



How, when, and why recipients and observers reward good deeds and punish bad deeds



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ABSTRACT

The strength of organizational norms often depends on consistent reciprocity, i.e., regular and expected rewards for good behavior and punishments for bad behavior. Varying reactions by direct recipients and third-party observers, however, present the potential for unmet expectations and organizational inconsistency. This paper suggests that these kinds of problems are not only common but predictable. To do so, we present and test a theoretical model of reward and punishment behaviors. Three experiments show that, as predicted, observers consistently punished more than direct recipients did and that direct recipients rewarded more than observers did. Experiments 2 and 3 provided additional insights, showing that observers felt a stronger obligation to punish but a weaker obligation to reward than recipients did. These markedly different approaches to rewards and punishments, and the inconsistencies that they produce, provide the basis for a variety of important organizational implications.

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Introduction

Thoughtful actors have a natural tendency to repeat behaviors that are rewarded and avoid behaviors that are punished. For example, children learn what's right, what's wrong, and what's appropriate when they experience a consistent pattern of encouragement, rewards, admonitions, and punishments (Kohlberg, 1963). Within organizations, elaborate incentive systems increase and direct employee motivation (Lawler & Porter, 1963). In essence, rewards for good deeds and punishments for bad deeds create environments that stimulate desirable behavior and deter undesirable behavior (Chen, 2012; Fuster & Meier, 2010; Podsakoff, Bommer, Podsakoff, & MacKenzie, 2006; Wayne, Shore, Bommer, & Tetrick, 2002). Clearly, however, some good deeds go unrewarded and some bad deeds go unpunished. This raises important questions about the contexts and conditions that lead to rewarding and punishing behavior.

The current research addresses these issues by focusing on the individuals who deliver rewards and punishments. More

specifically, we investigated how, when, and why both direct recipients and third-party observers of good and bad deeds chose to engage in rewarding and punishing behavior. *Direct recipients* personally experience the effects of good or bad deeds and, as a result, are likely to be motivated to respond. The constant presence of *third-party observers* in organizational settings (e.g., managers, supervisors, coworkers, and subordinates) also makes them important rewarders and punishers. Data also suggest that observers can play a critical role in the development and maintenance of norms of reciprocity (e.g., Fehr & Fischbacher, 2004a, 2004b; Rand & Nowak, 2013). To address these issues, we present and test a model which suggests that direct recipients and observers respond to good and bad deeds in markedly different ways.

Direct and indirect reciprocity

As Gouldner's (1960) seminal article noted, reciprocity is a universal norm with ancient roots, ranging from Marcus Cicero's observation "There is no duty more indispensable than that of returning a kindness", to the Old Testament's "eye for an eye, tooth for a tooth." The impulse to return a favor or punish harm continues to operate in modern society and modern organizations, from individuals returning kindnesses or revenging slights, to observers awarding bonuses, creating probation systems, engaging in

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organizational citizenship behavior (Organ, Podsakoff, & MacKenzie, 2006), and becoming whistleblowers (Bies & Tripp, 1996). Moreover, although rational choice models (Selten, 1965) suggest that the costs of rewarding good behavior and punishing bad behavior will reduce these acts of reciprocity, research findings indicate that reciprocity is extremely common, even in one-time interactions with strangers (Wang, Galinsky, & Murnighan, 2009; Wang, Sivanathan, et al., 2011).

An extensive literature focuses on direct and indirect reciprocity. Direct reciprocity involves the sequential action of two individuals: *A* helps or hurts *B* and *B* repays *A* in kind. Indirect reciprocity (also called generalized reciprocity or generalized exchange) includes an additional actor—a third-party observer (*C*).¹ Third parties are not directly involved in the initial interaction; instead, after observing it, they can choose to reward good behavior or punish bad behavior (e.g., *A* helps or hurts *B* and *C* responds by helping or hurting *A*; Rand & Nowak, 2013). Direct recipients and third-party observers both play important roles as reinforcers. As a result, our research investigates their positive and their negative reactions.

Research on direct reciprocity is considerable (Abbink, Irlenbusch, & Renner, 2000; Offerman, 2002; Wang & Leung, 2010; Wang, Leung, See, & Gao, 2011). In the Ultimatum Game, for instance, a proposer can offer any portion of their endowment to a responder, who then decides to accept or reject the offer. Studies have consistently found that, even in these one-shot contexts, responders tend to reject offers that are less than 30% of the endowment – an example of negative reciprocity – even though doing so results in lost payoffs for both parties (for a review, see Camerer, 2003). Positive reciprocity (e.g., rewarding) also occurs in one-shot interactions (for a review, see Gintis, Bowles, Boyd, & Fehr, 2005). In one-shot Trust Games, for example, people who receive money from anonymous counterparts typically send substantial amounts of money back to their senders, even though they can maximize their own outcomes by returning nothing (Berg, Dickhaut, & McCabe, 1995). Finally, in non-monetary contexts, e.g., a classic study by Lewin, Lippitt, and White (1939), abusive action by authority figures often stimulated negative reactions from their student groups. More generally, organizational behavior is filled with supervisors and subordinates directly reciprocating each other's positive and negative actions, both explicitly and discreetly (Gerstner & Day, 1997; Sparrowe & Liden, 1997).

Research on indirect reciprocity also shows that third-party observers are important contributors to organizational norms. Although they might easily ignore what they have seen, data consistently show that observers punish people who mistreat others, even at a personal cost (Fehr & Gächter, 2000; Kahneman, Knetsch, & Thaler, 1986; Rupp & Bell, 2010; Wang, Sivanathan, et al., 2011). Experimental participants who observed their partners being insulted, for instance, risked direct confrontation and sacrificed material benefits to retaliate against insulters (Meindl & Lerner, 1983). Organizational observers also tend to help people who have reputations for generosity and withhold help from people who have reputations for stinginess (Baker & Bulkley, 2014). Similarly, interactions among employee peers often involve positive and negative reciprocity, directly and indirectly (e.g., Baker, 2012). Journalists and analysts who portrayed organizations negatively, for instance, risked reprisal from CEOs and top management (Westphal & Clement, 2008; Westphal & Deephouse, 2011) and, in downsized organizations, employees who retained their jobs still

displayed substantive negative reactions toward their organization (Brockner, 1994; Brockner, DeWitt, Grover, & Reed, 1990).

On the positive side, industrial product designers routinely help each other solve design problems (Hargadon & Sutton, 1997), and fair treatment by managers can enhance team performance and individually motivated prosocial behavior (Qiu, Qualls, Bohlmann, & Rupp, 2009). Nowak and Sigmund (2005) also noted that positive reciprocity by observers is important: when reciprocal acts signal that an organization is fair to all of its employees, organizational commitment and citizenship behaviors increase (Cameraman, Cropanzano, & Vandenberghe, 2007; Masterson, Lewis, Goldman, & Taylor, 2000; Naumann & Bennett, 2000; Niehoff & Moorman, 1993), individually and within groups (Liao & Rupp, 2005; Simons & Roberson, 2003).

Punishing bad deeds

Direct recipients tend to have stronger emotional reactions to harm than observers do. Lind, Kray, and Thompson (1998), for instance, found that individuals rated a mild personal injustice as more unfair than another person's severe injustice, suggesting that direct recipients respond more to harm than observers do. This should lead them to be strong sources of punishments (e.g., Sheppard, Lewicki, & Minton, 1992; Tyler & Smith, 1998; Walster, Walster, & Berscheid, 1978). These emotional responses are conveniently consistent with a sense of moral duty to punish wrongdoers (Cropanzano, Goldman, & Folger, 2003; Folger, 1998; Folger & Cropanzano, 2001). For example, Wang and Leung (2010) found that East Asians felt a greater sense of obligation to punish wrongdoers than Americans did and therefore punished more. These findings are consistent with deontic justice – i.e., justice for the sake of justice – which is an important driver of individuals' senses of duty and moral obligation (Cropanzano et al., 2003; Folger, 1998; Folger & Cropanzano, 2001). Indeed, injustice can induce a deontic state that drives desires to punish wrongdoers (Folger, 2001). This discussion suggests that recipients may engage in stronger negative reciprocity than observers, due to both stronger visceral reactions and to social norms.

There are some reasons, however, to temper this expectation, particularly because countervailing social norms, e.g., “doing no harm” and “taking the high road,” can impede recipients' triggers to punish (Bandura, 1991; Osofsky, Bandura, & Zimbardo, 2005). These norms also provide personal benefits to the recipients of harm, for direct recipients who engage in costly punishment often suffer reduced outcomes (Dreber, Rand, Fudenberg, & Nowak, 2008). Indeed, retaliating directly risks additional negative reciprocity and an increasing cycle of vengeance, one of the most common causes of violence in primitive societies, organized crime and gangs, and geopolitical conflict (Davie, 1929; Nisbett & Cohen, 1996; Otterbein, 1970; Turney-High, 1971; Wright, 1965). Thus, direct punishment carries the risk of destabilizing a relationship, as well as stimulating additional negative effects. This may be why many people find avoidance psychologically easier than confrontation (Wang & Leung, 2010; Wang et al., 2009). Although the victims of bad behavior may experience strong emotions, whether to punish, and how much, can still be difficult decisions.

In contrast, observers of bad deeds experience considerably less conflict, and their reactions can be a critical deterrent of subsequent bad behavior. In fact, observers' punishments can effectively enhance cooperation in groups (Rockenbach & Milinski, 2006). Employees carefully observe their supervisor's treatment of other employees, for instance, to determine the fairness of their organization, even when the supervisor's actions do not directly affect them (Kray & Lind, 2002). Similarly, when the outcome of a trial contradicted individuals' moral beliefs, they were more likely to

¹ Although another form of indirect reciprocity exists, in which *A* helps *B* and *B* pays it forward by helping *C*, we do not examine this process here (see Baker & Bulkley, 2014; Nowak & Sigmund, 2005).

steal from unrelated individuals (Mullen & Nadler, 2008). Thus, the perception of unpunished negative acts can contribute to the emergence of even more negative behavior.

Unlike direct reciprocity, third-party punishments also tend to result in less retaliation and, therefore, less risk of stimulating a cycle of violence. Aquino, Tripp, and Bies (2006), for example, found that employees who had been hurt by another employee retaliated directly if they felt that their organization's procedural justice system was weak but they retaliated less when they believed that their justice system was strong. Negative acts of reciprocity can also reverberate through an organization, encouraging supervisors to feel a stronger sense of obligation to stop what might otherwise become increasingly damaging processes (Fukuyama, 1995; Miller & Kanazawa, 2000). Data also indicate that people care about procedural fairness because it signals social value and respect (e.g., Tyler & Blader, 2003), and communicates a group's continued regard for victims, increasing the likelihood that victims will continue to identify with the group (Okimoto & Wenzel, 2011). Thus, the norms that limit recipients' reciprocity of wrongdoing do not seem to apply as strongly to observers: observers' punishments of bad deeds are not just acceptable, they may be necessary for maintaining social order.

Witnesses of injustice, for instance, display a heightened desire for perpetrators to be held accountable (Folger, 2001), feelings that lead to a sense of duty and obligation to help victims (Cropanzano et al., 2003; Folger, 1998; Folger & Cropanzano, 2001). Research has also shown that observers are willing to sacrifice financial gain to punish a potential perpetrator even when they did not know the intended victim and had nothing to gain personally from the punishment (Turillo, Folger, Lavelle, Umphress, & Gee, 2002). More globally, when terrorist attacks have threatened the idea of a harmonious society, the Federal Judiciary's punishments of guilty defendants have intensified (Stein, Steinley, & Cropanzano, 2011).

Overall, direct recipients may face conflicting motivations, first to retaliate directly against those who harmed them, but also to conform to norms that discourage directly responding to malefactors. Conversely, observers may be driven by norms that encourage deontic responses. Thus, we predict that:

Punishment Hypothesis 1 (H1p): Third-party observers punish negative acts more than direct recipients do.

Punishment Hypothesis 2 (H2p): Third-party observers feel more obligated to punish negative acts than direct recipients do, which translates into stronger punishments.

Rewarding good deeds

Although directly punishing harm-doers has the potential to create intrapersonal conflict and interpersonal retaliation, returning a favor or rewarding a good deed is much easier. The direct recipients of a good deed face a simpler situation in which returning a favor is both normatively prescribed (Blau, 1963; Gouldner, 1960; Greenberg, 1980) and enjoyable (Andreoni, 1990). Similarly, approaching someone to directly reward them tends to be attractive rather than aversive (Eisenberger, Lynch, Aselage, & Rohdieck, 2004). Reciprocating a good deed also has other positive effects: it signals trust (Pillutla, Malhotra, & Murnighan, 2003), expresses gratitude (Tesser, Gatewood, & Driver, 1968), and helps relationships, contributing to a safe, supportive social environment (Macy & Sato, 2002). Westphal and his colleagues (Westphal & Clement, 2008; Westphal & Deephouse, 2011) have also suggested that top management uses these inclinations to their advantage by fostering positive relationships with journalists, analysts, and institutional investors, hoping for reciprocity that will increase positive coverage of their organizations. In short, the recipients of good deeds can be much more strongly motivated to reward good deeds

than recipients of bad deeds to punish bad (Wang et al., 2009), both as a result of internalized social norms and other personal reactions.

In addition to punishing, observers also reward (Nowak & Sigmund, 1998; Rand & Nowak, 2013), particularly because of reputational concerns (e.g., Baker & Bulkley, 2014; Milinski, Semmann, Bakker, & Krambeck, 2001). However, there is reason to believe that their rewarding behavior will be tempered as compared to recipients'. Because cooperation among humans is expected rather than unusual, observers may not feel that they need to reciprocate good deeds (Frauenfelder, 1974; Greenberg, Saxe, & Bar-Tal, 1978; Henrich, 2004; Sears, 1983; Zajonc & Burnstein, 1965). This runs in stark contrast to recipients who feel a strong need to establish equity by returning a favor (Regan, 1971); observers (who were not the beneficiaries of these positive acts) are likely to be less influenced by equity concerns. Observers' reticence to reward (compared to recipients) may also be driven by an awareness of group norms that suggests that prescriptions for moral action (e.g. people should tell the truth) are weaker than proscriptions against immoral action (e.g., people should not lie; Janoff-Bulman & Carnes, 2013). Overall, then, observers may feel a weaker drive to reward positive behaviors. Thus we predict that:

Reward Hypothesis 1 (H1r): Direct recipients reward positive acts more than third-party observers do.

Reward Hypothesis 2 (H2r): Direct recipients feel more obligated to reward positive acts than third-party observers do, which translates into stronger rewards.

The current research

The current research investigates people's motivations to reciprocate good and bad deeds by comparing and contrasting the reciprocal reactions of direct recipients and third-party observers. We predict that recipients and observers will have markedly different reactions to good and bad deeds, i.e., an interactive effect such that observers will reward less (H1r) and punish more (H1p) than recipients. We also predict that feelings of obligation will be the driving mechanism behind both observers' and recipients' rewarding and punishing behaviors.

We tested Hypotheses 1p and 1r in three experiments that used a variety of methodologies.² Experiment 1 asked individuals to recall an incident in which someone was kind or cruel to them, or to someone else, and how they responded to those actions. In Experiment 2, individuals took on the role of a recipient or an observer and indicated how much they would reward honesty or punish deception. In Experiment 3, participants incurred real, personal costs to reward honesty or punish deception. Experiments 2 and 3 tested Hypotheses 2p and 2r by assessing individuals' feelings of obligation. Experiment 3 also controlled for participants' sense of power in the relationship.

Experiments 2 and 3 used one-shot interactions in the lab, which allowed us to control for a variety of potentially confounding factors. While reciprocity is often embedded in complex, dynamic systems, one-shot interactions offer a useful approach for its study, for at least two reasons. First, as suggested by Rand and Nowak (2013), even in the absence of the contexts which usually drive cooperative behaviors, many people in one-shot anonymous laboratory games still choose to cooperate because, "the intuitions and norms that guide these decisions were shaped outside the laboratory by mechanisms for the evolution of cooperation" (p. 420). While debate exists concerning the strength of the

² We ran two additional studies whose results were consistent with our current findings. We do not report these studies here as their findings are redundant with our reported studies. They are available from the first author upon request.

cultural and genetic factors that have shaped these intuitions and norms, many situations evoke cooperation as an internalized default behavior. This suggests that a lifetime of experience influences behavior in one-shot lab contexts, including reciprocity. Indeed, de Waal (2008) notes, “[p]roximate and ultimate viewpoints do inform each other” (p. 280). Thus, while one-shot interactions in the lab do not encompass the complexity of many dynamic systems, they are not entirely free of them either. In fact, because these complex forces are likely to be weaker in one-shot lab environments, our research represents a conservative test of our hypotheses.

Second, even when people are making one-shot choices, there is always uncertainty about whether the situation is truly one-shot. While the probability of further interactions with one’s fellow participants is low, participants may share classes, majors, or enrollment, or encounter them outside the lab. Delton, Krasnow, Tooby, and Cosmides (2011) discuss this possibility extensively, noting, “. . . given the stochastic nature of the world, it might be correct to say that, at the time of the interaction, the interaction is not determinately either one-shot or repeated. Instead, an interaction only becomes one-shot retroactively at events that uniquely preclude additional interactions, such as the death of one of the parties” (p. 13336). Delton and colleagues also argued that the costs of reciprocating in a one-shot context were relatively low given the potential (however slight) for future interactions, and their situational models support this argument in finding that individuals are still very cooperative even when they believe they are in a one-shot interaction.³

Experiment 1

Method

Participants and design

We randomly assigned 145 paid participants (51 women; mean age = 28.01 years, $SD = 8.17$, range = 18–63; two individuals did not report demographics) on Amazon’s Mechanical Turk (MTurk), an online survey program, to one of four conditions in a 2 (context: recipient, observer) \times 2 (act: good deed, bad deed) between-subjects design.

Procedure

We asked participants to either recall an incident in their lives in which someone was kind (*good deed condition*) or cruel (*bad deed condition*), and when this behavior either happened to them (*recipient condition*) or to someone else (*observer condition*). After participants had recalled and described this incident, they progressed to a survey page in which they were asked to provide open-ended descriptions of their reactions.

Two independent coders read participants’ passages and rated the kindness (in the *good deed conditions*; 0 = not at all kind to 6 = extremely kind) and the cruelty (in the *bad deed conditions*; 0 = not at all cruel to 6 = extremely cruel) of the incident. A kind act coded as a 2 was: “One of my coworkers dropped a large pile of plates, and another coworker helped him pick them up and assured him that it wasn’t a big deal that some of the plates broke.” A kind act coded as a 6 was: “I was foreclosed out of my house and looking for a place to live. My brother said I could come stay with him. . . then he was called out of state to a job. He asked me look after his house while he continued to pay for the ongoing expenses.” An example of a cruel act coded as a 2 was, “[a coworker] . . . intentionally started rumors about us, and went out of her way to be vindictive. . .”. A cruel

act coded as a 6 was, “I was at a gas station and a father started beating his child because he couldn’t pay for gas.” The inter-coder correlation was $r(81) = .73$, $p < .001$, for the kind incident ratings, and $r(62) = .84$, $p < .001$, for the cruel incident ratings. Thus, we averaged the coders’ ratings, with higher numbers representing kinder and crueler incidents.

Two other independent coders rated the *intensity* of participants’ reactions to the incident from zero (e.g., “I did not respond. . .”) to 6 (extensive reactions). In the *good deed conditions*, a reward response coded as a 2 was: “I thanked them.” A reward response coded as a 6 was: “I responded by volunteering to stay at work later to help cover for the co-worker who was allowed to go home.” In the *bad deed conditions*, a punishment coded as a 2 was: “I responded courteously at first but eventually became annoyed and short with them. . .” A punishment coded as a 6 was: “I walked up and told him to stop. He refused so I took his child and gave her to the manager that had already called the police. He tried to attack me and I restrained him and placed him under citizen’s arrest.” The inter-rater correlation was $r(81) = .73$, $p < .001$ for the reward ratings, and $r(62) = .87$, $p < .001$ for the punishment ratings. We averaged the coders’ responses to form a scale measuring the intensity of participants’ response, with higher numbers representing greater rewards and harsher punishments. We also dichotomized the scales to identify whether participants chose to respond at all (frequency: 0 = no response vs. 1 = response, i.e., anything greater than zero).

Results and discussion

Fourteen participants observed or directly suffered from the cruelty of high-status individuals (e.g., their manager at work). These participants expressed a strong tendency to not respond. Because the force behind their reactions seemed to be external rather than internal, we dropped their data from the analyses, leaving a final sample of 131 participants (44 women, 1 individual did not report demographics). An analysis of variance (ANOVA) revealed no main effects and a significant interaction, $F(1, 127) = 6.53$, $p = .01$. Planned contrasts indicated that, for good deeds, recipients ($M = 2.34$, $SD = 1.11$) reported engaging in more intense rewards than observers did ($M = 1.61$, $SD = 1.71$), $t(127) = 2.01$, $p = .05$, $d = .51$, and for bad deeds, observers ($M = 2.05$, $SD = 2.32$) reported engaging in marginally more intense punishment than direct recipients did ($M = 1.30$, $SD = 1.46$), $t(127) = 1.66$, $p = .10$, $d = .39$.

To examine the frequency of responses, we ran a log-linear analysis of the proportion of individuals who responded across conditions. The test revealed a three-way context \times act \times reward/punish interaction, $\chi^2(1, N = 131) = 11.08$, $p = .001$. Chi-squared tests revealed that observers rewarded honesty less frequently than recipients (62% vs. 100%), $\chi^2(1, N = 78) = 18.91$, $p < .001$, and that observers and recipients did not differ in terms of the frequency of punishments (48% vs. 50%), $\chi^2(1, N = 53) = .03$, $p = .86$.

However, recall is clearly open to biases (Schacter, 1999). For example, we ran an ANOVA with the extremity of the incident as the dependent variable to see whether participants recalled more extreme acts depending on condition. The analysis yielded two marginally significant main effects. The observed incidents ($M = 3.94$; $SE = .94$) were rated as marginally more extreme than the incidents that were directly experienced ($M = 3.74$; $SE = .94$), $F(1, 126) = 3.11$, $p = .08$. Moreover, cruel incidents ($M = 3.97$; $SE = 1.14$) were rated as marginally more extreme than kind incidents ($M = 3.73$; $SE = .77$), $F(1, 127) = 3.46$, $p = .07$. A significant interaction also emerged, $F(1, 127) = 4.35$, $p = .04$. Whereas no difference emerged for kind actions, $t(127) = .26$, $p = .80$, observed cruel incidents ($M = 4.36$, $SD = 1.11$) were viewed as more extreme than experienced cruel incidents ($M = 3.72$, $SD = 1.10$),

³ We do however acknowledge that there is continuing debate about the rationale for reciprocity in one-shot interactions.

$t(127) = 2.47, p = .02$. Because of these effects, we ran an analysis of covariance (ANCOVA) that controlled for the extremity of the act. This analysis again yielded a significant interaction, $F(1, 126) = 6.86, p = .04$. Planned contrasts confirmed that recipients ($M_{adj} = 2.38; SE = .24$) reported engaging in more intense rewards than observers ($M_{adj} = 1.67; SE = .25$), $t(126) = 4.07, p = .05$. For bad deeds, however the difference between observers ($M_{adj} = 1.77; SE = .35$) and recipients ($M_{adj} = 1.35; SE = .27$) was only directional, $t(126) = .88, p = .35$.

These initial findings suggest that our model has potential, but might be influenced by biased recall. Thus, we conducted a more rigorous test of our hypotheses in two controlled experiments that allowed us to observe peoples' actual, as opposed to recalled, responses to acts of honesty and dishonesty. In each study, the impact of honesty and deception was monetarily equated and rewards and punishments were equally costly (Wang et al., 2009), eliminating the possibility that differences in the extremity of the acts or costs to reward or punish drove reciprocity.

Experiment 2

Experiment 2 used a business scenario of a positive or negative act (Wang et al., 2009) in which participants were direct recipients or third-party observers. It also tested whether feelings of obligation mediated the effects of both direct and indirect reciprocity on rewards and punishments. We also tested an alternative explanation – whether a person's emotions might explain their reward and punishment behavior. We expected that, as suggested by prior research, people who experience strong emotions (happiness following honesty; anger following deception) will be more likely to engage in reciprocity (Sheppard et al., 1992; Tyler & Smith, 1998; Walster et al., 1978).

Method

Participants, design, and procedure

The participants were 131 students (75 women, 4 unreported; mean age = 21.83 years, $SD = .99$, range = 20–26) from a U.S. university; they volunteered in return for extra course credit. They were randomly assigned to one of four conditions in a 2 (context: recipient, observer) \times 2 (act: honesty, deception) between-subjects design.

Manipulations

Participants read about someone who behaved either honestly or dishonestly in a business deal that caused them or another person to gain or lose money, respectively. In the *recipient* conditions, the scenario asked the participant to imagine that the scenario described their own experience, and that they had gained or lost money; in the *observer* conditions, the scenario indicated that they had learned about an interaction between Person A and Person B and that, due to Person A's honesty or dishonesty, Person B had gained or lost money (see Appendix A for full text; Wang et al., 2009). The honest and dishonest acts had the same impact (i.e., the person affected by the honesty or deception always ended up with \$100) and magnitude (i.e., the gain after honesty or the loss after deception was always \$50).

Reward/punishment. Participants were then asked, if given the opportunity, whether they would reward or punish their counterpart in the recipient condition or Person A in the observer condition. As in past research (Wang et al., 2009), the cost of administering a reward or a punishment was one-tenth of the resulting reward or punishment. Thus, they could pay \$5 to have a \$50 impact on the responsible actor. The reward/punishment

amounts and costs were presented on 11-point scales in equal increments, e.g., from "Punish the individual \$0 (at a cost of \$0)" to "Punish the individual \$100 (at a cost of \$10)." Their dollar amount of rewards/punishments (intensity) and whether they chose to reward/punish any dollar amount (frequency) were our dependent variables.

Felt emotion. After choosing whether and how much to reward or punish the other person, participants also responded to two items assessing their emotions (adapted from Wang et al., 2009). In the *deception* conditions, participants indicated how *angry* and *unhappy* they were; in the *honesty* conditions, participants indicated how *calm* and *happy* they were, all on scales from 0 (e.g., *least angry*) to 100 (e.g., *most angry*). Scores on each of these two-item measures were averaged to create felt emotion scales, such that higher numbers represented more extreme emotions: more positive in the *honesty* conditions and more negative in the *deception* conditions.

Felt obligation to reciprocate. Finally, participants also indicated their feelings of obligation by responding to this question: "After you found out the information about the other player's choice did you feel: 1 = *not at all obligated to respond* to 7 = *extremely obligated to respond*" (Wang et al., 2009).

Results and discussion

An ANOVA revealed an honesty/deception main effect, $F(1, 127) = 8.80, p = .004, d = .50$, and a significant interaction, $F(1, 127) = 5.18, p = .02$. Participants rewarded honesty ($M = \$66.67, SD = 34.45$) more than they punished deception ($M = \$48.09, SD = 39.26$). Further, observers ($M = \$57.94, SD = 35.66$) punished deception more than recipients did ($M = \$38.24, SD = 40.71$), $t(127) = 2.23, p = .03, d = .51$, and recipients rewarded honesty ($M = \$71.72, SD = 34.65$) more but not significantly more than observers did ($M = \$62.35, SD = 34.21$), $t(127) = 1.02, p = .31, d = .27$.

A log-linear analysis on the proportion of participants who responded across conditions revealed a three-way context \times act \times reward/punish interaction, $\chi^2(1, N = 130) = 9.48, p = .002$. Chi-squared tests indicate that observers, compared to recipients, punished deception more frequently (82.4% vs. 55.9%), $\chi^2(1, N = 68) = 5.58, p = .02$, and rewarded honesty marginally less frequently (100% vs. 88.2%), $\chi^2(1, N = 63) = 3.64, p = .06$.

Felt emotion

An ANOVA on felt emotion led to two main effects: recipients reported feeling stronger emotions ($M = 88.21; SD = 16.02$) than observers did ($M = 72.37; SD = 29.46$), $F(1, 127) = 27.11, p < .001, d = .67$, and people reported experiencing stronger emotions in the honesty ($M = 89.99; SD = 18.31$) than in the deception conditions ($M = 70.72, SD = 27.13$), $F(1, 127) = 27.11, p < .001, d = .83$. A significant interaction, $F(1, 127) = 6.00, p = .02$, indicated that recipients reported feeling stronger negative emotions when they were deceived ($M = 83.44; SD = 16.23$) than observers reported when they witnessed deception ($M = 58.00; SD = 29.96$), $t(127) = 4.89, p < .001, d = 1.06$. Note that this pattern occurred despite the fact that recipients punished less than observers did. In addition, recipients reported feeling stronger but not significantly stronger positive emotions ($M = 93.79; SD = 14.06$) than observers did in response to honesty ($M = 86.74; SD = 20.94$), $t(127) = 1.30, p = .20, d = .40$. Thus, people reported feeling more positively after honest action than they felt negatively after deceptive action. Recipients also reported feeling stronger emotions than observers did. This occurred particularly for negative emotions after deception, replicating previous research (Wang et al., 2009).

Table 1
Moderated path analysis results for rewards and punishments, Experiment 2.

	<i>b</i>	<i>SE</i>	<i>R</i> ²
Mediator variables models			
<i>Felt obligation</i>			
Constant	4.89***	.29	
Context (0 = Recipient; 1 = Observer)	-.45	.39	
Behavior (0 = Honesty; 1 = Deception)	-.90 ⁺	.39	
Context × Behavior	1.13 ⁺	.55	
Model summary			.04
<i>Felt emotions</i>			
Constant	93.79***	3.98	
Context (0 = Recipient; 1 = Observer)	-7.06	5.42	
Behavior (0 = Honesty; 1 = Deception)	-10.35 ⁺	5.42	
Context × Behavior	-18.38 ⁺	7.51	
Model summary			.29
Dependent variable model			
<i>Reward/punishment</i>			
Constant	15.61	13.73	
Context	-3.93	7.65	
Behavior	-22.29**	7.78	
Felt Obligation	14.04**	1.82	
Felt Emotions	-.13	.13	
Context × Behavior	10.70	11.13	
Model summary			.40
	Boot effect	Boot SE	Conf. interval
Conditional indirect effects			
<i>Felt obligation</i>			
Honesty	-6.39	6.34	[-19.85, 5.31]
Dishonesty	9.50	4.65	[.37, 18.51]
<i>Felt emotions</i>			
Honesty	.95	1.22	[-.63, 4.54]
Dishonesty	3.43	4.00	[-3.25, 12.90]

Note. *N* = 131.

Unstandardized regression coefficients are reported. Bootstrap sample size = 5000.

⁺ *p* ≤ .10.

^{*} *p* ≤ .05.

^{**} *p* ≤ .01.

^{***} *p* ≤ .001.

Felt obligation to reciprocate

An ANOVA on felt obligation yielded one significant effect, the interaction, $F(1, 127) = 4.30$, $p = .04$, with observers of deception ($M = 4.68$; $SD = 1.17$) reporting somewhat stronger feelings of obligation to punish deception than recipients did ($M = 4.00$; $SD = 1.48$), $t(118) = 1.85$, $p = .08$, $d = .51$; feelings of obligation in the honesty conditions revealed a difference in the predicted direction that was not significant (recipients: $M = 4.90$; $SD = 1.59$; observers: $M = 4.44$; $SD = 1.91$), $t(127) = 1.16$, $p = .25$, $d = .26$.

We predicted that felt obligation would mediate the effects of both direct and indirect reciprocity on the intensity of rewards (H2r) and punishments (H2p). Following Preacher, Rucker, and Hayes (2007), we conducted a moderated path analysis and tested for mediation using a series of linear regressions (see Table 1). Because past work has found that emotions play a role in punishment and reward decisions (Wang et al., 2009), the regressions included felt emotions as a parallel mediator.⁴ The first regression (Stage 1), with reported feelings of obligation as the dependent variable, resulted in a significant interaction effect ($b = 1.13$, $SE = .55$, $t(127) = 2.07$, $p = .04$) suggesting that the observer effect on feelings of obligation was moderated by honesty or deception. The second regression (Stage 2) demonstrated that stronger feelings of obligation led to stronger responses ($b = 14.04$, $SE = 1.82$, $t(125) = 7.73$, $p < .001$). A bootstrap procedure with 5000 samples (Shrout &

Bolger, 2002) in the deception conditions yielded a 95% bias-corrected interval of [.37, 18.51] suggesting that increased punishments by observers were driven by feelings of obligation; this supports Hypothesis 2p. However, in the honesty conditions, the analysis yielded a 95% bias-corrected interval of [-19.85, 5.31], failing to support Hypothesis 2r, which predicted that feelings of obligation would drive increased rewards by recipients. A bootstrap procedure with felt emotions as the mediator yielded confidence intervals that overlapped with zero in both the honesty and deception conditions, suggesting that the observer effects were not driven by felt emotions.

Thus, this study found support for Hypothesis 1p, that observers would punish more than recipients would. The results also supported Hypothesis 2p, that an obligation to reciprocate would increase punishments by observers. The data also indicate that individuals' emotions cannot account for these effects. Hypotheses 1r and 2r received more tepid support, with recipients rewarding good behavior more than observers but not significantly so, and their feelings of obligation to reciprocate failing to mediate.

Experiment 3

Experiment 3 moved beyond scenarios to test Hypotheses 1p and 1r in a context in which individuals' choices had direct monetary consequences for them. We used a modified version of Gneezy's (2005) Deception Game, in which one person can send a truthful or misleading message and the other chooses whether to believe it, with their decisions jointly determining their final

⁴ The results with felt obligation as the only mediator, with and without felt emotions as a control, remained significant.

monetary payoffs. In addition, we tested Hypotheses 2p and 2r using a multi-item measure of obligation.

It is possible that recipients who were deceived felt less powerful than observers of deception because they were transgressed against. Because people who feel powerful are more likely to take action (Galinsky, Gruenfeld, & Magee, 2003) and punish than people who feel less powerful (Van Prooijen, Coffeng, & Verneer, 2014; Wiltermuth & Flynn, 2013), a greater sense of power may account for our observed effects. As a result, Experiment 3 also investigated whether participants' sense of power could account for these effects.

Method

Participants and design

There were 206 participants (97 women; mean age = 29.68 years, $SD = 10.09$, range = 19–68). Sixty-two individuals from a U.S. university who received extra credit and 144 individuals on MTurk participated. All participants were informed that in addition to extra credit (undergraduates) or participation payment (MTurk), they would also receive the actual dollar amount they accumulated in the experiment. Thus, participants received the monetary payoffs that resulted from their choices (Range: \$3.60–4.00; $M = \$3.85$, $SD = .16$). As before, they were randomly assigned to one of the four conditions in a 2 (context: recipient, observer) \times 2 (act: honesty, deception) between-subjects design.

Introduction

Before they were assigned randomly to the observer or recipient conditions, all participants were told that: (1) they would be randomly assigned to the role of one of three players, A, B, or C, (2) the other two players were working on other computers at the same time as the participant, and (3) there would be an interaction between Player A and Player B, which Player C would observe.

Stage 1 for recipients

In the recipient conditions, all participants were told that they had been randomly assigned to the role of Player B. We always controlled Player A's choices. The instructions indicated that only Player As would see the payoff matrix, in which one option would give Player As more money than Player Bs and the other option would give them less. Player Bs, who did not see the payoff matrix, had the task of choosing Option A or B, thereby determining their final monetary payoffs.

The instructions also indicated that, prior to Player B's option choice, Player As would send Player Bs one of two messages: "Option A earns you more than Option B" or "Option B earns you more than Option A." The instructions made it clear that only one of these messages was accurate. Because we controlled Player A's choices, the message always indicated that Player B would do best by choosing Option A. Research (e.g., Gneezy, 2005) indicates that a majority of participants believe the message and choose Option A.

After their option choice, participants who chose Option A in the recipient-honesty condition were told that Player A's message was true, that they would receive \$4 in Stage 1, and that "you would have received 50% less if you had chosen Option B." Those who chose Option B saw a different ending: "Because you chose Option B, you received \$4 in Stage 1. You would have received 50% more if you had chosen Option A."

In the recipient-deception condition, participants who chose Option A were told that Player A's message was not true, that they would receive \$4 in Stage 1, and that "you would have received 50% more if you had chosen Option B." "Those who chose Option B were told that they would have received 50% less if you had chosen Option A." Thus, their payoffs were identical, but they were

framed as either a gain or a loss resulting from the truth or a lie, respectively.

Stage 1 for observers

In the observer conditions, participants were told that they had been randomly assigned to the role of Player C; we controlled the other two players' choices, and all participants observed Player B being on the receiving end of honesty or deception. The interaction between Player As and Bs duplicated the recipient conditions, with half of the Player As telling the truth (observer-honesty condition) and half not (observer-deception condition). In addition, Player Bs always believed Player As. Player Cs were shown the message sent to Player Bs in the recipient conditions (e.g., Player B received \$4/50% less because Player A's message was not true). Also, to keep their outcomes identical, all observers received the \$4 'observer payment.'

Stage 2

Stage 2 gave all of the participants the opportunity to use their \$4 payoff to reward or punish Player A. As before, rewarding or punishing cost 1/10th of its impact and everyone's choices were presented on a 9-point scale from \$0 to \$4, in 50 cent increments, from "Reward the individual \$0 (at a cost of \$0)" to "Reward the individual \$4 (at a cost of \$.40)." Our dependent variable was whether they chose to reward or punish, and their chosen amount of monetary reward or punishment, ranging from \$0 to 4.

Sense of power. Participants then rated how powerful they felt (3-items; e.g., "In Stage 2, how much power do you think you have?"; Anderson & Galinsky, 2006; Galinsky et al., 2013; $\alpha = .93$). All items were measured on 7-point scales (1 = *Not at All* to 7 = *Very Much*).

Felt obligation to reciprocate. They also indicated their feelings of obligation by responding to the item from Experiment 2, and five additional items: e.g., "To what extent do you think you should punish Player A?" 1 = *Not at All* to 7 = *Very Much*". These items were averaged to form a felt obligation scale ($\alpha = .93$).

Results and discussion

A main effect emerged: MTurk participants ($M = \$1.19$, $SD = 1.51$) were less likely to respond than undergraduates ($M = \$2.27$, $SD = 1.60$), $F(1, 198) = 24.34$, $p < .001$. This is not surprising as MTurkers participate in studies for payment and may be more motivated than undergraduates by potential payoffs. To rule out the possibility that undergraduates' reciprocation to observers vs. participants may differ from MTurkers' (e.g., due to different levels of moral development, or feelings of obligation) we also tested for interaction effects. Importantly, no significant interactions emerged as a function of participant type (undergraduates vs. MTurk; all F 's $< .44$); therefore, we collapsed across this factor in all subsequent analyses.

In the recipient conditions, 36 out of 105 participants did not choose Player A's suggested Option A, instead choosing Option B. To rule out the possibility that acceptance or rejection of Player A's message influenced subsequent reward and punishment decisions, we used this choice as an additional factor. In line with past work (Wang et al., 2009) that has found that choice did not influence subsequent decisions to reward and punish, a significant interaction did not emerge based on whether participants accepted Player A's message or not, $F(1, 202) = .78$, $p = .38$. This indicates that decisions to reward or punish Player A were not affected by participants' initial choice of option; as a result, we did not control for this factor in subsequent analyses.

A context \times act ANOVA yielded an honesty/deception main effect, $F(1, 202) = 15.10$, $p < .001$, $d = .54$, and a significant

interaction, $F(1,202) = 8.23, p = .005$. The main effect indicated that people rewarded honesty ($M = \$1.94, SD = 1.60$) more than they punished deception ($M = \$1.10, SD = 1.52$). The interaction indicated that observers punished deception marginally ($M = \$1.37, SD = 1.53$) more than recipients did ($M = \$.84, SD = 1.48$), $t(202) = 1.75, p = .08, d = .35$, and that recipients rewarded honesty ($M = \$2.29, SD = 1.61$) more than observers did ($M = \$1.59, SD = 1.53$), $t(202) = 1.35, p = .02, d = .45$. Thus, the pattern of the data supports Hypotheses 1p and 1r.

A log-linear analysis on the proportion of participants who responded across conditions revealed a three-way context \times act \times reward/punish interaction, $\chi^2(1, N = 206) = 4.42, p = .03$. Chi-squared tests found that observers, compared to recipients, punished deception marginally more frequently (54% vs. 35.9%), $\chi^2(1, N = 103) = 3.43, p = .06$, and rewarded honesty less frequently (66.7% vs. 76.9%), $\chi^2(1, N = 103) = 1.34, p = .25$, but not significantly so.

Sense of power

An ANOVA on people's sense of power demonstrated that punishers ($M = 5.18; SD = 1.43$) felt more powerful than rewarders did ($M = 4.71; SD = 1.58$), $t(202) = 5.29, p = .02, d = .31$, and that observers ($M = 5.13; SD = 1.41$) felt marginally more powerful than recipients did ($M = 4.76; SD = 1.61$), $t(202) = 3.21, p = .08, d = .24$. However, a significant interaction between these two variables did not emerge, $F(1,202) = .18, p = .67$.

Felt obligation to reciprocate

An ANOVA on people's feelings of obligation yielded an interaction, $F(1,202) = 8.82, p = .003$, with observers of deception ($M = 3.95; SD = 1.82$) reporting stronger feelings of obligation to punish deception than recipients did ($M = 3.25; SD = 1.58$), $t(202) = 1.99, p = .05, d = .41$, whereas recipients of honesty reported greater feelings of obligation to reward ($M = 4.59, SD = 1.93$) than observers did ($M = 3.81; SD = 1.79$), $t(202) = 2.21, p = .04, d = .42$.

As with Experiment 2, we tested whether felt obligation to reciprocate would mediate the effects of both recipients' and observers' reciprocity on the intensity of rewards (H2r) and punishments (H2p). A moderated path analysis was performed, with the regressions including sense of power as a parallel mediator (see Table 2). The first regression (Stage 1), with feelings of obligation to reciprocate as the dependent variable, resulted in a significant interaction ($b = -1.48, SE = .49, t(202) = 2.97, p = .003$), suggesting that the recipient vs. observer effect on feelings of obligation was moderated by honesty or deception. The Stage 2 regression demonstrated that stronger feelings of obligation to reciprocate led to stronger responses ($b = .53, SE = .05, t(200) = 10.92, p < .001$). In the honesty conditions, a bootstrap 95% bias-corrected interval of $[-.80, -.01]$ suggests that feelings of obligation drove recipients' increased rewards. In the deception conditions, a bootstrap 95% bias-corrected interval of $[.03, .72]$ suggests that feelings of obligation drove observers' increased punishments. Thus, these results support Hypotheses 2r and 2p, that feelings of obligation to reciprocate would decrease rewards and increase punishments by observers.

When people's sense of power served as the mediator, bootstrap procedures in the honesty and deception conditions yielded

Table 2
Moderated path analysis results for rewards and punishments, Experiment 3.

	<i>b</i>	<i>SE</i>	<i>R</i> ²
Mediator variables models			
<i>Felt obligation</i>			
Constant	4.59***	.25	
Context (0 = Recipient; 1 = Observer)	-.78*	.35	
Behavior (0 = Honesty; 1 = Deception)	-1.34***	.35	
Context \times Behavior	1.48**	.50	
Model summary			.07
<i>Sense of power</i>			
Constant	4.56***	.21	
Context (0 = Recipient; 1 = Observer)	.29	.30	
Behavior (0 = Honesty; 1 = Deception)	.39	.29	
Context \times Behavior	.18	.42	
Model summary			.04
Dependent variable model			
<i>Reward/punishment</i>			
Constant	-.08	.39	
Context	-.29	.24	
Behavior	-.74**	.25	
Felt Obligation	.53***	.05	
Sense of power	-.01	.06	
Context \times Behavior	.45	.35	
Model summary			.44
	Boot effect	Boot SE	Conf. interval
Conditional indirect effects			
<i>Felt obligation</i>			
Honesty	-.41	.20	$[-.80, -.01]$
Dishonesty	.37	.18	$ [.03, .72]$
<i>Sense of power</i>			
Honesty	-.003	.03	$[-.09, .03]$
Dishonesty	-.006	.03	$[-.10, .05]$

Note. $N = 206$.
Unstandardized regression coefficients are reported. Bootstrap sample size = 5000.
* $p \leq .10$.
* $p \leq .05$.
** $p \leq .01$.
*** $p \leq .001$.

non-significant intervals, suggesting that observers' greater sense of power cannot account for the results.⁵

Notably, while the percentages of money gained and lost (i.e., 50%) as a result of honesty or dishonesty did not differ between Experiments 2 and 3, the amounts did (i.e., \$2 vs. \$50). Given that this paper concerns relative judgments of actions, it is notable that the pattern of results was consistent across both experiments, such that the absolute amount of gain or loss did not influence the findings.

In Experiments 2 and 3, people rewarded good behavior more than they punished bad behavior. This reinforces past theorizing (Wang et al., 2009) that people respond behaviorally to positive behavior. However, the parties' relationship qualified this main effect. Across three experiments, observers punished deception more than recipients did (significantly and marginally so in Experiments 2 and 3, respectively, and directionally in Study 1); and with the exception of Experiment 2, they also rewarded honesty less. To be more thorough and increase statistical power (Cohn & Becker, 2003), we conducted a meta-analysis that included the data from all three experiments. The combined results led to a significant overall effect, with observers punishing more ($Z = -2.95; p = .003$) and rewarding less ($Z = 3.17; p = .002$) than recipients (see Table 3).

⁵ The results with felt obligation as the only mediator, with and without sense of power as a control, remained significant. Following the reviewers' suggestions, we also collected data on several potential alternative mediators, including participants' fears of retaliation (in the punishment conditions), their feelings of entitlement (which we adapted from the original trait-based measure (Zitek, Jordan, Monin, & Leach, 2010) by asking participants how they felt in the given situation), and Player A's perceived ethicality. No significant effects emerged. Further information on these data is available from the first author upon request.

Table 3
Meta-analysis summary.

Hypothesis	Effect size estimates (<i>r</i>)	Summary			
		<i>M</i>	<i>Z</i>	<i>p</i>	95% CI
<i>Rewards</i>					
Exp 1: Observer < Recipient	.25	.21	3.17	.002	.08, .33
Exp 2: Observer < Recipient	.13				
Exp 3: Observer < Recipient	.22				
<i>Punishments</i>					
Exp 1: Observer > Recipient	-.20	-.20	-2.95	.003	-.33, -.07
Exp 2: Observer > Recipient	-.25				
Exp 3: Observer > Recipient	-.17				

M represents the weighted average of the effect sizes. Heterogeneity tests were not significant.

Finally, these experiments provided support for our prediction that feelings of obligation were the psychological mechanism behind the punishment of deception (in Experiments 2 and 3) and the rewarding of honesty (in Experiment 3).

General discussion

Although rewards and punishments help to create organizational environments that stimulate good and deter bad behavior, the current results suggest that direct recipients and third-party observers reciprocate in decidedly different ways. Importantly, this research goes beyond comparing recipients' and observers' rewards and punishments to examine how and why they systematically differ. The data consistently supported Hypothesis 1p, that observers punished deceptive action more than recipients did, and Hypothesis 1r, that recipients rewarded honest action more than observers did. This occurred for both college and general population samples, in recall tasks, scenarios, and for consequential behavioral choices. The differential dynamics for observers and recipients of good and bad behavior are consistent and clear.

Experiments 2 and 3's results provided additional insight on the dynamics of punishment: observers felt stronger obligation to punish than recipients did, which suggests that observers are more responsive to norms for punishment than are recipients, who may be experiencing conflict between their individual inclinations and social norms. Thus, these findings suggest that the world is relatively simple for observers: they only need to react – behaviorally rather than emotionally – for the sake of social norms. For recipients, the action choices are not so simple: being a recipient boosts the intensity of their emotional reactions but does not help to clarify how they should respond. Instead, experiencing bad behavior seems to present them with a troublesome dilemma (Wang et al., 2009).

Experiment 3's results also support the idea that obligation drives positive reciprocity; however, in this case, it was the recipients who felt a stronger obligation to respond than observers. Unlike bad behavior, responding to good behavior is free of dilemmas for direct recipients – reciprocating good deeds is not only normatively prescribed, it also feels good to do so. Conversely, observers feel a weaker obligation to respond, as they are not subject to equity concerns, and the obligation to respond to positive moral actions is weaker than that for negative moral actions (Janoff-Bulman & Carnes, 2013).

These results also have a variety of organizational implications. For instance, these findings suggest that, if an organization wants to reduce malfeasance via punishment, then observers are likely to fill that role more effectively than victims are. Alternatively, to increase organizational rewarding behavior, systems might be created to allow employees to reward each other directly for cooperative, positive action (Baker & Bulkley, 2014). Finally, although these

findings focused on reward and punishment decisions among individuals, future research might examine whether this pattern of findings also applies to inter-group dynamics. Indeed, research on intergroup perceptions suggests that, for negative behaviors, observers tend to attribute out-group members' behaviors to dispositional rather than to situational causes (Hewstone, 1990), which can then result in greater punishments. Whether the same effect exists for positive behaviors and rewards is an open question.

These data also provide the basis for a broader understanding of the role of observers and the use of rewards and punishments as social control mechanisms. Organizational structures create fertile ground for indirect rewards and punishments, particularly when managers and supervisors work to improve their group's norms and/or resolve conflicts among their team's members. The present studies attest to the importance of the evolutionarily adaptive desire to encourage the collective good (Fehr & Fischbacher, 2004b). Further, the differing rewarding and punishing responses found in the current research will not necessarily produce correspondent effects on the targets of those rewards and punishments. That is, although our findings suggest that observers are more motivated to punish a wrongdoer than recipients are, the effectiveness of punishment (at least in increasing cooperation) appears to be stronger coming from a recipient rather than an observer (Balliet, Mulder, & Van Lange, 2011). Similarly, although our findings suggest that recipients are more motivated to reward a good deed than observers, Balliet et al.'s findings (2011) show that the effectiveness of rewards (in increasing cooperation) does not differ depending on who the rewards come from. Future research could explore how the effectiveness of rewards and punishments influences the extent to which they are employed.

Although we have identified feelings of obligation as an underlying mechanism behind observers' reciprocity, other phenomena may also mediate or moderate these effects. For example, as noted, observers may not reciprocate good deeds because cooperation among humans is expected rather than unusual (Frauenfelder, 1974; Greenberg et al., 1978; Henrich, 2004; Sears, 1983; Zajonc & Burnstein, 1965). Moreover, Wang and Leung (2010) found that differences in rewarding behaviors between Americans and East Asians were explained by differences in trust, not felt obligation, thus trust may be worth exploring as another critical factor in reciprocation. In addition, our initial experiment made clear that individuals – whether recipients or observers – are hesitant to punish bad behavior on the part of supervisors. While our effects remain even when controlling for recipients' and observers' sense of power, future research might explore how formal hierarchical roles within organizations influence direct recipients' and third-party observers' rewarding and punishing behavior. It is also worthwhile to examine people's reactions to the victims of

dishonesty. Although prior work has typically focused on the punishment of dishonest actors, research has also investigated the effects of compensating victims (see Darley & Pittman, 2003; Lotz, Okimoto, Schlösser, & Fetchenhauer, 2011). This is consistent with the notion that observer responses to norm violations could be driven by the obligation to communicate a group's regard for the victim (e.g., Okimoto & Wenzel, 2011) or a desire to portray a positive image of oneself to others (e.g., Pillutla & Murnighan, 1995). Thus, it would be fruitful to examine whether punishments and compensations differ in their effectiveness in maintaining victims' identification with their organization or in maintaining a person's image to the victim and to other organizational members (e.g., Adams & Mullen, 2012).

Our findings stand in contrast to those of Fehr and Fischbacher (2004b) who found that recipients were more likely to punish than observers. Our experiments, however, used very different methods. In the current paper, observers had an opportunity to reward or punish an individual they observed making a beneficial or harmful decision. In contrast, in Fehr and Fischbacher's (2004b) work, observers started by being recipients in a Dictator Game and were then given the opportunity to punish a different dictator with whom they had not interacted. It is not surprising that observers were hesitant to punish a stranger to the same extent as direct recipients who could punish someone who had just harmed them. In fact, these observers may have experienced an even more intense moral conflict than direct recipients did, as there are strong norms against punishing innocents. Clearly, it would be interesting for future research to examine the impact of past victimization on subsequent reciprocity.

As seen in the lines of reasoning for our hypotheses, the rationales for what leads to felt obligation to increase punishments but decrease rewards for observers are very different and clearly not symmetrical. Thus, we suggest that future research might fruitfully examine the dynamics of the forces driving felt obligation to reciprocate. For example, drives to reciprocate are likely to be more intense for observers who have been specifically selected to make a rewarding or punishing decision vs. observers who have encountered this opportunity by happenstance. It is also possible that one of the drivers of our observer effects is that observers can consider details of the social context more clearly than direct recipients can: because they are not directly involved, they can be more dispassionate in assessing the impact of their decisions on the initial actor, the recipient, and other individuals, including themselves. Thus, observers may perceive the broader implications that reciprocity can have for all of a group's members (Krupka & Weber, 2009), making it easier for their feelings of responsibility to influence them (Simpson & Willer, 2008). Self-interest can also encourage observers to respond: if they do not uphold social norms via punishment they risk reputational damage (dos Santos, Rankin, & Wedekind, 2013), especially in tightly-knit communities where reputations spread quickly. Future work might delve into the psychological underpinnings of felt obligation, and even manipulate it to further determine causality.

In conclusion, the present research highlights a variety of dynamic effects following honest and deceptive action. Our data raise new questions that might guide future research and enhance our understanding of rewards and punishments in a variety of interpersonal and collective contexts. The findings show how social norms are especially salient for observers, who respond to their feelings of obligation when bad deeds emerge, and for recipients, who respond to their feelings of obligation when good deeds emerge. Because the maintenance of organizational norms is grounded in consistent reciprocity, varying reactions by observers and recipients may create inconsistency and sow the seeds for unmet expectations. Our data provide potentially useful information for organizations that are seeking greater control over their

informal incentives, and a deeper understanding of how reciprocity may play out between employees.

Appendix

Scenarios from Experiment 2

Honesty [and deception] recipient scenarios

Imagine the following scenario: You recently completed a business deal with another individual. You just found out that the individual was honest [dishonest] about some key information regarding the deal. As a result, you [only] received \$100. You would have received 50% less [more] if the other individual had given you dishonest [honest] information.

You have a one-time opportunity to reward [punish] the individual, but responding will require you to spend your own money. You can choose to behave in a fashion that is equivalent to giving money to [taking money from] the individual at a 1:10 ratio. In other words, for every 10 cents you spend, you reward [punish] the individual 1 dollar. You can reward [punish] up to \$100.

After your choice, you WILL NOT interact with the individual again. The individual WILL NOT have the option to add money to [subtract money from] your bank. [You will not receive any money taken from the individual.]

How do you want to respond to the individual?

Honesty [and deception] observer scenarios

Imagine the following scenario: Two people recently completed a business deal and Person A was honest [dishonest] about some key information regarding the deal. As a result, Person B [only] received \$100. Person B would have received 50% less [more] if Person A had given Person B dishonest [honest] information.

You learned that Person A was honest [dishonest] towards Person B. You have a one-time opportunity to reward [punish] Person A, but responding will require you to spend your own money. You can choose to behave in a fashion that is equivalent to giving money to [taking money from] Person A at a 1:10 ratio. In other words, for every 10 cents you spend, you reward [punish] Person A 1 dollar. You can reward [punish] up to \$100.

After your choice, you WILL NOT interact with Person A again. Person A WILL NOT have the option to add money to [subtract money from] your bank. [You will not receive any money taken from Person A.]

How do you want to respond to Person A?

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