD = 1.39, 95% CI = [2.32, 3.17], p = .003). There was no significant difference in group identification between participants in the members-code and the no-code conditions (p = .78).

Though prior research has often not found a link between group identification and dishonesty (e.g., Hildreth et al., 2016), strong identification with one's group can encourage unethical behavior that benefits the group. For example, prior work shows that individuals act less ethically when they identify with their group and hold strong reciprocity beliefs than when they do not (Umphress, Bingham, & Mitchell, 2010). Given that we found differences on identification in this study, we then tested whether group identification mediated the relationship between the code-of-conduct condition and observed dishonesty. A bootstrapping test (with 10,000 iterations) using the we-code condition as the condition of reference revealed that the 95% bias-corrected CI for the size of the indirect effect included zero. In sum, we found no indirect effect of our manipulation on dishonesty through group identification.

Discussion

Overall, in this study, we found significant differences in cheating between the we-code condition and both the members-code and no-code conditions. Even though participants in the we-code condition identified more strongly with their group than did those in the other conditions, their level of identification did not predict their dishonesty. Though we did not find evidence for identification as a possible mechanism explaining why personal, communal language encourages dishonesty, we further explored the mediating role of perceptions of warmth, as well as group identification, in our next study.

Study 3a

In Study 3a, we examined whether the perception of a group's warmth mediates the effect of the language used in group communication and dishonesty. Once again, we included group identification to examine its role as a potential alternative psychological mechanism.

Method

Participants and design. We recruited 250 individuals through Mechanical Turk to participate in a two-part online study. Participants received $3 for completing both parts of the study a week apart and had an opportunity to earn an additional $10. Following a decision made prior

to conducting the study, we excluded the responses of 17 participants who failed an attention check in the first part. Out of the 234 participants, 217 (57.1% male; age: M = 35.7 years, SD = 10.3) completed both parts. Study 3a had a two-conditions, between-subjects design (members code, we code). To ensure high power, we aimed for a sample size of 100 participants per cell and recruited more participants in Part 1.

Procedure. In Part 1, participants were randomly assigned to two conditions (members code, we code). There were 112 in the we-code condition and 105 in the members-code condition. The manipulation was identical to the one used in the previous studies.

Perceptions of the group. After the manipulation, as in Study 1, participants responded to a number of questions in reference to the research group. They were provided with a list of 14 traits (adapted from Goodwin et al., 2014). Three of these traits were from the high-warmth–high-morality category (forgiving, kind, tolerant; α = .86), whereas two traits were from the high-warmth–low-morality category (warm, agreeable; α = .88). There were three moral character traits from Goodwin et al. (low warmth–high morality; fair, principled, responsible, α = .77), and six traits that captured the competence category (competent, effective, efficient, innovative, organized, logical; α = .89). Participants rated their perception of the "name of the research group" on each of these traits using a scale ranging from 1 (*not at all*) to 7 (*very much*). We also measured group identification with the same three items (α = .96) from previous studies. Afterward, participants completed a brief demographic questionnaire and then provided their e-mail address so they could be contacted to complete Part 2.

Dishonesty measure. One week later, an e-mail was sent to all participants who successfully completed Part 1 and provided a valid e-mail address. At Time 2, participants received a link based on their assigned condition to ensure that we controlled for the language used throughout the task. According to the condition to which participants had been assigned, they were briefly reminded of the code of conduct they had read a week earlier. Next, participants completed a task in which they could misreport their performance for extra money. The task involved unscrambling 10 word jumbles in 5 min, for which they would receive a $1 bonus for every jumble they reported to have solved correctly (adapted from Kouchaki & Gino, 2016). They had to indicate which word jumbles they successfully unscrambled without being asked to write down the unscrambled words (e.g., "O L A R C"). The instructions notified them that they had to unscramble the word jumbles in the order in which they appeared on
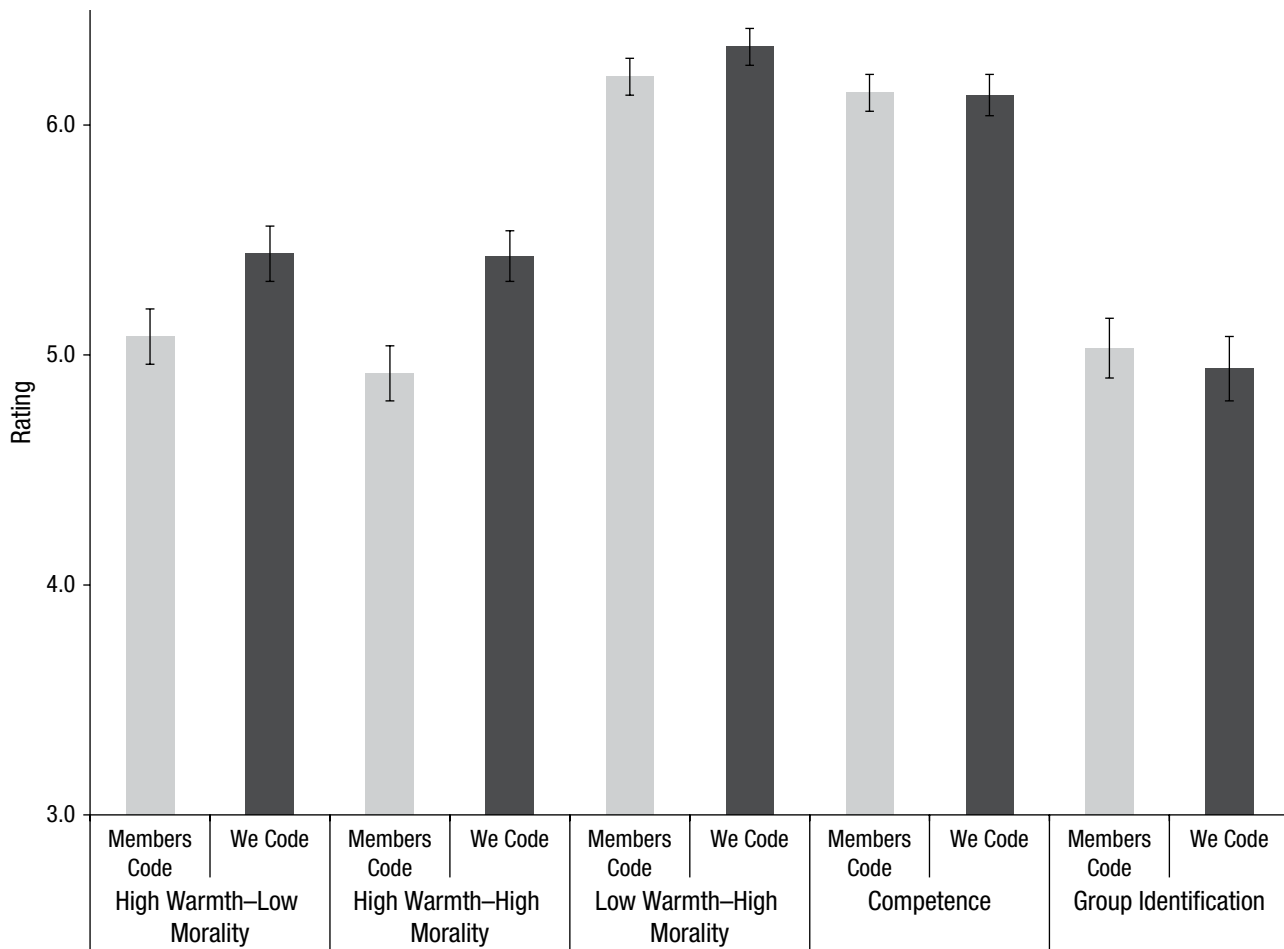
Fig. 5. Mean ratings of traits attributed to the group by participants in the two code conditions, separately for each of the five trait categories in Study 3a.

the screen in order to be paid. They were informed that if they successfully unscrambled the first six word jumbles but not the seventh, they would be paid only for the first six, even if they also successfully unscrambled the eighth word jumble.

Participants were given 10 word jumbles; however, the fifth word jumble was unsolvable. Given that solving the fifth one allowed participants to be paid for the rest of the word jumbles, they had an incentive to misreport their performance on this word jumble. We used participants' reported number of correctly solved word jumbles, starting from the fifth (unsolvable) word jumble, as the measure of cheating (a number between 0 and 6). Participants earned \$1 for every word; thus, with each additional word reported beyond the fourth word, participants earned more underserved money.

Results

Perceptions of the group's warmth. The language used affected perceptions of high warmth–low morality

(we code: $M = 5.44$, $SD = 1.23$, 95% CI = [5.21, 5.67]; members code: $M = 5.09$, $SD = 1.23$, 95% CI = [4.85, 5.32]), $t(215) = 2.13$, $p = .034$. On high-warmth–high-morality attributes, participants rated the group in the we-code condition higher ($M = 5.43$, $SD = 1.14$, 95% CI = [5.22, 5.64]) than the group in the members-code condition ($M = 4.92$, $SD = 1.28$, 95% CI = [4.67, 5.17]), $t(215) = 3.10$, $p = .002$ (see Fig. 5).

Perceptions of the group's morality and competence. Perceptions of moral character traits (i.e., low warmth–high morality; we code: $M = 6.34$, $SD = 0.70$, 95% CI = [6.21, 6.37]; members code: $M = 6.21$, $SD = 0.80$, 95% CI = [6.06, 6.37]), $t(215) = 1.27$, $p = .20$, and competence (we code: $M = 6.06$, $SD = 0.82$, 95% CI = [5.91, 6.22]; members code: $M = 6.06$, $SD = 0.79$, 95% CI = [5.91, 6.21]), $t(215) < 1$, were not significantly different.

Group identification. Group identification also did not differ significantly between conditions (we code: $M = 4.94$, $SD = 1.47$, 95% CI = [4.67, 5.22]; members code: $M = 5.03$, $SD = 1.37$, 95% CI = [4.76, 5.29]), $t(215) < 1$.

Cheating. We found that participants in the we-code condition reported solving a higher number of word jumbles after the fifth (unsolvable) one ($M = 4.13$, $SD = 1.94$) than did those in the members-code condition ($M = 3.44$, $SD = 2.38$), $t(215) = 2.34$, $p = .020$.

Mediation analyses. We first tested whether perceptions of high warmth–low morality mediated the relationship between our code-of-conduct conditions and observed dishonesty. On the basis of bootstrapping (with 10,000 iterations), we estimated the direct and indirect effects of condition via these perceptions on the dependent variable, cheating. In support of our prediction, the results suggested that perceptions of high warmth–low morality mediated the relationship between code condition and cheating (indirect effect: $b = 0.085$, $SE = 0.06$, 95% CI = [0.001, 0.240]). Similarly, perceptions of high warmth–high morality mediated the relationship between code condition and cheating (indirect effect: $b = 0.168$, $SE = 0.09$, 95% CI = [0.026, 0.376]). To test for the potential role of perceptions of moral character traits (low warmth–high morality), competence, and identification, we ran additional mediation analyses. None mediated the link between the code-of-conduct condition and observed dishonesty.

Discussion

In sum, this study provided direct evidence for the mediating role of perceptions of warmth (both high warmth–low morality and high warmth–high morality) but no evidence for group identification as a potential mediator. Even though we have examined the role of group identification in four studies, we did not find reliable differences in identification across conditions or any evidence of mediation. One reason for the lack of empirical support may be that, in our studies, we asked every participant to join a research team and to complete an onboarding process in which we varied the language used to refer to the group that the participants thought they would be joining. The effect of communal language might not have been strong enough to influence identification, as all participants felt equally identified with the group they joined. In real-world settings, though, when complemented by other forms of communication and behaviors, communal language may in fact result in higher levels of group or organizational identification. Future research could examine this possibility, as group identification may in fact influence the decision to act dishonestly, particularly within natural groups and organizations.

Study 3b

To comprehensively test whether perceptions of warmth contribute to dishonesty, we conducted a preregistered, high-powered study that provided participants with an opportunity to behave dishonestly by self-reporting performance to earn money. We measured perceptions of warmth as the result of the code-of-conduct condition and then tested whether perceptions of warmth predicted subsequent dishonest behavior.

Method

Participants and design. Three hundred one individuals (48.7% male; age: $M = 34.3$ years, $SD = 13.0$) at a university in the United States participated in this study for pay. Participants received $25 for completing an hour-long session and had an opportunity to earn an additional $5. This study had a two-conditions, between-subjects design (members code, we code). Before conducting the study, we preregistered it on the Open Science Framework (https://osf.io/69t4u). Thus, we ran laboratory sessions to achieve the predetermined number of participants (i.e., 300).

Procedure. Participants were randomly assigned to the two conditions. There were 151 in the we-code condition and 150 in the members-code condition. The code-of-conduct manipulation was similar to that in previous studies (see the Supplemental Material available online for the exact wording used).

Perceptions of the group. After the manipulation, participants responded to a number of questions about their group on a scale ranging from 1 (*not at all*) to 7 (*very much*). They were provided with the list of attributes from Studies 1a and 1b. Six of these traits were from the high-warmth–high-morality category (forgiving, helpful, kind, empathetic, cooperative, tolerant; $\alpha = .89$), and five were from the high-warmth–low-morality category (agreeable, warm, sociable, easygoing, playful; $\alpha = .88$). Four captured moral character traits (low-warmth–high-morality category; fair, principled, responsible, trustworthy; $\alpha = .87$) and six measured the competence category (competent, effective, efficient, innovative, organized, logical; $\alpha = .92$).

Dishonesty measure. Afterward, participants completed a matrix task similar to the one from Study 2c that allowed participants to self-report their performance and gave them an opportunity to overreport and be dishonest. Given that we previously found no difference in actual performance, we presented 10 solvable matrices to participants one by one. They were informed that they would receive $0.50 for each matrix they reported having solved.

Results

Perceptions of the group's warmth. Participants rated the group they were joining as higher on high-warmth–low-morality traits in the we-code condition ($M = 4.49$, $SD = 1.24$, 95% CI = [4.29, 4.69]) than in the members-code

condition (M = 4.10, SD = 1.43, 95% CI = [3.87, 4.33]), t(299) = 2.54, p = .012. Similarly, perceptions of high warmth–high morality differed between the code conditions (we code: M = 5.01, SD = 1.06, 95% CI = [4.84, 5.19]; members code: M = 4.60, SD = 1.28, 95% CI = [4.39, 4.81]), t(299) = 3.05, p = .002 (see Fig. 6).

Perceptions of the group's morality and competence. Once again, we found no significant differences in perceptions of moral character (we code: M = 5.67, SD = 1.04, 95% CI = [5.50, 5.84]; members code: M = 5.49, SD = 1.16, 95% CI = [5.30, 5.68], t(299) = 1.41, p = .160, or on perceptions of competence (we code: M = 5.49, SD = 1.07, 95% CI = [5.31, 5.66]; members code: M = 5.41, SD = 1.17, 95% CI = [5.23, 5.60]), t(299) < 1.

Cheating. When comparing cheating on the number of matrices reported as solved, we found no differences between conditions (we code: M = 5.99, SD = 2.79, 95% CI = [5.55, 6.44]; members code: M = 5.81, SD = 2.83, 95% CI = [5.36, 6.27]), t(299) = 0.56, p = .579.

Mediation analyses. Because the lack of a direct effect does not preclude an indirect effect (see MacKinnon, Krull, & Lockwood, 2000; Rucker, Preacher, Tormala, & Petty, 2011; Zhao, Lynch, & Chen, 2010), we next conducted mediation analyses to test whether each category of traits mediated the relationship between the language used in the code of conduct and cheating. The results, based on bootstrapping (with 10,000 iterations), supported our predictions, suggesting that perceptions of high warmth–low morality mediated the relationship between code condition and cheating (indirect effect: b = 0.098, SE = 0.06, 95% CI = [0.004, 0.242]). Similarly, perceptions of high warmth–high morality mediated the relationship between code condition and cheating (indirect effect: b = 0.114, SE = 0.07, 95% CI = [0.002, 0.278]).

However, we did not find evidence of mediation by perceptions of low warmth–high morality (indirect effect: b = 0.018, SE = 0.04, 95% CI = [–0.036, 0.116]) or by perceptions of competence (indirect effect: b = 0.008, SE = 0.03, 95% CI = [–0.034, 0.077]).

Thus, consistent with the findings of Study 3a and with our theorizing, Study 3b provided further evidence for the role of perceptions of warmth (both high warmth–low morality and high warmth–high morality) in explaining the relationship between personal, communal language and dishonest behavior. Perceptions of moral character (low warmth–high morality) and perceptions of competence, instead, did not mediate this effect.

Unlike in previous studies (Studies 2a, 2b, 2c, and 3a), we did not find a direct effect of our manipulation on dishonesty measure in the current study. Though emerging perspectives question the requirement that a total effect of *X* on *Y* needs to be present before assessing mediation (MacKinnon et al., 2000; Rucker et al., 2011; Zhao et al., 2010), we believe our measurement of mediator (perceptions) right before the dependent variable (cheating measure) may have contaminated responses to the dependent measure or drawn attention to the purpose of the study. Indeed, Sigall and Mills (1998) showed that sometimes the inclusion of a measure of mediator or a manipulation check can undermine the direct effect.

Study 4

So far, we relied on the language used in codes of conduct to infer the type of relationship perceivers presumably believe they have with the group they are joining. In Study 4, we aimed to provide direct evidence for the link between perceptions of warmth (specifically the perceptions of being forgiving, tolerant, agreeable, kind, and easygoing) and dishonesty by manipulating perceptions of warmth directly. We expected that perceptions of warmth (and not morality) would influence the expected magnitude of punishment and consequently increase the likelihood of misconduct.

Method

Participants and design. Two hundred individuals (52.5% male; age: M = 35.9 years, SD = 11.1) recruited through Mechanical Turk received $0.50 for completing this online study. The study had a two-conditions, between-subjects design (warm, neutral). Prior to conducting the study, we planned to stop data collection after recruiting about 100 participants for each condition. This study was preregistered on the Open Science Framework (https://osf.io/jdbcu).

Procedure. Participants were randomly assigned to two conditions (warm, neutral). There were 99 in the warm condition and 101 in the neutral condition. They were asked to imagine themselves in a described situation as if they were actually experiencing it. All participants read that a few months ago, they started a new job as an office assistant at a large company in Chicago, where they were responsible for answering the front desk phone and taking care of administrative tasks for a few of the office managers. They further read,

> After a week of job training, you feel confident and comfortable in your position at the office. Although it is not your dream job, the pay is good for an assistant position and you enjoy the people and culture at the company. You feel like you could see
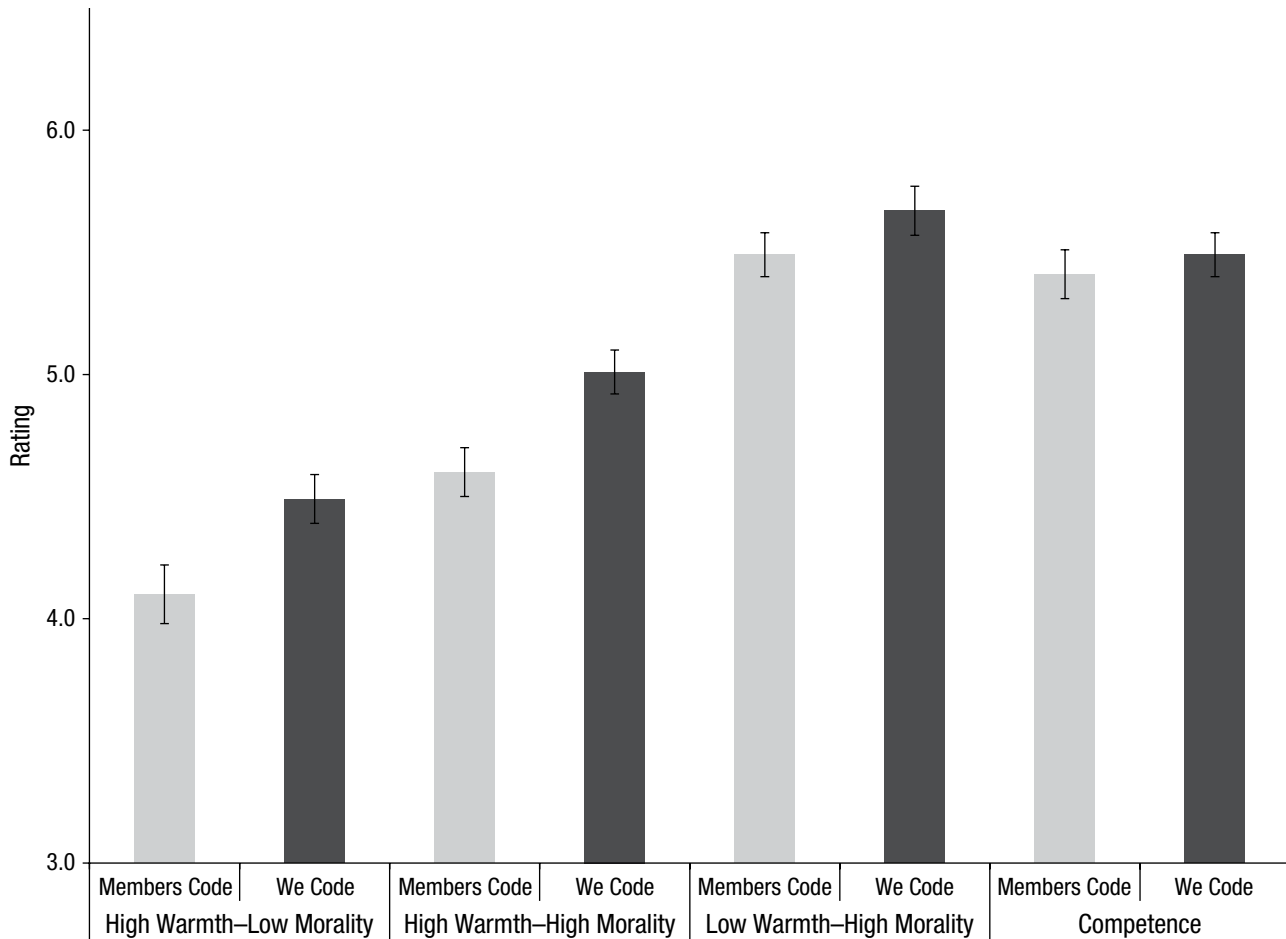
Fig. 6. Mean rating of traits attributed to the group by participants in the two code conditions, separately for each of the four trait categories in Study 3b.

yourself staying in this job for a while. Your workload is manageable; often, you find yourself with free time after your daily tasks are complete.

In the warmth condition, the participants then read some additional information intended to manipulate perceptions of warmth: "The office managers are very agreeable and easy-going. Everyone is kind to one another and playful around the office. Even when mistakes are made, the response is forgiving."[2]

After reading the scenario, participants in both conditions read a description of an unethical act and were asked to indicate the likelihood of them engaging in it. They read,

The company policy warns against personal internet use. Although the company does not restrict or monitor any websites to your knowledge, it is prohibited in the company code of conduct. You find yourself getting bored towards the end of each day. You have holiday shopping to get done and you figure you could online shop to save time. How likely are you to engage in this behavior?

They responded to this question on a scale from 1 (*extremely unlikely*) to 7 (*extremely likely*). At the end, as a manipulation check, we asked participants to rate the group on 21 traits, as in our previous studies (high warmth–high morality, $\alpha = .93$; high warmth–low morality, $\alpha = .92$; moral traits, $\alpha = .87$; competence, $\alpha = .92$).

Results

Participants in the warmth condition ($M = 3.88$, $SD = 1.96$, 95% CI = [3.50, 4.26]) were more likely to engage in the morally questionable behavior than those in the neutral condition ($M = 3.22$, $SD = 1.93$, 95% CI = [2.83, 3.61]), $t(198) = 2.40$, $p = .017$. The warmth condition was rated higher on high-warmth–low-morality traits ($M = 5.34$, $SD = 1.24$, 95% CI = [5.10, 5.59]) than the neutral condition ($M = 4.39$, $SD = 1.21$, 95% CI = [4.15,

4.63]), $t(198) = 5.53$, $p < .001$. Similarly, perceptions of high warmth–high morality differed between conditions (warmth: $M = 5.23$, $SD = 1.20$, 95% CI = [4.99, 5.46]; neutral: $M = 4.53$, $SD = 1.12$, 95% CI = [4.30, 4.75]), $t(198) = 4.24$, $p < .001$.

However, we found no significant differences between conditions on perceptions of moral character (warmth: $M = 5.33$, $SD = 1.06$, 95% CI = [5.12, 5.54]; neutral: $M = 5.23$, $SD = 1.08$, 95% CI = [5.01, 5.44]), $t(198) = 0.67$, $p = .501$, or competence (warmth: $M = 5.19$, $SD = 1.02$, 95% CI = [4.929, 5.39]; neutral: $M = 5.30$, $SD = 1.07$, 95% CI = [5.08, 5.51]), $t(198) = 0.70$, $p = .482$. These results confirm that we were able to successfully manipulate perceptions of the group's warmth and not its competence or moral character. The findings from this study provide direct evidence that exclusively manipulating warmth perceptions can increase morally questionable behaviors.

Study 5

Focusing on a large sample of companies, we aimed to examine the relationship between the language used ("we" vs. "employees") in codes of conduct and corporate illegality. Most firms' codes of conduct consist of a company vision and a letter from the CEO. Our conversations with ethics officers led us to believe that employees typically have access to the code of conduct and that it is the basis for additional forms of communication, such as ethics training. Thus, it is an important document that exemplifies the type of relationship that exists between the firm and its members, as well as the corporate culture.

Pilot study

On the basis of our conversations with ethics officers and employees across a few different organizations, we believe that most employees read their company's code of conduct at some point during their tenure, although to varying degrees. We conducted a survey to test this assumption by recruiting a large sample ($N = 1,916$) of full-time employees (55.6% male; age: $M = 36.9$ years, $SD = 10.7$) across the United States, who received $5 for completing the survey. Among the 1,916 full-time employees we recruited, 1,445 (75%) indicated that their organization had a written code of conduct. Among these 1,445 employees, 1% indicated that they did not know how employees could access the code of conduct or indicated that it was not available. Among the rest, 50.7% indicated that they had received a hard copy of the code of conduct, 48.1% indicated it was on the organization's internal server, 34.1% noted its availability online, and 31.1% reported that portions of the code were posted in public spaces in the company. As

for when they last read or reviewed their company's code of conduct, 57.4% reported that they read or reviewed the code within that last year, 22.6% reported reviewing it within the last 5 years, 10% reported they had not read or reviewed it when they were hired, 1.4% noted that they reviewed it more than 5 years ago, 6% said they did not recall, and only 2.4% said they had not read or reviewed it. Given these responses from a sample of working adults, we feel confident in our assumption that employees have access to their company's code of conduct and generally have been exposed to its language at some point during their employment. Thus, given the results of our previous studies, we expected the use of personal, communal (i.e., "we") versus impersonal (i.e., "group members") language in a company's code of conduct to affect employees' perceptions of their organization and their unethical behavior, operationalized in this study as corporate illegality.

Method

Our sample of companies consisted of all manufacturing firms that were part of the S&P 500 stock market index between 1990 and 2012. The S&P 500 provides financial information and analysis, and it is "one of the most commonly followed equity indices, and many consider it to be one of the best representations of the U.S. stock market" (https://en.wikipedia.org/wiki/S%26P_500_Index). Of the 210 firms in our sample, we could not readily identify a code of conduct for 22 firms and did not receive any e-mail response after querying investor relations. Thus, our S&P 500 manufacturing-firm sample consisted of 188 firms for which we had access to a copy of their code of conduct.

Independent variable. Two research assistants blind to our research questions and hypotheses coded each code of conduct on a dichotomous variable: 1 if the focal firm's code of conduct primarily used "we" language and 0 if it mainly used "member" or "employee" language. The research assistants were trained by one of the authors about the distinctions between the two categories. They categorized a sample of codes of conduct from *Fortune* magazine's Top 50 companies as practice and discussed the coding with one of the authors. For all of the firms in our sample, the research assistants read the code and then categorized it. There was very high agreement (> 87%) between the research assistants' ratings. Inconsistencies were resolved through discussion among the two research assistants and one of the authors.

Dependent variable. We closely followed the procedures of Mishina, Dykes, Block, and Pollock (2010), searching various media sources to identify any type of

corporate illegality, such as environmental violations, anticompetitive actions, false claims, and fraudulent actions. We created a dichotomous variable and coded it as 1 if we identified any incident of corporate illegality in a given year and 0 otherwise. Our search identified 873 incidents of corporate illegality with settlement or conviction between 1990 and 2012. No cases were excluded. Given that we used a dichotomous measure—that is, whether or not a target firm engaged in any incident in a given year—these 873 incidents correspond to 542 firm-year observations, coded as 1 in our data set.[3]

Control variables. Following Mishina et al. (2010), we controlled for firms' size and levels of slack resources, which may affect the propensity to engage in corporate illegality. In addition, we included year indicators to control for systematic differences in the incidence of corporate illegality. Firm size was operationalized as the natural

logarithm of number of employees annually. We controlled for three types of slack resources because firms with more slack resources have less need to pursue illegal activities, which pose the greatest risk to sustained performance. The first one, absorbed slack, was measured as the ratio of administrative expenses to sales; the second one, unabsorbed slack, was measured as the ratio of cash and marketable securities to liabilities; and the third, potential slack, was measured as the ratio of debt to equity. Finally, we included the total number of words in the code of conduct as another control variable to account for potential systematic differences and to provide a more conservative test of our hypothesis.

Results

Table 1 provides correlations and descriptive statistics for each of the variables in this study. Table 2 presents

Table 1. Correlations and Descriptive Statistics (Study 5)

Variable	*M*	*SD*	1	2	3	4	5	6	7	8	9	10	11	12
								Correlations						
1. Corporate illegality	.128	.334	—											
2. "We" code	.368	.482	.095	—										
3. 1990 dummy	.043	.204	.047	.007	—									
4. 1991 dummy	.043	.204	.042	.009	−.056	—								
5. 1992 dummy	.043	.204	.031	.006	−.056	−.056	—							
6. 1993 dummy	.043	.204	.036	.006	−.056	−.057	−.057	—						
7. 1994 dummy	.043	.204	.043	.008	−.057	−.057	−.057	−.057	—					
8. 1995 dummy	.043	.204	.042	.008	−.056	−.056	−.056	−.057	−.057	—				
9. 1996 dummy	.043	.204	.038	.009	−.056	−.056	−.056	−.057	−.057	−.056	—			
10. 1997 dummy	.043	.204	.036	.009	−.056	−.057	−.057	−.057	−.057	−.057	−.057	—		
11. 1998 dummy	.043	.204	.018	.011	−.056	−.056	−.056	−.057	−.057	−.056	−.056	−.057	—	
12. 1999 dummy	.043	.204	.022	.011	−.055	−.055	−.055	−.056	−.056	−.055	−.055	−.056	−.055	—
13. 2000 dummy	.043	.204	.009	.011	−.051	−.051	−.051	−.052	−.052	−.051	−.051	−.052	−.051	−.051
14. 2001 dummy	.043	.204	−.014	.007	−.049	−.049	−.049	−.050	−.050	−.049	−.049	−.050	−.049	−.049
15. 2002 dummy	.043	.204	−.019	−.011	−.048	−.048	−.048	−.048	−.049	−.048	−.048	−.048	−.048	−.047
16. 2003 dummy	.043	.204	−.013	−.005	−.047	−.047	−.047	−.048	−.048	−.048	−.047	−.048	−.048	−.047
17. 2004 dummy	.043	.204	−.017	−.007	−.047	−.047	−.047	−.048	−.048	−.047	−.047	−.048	−.047	−.047
18. 2005 dummy	.043	.204	−.020	−.008	−.047	−.047	−.047	−.047	−.047	−.047	−.047	−.047	−.047	−.046
19. 2006 dummy	.043	.204	−.026	−.009	−.046	−.046	−.046	−.046	−.046	−.046	−.046	−.046	−.046	−.045
20. 2007 dummy	.043	.204	−.023	−.011	−.045	−.045	−.045	−.045	−.046	−.045	−.045	−.045	−.045	−.044
21. 2008 dummy	.043	.204	−.044	−.012	−.044	−.044	−.044	−.044	−.044	−.044	−.044	−.044	−.044	−.043
22. 2009 dummy	.043	.204	−.061	−.010	−.043	−.043	−.043	−.043	−.043	−.043	−.043	−.043	−.043	−.042
23. 2010 dummy	.043	.204	−.070	−.019	−.042	−.042	−.042	−.043	−.043	−.043	−.042	−.043	−.043	−.042
24. 2011 dummy	.043	.204	−.074	−.015	−.042	−.042	−.042	−.042	−.042	−.042	−.042	−.042	−.042	−.041
25. 2012 dummy	.043	.204	−.078	−.016	−.041	−.041	−.041	−.042	−.042	−.041	−.041	−.042	−.041	−.041
26. Log number of employees	3.146	1.010	.252	.112	−.009	−.010	−.013	−.016	−.018	−.022	−.020	−.014	−.015	−.004
27. Absorbed slack	0.230	0.140	−.073	.159	−.037	−.028	−.015	−.020	−.020	−.024	−.024	−.020	−.009	.000
28. Unabsorbed slack	0.324	0.568	−.053	.050	−.045	−.045	−.039	−.048	−.048	−.076	−.073	−.068	−.074	−.078
29. Potential slack	−0.126	35.35	.009	.015	.002	.003	.000	.004	.004	.005	.002	.003	.007	.003
30. Total number of words	8,277.827	5,280.827	.092	.310	.013	.006	.010	.008	.012	.012	.003	.004	.008	.016

(continued)

Table 1. *(continued)*

Variable	Correlations																
	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29
1. Corporate illegality																	
2. "We" code																	
3. 1990 dummy																	
4. 1991 dummy																	
5. 1992 dummy																	
6. 1993 dummy																	
7. 1994 dummy																	
8. 1995 dummy																	
9. 1996 dummy																	
10. 1997 dummy																	
11. 1998 dummy																	
12. 1999 dummy																	
13. 2000 dummy	—																
14. 2001 dummy	−.045	—															
15. 2002 dummy	−.044	−.042	—														
16. 2003 dummy	−.043	−.042	−.041	—													
17. 2004 dummy	−.043	−.042	−.040	−.040	—												
18. 2005 dummy	−.043	−.041	−.040	−.040	−.040	—											
19. 2006 dummy	−.042	−.040	−.039	−.039	−.039	−.038	—										
20. 2007 dummy	−.041	−.040	−.038	−.038	−.038	−.038	−.037	—									
21. 2008 dummy	−.040	−.039	−.037	−.037	−.037	−.037	−.036	−.035	—								
22. 2009 dummy	−.039	−.038	−.037	−.036	−.036	−.036	−.035	−.034	−.034	—							
23. 2010 dummy	−.039	−.037	−.036	−.036	−.036	−.036	−.035	−.034	−.033	−.033	—						
24. 2011 dummy	−.038	−.037	−.036	−.035	−.035	−.035	−.034	−.034	−.033	−.032	−.032	—					
25. 2012 dummy	−.038	−.036	−.035	−.035	−.035	−.035	−.034	−.033	−.032	−.032	−.031	−.031	—				
26. Log number of employees	−.001	−.001	.010	−.008	−.003	−.005	.005	.016	.030	.036	.032	.036	.033	—			
27. Absorbed slack	.016	.010	.021	.017	.024	.015	.016	.021	.024	.021	.029	.014	.010	−.136	—		
28. Unabsorbed slack	−.019	.007	.015	.043	.058	.089	.083	.061	.044	.043	.119	.105	.093	−.226	.241	—	
29. Potential slack	.005	.007	.011	−.001	−.029	.020	.003	.002	.031	−.035	−.019	−.041	.000	.000	.011	−.007	—
30. Total number of words	.002	−.008	−.008	−.008	−.004	−.004	−.012	−.012	−.008	−.005	−.017	−.014	−.016	.111	.198	.030	.003

the results of our analyses predicting corporate illegality. We predicted that the "we" version (coded as 1), relative to the "member" or "employee" version (coded as 0), would be positively related to a firm's propensity to engage in corporate illegality. In fact, the code-of-conduct variable was positively related to corporate illegality. Even after controlling for the number of words and other variables, we found a firm's code of conduct to be positively related to its propensity to engage in corporate illegality.

General Discussion

Our study joins an emerging literature that has begun to explore factors responsible for curbing and reducing unethicality in organizations and society more broadly. Research in behavioral ethics has suggested revisiting and revising traditional enforcement instruments, such as codes of conduct and corporate culture, to see whether they can be improved to better address the dishonesty of "good" people. Using multiple methods and settings, we have shown that perceptions of group warmth through subtle changes in the language used in group communications ("we" vs. "employees") can have a sizable impact on individuals' ethical behavior. Thus, in contrast to the current trend of looking for nudges that might enforce ethical behavior (e.g., priming the Ten Commandments), our study shows that placing an emphasis on enforcement could go a long way toward improving ethical conduct.

Our research contributes to the literature on moral psychology and ethics in important ways. First, we

Table 2. Multiple Estimates of Logistic Regression Models Predicting Corporate Illegality (Study 5)

Predictor	Model 1		Model 2		Model 3	
	Estimate	SE	Estimate	SE	Estimate	SE
Intercept	0.212***	0.026	−0.093*	0.037	−0.107**	0.038
"We" code	0.052***	0.011	0.043**	0.013	0.034*	0.014
1991 dummy	−0.009	0.036	−0.007	0.039	−0.006	0.039
1992 dummy	−0.025	0.036	−0.024	0.039	−0.023	0.039
1993 dummy	−0.017	0.036	−0.013	0.039	−0.013	0.039
1994 dummy	−0.010	0.036	−0.002	0.039	−0.002	0.039
1995 dummy	−0.010	0.036	0.002	0.039	0.002	0.039
1996 dummy	−0.018	0.036	−0.004	0.039	−0.003	0.039
1997 dummy	−0.023	0.035	−0.012	0.039	−0.011	0.039
1998 dummy	−0.051	0.035	−0.038	0.039	−0.037	0.039
1999 dummy	−0.043	0.035	−0.035	0.039	−0.035	0.039
2000 dummy	−0.084*	0.036	−0.063	0.041	−0.061	0.041
2001 dummy	−0.123***	0.036	−0.107*	0.041	−0.104*	0.042
2002 dummy	−0.138***	0.036	−0.120**	0.042	−0.117**	0.042
2003 dummy	−0.131***	0.036	−0.105*	0.043	−0.103*	0.043
2004 dummy	−0.136***	0.036	−0.116**	0.043	−0.114**	0.043
2005 dummy	−0.135***	0.036	−0.127**	0.043	−0.126**	0.043
2006 dummy	−0.153***	0.036	−0.144***	0.044	−0.141**	0.044
2007 dummy	−0.153***	0.036	−0.142**	0.044	−0.139**	0.044
2008 dummy	−0.182***	0.036	−0.190***	0.045	−0.189***	0.046
2009 dummy	−0.200***	0.036	−0.231***	0.045	−0.229***	0.045
2010 dummy	−0.218***	0.036	−0.261***	0.046	−0.257***	0.046
2011 dummy	−0.224***	0.036	−0.273***	0.046	−0.270***	0.046
2012 dummy	−0.230***	0.036	−0.281***	0.046	−0.278***	0.046
Log number of employees			0.106***	0.007	0.104***	0.007
Absorbed slack			−0.123*	0.048	−0.144**	0.049
Unabsorbed slack			0.056***	0.013	0.055***	0.013
Potential slack			0.000	0.000	0.000	0.000
Total number of words					0.000*	0.000

Note: Estimates are unstandardized.
*$p < .05$. **$p < .01$. ***$p < .001$.

demonstrated a direct link between perceptions of warmth and dishonesty. Relatively few studies have examined the perceptions of warmth and competence of a recipient or evaluators in moral decision making. Our article highlights an important challenge that groups face: being perceived as warm and accepting while still signaling to members that any misbehavior will have consequences. Second, instead of focusing on increasing the salience of morality (the adoption of codes of conduct or triggering morality thoughts more generally), we focused on the perceptions of enforcement (the content of these codes of conduct or the emphasis on strong enforcement) to better understand what type of language is most effective at deterring unethical behavior.

Third, this article also adds insight to the broader debate in enforcement theory of whether to address people's moral or extrinsic motivations. The classic argument against the stricter approach that focuses on punishment as its main tool of enforcement is that it crowds out

people's intrinsic motivations and hence reduces the quality of their moral behavior (Bohnet, Frey, & Huck, 2001; Gneezy & Rustichini, 2000). Our focus on language effects provides an important new way to think about how to optimize the use of sanctions. We have shown that language that sends subtle cues to employees that an organization is serious about enforcing ethical behavior might avoid some of the negative consequences of the rigid use of sanctions discussed by scholars. In other words, our findings suggest that subtle cues communicating a group or community culture, beyond enforcement practices and the size of sanctions, could undermine the ethicality of people's decisions. This effect should be considered in theoretical and policy discussions of ethical decision making in the workplace.

The conclusions drawn from our results should be considered in light of the limitations of these studies. Even though we used multiple methods and diverse sample populations, our study designs

potentially limit the generalizability of the findings. The use of controlled experiments helped us establish causality and illuminate the psychological mechanisms explaining our proposed effects but did not allow us to explore the role of alternative mechanisms as closely. For instance, in our studies, we examined the role of group identification but did not manipulate it explicitly. Future research could test the relationships we proposed using other methodological approaches and strategies to promote identification. The effect of language on identification and how identification influences dishonest behavior remain unclear. Additionally, in our experimental studies, we used the same manipulation of personal versus impersonal language. This potentially limits the contribution of our work to the literature. Future research could extend our findings by using other manipulations of the type of language used not only in codes of conduct but also in other forms of communication with people who are about to join groups or organizations.

Another limitation of our studies may be the lack of explicit expectations to be caught or punished. Most of our studies, except Study 2b, used paradigms similar to those of previous psychological research (Mazar et al., 2008): Participants completed tasks anonymously without providing any identifiable personal information and thus had no explicit expectations for punishment that could be enforced. The extent to which the effects we found hold under explicit punishment expectations needs further investigation. In fact, research has shown that cheating is affected when all concerns about being exposed as a cheater are eliminated (Kajackaite & Gneezy, 2017). This could have important implications for the ecological validity of our findings and should be further examined. Finally, we tested our predictions across many studies and contexts, and the effect of the language manipulation we used was rather weak. This can be attributed to our subtle manipulations. Though our effects were not very strong, we believe them to be meaningful and of practical importance. In fact, as we found in our field experiment, differences in the language used in codes of conduct changed individuals' behavior. Future research could investigate whether stronger language manipulations would lead to stronger results, thus advancing our understanding of the potential perils of communal language.

Action Editor

Ayse K. Uskul served as action editor for this article.

Author Contributions

M. Kouchaki, F. Gino, and Y. Feldman developed the research idea and designed the studies. M. Kouchaki and F. Gino conducted the studies and analyzed the data. M. Kouchaki

drafted the manuscript, and the other two authors provided critical revisions. All authors approved the final version of the manuscript for submission.

ORCID iD

Maryam Kouchaki https://orcid.org/0000-0003-1292-5134

Supplemental Material

Additional supporting information can be found at http://journals.sagepub.com/doi/suppl/10.1177/0956797619882917

Open Practices

Data and materials for the present studies have not been made publicly available. The design and analysis plans for the following studies were preregistered on the Open Science Framework: Study 1b (https://osf.io/epsva), Study 3b (https://osf.io/69t4u), and Study 4 (https://osf.io/jdbcu). The complete Open Practices Disclosure for this article can be found at http://journals.sagepub.com/doi/suppl/10.1177/0956797619882917. This article has received the badge for Preregistration. More information about the Open Practices badges can be found at http://www.psychologicalscience.org/publications/badges.

Notes

1. Although, to our knowledge, no research directly links warmth perceptions of a decision maker or group to rule breaking and other unethical behavior, some research has explored related questions. The most similar study is one by Azevedo, Panasiti, Maglio, and Aglioti (2018), who manipulated the perceptions of warmth and competence of a recipient in an anonymous dictator game that offered participants opportunities to lie. In this game, participants had total control over both their own and the recipient's payoff. Participants were told that they were playing against four opponents, each representing one of four categories and described in a different way: high warmth–high competence, high warmth–low competence, low warmth–low competence, or low warmth–high competence. For instance, the low warmth–low competence opponent was described as a low-class Eastern European migrant, whereas the high warmth–high competence opponent was a friendly music student. The results showed independent effects for warmth and competence; specifically, individuals were less likely to deceive opponents seen as warm and more likely to lie to highly competent

ones. Thus, participants engaged in different levels of deception on the basis of the perceived characteristics of the person with whom they were interacting.

2. Before conducting the study, we ran a pilot study to make sure individuals viewed the two groups differently. After presenting the description, we asked participants to rate the group with a list of 21 traits from our previous studies (high warmth–high morality, α = .92; high warmth–low morality, α = .90; moral traits, α = .87; competence, α = .92). As we expected, the warmth condition was rated higher on high-warmth–low-morality traits ($M = 5.77$, $SD = 1.02$) than the neutral condition ($M = 5.37$, $SD = 1.04$), $t(198) = 2.75$, $p = .006$. Similarly, perceptions of high warmth–high morality differed between conditions (warmth: $M = 5.72$, $SD = 0.93$; neutral: $M = 5.37$, $SD = 1.00$), $t(198) = 2.52$, $p = .013$. We found no significant differences between conditions on perceptions of moral character (warmth: $M = 5.38$, $SD = 0.95$; neutral: $M = 5.52$, $SD = 1.00$), $t(198) = 0.96$, $p = .337$, and perceptions of competence (warmth: $M = 5.21$, $SD = 0.94$; neutral: $M = 5.36$, $SD = 1.06$), $t(198) = 1.03$, $p = .304$. The findings from this pilot study confirm that we were able to successfully manipulate perceptions of the group's warmth and not its competence or moral character.

3. To assess the robustness of our results, we also created a measure of corporate illegality with the sum of the number of violations every year in each of the categories coded (environmental violations, anticompetitive actions, false claims, and fraudulent actions). This variable reflects the total number of illegal incidents per year. We conducted additional analyses, the results of which are available in the Supplemental Material. The results are similar to the findings reported here.

References

3M. (2016). *Be 3M: Code of conduct global handbook*. Retrieved from http://multimedia.3m.com/mws/media/882686O/code-of-conduct-handbook.pdf

Aaker, J., Vohs, K. D., & Mogilner, C. (2010). Nonprofits are seen as warm and for-profits as competent: Firm stereotypes matter. *Journal of Consumer Research*, *37*, 224–237.

Azevedo, R. T., Panasiti, M. S., Maglio, R., & Aglioti, S. M. (2018). Perceived warmth and competence of others shape voluntary deceptive behaviour in a morally relevant setting. *British Journal of Psychology*, *109*, 25–44.

Becker, G. S. (1968). Crime and punishment: An economic approach. In R. Witt, A. Clarke, & N. Fielding (Eds.), *The economic dimensions of crime* (pp. 13–68). London, England: Palgrave Macmillan.

Bernhard, H., Fehr, E., & Fischbacher, U. (2006). Group affiliation and altruistic norm enforcement. *American Economic Review*, *96*, 217–221.

Bohnet, I., Frey, B. S., & Huck, S. (2001). More order with less law: On contract enforcement, trust, and crowding. *American Political Science Review*, *95*, 131–144.

Clark, M. S. (1984). Record keeping in two types of relationships. *Journal of Personality and Social Psychology*, *47*, 549–557.

Clark, M. S., & Mills, J. (1979). Interpersonal attraction in exchange and communal relationships. *Journal of Personality and Social Psychology*, *37*, 12–24.

Clark, M. S., Mills, J., & Powell, M. C. (1986). Keeping track of needs in communal and exchange relationships. *Journal of Personality and Social Psychology*, *51*, 333–338.

Clark, M. S., Ouellette, R., Powell, M. C., & Milberg, S. (1987). Recipient's mood, relationship type, and helping. *Journal of Personality and Social Psychology*, *53*, 94–103.

Doosje, B., Ellemers, N., & Spears, R. (1995). Perceived intragroup variability as a function of group status and identification. *Journal of Experimental Social Psychology*, *31*, 410–436.

Feldman, Y. (2018). *The law of good people: Challenging states' ability to regulate human behavior*. Cambridge, England: Cambridge University Press.

Fiske, S. T., Cuddy, A. J., Glick, P., & Xu, J. (2002). A model of (often mixed) stereotype content: Competence and warmth respectively follow from perceived status and competition. *Journal of Personality and Social Psychology*, *82*, 878–902.

Gächter, S., Renner, E., & Sefton, M. (2008). The long-run benefits of punishment. *Science*, *322*, 1510.

Gneezy, U., & Rustichini, A. (2000). A fine is a price. *The Journal of Legal Studies*, *29*, 1–17.

Goodwin, G. P., Piazza, J., & Rozin, P. (2014). Moral character predominates in person perception and evaluation. *Journal of Personality and Social Psychology*, *106*, 148–168.

Hildreth, J. A. D., Gino, F., & Bazerman, M. (2016). Blind loyalty? When group loyalty makes us see evil or engage in it. *Organizational Behavior and Human Decision Processes*, *132*, 16–36.

Kajackaite, A., & Gneezy, U. (2017). Incentives and cheating. *Games and Economic Behavior*, *102*, 433–444.

Kish-Gephart, J. J., Harrison, D. A., & Treviño, L. K. (2010). Bad apples, bad cases, and bad barrels: Meta-analytic evidence about sources of unethical decisions at work. *Journal of Applied Psychology*, *95*(1), 1–31.

Kouchaki, M., & Gino, F. (2016). Memories of unethical actions become obfuscated over time. *Proceedings of the National Academy of Sciences, USA*, *113*, 6166–6171.

Kouchaki, M., & Smith, I. H. (2014). The morning morality effect: The influence of time of day on unethical behavior. *Psychological Science*, *25*, 95–102.

MacKinnon, D. P., Krull, J. L., & Lockwood, C. M. (2000). Equivalence of the mediation, confounding and suppression effect. *Prevention Science*, *1*, 173–181.

Mazar, N., Amir, O., & Ariely, D. (2008). The dishonesty of honest people: A theory of self-concept maintenance. *Journal of Marketing Research*, *45*, 633–644.

Mills, J., & Clark, M. S. (1982). Exchange and communal relationships. *Review of Personality and Social Psychology*, *3*, 121–144.

Mishina, Y., Dykes, B. J., Block, E. S., & Pollock, T. G. (2010). Why "good" firms do bad things: The effects of high aspirations, high expectations, and prominence on the incidence of corporate illegality. *Academy of Management Journal*, *53*, 701–722.

Paine, L., Deshpande, R., Margolis, J. D., & Bettcher, K. E. (2005). Up to code: Does your company's conduct meet world-class standards? *Harvard Business Review*, *83*, 122–133.

Rucker, D. D., Preacher, K. J., Tormala, Z. L., & Petty, R. E. (2011). Mediation analysis in social psychology: Current practices and new recommendations. *Social and Personality Psychology Compass*, *5*, 359–371.

Sánchez-Pagés, S., & Vorsatz, M. (2007). An experimental study of truth-telling in a sender–receiver game. *Games and Economic Behavior*, *61*, 86–112.

Shariff, A. F., & Norenzayan, A. (2011). Mean gods make good people: Different views of God predict cheating behavior. *The International Journal for the Psychology of Religion*, *21*, 85–96.

Sigall, H., & Mills, J. (1998). Measures of independent variables and mediators are useful in social psychology experiments: But are they necessary? *Personality and Social Psychology Review*, *2*, 218–226.

Simmons, J. P., Nelson, L. D., & Simonsohn, U. (2013, January). *Life after P-hacking*. Paper presented at the Meeting of the Society for Personality and Social Psychology, New Orleans, LA. Retrieved from https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2205186

Umphress, E. E., Bingham, J. B., & Mitchell, M. S. (2010). Unethical behavior in the name of the company: The moderating effect of organizational identification and positive reciprocity beliefs on unethical pro-organizational behavior. *Journal of Applied Psychology*, *95*, 769–780.

Weick, K. E. (1979). Cognitive processes in organizations. In B. Staw (Ed.) *Research in Organizational Behavior* (Vol. 1, pp. 41–74). Amsterdam, The Netherlands: Elsevier.

Wells Fargo. (n.d.). *Our code of ethics and business conduct: Living our vision, values & goals*. Retrieved from https://www08.wellsfargomedia.com/assets/pdf/about/corporate/code-of-ethics.pdf

Zhao, X., Lynch, J. G., Jr., & Chen, Q. (2010). Reconsidering Baron and Kenny: Myths and truths about mediation analysis. *Journal of Consumer Research*, *37*, 197–206.