Improving Consumer Wellbeing with Subtle Interventions to Decision Context

by

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DEDICATION

To my family.

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ABSTRACT

I investigate ways to utilize decision context to influence decision making and behavior, particularly in domains concerning personal or societal wellbeing. In my dissertation, I explore two distinct, novel contextual manipulations that can 1) improve consumers' tendency to precommit to beneficial behaviors and 2) increase consumers' likelihood of behaving prosocially. In my first essay, I empirically explore the impact of a perceived time lag on consumers' decisions to precommit to making goal progress in the future. Past research shows that persistence toward a goal is greater near its completion than its initiation (the goal gradient effect). I find that the goal gradient effect is reduced in precommitment contexts because a time lag between precommitment and progress dates reduces affect associated with goal progress (e.g., the pain of failing goal completion). I show that contracting the perceived length of a lag restores the goal gradient effect for precommitment. In my second essay, I investigate how contextual information about reference groups influences conformity to the group's prosocial norms. To the extent that norms are derived from culture, normative influences on persuasion would vary for message recipients depending on their cultural orientation. I find that consumers with interdependent self-construal are more likely to follow norms of social reference groups (e.g., citizens). On the other hand, consumers with independent self-construal are more likely to follow norms of situational reference groups (e.g., strangers in this room). Underlying psychological processes, theoretical contribution, and managerial implications are discussed.

CHAPTER 1

Introduction

My dissertation investigates how marketers can utilize contextual factors to motivate consumers' responses that benefit consumer and societal wellbeing. Despite their best intentions and external assistance, many people still fail to get their health routinely checked, volunteer for social causes they care about, or recycle. Despite marketers' efforts directed at improving consumers' decisions and behavior, a striking gap still exists between what should be done and what is being done. A better understanding of how context can inhibit or promote consumer wellbeing is needed. Thus, I investigate ways to utilize decision context to influence decision making and behavior, particularly in domains concerning personal and societal wellbeing. In my dissertation, I explore two distinct, novel contextual manipulations that can 1) improve consumers' tendency to precommit to beneficial behaviors and 2) increase consumers' likelihood of following prosocial norms.

In my first essay, "Leaps and Bounds: Temporal Influences on Precommitment During Goal Progress," I examine how perceived closeness of time shapes consumers' precommitment to initiate or complete goals. The goal gradient effect suggests that it is difficult to initiate progress towards goals, but goals become more motivating as we near completion. This phenomenon has been explained by the curvature in the value function, which is more S-shaped for affect-rich than for affect-poor decisions. When we near goal completion, making progress delivers a much more psychological payoff than making the equivalent amount of progress near

initiation. Historically, this phenomenon has been observed for scenarios in which goal progress will imminently occur. In real life, however, we often plan our goal progress ahead of time, and marketers must ask us for precommitment to future action on our goal progress (e.g., requiring sign-ups for volunteering at a soup kitchen prior to the volunteering date). I test the robustness of the goal gradient effect when consumers precommit to making progress and explore the impact of "bounding" time on consumers' decisions to precommit to progress toward a goal.

First, I find that the traditional goal gradient effect is reduced in precommitment contexts (e.g., decisions to make volunteering progress that occurs in the future). This is because when individuals decide whether to initiate or complete a goal ahead of time, they feel that the future time point at which they will take action on goal progress is temporally distant from the current time point at which they make the precommitment decision. Such a time lag between precommitment and action on goal progress makes individuals feel farther away from and feel reduced affect about the future goal progress. I propose that a time lag alters the curvature of the value function because diminished affect reduces the curvature of the value function. Thus, the farther in the future that goal progress seems to be, the weaker the goal gradient effect. This has a deleterious effect on precommitment to progress near the completion of a goal.

Next, I show that reduced precommitment for goal completion is not inevitable when consumers plan their future goal progress. Perceptions of time between a commitment decision and goal progress are subjective and can be subtly manipulated to reduce the perceived time until progress will occur. Specifically, a bounded time lag has a more salient end than an unbounded time lag does, making the bounded time lag feel closer in time. For example, a time lag followed by a month rollover makes the preceding dates seem to be ending more quickly. In six experiments, I demonstrate that bounding the time between precommitment and subsequent goal

progress can reduce the perceived time until the opportunity to make goal progress and thus increase participants' persistence near goal completion. I use subtle cues to bound time, such as a month rollover, and show that this is sufficient to contract the perceived length of a time lag, restoring the goal gradient effect. This work not only reveals an important boundary to a robust phenomenon, but also uncovers a simple and powerful way to restore the beneficial characteristics of the goal gradient effect in prosocial advocacy and other contexts.

In my second essay, "Cultural Orientation Differentially Impacts the Effectiveness of Descriptive Norm Appeals to Promote Prosocial Behavior," I investigate how marketers can make better decisions about which reference group's norms to include in messages to more effectively promote prosocial behavior. Previous research has examined different aspects of reference groups, with some emphasizing their social relevance (e.g., referencing citizens) and others emphasizing their situational relevance (e.g., referencing strangers who have been in the same situation). When are descriptive norms of one type of reference group more likely to be persuasive than the other? In addressing this question, I investigate how contextual information about a reference group influences conformity to the group's prosocial norms. Specifically, I focus on the role of cultural orientation through the lens of self-construal and consider how decision context interacts with self-construal to impact prosocial behavior.

To the extent that norms are derived from culture, normative influences on persuasion would vary for message recipients depending on their cultural orientation. I propose that depending on a consumer's cultural background, appeals that employ norms of situationally relevant reference groups (e.g., strangers in the same situation) and socially relevant reference groups (e.g., citizens) elicit different levels of conformity to prosocial norms. Five studies across various domains of prosocial behavior (e.g., reusing towels, helping others, and donating money)

provide convergent evidence that in relatively private decision contexts, individuals with independent (interdependent) self-construal are more persuaded by norms of situational (social) reference groups.

Independents, who prioritize being distinct and unique, are inclined to perceive behavior that accords with norms of a situational reference group as being distinctive and therefore more persuasive. By contrast, interdependents, who value interconnectedness and belonging, are more susceptible to norms of a social reference group with which they identify. Independents identify with both groups similarly, and the level of identification has less influence on prosocial behavior. In public decision contexts, there appears to be a shift in how independents and interdependents respond to reference groups featured in descriptive norm appeals. Among independents, there is a greater need for uniqueness relative to the members of the situationally relevant group, and thus they are less likely to find its norms persuasive. Interdependents, however, have higher sensitivity and conformity to the group that is proximate to the situation at hand (i.e., situationally relevant reference group) given their propensity for attunement to contextual surroundings and the relationship with people in their environment.

This work makes a unique theoretical contribution by revealing cultural variability in group identification and perceived distinctiveness that systematically influence various types of prosocial behavior. In addition, it suggests simple yet powerful ways in which marketers and policymakers can increase the persuasive appeals of normative messages employing descriptive norms. The findings reveal how marketers and policymakers need to account for cultural orientation and decision context when developing persuasive norm appeals to promote prosocial behavior. Effective marketing strategies to increase persuasion and conformity will account for the cultural orientation of the target market. Thus, firms should correctly identify the target group

of their marketing campaigns that contain descriptive normative appeals. Firms that opt for mass marketing strategies targeting a more diverse group of people can deliberately prime independence or interdependence and employ descriptive norm information about a situationally or socially relevant reference group, respectively, especially for decisions that are primarily made in private.

In conclusion, my dissertation makes valuable theoretical contributions by investigating important contextual factors for developing a better understanding of the power of the goal gradient effect and normative influences. By examining subtle but impactful interventions that facilitate prosocial behavior, I provide practical insights for policy makers, charities, and business practitioners.

CHAPTER 2

Leaps and Bounds: Temporal Influences on Precommitment During Goal Progress

Consumers often struggle to persist in progress toward a goal. From simply reading for an hour a day to saving for retirement, past research show that consumers find it difficult to start and stay on track (Chandran and Menon 2004; Hershfield et al. 2011; Tonietto, Malkoc, and Nowlis 2019). This has been attributed to failures in self-control (Thaler 1980, 1999). Marketers have tried to solve this problem for decades, and several solutions have been identified.

One way to help consumers overcome self-control problems during goal pursuit is to require precommitment to goal progress before they even take action. For example, consumers who struggle to save can benefit from precommitting to save a portion of their income through the Save More Tomorrow program (Thaler 1980). Precommitment locks them into a schedule of saving more each period and limits consumers' exposure to temptation rather than repeatedly subjecting them to temptation at the moment of action (e.g., at the time of receiving their paycheck). Another moderator of weak self-control, even in the absence of precommitment, is proximity to goal completion: Once completion is near, persistence increases in a phenomenon explained by the value function called the goal gradient effect (e.g., Kivetz, Urminsky, and Zheng 2006). Marketers can leverage this phenomenon to help consumers achieve their goals. For example, consumers who receive a 12-punch loyalty card with two "starter" punches have a sense of progress toward receiving a free reward and are more likely to complete the card than

consumers who receive a 10-punch loyalty card with no starter punches (Kivetz et al. 2006; Nunes and Dréze 2006).

Given that the goal gradient effect and precommitment each help marketers facilitate goal completion, one might be inclined to use both interventions simultaneously to maximize the odds of success. However, in this work, I argue and demonstrate that these two moderators of goal progress do not produce synergic effects on persistence in goal pursuit. Indeed, I propose and show that the goal gradient effect is reduced for precommitment.

Imagine a consumer who wants to meet a goal of volunteering for four weeks. Some volunteering organizations ask volunteers to repeatedly sign up for volunteering in advance (e.g., sign up at the beginning of each week for the following weekend). Notice that this scenario combines the potential for motivation via the goal-gradient with precommitment. The goal gradient literature suggests that the volunteer will be more motivated to sign up to complete the four-week goal than to initiate it. That is, the volunteer should be more likely to sign up for the fourth weekend of volunteering than the first weekend of volunteering. However, I note that precommitment is inherently time-lagged from goal progress (e.g., the volunteer is signing up ahead of each action). Critically, this time lag attenuates the goal gradient effect. Thus, I propose that individuals are not more motivated to precommit to goal completion than goal initiation.

In the present chapter, I provide evidence that the goal gradient effect is indeed reduced in contexts requiring precommitment (that is, when goal progress will not occur until a date in the future). I show that a context that specifies when in the future goal progress will occur actually makes the date of progress seem remote. Things that feel remote elicit less affect than those that are not (e.g., Bruehlman-Senecal and Ayduk 2015; Chang and Pham 2012). Thus, the subjective feeling of remoteness weakens the goal gradient effect, consistent with an affect-poor

value function. The greater the subjective distance between the precommitment decision and the date of progress, the weaker the goal gradient effect, even when the objective time between the date of precommitment and the date of goal progress is held constant.

With the understanding of why precommitment can undermine the goal gradient effect, I demonstrate how marketers can, with a subtle intervention, contract perceived time and restore the goal gradient effect. To return to my earlier example, I do not see the goal gradient effect when the volunteer requires deciding to precommit to goal progress at a future date. However, because perceptions of time are malleable, I can restore the goal gradient effect (thereby increasing a volunteer's motivation to precommit to complete the last week of volunteering) by making that date of the volunteering feel closer to the date of the precommitment. Once again, this effect is consistent with the shape of the value function that underlies the goal gradient effect. Things that are near elicit more affect than things that are far (e.g., Bruehlman-Senecal and Ayduk 2015; Chang and Pham 2012). The value function is more curved near the reference point when participants have heightened affect (Hsee and Rottenstreich 2004). It follows, then that if the date of the last volunteering feels less remote, a volunteer will feel more motivated to avoid the pain of failing to achieve a proximate goal.

THEORETICAL DEVELOPMENT

The Goal Gradient Effect

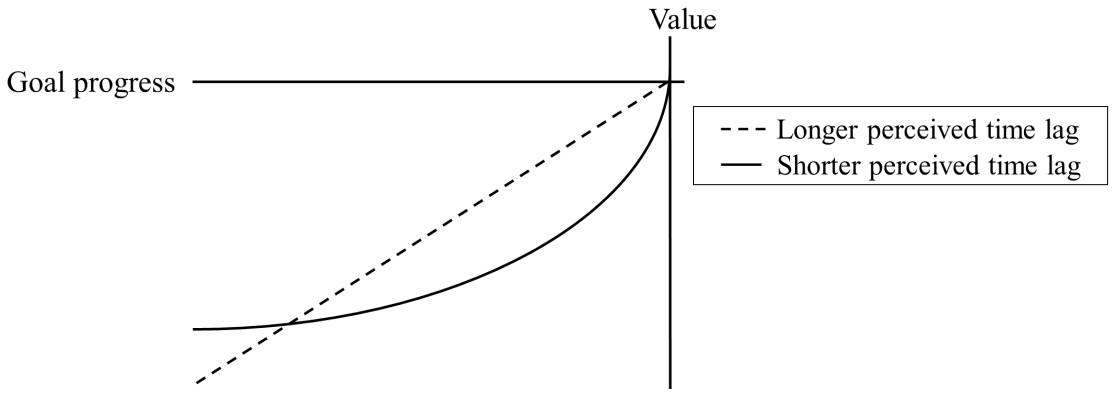
The importance of goal completion to consumers is a well-studied area. The goal gradient literature has shown that people are less likely to persist early in a task than near completion of the task (e.g., Kivetz et al. 2006; Louro, Pieters, and Zeelenberg 2007; Nunes and Dréze 2006;

Soman and Shi 2003). The effect is neatly explained by the prospect theory value function (Kahneman and Tversky 1979; Tversky and Kahneman 1991). The reference point is goal completion and thus the progress toward the goal occurs in the loss quadrant of that function (Heath, Larrick, and Wu 1999) (See Figure 2.1, in which the solid line illustrates the shape of the classic value function in the loss quadrant). The value function is convex and steepest near the reference point. Because of the diminishing sensitivity of the value function, progress made near the reference point is subjectively more valuable than the equivalent amount of progress made far from the reference point (i.e., at task initiation). This steepness reflects the fact that consumers anticipate more pain if they fail to attain their goal when they are close to completion than when they are far from it. Motivation to avoid this pain explains why persistence increases as people near goal achievement (Heath et al. 1999). Consequently, though the curvature of the value function can be an impediment when consumers begin goal progress, it actually benefits them when they near completion (e.g., Kivetz et al. 2006). For example, rats run faster as they near a reward (Hull 1932, 1934) and pull an apparatus more forcefully when food is closer than when it is farther away from them (Brown 1948). People exert greater pressure on a metal plate as they approach the end of an anagram task (Förster, Higgins, and Idson 1998). They are more likely to donate as charitable campaigns near completion (Cryder, Loewenstein, and Seltman 2013) and visit a store more frequently as they approach the redemption of a reward in a loyalty program (Kivetz et al. 2006; Nunes and Dréze 2006).

Further evidence for the value function explanation for the goal gradient effect comes from research that manipulates the position of the reference point. Highlighting accumulated progress and thus shifting the reference point from the final completion to the initial state reduces the goal initiation problem (Bonezzi, Brendl, and De Angelis 2011; Koo and Fishbach

2012). Range goals also motivate goal initiation and completion by allowing the reference point to be either the lower or the upper endpoint of the range (Scott and Nowlis 2013; Wallace and Etkin 2017a, 2017b).

Figure 2.1. Value functions for shorter versus longer perceived time lag between precommitment and future goal progress



Notes: The solid line represents the classic value function in the loss quadrant. Differences in the degree of curvature between the solid and dotted lines are also aligned with affect-rich and affect-poor contexts respectively.

Though it is a lynchpin of the goal gradient effect, the curvature of the value function is not always observed. Notably, when consumers experience little affect, the value function takes a more normative, linear shape, as represented by the dotted line in Figure 2.1 (Hsee and Rottenstreich 2004). The value function has been modified in this way via priming (Hsee and Rottenstreich 2004; Lerner, Small, and Loewenstein 2004), taking a third-person perspective (Faro and Rottenstreich 2006; Kurt and Inman 2012), and simply reducing the affectiveness of the stimuli (Hsee and Rottenstreich 2004; Smith, Faro, and Burson 2012)[1]. For example, people

[1] Third-person scenarios have been used to reduce affect in decision making, and the curvature of the value function depends a great deal on affect, so one might wonder how the Heath et al. (1999) scenarios were able to produce such strong goal gradient results. The Heath et al. (1999) demonstrations of differential persistence toward a goal are elegant but heavy-handed. The scenarios use hypothetical protagonists and within-subjects designs. Participants chose the protagonist who would be more likely to

are more emotionally involved when estimating the worth of their own (as opposed to others') goods, so they tend to set higher asking prices for their own possessions and underestimate the asking prices of other sellers (Kurt and Inman 2012). They also demonstrate less loss aversion when they evaluate choices for others (vs. themselves) (Polman 2012). Framing recipients of charity as a single identifiable unit (e.g., a family) triggers affective processing and greater donations than when the same recipients are de-unitized (e.g., six children) (Smith et al. 2012). Empathy increases people's ability to predict others' risky choices (Faro and Rottenstreich 2006). Overall, research shows that reducing the affect associated with judgments makes the value function less curved, which implies a weak goal gradient effect.

Manipulating temporal distance can also reduce affect (Bruehlman-Senecal and Ayduk 2015; Chang and Pham 2012; Loewenstein 1996; Metcalfe and Mischel 1999), though this has not been applied to the goal gradient context. For example, consumers experience less affect when they can apply promotional discounts to delayed as opposed to immediate consumption (Lee and Tsai 2014). The close relationship between temporal distance and intensity of affect documented in the literature strongly suggests that there is less curvature in the value function for distant than for near events. Considering the close association between affect and temporal distance, I propose that precommitment decisions that precede a delayed action on goal progress will show a reduced goal gradient effect. Simply asking for goal-persistence commitments in advance could reduce the convexity of the value function and undermine the goal gradient effect that increases the likelihood of goal completion.

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persist. This might in part explain why results reflect the value function even though the people they were evaluating may have been emotionally remote to the participants.

Precommitment

Precommitment is a strategy employed to restrict future choice and commit to good behavior (Laibson 1997; Strotz 1955; Thaler 1980; Wertenbroch 1998). It was first studied by economists like Strotz (1955) and was expanded by scholars like Richard Thaler to the field of economics and behavioral sciences. Recently, it has started grabbing marketing scholars' attention (e.g., Schaefer, Rao, and Mahajan 2018).

Precommitment is ubiquitous in consumption decisions. For example, consumers using retailers like Groupon and Living Social precommit to consuming their purchases at a later point (Aydinli, Bertini, and Lambrecht 2014). In the context of goals and motivation, research in a variety of domains has shown the benefits of precommitment for self-control (e.g., smoking cessation in Giné, Karlan, and Zinman 2010). Pursuing a goal requires self-control, and consumers who engage in precommitment can protect themselves from choosing undesirable alternatives (e.g., consuming alcohol) and instead focus more on goal pursuit (Thaler 1999). Consumers who open savings accounts prefer to have precommitment devices that require deposits and forbid early withdrawals (Ashraf, Karlan, and Yin 2006; Thaler and Benartzi 2004). Compared to those who did not, those who precommitted achieved more savings 12 months after opening their account (Ashraf et al. 2006). Similarly, consumers who agreed with the binding precommitment to increase their purchases of healthy food showed a 3.5% increase in their purchases of healthy groceries over six months relative to those who did not (Schwartz et al. 2014). As these studies suggest, precommitment can benefit consumers, but also be profitable for marketers. For example, precommiting to a small investment can be sufficient to decrease consumers' propensity to search and switch brands (Zauberman 2003).

One noteworthy feature of precommitment is that it necessarily includes a time lag between precommitment and action (hence the "pre" in precommitment). Participants in Schwartz et al. (2014), for example, precommitted to their goal of healthier food purchases for the next six months. As I have already noted, temporal separation between precommitment and action reduces the intensity of affect. For example, the emotional benefits of purchasing indulgent products at discounted prices diminishes when the consumption window lengthens (Matherly, Ghosh, and Joshi 2019).

Research has predominantly focused on whether precommitment is helpful in goal progress. While extant literature has demonstrated the positive effects of precommitment on consumers' behavior, research on the factors that influence consumers' likelihood of precommitment is relatively scarce. Consequently, I investigate how stages of goal progress impact the value of asking for precommitment. While precommitment helps consumers follow through goal progress in general, I theorize that a time lag between the precommitment and the date of goal progress can reduce precommitment as consumers near goal completion. This is of course undesirable for consumers who wish to complete goals in the future.

Perceived Time Lag and the Goal Gradient Effect

Events that are objectively close are associated with greater affect than those that are objectively distant (Bruehlman-Senecal and Ayduk 2015; Chang and Pham 2012; Lee and Tsai 2014; Loewenstein 1996; Metcalfe and Mischel 1999). Curvature in the value function is also contingent on affect (Hsee and Rottenstreich 2004). Thus, one would expect greater steepness in the value function for near events than for distant events (as is represented by the solid versus dotted lines respectively in Figure 2.1). In other words, there should be a stronger goal gradient effect in near-term contexts than in lagged ones.

Parallel research supports this theorizing. Work on motivation has found that people have significantly less motivation to work toward a goal when the time of the action date feels subjectively distant than when it feels near (Peetz, Wilson, and Strahan 2009). Students who perceived an exam and graduation to be less (vs. more) proximate in time experienced less academic motivation (Peetz et al. 2009). Hence, I hypothesize that perceptions of a time lag between the date of precommitment and the date of goal progress will moderate the goal gradient effect. Specifically, while I expect that the goal gradient effect will be strong when I leave the date of goal progress ambiguous, I hypothesize that when I explicitly state that goal progress would occur *on a particular date in the future*, there will be no difference between precommitment to initiate progress and precommitment to complete the goal. In other words, the goal gradient effect—the tendency for completion to elicit more persistence than initiation—will diminish when people must precommit to making goal progress at a specific future date.

H1: Precommitment weakens the goal gradient effect. Specifically, when people must precommit, their motivation to precommit to goal completion and goal initiation will not differ.

Presumably, imagining making progress toward a goal at a specific future date reduces the affect associated with the prospect of making progress and thereby reduces the curvature of the value function. I suspect that asking consumers to precommit to future goal progress highlights the distance between the precommitment date and the progress date and unfortunately attenuates the goal gradient effect. However, the deleterious consequence of a time lag is not inevitable because perceptions of time are subjectively malleable. Perceptions of the temporal distance until goal progress can vary regardless of the objective time lag (Le Poidevin 2015).

Contracting the precommitment-progress lag, even just subjectively, might restore affect, value function curvature, and consequently the goal gradient effect.

Research on time perception offers numerous levers for contracting subjective time. For instance, time perception is influenced by the number of events occurring during an interval (May 2017), whether the time period is "bounded" by a concrete event (Tonietto et al. 2019), or whether there is a causal relationship between two events (Faro, Leclerc, and Hastie 2005; Faro, McGill, and Hastie 2010). I believe that perceived time, not simply objective time, is what matters in goal progress contexts. That is, even if an objective time lag remains constant, events that feel closer will trigger more affect than those that feel farther away and will thus generate more curvature in the value function. Figure 2.1 illustrates this theory. The solid line represents a classic value function, while the dotted line shows a value function for future events. I theorize that the extent of a perceived time lag until future goal progress will influence the curvature of the value function: the shorter the perceived lag, the greater the curvature of its value function will be (as in the solid line).

These depictions of shorter versus longer perceived lags align with differences in value function curvatures for decisions involving strong versus weak affect (Hsee and Rottenstreich 2004; Kurt and Inman 2012). Note that the slope of the value function gets steeper as it reaches the reference point. This means that the same amount of goal progress is less valuable when precommiting to goal initiation (the left side of the function) than it is when precommiting to goal completion (the right side of the function). However, the slope is relatively more constant (monotonic) for the longer perceived-time-lag function. This means that at the time of precommitment, the same amount of goal progress is equally valuable at goal initiation and goal completion when people imagine it occurring in the distant future. Thus, different levels of the

curvature in functions for a longer versus a shorter perceived time lag have different implications for the goal gradient effect in precommitment contexts.

Building on my theorizing about changing curvatures in the value function, I further test the goal gradient effect for precommitments by comparing shorter and longer perceived time lags until goal progress. Specifically, I examine the effects of bounding time lags on participants' perceptions of time until goal progress and thus on the goal gradient effect. Bounding a time period with a new event contracts its perceived length: Conference attendees who planned to attend a presidential address later in the day considered the available time until the address to be shorter than those who planned not to attend (Tonietto et al. 2019). The presidential address provided a hard stop to the preceding time period, creating a salient conclusion to the current time flow.

Even when nothing is scheduled, time can feel naturally bounded simply by how I define it. For example, every hour ends when it rolls over to the next hour. Days end when nights begin. February has ended when March begins. Thus, a time lag may feel bounded simply because it is followed by a new time period. While past work has not explored such abstract boundaries and has focused on commitments to engage in a task during a bounded or unbounded time period, it stands to reason that perceptions of a time lag (the time *until* the opportunity to act) will show similar effects. That is, people might feel that opportunities to make goal progress are closer if they occur after a bounded time lag than if they do not. This contraction of time for a naturally bounded (vs. unbounded) time lag will increase consumers' precommitment to completing their goals.

H2: Naturally bounding a time lag will shorten the perceived length between precommitment and goal progress.

H3: Bounding a time lag will moderate the goal gradient effect for precommitment.

- H3a: Naturally bounding a time lag will restore the goal gradient effect for precommitment because it contracts perceived time and elicits affect, restoring the convexity in the value function. Specifically, people will precommit more to completing than to initiating goal progress opportunities that follow a bounded time lag.
- H3b: Using an unbounded time lag will weaken the goal gradient effect for precommitment because it does not contract time and reduces affect, removing the convexity in the value function. Specifically, people will not precommit more to completing than to initiating goal progress opportunities that follow an unbounded time lag.

In summary, this chapter examines whether a time lag between precommitment and action attenuates the goal gradient effect, and how it might be perceptually contracted to restore the goal gradient effect even for precommitment. I predict that because of the time lag that exists between dates of precommitment decision and goal progress, precommitting to completion will not be more motivating than precommittng to initiation (H_1). In Study 1, I test whether a time lag attenuates the goal gradient effect for precommitment. To restore the goal gradient effect for precommitment, I bound a time lag to shorten the perceived time lag (H_2). In Study 2, I test this by simply utilizing the rollover to a new month. Furthermore, because nearer events elicit greater affect, goal completion opportunities that are scheduled to occur after a bounded time lag will be more motivating than goal initiation opportunities (H_{3a}), which I test in Studies 3-5b. However, because events evaluated in the future elicit less affect, upcoming opportunities for initiation and completion will be similarly motivating when they will occur after an unbounded time lag (H_{3b}).

I hypothesize and find that a naturally bounded time lag increases precommitment for goal completion more than for initiation.

STUDY 1

In this experiment, I vary the presence of a time lag between precommitment and goal progress. I expect to find an attenuated goal gradient effect for precommitment in line with an affect-poor value function.

Method

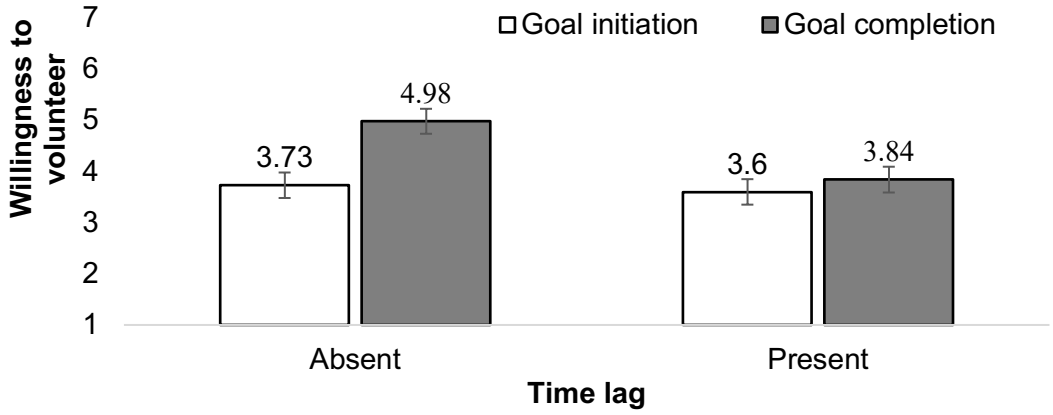
Study 1 was a 2 (time lag: absent, present) x 2 (goal progress: initiation, completion) between-subjects design in the domain of volunteering at a soup kitchen, modeled after Heath et al. (1999). Undergraduates (N = 181, M_{age} = 20.12, SD_{age} = .90, 103 males and 78 females) participated in the study to fulfill course requirements.

Participants imagined that they had decided to donate more time to charity and joined a volunteer community cooperative that encouraged volunteers to commit to working for four consecutive weeks and renewed members' membership every four weeks. Those in the initiation goal progress condition imagined that they had not yet begun volunteering and that they had an opportunity to volunteer at the beginning of the new volunteering cycle. Those in the completion condition imagined that they had already volunteered for three consecutive weeks and that they had an opportunity to volunteer at the end of the current volunteering cycle. Participants in the time lag condition imagined that the current date was June 17 and that they would have to precommit in order to volunteer on June 23. All participants reported their willingness to volunteer on a 1 (extremely unlikely) to 7 (extremely likely) scale.

Results

An ANOVA yielded significant main effects of time lag ($F(1, 177) = 6.54$, $p = .011$) and degree of goal progress ($F(1, 177) = 8.94$, $p = .003$). Participants in the goal completion stage were more willing to volunteer than those in the initiation stage, consistent with the goal gradient effect in general (M = 4.43, SD = 1.81 vs. M = 3.67, SD = 1.60). However, participants were less willing to volunteer when a time lag was present than when it was absent (M = 3.72, SD = 1.74 vs. M = 4.37, SD = 1.71). As predicted, the interaction between time lag and goal progress was significant ($F(1, 177) = 4.08$, $p = .045$; see Figure 2.2). The goal gradient effect was strong in the absence of a time lag ($F(1, 177) = 12.76$, $p < .001$), but in the presence of a time lag, participants' willingness to volunteer in the initiation versus completion conditions did not differ in the presence of a time lag ($F(1, 177) = .46$, $p = .497$), supporting Hypothesis 1.

Figure 2.2. Willingness to volunteer by time lag and goal progress in Study 1. Error bars reflect standard errors of the mean.



Discussion

When participants estimated their likelihood of goal-persistence toward a four-week volunteering goal after having completed either zero or three weeks, the goal gradient effect was strong only when the date of the next volunteering opportunity was ambiguous (as in Heath et al.

1999). However, when the date of goal progress was explicitly lagged, there was no difference between precommitment to initiate progress and precommitment to complete progress. In other words, my findings suggest that nearing goal completion is not particularly motivating for consumers who precommit to goal progress.

The findings are consistent with my theorizing around the value function. In the absence of a time lag, the goal gradient effect that I observed is analogous to the steeper slope near goal completion than initiation, as in the affect-rich, solid value function in Figure 2.1. However, in the presence of a time lag, the goal gradient effect disappeared. Because a time lag reduces the intensity of affect and the convexity of the value function, the value function has more constant slopes across goal progress, as in the affect-poor, dotted value function in Figure 2.1. A time lag thus reduces the differences in persistence toward initiating versus completing goal progress, removing the goal gradient effect. As in Study 1 and the rest of studies, meaningful comparisons are made in persistence (i.e., theorized slopes) between goal initiation and completion within each time lag condition (i.e., the same value function). However, meaningful comparisons between slopes of different value functions at particular goal progress (e.g., comparing the level of persistence toward goal completion when a time lag is present vs. absent) are not feasible without figuring out the intersecting point of two value functions. Determining the exact value function equations is out of the scope of the current research, and thus I do not emphasize the latter comparisons in the analyses.

Given that precommitment helps consumers follow through (e.g., Schwartz et al. 2014) and that the goal gradient effect helps consumers who are near their goal complete it (e.g., Kivetz et al. 2006), ideally both levers could be useful. Thus, in the next set of studies, I focus on restoring the goal gradient effect for precommitment by contracting a perceived time lag.

STUDY 2

In this experiment, I attempted to "bound" the time lag intrinsic to precommitment. I test a new, naturally occurring boundary. Though past research has shown that bounded time (e.g., a time period bounded by a scheduled meeting) leads consumers to perceive less time within that time period, I took a more subtle approach. I simply use dates as a boundary.

To illustrate, consider the volunteering scenario from Study 1. The participant faced a precommitment decision for goal progress that would occur after a few days. The time until the volunteering date represents a lag between the precommitment and the actual goal progress. The goal gradient literature would argue that persistence depends on how much progress has been achieved. Thus, precommitment would be greater after three weeks of volunteering were completed than when no progress had begun. However, Study 1 shows that this asymmetry does not appear when participants are presented with actual dates. I believe this is because the presence of future dates leads to an impression of a substantial time lag until action, which removes affect from the anticipated action. However, I believe I can bound that time lag and thus make it feel shorter. I introduce a subtle, natural bound—a temporal rollover.

Participants in Study 2 were randomly assigned to one of two time lag conditions (unbounded or bounded). They saw the October-November calendars. Those in the unbounded time lag condition imagined that today was October 22th and the action date would be October 26th. Those in the bounded time lag condition imagined that today was October 28th and that the date of interest was November 1st. The 26th does not naturally bound the lag in October. However, the change from one month to the next should make the lag feel bounded. This is

analogous to the unbounded time conditions in Tonietto et al. (2019) in which participants' time (e.g., from 6 pm to 9 pm) was not bounded with a scheduled event (e.g., hanging out with friends).

I believe that a time lag between two dates within the same month represents a continuous flow of time and thus feels subjectively longer than an objectively equivalent time period that has a salient end to its time flow with a month change. Therefore, I intentionally chose the first of the month as the date of upcoming progress rather than the last day of the preceding month. Intuitively, when something new is approaching (like the first of the next month), the intervening end becomes salient. Additionally, because months end on different dates, they may not naturally bound time as well as the 1st of a subsequent month. In fact, when I asked participants in a pretest to indicate the total number of days in each of the 12 months presented in a random order, results showed that only 19 out of 113 participants (M_{age} = 20.86, SD_{age} = .81) were able to correctly report the correct number of days for each month. I predict that merely bridging a change in months with the temporal lag will lead to perceptions of a shorter lag.

Method

Mechanical Turk workers residing in the United States (N = 182, M_{age} = 36.79, SD_{age} = 12.61, 101 males and 81 females) participated in this study in exchange for a nominal fee. They saw the same calendars, but they were randomly assigned to either a bounded or unbounded time lag condition. I measured the perceived length of the time lag using six items on seven-point scales (extremely quickly – extremely slowly, extremely short – extremely long, extremely near – extremely far, ○ – ○○, extremely close – extremely far away, exactly the same – completely different; α = .897). Participants also reported the objective length of the time lag by computing

the number of days left until the action date and rated how difficult this computation was for them (1=extremely difficult, 7= extremely easy).

Results

An ANOVA on perceived time lag as predicted by whether it was naturally bounded or not revealed the expected main effect ($F(1, 180) = 8.95$, $p = .003$). The bounded time lag was perceived as shorter than the unbounded time lag was (M = 2.56, SD = .91 vs. M = 3.05, SD = 1.23), supporting Hypothesis 2. Given that difficult calculations can lead to smaller difference estimates (Thomas and Morwitz 2009), the differences in time perception might be due to a confounding role of mathematical difficulty. However, results were consistent even when I controlled for the perceived difficulty of the calculation ($F(1, 179) = 11.42$, $p = .001$). Finally, as in other work on bounded time perceptions (Tonietto et al. 2019), estimates of objective time lag did not differ by condition ($F(1, 180) = 2.53$, $p = .113$).

Discussion

Although bounded and unbounded time lags did not differ in objective terms, participants *felt* that they did. These results are highly consistent with those of Tonietto et al. (2019) and suggest that the properties of time bounded by a scheduled event (a hard stop) extend to time periods that are naturally bounded in a quite subtle way (e.g., by a new month). In the next study, I test the impact of this temporal contraction on precommitment to goal progress.

STUDY 3

Bounding a time lag makes the lag feel shorter and thus goal progress after that lag more proximate. Decisions for the near term involve more affect than those for the far future

(Bruehlman-Senecal and Ayduk 2015; Chang and Pham 2012; Lee and Tsai 2014; Loewenstein 1996; Metcalfe and Mischel 1999). Consequently, I anticipate bounded time lags to restore the goal gradient effect. Specifically, I hypothesize that the goal gradient effect will be stronger when precommitment decisions occur regarding a bounded than an unbounded lag, consistent with a pattern of participants evaluating goal progress with an affect-rich versus an affect-poor mindset. As a result, the goal completion stage will be more motivating than the goal initiation stage when the time lag between precommitment and action is bounded, but not when the lag is unbounded (as in Figure 2.1).

Method

Mechanical Turk workers residing in the United States (N = 328, M_{age} = 38.75, SD_{age} = 12.59, 180 males and148 females) participated in the experiment in exchange for a nominal fee. They imagined a volunteering scenario at a soup kitchen, adapted from Heath et al. (1999). I manipulated the time lag (unbounded versus bounded) and also the extent of goal progress (initiation versus completion).

Participants imagined it was currently four days before a volunteering date. This date in the bounded (unbounded) time lag condition was February 1st (Feb. 29th). Next, they were told: "Imagine that you have decided to donate more time to charity and joined a volunteer community cooperative. As a member of the cooperative, you have signed on to volunteer at a local soup kitchen each Sunday, although there is, of course, no way for them to enforce your commitment. However, in order to increase continuity and stability, the co-op asks volunteers to commit to working for 4 consecutive Sundays. The co-op renews members' membership every 4 weeks."

Participants in the goal initiation condition imagined that they had not yet begun volunteering and thus the 1st progress date was the start of the new four-week volunteering cycle. Those in the goal completion condition were asked to imagine that they had already volunteered for three consecutive Sundays and that the progress date was the end of the current volunteering cycle. All participants were given a monthly calendar that denoted the upcoming date as either the beginning or end of the volunteering cycle. Participants indicated their likelihood of precommitting to volunteer on the progress date by rating the degree to which they would likely opt out of volunteering at the soup kitchen on the upcoming Sunday (1 = "Extremely unlikely [I will volunteer]", 7 = "Extremely likely [I won't volunteer]"). They also rated the perceived distance between today and the volunteering date on three seven-point scales ([approaching] very quickly-very slowly, very short-very long, very near-very far; $\alpha = .902$). As an attention check, participants were asked to recall whether they were beginning or ending a four-week volunteering cycle on the progress date.

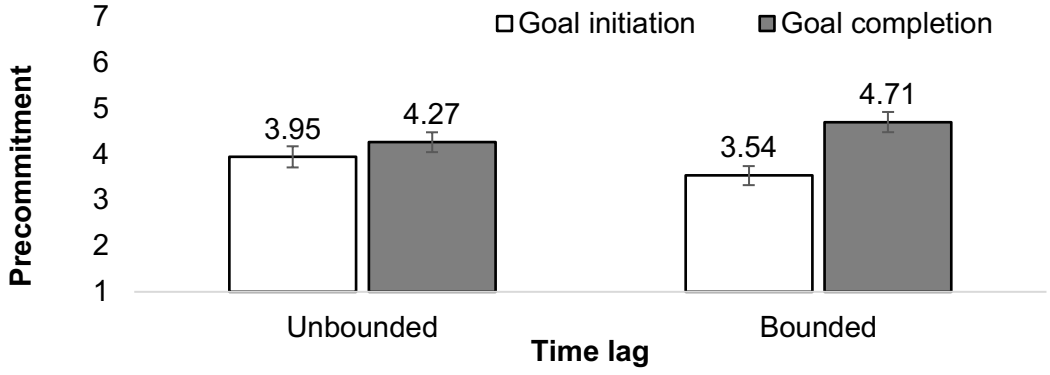
Results

Seventeen participants who failed the attention check question were removed from the analysis, leaving 311 participants. The main dependent variable was reverse coded so that higher values represented greater precommitment to volunteering. I predicted that when the time lag until the progress date was not bounded (and thus seemed long), participants would show a weak goal gradient effect. When the time lag was naturally bounded by the month rollover, however, participants would perceive the lag as shorter and show a strong goal gradient effect, in line with an affect-rich, convex value function.

I tested these hypotheses with an ANOVA predicting the effects of goal progress (initiation or completion) and time lag (naturally bounded or not) on perceived time until the

progress date and precommitment to volunteering. Replicating Study 2, the main effect of time lag was significant on perceived time ($F(1, 307) = 5.33$, $p = .022$). As predicted in Hypothesis 2, perceived time lag until the action date was contracted when it was naturally bounded with a month rollover relative to when it was not (M = 2.29, SD = .94 vs. M = 2.56, SD = 1.07, respectively). Replicating past goal gradient literature, the main effect of goal progress on precommitment to volunteering was significant ($F(1, 307) = 11.51$, $p = .001$): Nearing completion led to greater precommitment than did goal initiation (M = 4.47, SD = 1.87 vs. M = 3.74, SD = 2.00). However, this effect was qualified by the predicted significant interaction of goal progress and time lag ($F(1, 307) = 3.75$, $p = .054$; see Figure 2.3). When the next volunteering date was not after a naturally bounded lag, the degree of goal progress did not affect precommitment ($M_{completion} = 4.27$, $SD_{completion} = 1.82$ vs. $M_{initiation} = 3.95$, $SD_{initiation} = 2.09$; $p = .293$). However, when the next volunteering date was after a naturally bounded time lag, the completion (M = 4.71, SD= 1.92) rather than the initiation (M = 3.54, SD= 1.90) of goal progress resulted in greater precommitment to volunteer ($F(1, 307) = 13.63$, $p < .001$). That is, the goal gradient effect persisted only when the volunteering date followed a bounded time lag. These results Hypothesis 3.

Figure 2.3. Precommitment by time lag and goal progress in Study 3. Error bars reflect ±1 standard error of the mean.



To test the proposed process that naturally bounding a time lag contracted the delay between a precommitment decision and a progress date, I looked at the mediating role of perceived length of time in the relationship between type of time lag (0 = unbounded, 1 = bounded) and precommitment given goal progress (moderator: 0 = initiation, 1 = completion). A PROCESS model 15, with 5,000 bootstrapped samples and a 95% confidence interval revealed that the moderated mediation was significant, supporting my hypothesis (b = .16, SE = .09, 95% CI = [.02, .37]). The indirect effect of the lag manipulation (bounded or unbounded) on likelihood of precommitting was significant at initiation (b = -.13, SE = .07, 95% CI = [-.28, -.02]) but not at completion (b = .03, SE = .05, 95% CI = [-.04, .15]). That is, the slopes of the value functions between the naturally bounded versus unbounded lag conditions (the solid versus dotted lines respectively in Figure 2.1) differed depending on whether the upcoming goal progress was at initiation or completion. Differences in the steepness of slopes capture differences in the subjective value of equivalent goal progress: In the goal initiation condition, longer perceived time due to the unbounded lag (vs. bounded) increased precommitment. In the goal completion condition, the differences in the slopes of the dotted and solid lines near the reference point in Figure 2.1 were modest. More importantly, the contrast between the indirect effects at completion and initiation was significant (*contrast* = .16, SE = .09, 95% CI = [.02, .36]), showing that shorter perceived time due to the bounded (vs. unbounded) time lag increased precommitment more at completion than at initiation. This supports Hypothesis 3 that bounding a time lag between precommitment and action on goal progress strengthens the goal gradient effect by contracting perceived time.

Discussion

These results illustrate that evaluating upcoming progress that follows a naturally bounded time lag restores the goal gradient effect. This is because bounding a time lag leads to temporal contraction which increases participants' propensity to favor persistence near completion over initiation.

I used a subtle, naturally occurring boundary—a new month. I argue the first of a new month is a better way to close the door on the previous month than the last day of the month because there is no ambiguity about the completion of the month. But one might suspect that the rollover to a new month is not so much about bounding the preceding time period as signifying a "fresh start." Indeed, research finds that salient dates can make people feel as if they can turn over a new leaf on that date and start over (Dai, Milkman, and Riis 2015; Peetz and Wilson 2013). This in turn increases people's motivation to initiate goal progress, as long as it is made quite salient that it is an opportunity for a fresh start (Dai et al. 2015). The results of Study 3 disconfirm this alternative account. If participants perceived the new month as a fresh start, they should have shown more of a tendency to commit to volunteering at the initiation stage than those without the naturally bounded lag. I do not see this pattern, suggesting that either fresh start does not play a role, or that even if it does, it is overwhelmed by the magnitude of the opposing effect of perceived time lag.

Study 3 used a volunteering scenario that might elicit effects on precommitment that are attributable to participants' prosocial inclinations. In fact, prior research has identified latent tendencies toward a fresh start mindset, and those who have stronger beliefs tend to be more prosocial (Price et al. 2017). It is possible that the 1st of a month elicits stronger fresh start mindsets than other days of a month among a subset of participants who tend to see opportunities

for fresh starts. Those with strong fresh start mindsets might even simply see each volunteering opportunity as a fresh start. These possibilities could partially explain or potentially simply weaken the effects I have attributed to precommitment, the goal gradient effect, and bounding. In the next study, I try to disentangle my account from the potentially countervailing (and thus dampening) effect of latent fresh start mindset by controlling for the propensity to see a fresh start.

STUDY 4

This study was designed to increase the realism of the previous scenarios by asking participants about dates that were four days away from the actual experiment date rather than imaginary dates, but also to control for potentially dampening of the effects stemming from individual differences in fresh start mindset. Although a fresh start mindset might have an independent impact on propensity to precommit to volunteering, I expect it to be a distinct effect from the impact of goal progress and time lag boundedness.

Method

Study 4 was a 2 (goal progress: initiation, completion) x 2 (time lag: bounded [July 1st], unbounded [July 15th and July 29th]) between-subjects experiment, collected in three waves (June 28th, July 12th, and July 26th). Three hundred and one people (M_{age} = 31.84, SD_{age} = 17.60, 136 males and 165 females) residing in the United States participated using their non-mobile device via the online research platform Lucid in exchange for a nominal fee.

Though I included 2 unbounded goal progress dates (July 14th and 29th), the rest of the procedure was similar to that of Study 3. Participants were informed that the study was about

how consumers manage their schedules. Those in the goal initiation and bounded [unbounded] time lag condition imagined that they had not yet begun volunteering on Sundays and that July 1st [15th or 29th] was the start of the new volunteering cycle. Those in the goal completion and bounded [unbounded] time lag condition imagined that they had already volunteered for three consecutive Sundays and that July 1st [15th or 29th] was the end of the current volunteering cycle. As in the previous studies, participants were provided with a monthly calendar that marked the action date as the beginning or end of the volunteering cycle.

I measured precommitment by asking participants to rate their willingness to volunteer on the action date using two items—the degree to which they were likely to volunteer and to opt out of volunteering on the action date—each on a seven-point scale. Finally, participants completed the six-item fresh start mindset scale (Price et al. 2017).

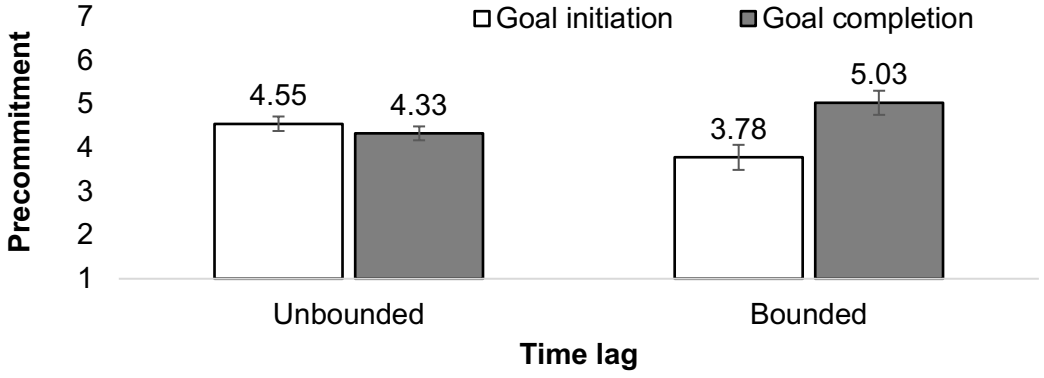
Results

The opt out response was reverse-coded and averaged with the other precommitment measure ($\alpha = .779$). Unsurprisingly, because neither the 15th nor the 29th naturally bounds the time lag, precommitment did not differ between these dates for either initiation ($M_{15th} = 4.44$, $SD_{15th} = 1.88$ vs. $M_{29th} = 4.67$, $SD_{29th} = 1.70$; $p = .501$) or completion ($M_{15th} = 4.21$, $SD_{15th} = 1.64$ vs. $M_{29th} = 4.38$, $SD_{29th} = 1.77$; $p = .602$), and thus they were collapsed.

An ANCOVA showed that the covariate fresh start mindset significantly increased precommitment to volunteering ($F(1, 296) = 11.74$, $p = .001$), replicating past work that greater fresh start mindset increases prosocial behavior (Price et al. 2017). Despite the large main effect of fresh start mindset on goal persistence, I also replicated my previous findings. Specifically, the main effect of goal progress was strong after controlling for the individual difference ($F(1, 296) = 5.01$, $p = .026$). Importantly, the goal gradient effect was qualified by a significant

interaction of goal progress and time lag ($F(1, 296) = 10.26$, $p = .002$; see Figure 2.4), replicating Study 3. Even after controlling for participants' latent fresh start mindsets, I again found a goal gradient effect only when the time lag until the volunteering date was naturally bounded ($F(1, 296) = 9.87$, $p = .002$). The goal gradient effect weakened when the time lag was unbounded ($F(1, 296) = .94$, $p = .334$). In other words, participants were more willing to precommit to completing than to initiating a volunteering cycle only when the time lag until the volunteering date was bounded (M = 5.03, SE = .17 vs. M = 3.78, SE = .16). When the volunteering date followed the unbounded time lag, however, precommitment to completion was not significantly greater than precommitment to initiation (M = 4.31, SE = .29 vs. M = 4.56, SE = .28). Results were nearly identical when fresh start mindset was not included as a covariate (i.e., interaction remained significant with $F(1, 297) = 10.86$, $p = .001$).

Figure 2.4. Precommitment by time lag and goal progress in Study 4. Means are adjusted means controlling for fresh start mindset, and error bars represent ±1 standard error of the mean.



Discussion

As in Study 3, only when progress was scheduled after a bounded time lag did a goal gradient effect appear. This effect was robust, holding when real dates were used and when individual differences were controlled for. I have shown consistent findings by comparing time

lags that are naturally bounded versus unbounded. Despite consistent findings, the effect I am documenting may be due to my operationalization of a bounded time lag. Although I used the 1st of a month to indicate that the time lag was bounded, I did not explicitly highlight the progress date as "the new month" in either verbal or visual formats.

In the next study, I further probe the potential role of fresh starts in my research contexts and use the underlying mechanism of perceived time lag as a moderator. If contracting a perceived time lag plays a critical role in restoring the goal gradient effect for precommitment, as I have proposed, a bounded time lag that feels longer should not restore the goal gradient effect. Consider the work of Tu and Soman (2014). They showed that perceptually bounding a time period separates time into two categories (e.g., by creating a visual block around a week to separate one week from the next). Such bounding of time can make the next category feel like a salient fresh start and can influence motivation for goal initiation. I predict, therefore, these heavy-handed bounding will have a result that differs from natural bounding. That is because the mechanisms differ: Natural boundaries decrease a perceived time lag while perceptual boundaries increase a perceived time lag. I therefore compare a time lag that is naturally bounded to an equivalent lag that is also bounded by a perceptual border that splits time into multiple categories. I present participants with the same dates but perceptually separated them by presenting two separate monthly calendars in the perceptual boundary condition. Consumers are often presented with different types of calendars where dates of multiple months are represented in a continuous format or separated in a monthly format. I utilize these different ways of visualizing dates in the next study to compare the power of naturally bounding a time lag versus highlighting a bounded time lag that creates two distinct time periods.

STUDY 5A

Thus far, I have subtly bounded a time lag by using the rollover to a new month. I have shown that the goal gradient effect in precommitment is stronger after a bounded than an unbounded temporal lag because the perceived distance until the progress date is contracted. Studies 5A and 5B provide more support for my proposition that only when the lag feels shorter does the goal gradient effect appear in precommitment. That is, boundaries in themselves are not sufficient to restore the goal gradient effect. They must produce a temporal contraction that would restore the curvature in the value function.

To test this hypothesis, I examine a new temporal boundary manipulation. This one makes the rollover from one month to the next visually salient. Perceptually, the day of action seems spatially distant from the day on precommitment. Notably, perceptually bounding time in this way should have the opposite effect on perceptions of lag and goal persistence from naturally bounding time. Greater spatial distance increases perceived temporal distance (Kim, Zauberman, and Bettman 2012). Even when physical distance is held constant, the perceived distance between two objects is greater if they belong in different perceptual units (Coren and Girgus 1980). Specifically, individuals tend to separate a salient fresh start or a new beginning from their current temporal category. As a result, they imagine themselves at that future fresh start/new beginning to be distinct from their current self (Dai et al. 2015; Peetz and Wilson 2013). Thus, a perceptual boundary will exacerbate the time lag effect on goal progress, consistent with an affect-poor value function.

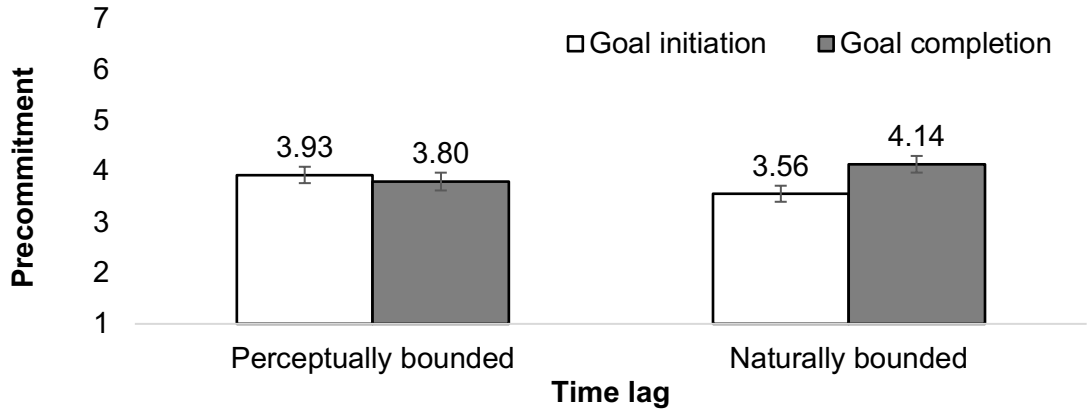
Recall that the longer a lag until goal progress, the less affect rich (curved) the value function is. Only in the presence of a time lag that is more subtly and naturally bounded do I

expect to find a goal gradient effect for precommitment because the lag feels shorter and makes the decision more affect-rich. I test this prediction in Study 5A. As in Study 4, I ask about a volunteering date that is just a few days beyond the experiment date and control for participants' fresh start mindsets.

Method

Study 5A was a 2 (goal progress: initiation, completion) x 2 (time lag: naturally bounded, perceptually bounded) between-subjects experiment. Mechanical Turk workers (N = 444, M_{age} = 37.13, SD_{age} = 12.05, 237 males and 207 females) residing in the United States participated on August 30 in the experiment in exchange for a nominal fee. The scenario was adapted from my previous experiments, but this time, all participants imagined that the volunteering date was September 1. Participants in the goal initiation condition imagined that they had not yet begun volunteering on Saturdays and that September 1 was the start of the new four-week volunteering cycle. Those in the completion condition imagined that they had already volunteered for three consecutive Saturdays and that September 1 was the end of the current volunteering cycle. I provided monthly calendars that marked September 1 as the beginning or end of the volunteering cycle. Critically, the calendars differed between the two time lag conditions (see the Appendix for the stimuli). In the naturally bounded condition, participants saw no visual break between August and September. Participants in the perceptually bounded condition saw a calendar that had a clear visual break between the August and September calendars. I measured precommitment to volunteering using two items with seven-point scales (likelihood of volunteering and opting out of volunteering at the soup kitchen on September 1). Participants answered a six-item fresh start mindset scale (Price et al. 2017).

Figure 2.5. Precommitment by time lag and goal progress in Study 5A. Means are adjusted means controlling for fresh start mindset, and error bars represent ±1 standard error of the mean.



Results

The precommitment variables were combined as before (α = .887). An ANCOVA showed once again that the fresh start mindset covariate significantly increased precommitment to volunteering ($F(1, 439) = 5.52$, $p = .019$). Neither the main effect of goal progress nor time lag was significant, however, suggesting no goal gradient effect overall. Critically, the interaction of goal progress and time lag was significant ($F(1, 439) = 4.80$, $p = .029$; see Figure 2.5). As predicted, even after controlling for participants' latent fresh start mindset, the goal gradient effect was strong only when the progress date followed a naturally bounded time lag ($F(1, 439) = 6.59$, $p = .011$). Participants precommitted more for completing than for initiating a volunteering cycle when the progress date followed a naturally bounded time lag (M = 4.16, SE = .16 vs. M = 3.54, SE = .18). On the other hand, when the progress date (September 1) was also perceptually separated from the current date by a visual break, participants' precommitment to initiation and completion did not differ (M = 3.93, SE = .17 vs. M = 3.80, SE = .16; $p = .571$). In other words, while the naturally bounded time lag produced results similar to those of my previous experiments, the perceptually bounded time lag condition produced results similar to those

observed for an unbounded time lag. The results were nearly identical when I did not include fresh start mindset as a covariate (i.e., interaction remained significant with $F(1, 440) = 4.82$, $p = .029$).

Discussion

I replicated my previous findings in the naturally bounded time lag condition. The results of Study 5A showed a strong goal gradient effect only among participants who made precommitment decisions for a future date that was only bounded by the rollover, not also perceptually bounded. These results support my theorizing that a contracted temporal lag is a necessary factor of a goal gradient effect when people must commit to goal progress for a time in the future.

STUDY 5B

Study 5A showed a stronger goal gradient effect in precommitments for a naturally bounded, and not for a perceptually bounded, time lag. Now, I test whether these differences are indeed driven by the contracted perceived time and intensified affect caused by naturally bounding a time lag. I simplify the design and explore only the goal completion condition from Study 5A.

Method

Participants were randomly assigned to either a naturally bounded or perceptually bounded time lag condition. Mechanical Turk workers ($N = 165$, $M_{age} = 42.77$, $SD_{age} = 12.88$, 85 males and 80 females) participated in the experiment in exchange for a nominal fee. The scenario was adapted from Study 5A. The scenario and measures were identical to the completion

conditions of Study 5A. However, I also measured participants' perceived distance between today and the progress date (extremely near-extremely far, ◯ – ◯◯; α = .663) and the intensity of affect ("From today, how weak or strong are your emotions when you are imagining volunteering on September 1?"; extremely weak-extremely strong). Finally, participants answered a six-item fresh start mindset scale (Price et al. 2017).

Results

The precommitment variables were combined as before (α = .915). An ANCOVA showed once again that fresh start mindset significantly increased precommitment to volunteering ($F(1, 162) = 4.21$, $p = .042$). Fresh start mindset also significantly intensified the affect participants felt when they imagined the future goal completion ($F(1, 162) = 5.78$, $p = .017$). Importantly, the main effect of time lag was significant on precommitment ($F(1, 162) = 6.16$, $p = .014$), perceived time lag ($F(1, 162) = 5.43$, $p = .021$), and emotional intensity ($F(1, 162) = 5.87$, $p = .017$). As predicted, even after controlling for participants' latent fresh start mindset, participants precommitted more when the volunteering completion date followed a naturally bounded time lag than a perceptually bounded time lag (M = 4.87, SE = .20 vs. M = 4.19, SE = .19). A naturally bounded time lag contracted perceived time between today and the completion date ($M_{naturally} = 2.10$, $SE_{naturally} = .11$ vs. $M_{perceptually} = 2.45$, $SE_{perceptually} = .11$) and intensified participants' affect as they thought about goal completion ($M_{naturally} = 4.63$, $SE_{naturally} = .15$ vs. $M_{perceptually} = 4.11$, $SE_{perceptually} = .15$).

To test the mediating roles of perceived time lag and emotional intensity in the impact of time lag (0 = perceptually bounded, 1 = naturally bounded) on precommitment to completing a volunteering cycle, I conducted a serial mediation test using PROCESS Model 6, with 5,000 bootstrapped samples and a 95% confidence interval. Results revealed that the serial mediation

was significant (b = .06, SE = .04, 95% CI = [.001, .14]). In other words, a naturally (vs. perceptually) bounded time lag contracted perceived time between today and the progress date, which intensified affect when participants thought about completing a volunteering cycle on September 1 and subsequently increased precommitment to completing a volunteering goal. The pattern of results was nearly identical when I did not include fresh start mindset as a covariate (i.e., the serial mediation remained significant with b = .06, SE = .04, 95% CI = [.001, .14]).

Discussion

Like an unbounded time lag, a perceptually bounded time lag did not contract time. Study 5B replicates earlier finding that only a naturally bounded time lag contracts perceived time between precommitment and goal progress. Importantly, Study 5B also shows that this contracted time lag indeed intensifies affect about the future and thereby increases the likelihood of precommitting to future goal progress. These results support my theorizing that perceived time lag affects the curvature of the value function in line with an affect-rich context, with important consequences for the goal gradient effect.

GENERAL DISCUSSION

Results from six studies show that the goal gradient effect is absent in precommitment. Precommitment to goal persistence depends on how proximate the progress date feels to individuals—the farther in the future it feels, the less likely it is that the goal gradient effect will appear. This is because decisions about the future involve little affect, which reduces the curvature of the value function that underlies the goal gradient effect. Subtle interventions, such

as naturally bounding a time lag with a month rollover, that reduce the perceived length of the lag can increase affect and restore the goal gradient effect.

My research makes important theoretical contributions to the literatures of precommitment, goals, and bounded time. To begin with, in the precommitment literature, past research has predominantly focused on studying positive effects of precommitment on consumers' follow-through. For example, precommitment motivated consumers to quit smoking (Giné et al. 2010), increase savings (Ashraf et al. 2006; Thaler and Benartzi 2004), and purchase healthier groceries (Schwartz et al. 2014). Rather than studying the positive consequences of precommitment, however, I examine factors that influence precommitment decisions and conditions under which precommitment may hurt consumers.

First, goal progress significantly influences precommitment, and goal pursuers who are close to goal completion may not benefit from precommitment. Marketers often repeatedly ask consumers to precommit at different stages of their goal pursuit (Schaefer et al. 2018). For example, a health enthusiast who has a goal of attending yoga classes may face a weekly precommitment decision of whether to sign up for an upcoming class. My findings show that goal pursuers who near goal completion are less likely to precommit than those who are at the beginning of goal pursuit. Given that precommitment increases consumers' follow-through (e.g., Schwartz et al. 2014), my findings suggest that precommitment may not benefit consumers who are close to completing their goals.

Second, I extend the notion that a time lag is a critical component that determines consumers' likelihood of precommitting, but with a cautionary perspective. A time lag is inherent to precommitment. Without precommitment and thus without a time lag, consumers feel the pain of taking challenging action at the moment of making goal progress. On the other hand,

precommitment helps consumers fight off self-control problems because a time lag that exists between precommitment and action reduces negative affect consumers may have toward making challenging progress (Thaler 1980). A time lag helps consumers focus on their goal rationally. Thus, a time lag has been perceived to motivate consumers to precommit to a goal. My research cautions that a time lag does not always motivate precommitment to any goal progress. Just as a time lag reduces negative affect associated with making challenging progress, for consumers who are near goal completion, it also reduces negative affect associated with failing goal completion. Because of a close relationship between time and affect, my research clarifies how goal progress shapes the direction to which a time lag influences precommitment decisions.

A close examination of the effect of perceived time lag on precommitment to goal progress advances our understanding of the goal gradient effect. I build on Heath et al.'s (1999) use of the value function in explaining the goal gradient effect by extending their contexts to those that include time and affect. The goal gradient effect is neatly explained by the curvature in the value function, which has more curvature in affect-rich than in affect-poor contexts (Dhar and Wertenbroch 2000; Hsee and Rottenstreich 2004; Kurt and Inman 2012). As events that occur at a more proximate time in the future elicit greater affect (e.g., Matherly et al. 2019), I theorized that a time lag between precommitment and future goal progress reduces the curvature of the value function. Indeed, my findings show that consumers are no longer more motivated to complete (vs. initiate) a goal when they have to precommit to it in advance. In line with my proposition that a perceived time lag influences the curvature of the value function, I show that naturally bounding a time lag contracts the perceived length of the lag and restores the goal gradient effect.

Finally, my research extends past work on bounded time in three ways. First, I look at subtly bounded time rather than explicit "hard stops" (Tonietto et al. 2019) or landmark boundaries that cause categorical breaks like birthdays (e.g., Dai, Milkman, and Riis 2014; Tu and Soman 2014). Second, I explore whether temporal contraction affects the decision to commit to goal progress *after* a time lag in a similar way that it affects propensity to engage in a task during the lag itself (in contrast to Tonietto et al. 2019). Finally, I explicitly focus on the goal gradient effect and thus vary the degree of progress that has already been made toward a goal. That is, I look not only at the propensity to opt into a new task (as in Tonietto et al. 2019), but also at the propensity to complete an existing task (as in Tu and Soman 2014).

Importantly, my studies complement past work on bounded time. For example, participants in Tonietto et al.'s (2019) experiments appeared to worry that they would not complete a task during the bounded time period. Research on the goal gradient effect suggests that nearing but not achieving task completion is particularly painful, as it reflects a steep area in the value function (loss aversion). Thus, it is possible that participants felt that initiation (which has little payoff according to the value function) was demotivating. Indeed, when tasks were presented such that completion seemed certain (e.g., by dividing one task into multiple shorter tasks), participants showed a greater tendency to opt into the task. Though I explored precommitment to initiation and completion *after* the bounded period rather than within it, I believe that my work sheds some light on these past results.

My findings have practical and managerial implications for marketers. Given that precommitment gives marketers opportunities to increase profits (e.g., by requiring a down payment or a ticket purchase whether or not consumers ultimately follow through), marketers can manipulate the duration of the consumption window to increase consumers' likelihood of

precommitment. Specifically, my findings imply that consumers are more motivated to precommit to goal completion than to goal initiation when the time lag between precommitment and the goal progress date is shorter. On the other hand, their motivation to precommit to initiation increases when they feel that the progress date belongs to a new time period (i.e., the time lag feels longer). This implies that if dates are externally imposed (e.g., if a personal trainer designs a client's workout and dietary schedule), marketers can actively utilize different sets of time points for initiation and completion. Specifically, for consumers who are near their final consumption stage (e.g., attending the last yoga class), marketers can benefit by contracting the time lag between sign-up dates and consumption dates. For consumers who are near their initial consumption stage (e.g., attending the first yoga class), marketers can benefit by lengthening the time lag between sign-up dates and consumption dates. In addition, online retailers like Groupon and LivingSocial that frequently re-offer previously successful products might benefit by contracting (vs. lengthening) the consumption window for products that consumers purchase for the last (vs. first) time.

In addition to manipulating the actual time lag or consumption window, marketers can use subtle visual cues to change how the equivalent lag feel to encourage precommitment. For example, marketers can increase consumers' motivation to precommit to goal progress by presenting an action date as occurring after an unbounded or bounded time lag. For apps or websites that help individuals achieve their goals, providers can customize how the dates used in reminders look depending on whether consumers are in their initiation or completion stage. For consumers who are approaching goal initiation, marketers can perceptually separate the potential action dates from the current time period (e.g., by creating a visual border between the two time periods). For consumers who are approaching goal completion, providers can ask consumers to

choose as their deadlines dates that belong in the same category (e.g., within this month) or that fall just after naturally bounded periods.

Limitations and Future Research

My findings have consistently shown that when people must precommit to goal progress at a later date, a time lag that feels shorter strengthens the goal gradient effect. I operationalized time lag by unbounding, naturally bounding, and perceptually bounding it and showed that only naturally bounding a lag by a month rollover strengthens the goal gradient effect. When that temporal lag is not bounded, or when it is bounded but is also perceptually separated from the progress date, the goal gradient effects is not present. I used a variety of dates (e.g., October 26, February 29, July 15, and July 29), and most of the dates I used in the unbounded temporal lag condition were near the end of a month. Because research has suggested that greater processing fluency increases persuasion (e.g., Lee and Aaker 2004), one might speculate that approaching goal completion towards the end (vs. the beginning) of a month would enhance processing fluency and be more motivating. Although my findings suggest that motivation to precommit to completion is not significantly greater than motivation to precommit to initiation for dates framed as unbounded time lags, it is possible that the impact of affect-poor decision making is simply overwhelming these effects. Future research might try to detect the opposing effect of processing fluency hypothesized by other researchers.

Although the last day of a month does not naturally bound a time lag as much as the first day of the subsequent month, it is possible to make the end of a month (e.g., February 29th) salient as bounded and thereby strengthen the goal gradient effect. Studies 5A and 5B showed that creating a perceptual separation between months leads to a categorization effect: people presumably perceived the 1st as belonging to a different temporal category than the present

category. This boosted their precommitment to goal initiation in the future on the 1st. Likewise, it is plausible that such visual separation can lead people to perceive the final date of a month as belonging to the current temporal category and reduce the perceived temporal distance between the present and the month's end. Future research on the impact of bounded time on the goal gradient effect can further examine bounding time using a month rollover and broaden the operationalization of bounding time by exploring other types of naturally bounded time periods. Besides bounding time, other ways to manipulate perceived time or affect can further be used to test the effect of a time lag on goal gradient effects in precommitment.

Finally, despite the consistency of findings, one might wonder why having a time period naturally bounded by the 1st of a month did not increase the feeling of "fresh starts" in my studies and increase precommitment for initiation. I argue 1) that it might have, but that the effect I have documented outweighs the fresh start effect, and 2) that this may be due to my operationalization of a bounded time lag. Related to the latter point, when I explicitly highlighted the progress date as "the new month" by using perceptually separated calendars, people's motivation to precommit to initiate goal progress increased. This suggests that participants might have perceived the date as a fresh start. Future research could examine whether and how latent fresh start mindsets and priming a fresh start time period affect motivation and behavior, particularly in precommitment.

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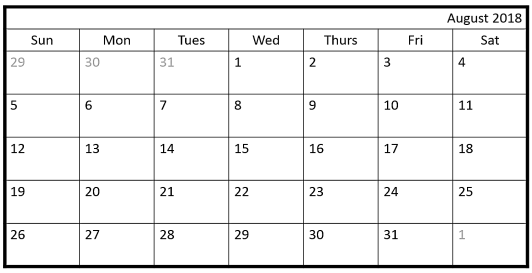
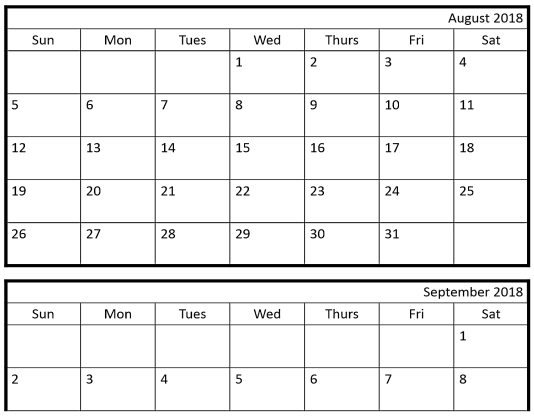
APPENDIX

STIMULI FOR STUDY 5A



August 2018

Sun	Mon	Tues	Wed	Thurs	Fri	Sat
29	30	31	1	2	3	4
5	6	7	8	9	10	11
12	13	14	15	16	17	18
19	20	21	22	23	24	25
26	27	28	29	30	31	1



August 2018

Sun	Mon	Tues	Wed	Thurs	Fri	Sat
			1	2	3	4
5	6	7	8	9	10	11
12	13	14	15	16	17	18
19	20	21	22	23	24	25
26	27	28	29	30	31	

September 2018

Sun	Mon	Tues	Wed	Thurs	Fri	Sat
						1
2	3	4	5	6	7	8

Notes: The calendar format on the left (right) was used for the naturally bounded (perceptually bounded) time leg condition. September 1st was marked as either the start or the end of the volunteering cycle.

CHAPTER 3

Cultural Orientation Differentially Impacts the Effectiveness of Descriptive Norm Appeals to Promote Prosocial Behavior

"Freedom over fear," said one protester's sign in a protest against the stay-at-home order during the COVID-19 pandemic (Yan 2020). As of June 2020, the COVID-19 pandemic has taken more than 500,000 lives across the world (World Health Organization 2020). Although everyone around the globe shares the goal of fighting against the pandemic, cross-cultural differences have emerged in how people adopt and respond to new norms in society. While the number of COVID-19 cases initially spiked in Eastern countries, these countries were able to flatten the curve. People in countries like South Korea showed strong conformity to the norms of facial coverings, social distancing, and voluntarily testing (McCurry 2020).

On the other hand, in Western countries, where independent cultural orientation dominates, the number of COVID-19 cases rapidly increased and resulted in relatively high death rates. People in various parts of the United States protested against stay-at-home orders, demanding freedom (Yan 2020). While acknowledging that there are undoubtedly multiple explanations for these cross-cultural differences, I want to underscore the need for an improved understanding of the mechanism by which norms influence prosocial behavior across different cultures and decision contexts. I address this need by examining people's susceptibility to descriptive norms through the lens of what is widely acknowledged as a core dimension of cultural variability, independent or interdependent self-construal.

Prior findings have documented the ways in which social norms guide and influence prosocial behavior in a variety of domains including recycling, reusing hotel towels, and purchasing sustainable products (e.g., Cialdini, Reno, and Kallgren 1990; Goldstein, Cialdini, and Griskevicius 2008; Melnyk et al. 2013). Normative appeals often contain descriptive norms, which provide information about how most people would behave in a particular setting (Cialdini, Reno, and Kallgren 1990). The situation-specific nature of the normative information communicates what others in the same situation do and thereby suggests the appropriate behavior. Whereas extensive research has shown the persuasive appeal of descriptive norms compared to non-normative messages (e.g., Nolan et al., 2008; White and Simpson 2013), research on the normative influence of different reference groups has produced mixed results.

A preponderance of evidence exists that a psychological affiliation with the reference group is necessary to trigger conformity to group norms (e.g., Harmeling et al. 2017; Turner 1991). When individuals lack a shared perception of collective identity with the reference group, they are not motivated to conform to group norms (Morgan and Laland 2012). The persuasive effect of normative appeals on behavior increases when the reference group is more important to the target individual. Indeed, much of the literature on social norms has focused on the normative influence exerted on target individuals by groups they socially and personally identify with (Cialdini and Trost 1998). These studies point to the meaningfulness of a reference group to one's social identity as a critical determinant of normative influence.

However, the emphasis on the shared collective identity is seemingly at odds with studies that have compared socially relevant versus situationally relevant reference groups. For example, in an influential study conducted in hotel settings by Goldstein, Cialdini, and Griskevicius (2008), a persuasive appeal conveying the norms of a group relevant to the situation (other hotel

guests who had stayed in the same room) increased consumers' towel reuse more than the norms of a group whose social identity was meaningful to them (fellow citizens). In other words, normative information about a relatively unimportant reference group that was relevant to the targets' immediate circumstances exerted greater influence than an important reference group that was not directly relevant to the situation. Another study conducted in the context of timeshare condominium units also revealed that a more situationally specific reference group leads to directionally greater participation in environmental conservation (Schultz, Khazian, and Zaleski 2008). These findings suggest that normative information about a reference group that is not personally meaningful can nonetheless be persuasive if it provides information that is unique to the particular situation. Although the previous research, at first glance, would appear to be inconsistent with research based on identity-based accounts, I assert that this can be explained by considering the role of self-construal and whether the conditions under which decisions about one's prosocial behavior are made in private or public.

The present chapter investigates how independent versus interdependent self-construal impacts a person's susceptibility to descriptive norm information about a reference group in persuasive appeals. I focus on settings involving prosocial behavior for which individuals lack a generally shared understanding about what would be considered normative in those settings. Indeed, these are the very situations in which descriptive norms would be particularly informative and useful as guides to behavior for targeted individuals. In such settings, how does self-construal impact a person's responsiveness to descriptive norms of a reference group that is meaningful to their social identity versus one that is relatively unimportant to their identity but relevant to the situation at hand? In so doing, I seek to explain the seemingly divergent patterns of prior findings by positing that the way people construe themselves serves to moderate their

responses to descriptive norm appeals.

Building on literatures that distinguish between interdependent and independent self-construal, I propose that descriptive norms of situationally versus socially relevant reference groups exert different levels of persuasive appeals depending on the target's self-construal. Specifically, people with independent self-construal tend to follow the norms of a reference group that shares situational similarities with their local setting or circumstances (i.e., a situationally relevant group) even if that group is not important to their self-identity. To the extent that the group is perceived to be distinctive and unique to the situation, independents whose natural tendencies are to prioritize uniqueness and individualism are likely to be persuaded by the norms of a situationally relevant reference group. This is especially likely to occur in private as opposed to public settings. In the latter, the presence of others in the immediate environment would be more likely to activate a propensity by independents to be unique and distinctive compared to those immediately around them so that the norms of a situationally relevant group become less persuasive.

In contrast, people with interdependent self-construal tend to follow the norms of a reference group that represents an important social identity (i.e., a socially relevant group), even when the social identity is not directly relevant to the unique situation at hand. Because an interdependent orientation involves maintaining a sense of connection with important others, how one is related and linked to others is a primary and chronic driver of normative behavior in multiple domains and circumstances (Riemer et al. 2014). I also consider how the public versus private decision context would influence interdependents' responses to descriptive norms of socially- versus situationally-relevant reference group. Whereas interdependents are more likely to be persuaded by socially relevant reference groups in private, I suggest that they will be more

susceptible to descriptive norms of situationally relevant reference groups in public. I reason that because of their tendency to allocate more attention to their contextual surroundings (Nisbett et al. 2001), they will become more attuned to the norms of the situational reference group that is situated in their immediate public decision context.

THEORETICAL DEVELOPMENT

One of the most common ways to differentiate one culture from another is by how people mentally represent the self: as independent or interdependent (Markus and Kitayama 1991). While those with an independent orientation tend to perceive and understand themselves to be separate from others, those with an interdependent orientation tend to focus on relationships with meaningful others and reference others with whom they identify. An independent mode of the self is more prominent in Western cultures, whereas an interdependent model of the self is more common in Eastern cultures (Han and Shavitt 1994; Markus and Kitayama 1991; Riemer et al. 2014). Within a given national culture, individual differences in chronic self-construal also exist so that some individuals are more independent or interdependent than others (Singelis 1994). Notably, the two types of self-construal can be activated by priming individuals to construe themselves as independent or interdependent in the moment (e.g., Aaker and Williams 1998; Trafimow, Triandis, and Goto 1991). In the following sections, I theorize how self-construal would influence people to react differently to norms of situationally versus socially relevant (hereafter labeled “situational” and “social”, respectively) reference groups.

Independent self-construal and Perceived Distinctiveness of Situational Reference Group Norms

Independents have a motivational drive to focus on differentiation and a relatively greater need to be unique (Aaker and Maheswaran 1997). Their behavior reflects a tendency to act in a way that promotes self-expression, uniqueness, and self-reliance (Snibbe and Markus 2005). For example, when deciding whether to donate to charity, independents donated more to one that performed above than below average because the charity stood out in competence (Allen, Eilert, and Peloza 2018). While independents value expressing their own attitudes and autonomy, that does not mean that they are not likely to follow group norms.

Descriptive norms can have a strong impact on people's intentions and behavior. Compared to marketing messages that do not contain normative information, those with descriptive norms can be more effective in persuasion. For example, in the context of a hotel stay, guests engaged in more conservation behavior during their stay when they saw messages with descriptive norm appeals compared to those with pro-environmental appeals (Goldstein, Cialdini, and Griskevicius 2008; Schultz, Khazian, and Zaleski 2008). White and Simpson (2013) also showed that independents are more persuaded by descriptive norms than by injunctive norms to follow environmentally sustainable behavior.

Despite the extensive research on descriptive norms, it is as yet unclear whether independents would be more persuaded by situational or social reference groups. Studies conducted in cultures with an independent orientation have found that reference groups with whom the targets strongly identify had strong normative effects on targets' behavior (Abrams and Hogg 1990; Hogg and Reid 2006; Turner 1991). It follows then that when independents are motivated by the need for social affiliation or belongingness, they would prioritize norms of a

meaningful reference group. However, the normative behavior of independents in a variety of situations tends to be guided by their own attitudes, personal preferences, individual goals, and feelings about the self (e.g., sense of control, self-confidence, self-esteem) (Riemer et al. 2014). Individual agency within such contexts means that norm adherence to reference groups is less consistent across different situations. Whereas social reference groups represent chronically important collective identities, situational reference groups are inherently distinctive and unique to the situation at hand. Independents' natural tendency to prioritize uniqueness and individualism are thus more likely to lead to the perceptions of behavior in accordance with descriptive norms of situational reference groups as being distinctive and, therefore, more persuasive even if they do not personally identify with the group.

I hypothesize that for independents, the norms of situational reference groups will be more persuasive than those of social reference groups. I expect that independents' greater conformity to the norms of situational (vs. social) reference groups is driven by the perception that doing so is consistent with being distinctive. For interdependents, who tend not to be motivated by uniqueness, I do not expect perceived distinctiveness of behaving in a manner consistent with situational reference group norms to supersede their inclination to conform to the norms of a meaningful social reference group.

Interdependent self-construal and Group Identification

Interdependents have a motivational drive to focus on similarity and have a relatively greater need to blend in with other people (Aaker and Maheswaran 1997). To maintain their self in relation to others, they seek to behave in accordance with prescribed roles and attune themselves to situations and patterns of agency that require this behavior, often automatically and effortlessly. Because of their need for belongingness, when their collective self (e.g.,

University of X student) is threatened, they even consume identity-linked products in order to access a repertoire of social identities (White, Argo, and Sengupta 2012). While interdependents prioritize accommodating to others' expectations, they are not equally attentive to all groups.

Importantly, interdependents' behavior differs towards groups that they identify with versus those they do not. For example, interdependents engaged in more prosocial behavior toward others with whom they identified with (vs. not) because they believed that it would contribute to their own happiness (Duclos and Barasch 2014). Because interdependents' sense of the self is tightly connected with group identity, the number of important reference groups is relatively small, and such groups usually include others who share a common destiny, such as family members, friends, co-workers, or fellow citizens (Bontempo, Lobel, and Triandis 1990). Such groups are more meaningful to interdependents than to independents and exert a stronger influence on them, as reflected in the subordination of personal goals to the goals of a few large ingroups among interdependents (Triandis et al. 1988). Thus, it seems unlikely that interdependents are susceptible to any normative influence across the board. Instead, differences in the extent to which interdependents identify with a reference group should have differential impact on conformity to descriptive norms involving prosocial behavior. I expect interdependents, who are more attuned to relationships than independents, to be particularly sensitive to the descriptive norms of socially meaningful reference groups.

When people encounter normative messages, they do not necessarily have interpersonal relationship with the groups referenced in normative messages. However, research has shown that people can feel interconnected not only with close others but also with groups that comprise ethnicities and nationalities they share without direct relationships (Aron, Aron, and Smollan 1992; Tropp and Wright 2001). Such greater personal identification with a reference group leads

to behavioral consequences. For example, greater group identification increased charitable donations to a group with which people had no previous relationship (Winterich, Mittal, and Ross 2009). It can also increase conformity to norms at a collective level, even in the absence of interpersonal relationships (Brewer and Gardner 1996). Indeed, when people have a strong motivation for social connectedness, they are more likely to follow descriptive norms of a more familiar reference group (Kwan, Yap, and Chiu 2015).

Thus, I hypothesize that interdependents are more likely to include socially rather than situationally relevant others in the self and for this group identification to drive their norm adherence to prosocial behavior. In other words, I expect that interdependents' greater conformity to the norms of social rather than situational reference groups is driven by their stronger identification with the former than the latter. In addition, I expect interdependents and independents to differ in their likelihood of identifying with a group referenced in normative messages. For people with a strong motivation for distinctiveness (e.g., independents), the level of familiarity of the group does not influence their conformity to descriptive norms (Kwan, Yap, and Chiu 2015). Thus, I do not expect group identification to influence independents' norm adherence to prosocial behavior.

I summarize my hypotheses as following:

H1: The effect of descriptive norms on prosocial behavior is contingent on self-construal. For independents, descriptive norm information about a situational (vs. social) reference group will exert greater influence on prosocial behavior. For interdependents, descriptive norm information about a social (vs. situational) reference group will have greater influence on prosocial behavior.

H2: The perceived distinctiveness of the reference group will mediate the effects of reference

groups' descriptive norms on independents' prosocial behavior.

H3: The group identification will mediate the effects of reference groups' descriptive norms on interdependents' prosocial behavior.

Role of Decision Context

I further consider how cultural differences in the persuasive impact of normative information on prosocial behavior will be affected by whether the decision context is private or public. Decision context is important to consider insofar as situational factors have a significant influence on people's behavior (Burnkrant and Cousineau 1975; Johar, Moreau, and Schwarz 2003; Simpson, White, and Laran 2018). Compared to private, public decision contexts increase people's awareness of others' expectations (Lerner and Tetlock 1999; Ratner and Kahn 2002; White and Peloza 2009), motivation to manage impressions (Argo, White, and Dahl 2006), and concerns about negative evaluations by others (Dahl, Manchanda, and Argo 2001; Ratner and Hamilton 2015). Consequently, in public, people tend to increase their prosocial behavior (Fisher and Ackerman 1998; White and Peloza 2009).

Given the differences between independents and interdependents in their social motives to be unique versus blend in, respectively, I propose that the decision context plays a potentially important role in how they respond to descriptive norms of social versus situational reference groups. Unlike a decision made in private, one made in a public setting produces the need for people to consider how the decision will be perceived by others in determining how they behave (Leary and Kowalski 1990). I expect that heightened sensitivity to immediate surroundings will influence the underlying mechanisms of perceived distinctiveness and group identification, and thereby change how independents and interdependents react to the norms of situational and social reference groups.

For independents, I hypothesized they will be more susceptible to the normative influence of situational (vs. social) reference groups because they perceive the groups to be distinct to the decision setting. They tend to view the descriptive norms of situational reference groups described in the persuasive appeals as being consistent with their self-perceptions as unique and distinct individuals. I expect this to be the case especially in private settings in which a decision is made in the absence of what may feel like direct scrutiny by others in the immediate environment. However, in public settings where there are direct observers within close physical proximity, the perspective of interdependents may reflect a heightened awareness and concern over how one is regarded by direct observers. Here, individuals may prioritize behaving in a unique way compared to those around them and thereby become less likely to adhere to situational reference group's norm in public than in private settings. In other words, public decision contexts would not necessarily increase independents' conformity to norms of the situational reference group as they are likely to perceive less distinctiveness in behaving in a manner consistent with this group. Despite people's general tendency to increase their prosocial behavior in public, I hypothesize that independents' conformity to situational reference groups' prosocial norms will not increase in public compared to private settings. On the other hand, I posit that public decision contexts may, in fact, increase independents' perceptions of distinctiveness of normative behaviors by social reference groups because they include others who are more distant from immediate surroundings. Thus, I might expect that independents will be more persuaded by descriptive norms of social reference groups in public than in private decision contexts. More formally, I propose the following hypothesis:

H4: Descriptive norm information about a social reference group will be more persuasive to independents in public than in private. Descriptive norm information about a situational

reference group will not increase independents' prosocial behavior in public than in private.

For interdependents, nonconformity represents deviance (Kim and Markus 1999), and thus I expect that social (vs. situational) reference groups with whom they strongly identify will exert greater influence in norm adherence. However, one may argue that my hypothesis is at odds with interdependents' greater sensitivity to contexts (Jain, Desai, and Mao 2007; Kühnen and Oyserman 2002; Nisbett et al. 2001). In other words, interdependents' high level of sensitivity to the context suggests that interdependents may be more likely to recognize low fit between social reference groups and the immediate situations and thus to exhibit a contrast effect. I propose that decision context plays an important role in reconciling these opposing predictions. Research has shown that interdependents strongly believe in fate in private but in the concept of face in public (Chan, Wan, and Sin 2009). This suggests that they become more aware of their surroundings and how others perceive them when in public. They are prone to experience dissatisfaction with their social failures in public compared to in private. Motivated to manage their impressions in front of others immediately surrounding them, they would be more motivated to maintain harmony with situational reference groups. Thus, I hypothesize that interdependents follow situational reference groups' norms of prosocial behavior more in public than in private. The sensitivity and attunement to others in their immediate environment would serve to decrease their identification with social reference groups. Accordingly, I expect that interdependents' conformity to social reference groups' prosocial norms will not increase in public compared to private decision contexts.

H5: Descriptive norm information about a situational reference group will be more persuasive to interdependents in public than in private. Descriptive norm information about a social

reference group will not increase interdependents' prosocial behavior in public than in private.

In five studies, I show that prosocial behaviors of independents and interdependents are differentially influenced by descriptive norms of situational versus social reference groups. I show the predicted differences in Study1 and Study 2. Study 3 tests the robustness of the findings in a field setting that employs different operationalizations of self-construal and reference groups. Study 4 investigates the proposed mechanisms underlying the findings in Studies 1 through 3. In particular, I examine whether the perceived distinctiveness of abiding by the situational reference group norms drives independents' susceptibility to descriptive norms of a situational (vs. social) reference group; and whether the degree of group identification drives interdependents' stronger conformity to descriptive norms of a social (vs. situational) reference group. I rule out alternative explanations involving perceived temporal impact and the size of reference group. In study 5, I investigate the moderating role of public versus private decision contexts on how self-construal influences prosocial behavior in response to descriptive norms of social versus situational reference groups.

STUDY 1

Study 1 tests my basic prediction that descriptive norms about a situational reference group will be more persuasive to independents, and that descriptive norms about a social reference group will be more persuasive to interdependents in promoting prosocial behavior (H_1).

Method

The study was a 2 (self-construal: independent, interdependent) x 3 (reference group: control, social, situational) between-subjects design. I operationalized self-construal as national cultural values. Extant literature views the United States and South Korea as typifying independent and interdependent cultures (Hofstede 1980; Markus and Kitayama 1991; Singelis 1994). In line with prior research (e.g., Kim and Drolet 2003; Lalwani 2009), I conducted a cross-cultural experiment by recruiting 216 students from a large university in the U.S. (an independent culture) and 119 students from a large university in South Korea (an interdependent culture). The study materials used in Korea were translated from English to Korean and then back-translated by bilingual speakers of English and Korean who were blind to the hypotheses.

Participants were recruited for a study on environmental conservation. I adapted the task used by Goldstein, Cialdini, and Griskevicius (2008), which assessed towel reuse in a U.S. hotel in response to messages containing descriptive norms of different reference groups. I deliberately used a similar task, albeit not in a field setting, to ensure that I could replicate the results of the study conducted by Goldstein and colleagues for independents and to directly compare the result for interdependents. Participants read a hypothetical scenario about staying in a hotel room during vacation in which they imagined seeing a message next to the towel racks in the bathroom. The message contained descriptive norm information about towel reusage. Participants were randomly assigned to one of three reference group conditions. The message in the social (situational) reference group condition encouraged participants to follow the majority of fellow citizens (guests who had stayed in the same room) in reusing towels to help save the environment. The control condition encouraged towel reuse to help save the environment, without giving information about reference groups.

After reading the scenario, participants indicated how likely they would be to reuse towels. Next, participants indicated the degree to which they usually reuse towels during hotel stays as a potential covariate. As a manipulation check for reference group, I adapted the procedure from Goldstein, Cialdini, and Griskevicius (2008). Specifically, participants rated the importance of being a helpful person (control reference group), citizen (social reference group), and a hotel guest in the same room (situational reference group) to their own identity (1 = not at all, 7 = very much). Finally, they shared their demographic information and were then debriefed.

Results

Two Korean participants who failed to complete the study were excluded from the analysis. The remaining sample included 333 participants (M = 23.09 years old, SD = 3.59, 65% female for the Korean sample; M = 19.84 years old, SD = .86, 49% female for the U.S. sample).

Participants' prior towel reuse frequency significantly differed between cultures ($F(1, 331) = 33.56$, $p < .001$). Americans (M = 4.67, SD = 1.96) had a greater frequency of reusing towels prior to their participation in the study than Koreans (M = 3.42, SD = 1.73) did. Thus, I conducted a two-way (self-construal x reference group) ANCOVA on willingness to reuse towels after controlling for participants' habitual levels of towel reuse. Results from the ANCOVA test yielded main effects of prior towel reuse frequency ($F(1, 326) = 295.26$, $p < .001$) and self-construal ($F(1, 326) = 15.57$, $p < .001$) on towel reuse. Indeed, participants' prior towel reuse frequency significantly influenced their intention to reuse towels, and Americans (M = 5.30, SD = 1.79) expressed a significantly greater willingness to reuse towels than did Koreans (M = 3.86, SD = .1.85). The main effect of reference group was not significant ($p = .158$). Importantly, the effect of self-construal was qualified by a significant interaction ($F(2, 326) = 4.21$, $p = .016$).

For Americans, messages about the reference groups differed in their persuasive effect on towel reuse ($F(2, 326) = 3.21$, $p = .042$). A planned contrast revealed that messages referencing same-room guests (M = 5.58, SD = 1.68) were significantly more influential than those referencing citizens (M = 5.29, SD = 1.77) in increasing participants' willingness to reuse towels ($F(1, 326) = 4.06$, $p = .045$). The standard environmental message (M = 5.04, SD = 1.89) and the message about citizens did not differ in their impact on towel reuse likelihood ($p = .740$). The message about same-room guests was significantly more persuasive than the standard environmental message in increasing participants' towel reuse likelihood ($F(1, 326) = 5.53$, $p = .019$). For Koreans, the reference group had a marginally significant main effect on towel reuse likelihood ($F(2, 326) = 2.90$, $p = .057$). As expected, a planned contrast revealed that messages about citizens (M = 4.44, SD = 1.92) were significantly more persuasive than messages about same-room guests (M = 3.68, SD = 1.53) in prompting greater willingness to reuse towels ($F(1, 326) = 4.46$, $p = .035$). The standard environmental message (M = 3.47, SD = 1.98) and the message about same-room guests did not differ in their effect on towel reuse ($p = .980$). The message about citizens was more persuasive than the standard environmental message in increasing participants' willingness to reuse towels ($F(1, 326) = 4.23$, $p = .040$).

As a manipulation check, I tested whether participants perceived citizens to be more important to their identities than same-room guests. I conducted an ANCOVA to test the effect of reference group on perceived importance of the reference group identity after controlling for culture. Culture did not influence the manipulation check ($p = .303$). As expected, the main effect of reference group was significant on the perceived importance of group identity ($F(1, 329) = 40.35$, $p < .001$). Participants perceived the citizen identity ($M = 4.99$, $SD = 1.56$) to be most important, followed by the helpful person identity ($M = 3.90$, $SD = 1.49$) and then by the

hotel guest identity ($M = 3.13$, $SD = 1.60$). Given that participants perceived the identity of the social reference group to be more important than the situational reference group ($p < .001$), the manipulation check was successful.

Discussion

Findings from Study 1 provided initial support for my hypothesis by showing that descriptive norms of situational and social reference groups exert different levels of persuasiveness on willingness to help for independents and interdependents. The situational reference group of study participants was more influential than the social reference group of college students in increasing independents' willingness to help. On the other hand, the social reference group was more persuasive than the situational reference group for interdependents.

STUDY 2

Study 2 tests the generalizability of the findings from Study 1. Whereas I conducted a cross-cultural study in Study 1, I manipulate self-construal in Study 2. By utilizing a different context of prosocial behavior, I further test the hypothesis that there are cultural differences in how individuals respond to the persuasive appeals of social and situational group norms.

Method

Upper-level undergraduates (N = 304; 139 females) were recruited from a subject pool at a large North American university in a 2 (self-construal: independent, interdependent) x 3 (reference group: situational, social, control) between-subjects study.

Participants came into the Behavioral Lab at the university to take part in an experimental session. During the session, they completed two tasks that were ostensibly unrelated. In the first

task, they were randomly assigned to an independent or interdependent self-construal condition. I adopted a self-imagery procedure in a text comprehension task used in Trafimow, Triandis, and Goto (1991). Participants took the perspective of a Sumerian warrior and imagined making a decision about whom to put in command of the detachment for a new war. Those in the independent condition imagined choosing a talented general who would greatly increase their prestige. Those in the interdependent condition imagined choosing a family member who would increase the power and prestige of their family. Prior research has established the validity of this priming method for manipulating self-construal (Mandel 2003; Torelli and Kaikati 2009; Trafimow, Triandis, and Goto 1991).

The second task involved volunteering for a freshman tutoring program. Participants viewed a poster, ostensibly from the university's tutoring center, that encouraged them to volunteer. They were randomly assigned to view a poster containing one of three types of information: 1) descriptive norms of college students (i.e., a social reference group), 2) descriptive norms of fellow study participants in the Behavioral Lab (i.e., a situational reference group), or 3) no reference to norms of any group (i.e., control). The posters in the situational and social reference group conditions stated that 71.3% of respondents in a survey conducted by the Tutoring Center wanted to volunteer for the freshman tutoring program. This information was followed by the situational (social) reference group manipulation with the statement, "You can join your fellow study participants in this Behavioral Lab (college students) in showing your care for others by sharing your knowledge in any subject to help freshmen who are in need of tutoring." The poster in the control condition did not provide any normative information and simply encouraged participants to help freshmen who need tutoring.

After reading the poster, participants were reminded that their responses were strictly

private and anonymous, and that they should answer the questions as honestly as possible. Then, they rated how willing they would be to share study materials and to share tips with freshmen in the tutoring program, responding to each item using a seven-point Likert scale (1 = not at all, 7 = very much). I used the average of these two items as my dependent variable of willingness to help (α = .81). Such measures of conformity intentions have been used in prior research (e.g., Allen, Eilert, and Peloza 2018; White and Simpson 2013). As a manipulation check for reference group (Goldstein, Cialdini, and Griskevicius 2008), participants rated how important and how meaningful the identity of the reference group was to them (1 = not at all, 7 = very much; α = .93). Those in the control condition answered these questions about a helpful person.

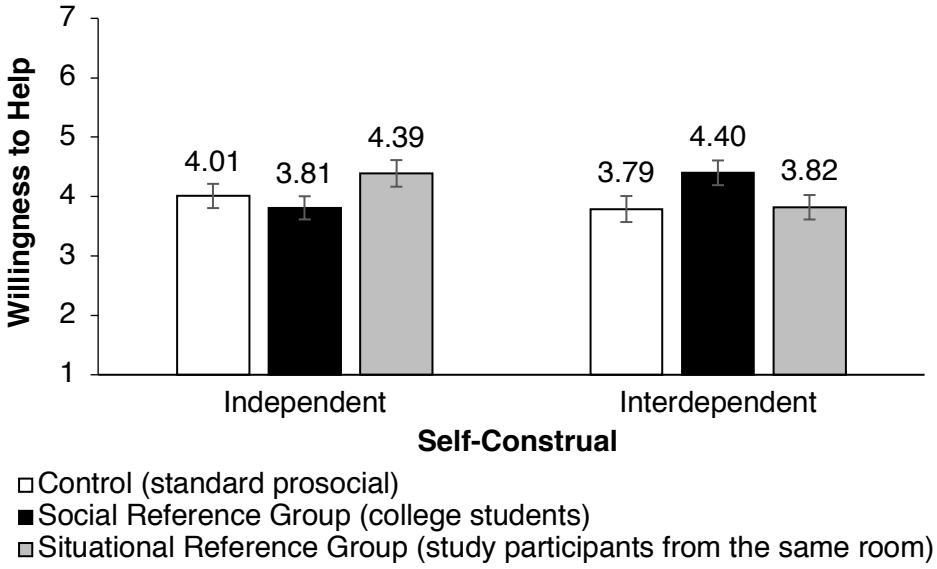
Results

I conducted a 2 (self-construal) x 3 (reference group) ANOVA to examine the effects of self-construal and reference group on willingness to help. Neither the main effect of self-construal nor the reference group was significant (*p*s > .50). As predicted, the interaction effect of self-construal and reference group was significant ($F(2, 298) = 4.12$, $p = .017$). To examine the hypothesized joint effect of self-construal and reference group on willingness to help, I conducted planned contrasts between situational and social reference groups within each self-construal condition (see Figure 3.1).

For independents, descriptive norm information about the situational reference group (M = 4.39, SD = 1.48) was more persuasive than the social reference group (M = 3.81, SD = 1.59) in increasing participants' willingness to help ($F(1, 298) = 3.75$, $p = .054$). However, for interdependents, descriptive norm information about the social reference group (M = 4.40, SD = 1.38) was more persuasive than the situational reference group (M = 3.82, SD = 1.54; $F(1, 298) = 3.97$, $p = .047$). These results supported H_1. Additional analyses revealed that for independents,

the effects of persuasive appeals on willingness to help in the control condition (M = 4.01, SD = 1.38) did not differ from the descriptive norm appeals in the situational and social reference group conditions (*p*s > .20). For interdependents, the message in the control condition (M = 3.79, SD = 1.53) was less persuasive than norm appeals in the social reference group condition ($F(1, 298) = 4.04$, $p = .045$), but equivalent in persuasiveness to those in the situational reference group condition ($p = .937$).

Figure 3.1. Willingness to help, by self-construal and reference group, in Study 2. Error bars reflect ±1 standard error of the mean.



Manipulation checks showed that reference group significantly influenced participants' assessments of identity importance ($F(2, 301) = 163.41$, $p < .001$). Participants perceived a helpful person identity (control) to be most important (M = 5.13, SD = 1.30). As expected, participants perceived the identity of the college student (M = 4.76, SD = 1.46) as more important than the study participant in this behavioral lab (M = 1.97, SD = 1.93; $F(1, 301) = 221.59$, $p < .001$).

Discussion

Findings from Study 2 further provide support for my hypothesis by showing that descriptive norms of situational and social reference groups exert different levels of persuasiveness on willingness to help for independents and interdependents. One might, however, reasonably argue that the results may have been driven by participants' attention to the difference in size between social and situational reference groups. Although I used the phrase "71.3% of respondents in a survey conducted by the Tutoring Center" in both social and situational reference group conditions in order to keep the size of the respondents vague on purpose, it is possible that participants perceived a greater number of respondents in the social reference group than the situational reference group. Nevertheless, if participants were influenced by the size of the reference group, I would have expected a different pattern of results than were obtained for interdependents. Research has shown that while independents are more likely to follow a group's preferences when the group is smaller, interdependents tend to consider a group's preferences regardless of the size of the group (Wu, Moore, and Fitzsimons 2019). Thus, the latter account is inconsistent with my finding that the social reference group is more persuasive than the situational reference group for interdependents. I nonetheless seek to rule out, in the next study, the role of the size of group as a potential explanation for the observed effects.

One limitation of Study 1 and Study 2 is that my findings were based on participants' prosocial intentions rather than actual behavior. Although previous research has shown similar findings between measures of intentions and behavior (e.g., Allen, Eilert, and Peloza 2018; White and Simpson 2013), there also exists research showing inconsistencies between the measures (Nolan et al. 2008; Pronin, Berger, and Molouki 2007). Thus, the subsequent studies

test the hypotheses by measuring real behavior.

STUDY 3

Study 3 builds on Study 2 in four ways. First, it seeks to replicate the findings of a scenario-based study by conducting a field experiment at a hotel. Second, it strengthens the robustness of my findings by employing different stimuli for manipulating self-construal and reference group. Third, it rules out the alternative explanation concerning the role of the group size by specifying the same size for situational and social reference groups. Fourth, it provides for generalization of my findings by utilizing a different prosocial context (reusing towels during stayovers). Consistent with my previous findings, I expect to discover the highest towel reuse in response to descriptive norms of the situational reference group among independents, and to descriptive norms of the social reference group among interdependents.

Method

I selected a hotel with 105 guest rooms owned by a large North American university as the site of a 2 (pre- and post-intervention) x 5 (towel reuse signs) field experiment. The hotel features boutique style guest rooms with one full size bed and accommodates only people who have an affiliation with the university, as a current student, faculty, staff, or alumnus. Because the guests shared a university affiliation and each guest room had the same design that accommodated primarily single individuals, this hotel afforded us a more controlled environment compared to other hotels.

Over a 39-day span between February and March 2020, I collected towel reuse data on 409 occupancies which involved more than one night's stay. Among 409 occupancies, all but

two had one guest staying in the room. All towel reuse data were collected by the hotel's room attendants who were thoroughly trained prior to the data collection but remained blind to the hypotheses.

I began data collection by measuring the baseline towel reuse rates, and I introduced my intervention (towel reuse signs) at the beginning of March. Thus, the data contained baseline towel reuse rates prior to the intervention (N = 274) and towel reuse rates impacted by the intervention (N = 135). Despite the agreement with the hotel to collect the data throughout April, data collection was abruptly ended in mid-March because of COVID-19, and thus the sample size following the intervention was smaller than I had anticipated.

The intervention comprised one of five sets of signs urging guests' participation in the towel reuse program (see the Appendix for the stimuli). Each of the 105 guest rooms was assigned to have one of these sets of towel reuse signs. In each guest room, I introduced a sign on the nightstand and replaced the previous towel reuse sign on the bathroom doorknob with mine. The original towel reuse signs on bathroom doorknobs that existed prior to the intervention had the following messages, "Using linens more than once can save thousands of gallons of water each day, reduce energy consumption and detergent waste. If you prefer to have your towels replaced, please leave them on the door to be laundered. Thank you for helping us conserve!" In addition to changing the message, I changed the design of the signs to be more visible and visually appealing.

Similar to the original towel reuse signs that I replaced, the signs in the control condition encouraged towel reuse without containing any descriptive norm information and manipulated neither self-construal nor reference group. However, the other four signs contained norm information about a reference group (situational, social) and also manipulated self-construal

(independent, interdependent). The signs in the situational reference group condition included normative information of other guests who stayed in the same room as the guest, and those in the social reference group included normative information of people who were affiliated with the university. Unlike the previous studies in which I left the size of the reference group vague, I specified the size of the group to be consistent as 75% of 292 guests who stayed in the same room (situational) or 292 university affiliates (social).

I manipulated self-construal using ad appeals (Aaker and Williams 1998; Allen, Eilert, and Peloza 2018; Han and Shavitt 1994; Ma, Yang, and Mourali 2014). Specifically, towel reuse signs that primed independent self-construal contained "you" and "your" pronouns, included pictures of an individual, and included the following message, "You are a product of your environment, so choose the environment that will best develop you toward your objective." Signs that primed interdependent self-construal contained "we" and "our" pronouns, included pictures of a group of people, and included the following message, "We are a product of our environment, so choose the environment that will best sustain our connected world."

For the dependent variable, I used the average towel reuse rates which was calculated as the total number of days when a guest reused towels divided by the total number of days in occupancy, excluding the checkout date. It ranged from 0, meaning that a guest did not reuse towels during the entire stay (i.e., requested towels to be laundered everyday), to 1, meaning that a guest reused towels during the entire stay (i.e., never requested towels to be laundered). For example, if a guest checked in on March 3, reused towel on the 4th, and checked out on the 5th, the average towel reuse rates would for the guest would be 1 divided by 1.

Results

I conducted a 2 (pre- and post-intervention) x 5 (towel reuse sign) ANOVA to examine

whether and which of the five intervention signs increased guests' towel reuse at the hotel. The main effect of towel reuse sign was not significant on guests' average towel reuse ($F(4, 399) = 1.01$, $p = .402$). The main effect of intervention was significant ($F(1, 399) = 36.0$, $p < .001$). Guests' average towel reuse increased after the intervention of five towel reuse signs were introduced ($M_{pre} = .25$, SD = .41vs. $M_{post} = .50$, SD = .40). Importantly, the interaction of intervention and towel reuse sign was significant on guests' average towel reuse ($F(4, 399) = 3.07$, $p = .016$).

Planned contrasts revealed that towel reuse rates in guest rooms with the signs that primed independent self-construal and included descriptive norm information about the social reference group did not differ between pre- and post-intervention ($M_{pre} = .26$, SD = .41 vs. $M_{post} = .38$, SD = .41; $F(1, 399) = 1.38$, $p = .240$). Towel reuse rates in rooms with the signs that primed interdependent self-construal and referred to norms of the situational reference group also did not differ between pre- and post-intervention ($M_{pre} = .34$, SD = .44 vs. $M_{post} = .43$, SD = .40; $F(1, 399) = .85$, $p = .358$). For these guest rooms, results suggest that towel reuse signs that contained normative information did not increase towel reuse compared to original signs that did not contain normative information. However, as predicted, towel reuse rates in guest rooms with the signs that primed independent self-construal and included normative information about the situational reference group increased after the intervention ($M_{pre} = .18$, SD = .38 vs. $M_{post} = .67$, SD = .38; $F(1, 399) = 27.27$, $p < .001$). Towel reuse rates also increased in rooms where signs that primed interdependent self-construal and referred to norms of the social reference group were introduced ($M_{pre} = .27$, SD = .42 vs. $M_{post} = .56$, SD = .38; $F(1, 399) = 7.67$, $p = .006$). These results thus provided support for H_1. In guest rooms assigned to display control signs, there was an unexpected increase in towel reuse after the intervention ($M_{pre} = .18$, SD = .37 vs.

M_{post} = .48, SD = .41; $F(1, 399) = 11.34$, $p = .001$). I speculate that this could have been due to differences in visual appearance between the original signs and the control signs.

In addition, also as predicted, while guests rooms did not differ in guest's towel reuse rates prior to the intervention ($F(4, 399) = 1.83$, $p = .122$), they exhibited marginally different levels of towel reuse after the intervention was introduced ($F(4, 399) = 2.16$, $p = .072$). Although the sample size planned for after the intervention fell unexpectedly short of what I had planned due to the COVID-19 pandemic, I further tested my hypothesis by conducting planned contrasts among the sign conditions after the intervention. Results revealed that in guest rooms with signs that primed independent self-construal, norms of other guests who stayed in the same room (i.e., situational reference group; M = .67, SD = .38) were significantly more effective than those of university affiliates (i.e., social reference group; M = .38, SD = .41) in increasing guests' towel reuse ($F(1, 399) = 6.93$, $p = .009$). In guest rooms with the signs that primed interdependent self-construal, norms of university affiliates (M = .56, SD = .38) were more effective than those of other guests who stayed in the same room (M = .43, SD = .40) in increasing guests' towel reuse, although difference did not reach significance ($p = .240$).

Discussion

Study 3 replicated findings from Studies 1 and 2 in a field setting which utilized different manipulation methods and kept the size of reference groups consistent. The present findings provided further support for H_1 that descriptive norms of a situational (vs. social) reference group are more persuasive for independents, and that descriptive norms of a social (vs. situational) reference group are more persuasive for interdependents. The towel reuse rates were highest among guests who were primed with independent self-construal and saw the norms of the situational reference group (other guests who stayed in the same room), and among guests who

were primed with interdependent self-construal and viewed the norms of the social reference group (university affiliates). Lending further confidence in my findings are the consistent results of Study 1 that I conducted cross-culturally using a hotel-stay scenario.

STUDY 4

In Study 4, I sought to replicate my findings in the context of donations made in response to COVID-19. Moreover, Study 3 tests the underlying mechanisms of different levels of persuasiveness of descriptive norms about situational versus social reference groups among independents and interdependents. Specifically, I examine the mediating roles of perceived distinctiveness of the reference group for independents (H_2) and group identification for interdependents (H_3). I expect independents to perceive greater distinctiveness in following a situational (vs. social) reference group, which influences them to follow this group's norms in behaving prosocially. I expect interdependents to identify more with a social (vs. situational) reference group, which influences the interdependents to follow their prosocial norms. I also test and rule out alternative mechanisms, namely, the perceived temporal impact of conformity and the size of contribution.

Method

Mechanical Turk workers (N = 402; M = 39.54 years old, SD = 14.01; 196 females) who are U.S. citizens participated in a 2 (self-construal: independent, interdependent) x 2 (reference group: situational, social) between-subjects study in exchange for payment of $.55. Before the study began, participants were reminded that their responses would remain private and anonymous and that they would be invited at the end for a chance to make a real donation.

The study began with the manipulation of self-construal using the same reading task as in Study 2 (Trafimow, Triandis, and Goto 1991). Next, participants performed a filler task that was later used to prime the situational reference group. The task involved sharing their ideas on designing the virtual Escape Room, which is analogous to the real-life Escape Room in that a team of players cooperatively discover clues and solve puzzles in order to escape a room. Participants' job was to come up with three best locations where it would be difficult for players of the game to find clues. They were told that they would be randomly assigned to enter one of the five following virtual Escape Rooms: Supernatural Mystery, Go Back in Time, Ancient Egypt, Superhero, and Zombie. In reality, all participants virtually entered the Go Back in Time Escape Room and completed the task of identifying three locations to best hide clues.

After participants finished the tasks, they saw one of four ads soliciting donations to the United Way's COVID-19 Community Response and Recovery Fund to help give those hit the hardest across a spectrum of issues, assistance with housing and utility payments. I manipulated reference group and self-construal in the donation ads. To manipulate reference group, the ads in the situational reference group condition included descriptive norm information about study participants who entered the Go Back in Time Escape Room, and those in the social reference group included norm information about citizens. Donation ads stated that 73% of 382 people in the situational or social reference group made donations in the response to COVID-19 last month. In addition to the earlier reading task adapted from Trafimow, Triandis, and Goto (1991), I bolstered the manipulation of self-construal by presenting ad appeals used in Study 3. Specifically, I added the sentences, "As an individual, you can make a difference," and "Together, I can make a difference" in the situational and social reference group conditions, respectively.

Upon seeing the donation ad, participants were thanked for their participation and offered a bonus payment of $.20. If they chose to donate, they were to indicate how much of the bonus payment they wanted to donate to the COVID-19 fund set up by United Way. Any amount that they did not donate was theirs to keep. Participants' donation amounts served as the main dependent variable. After making donation decisions, they answered questions that measured the underlying processes. For perceived distinctiveness, they answered how they perceived making donations with the reference group to be on three 11-point items (1 = too unoriginal, 6 = perfectly original, 11= too eccentric; 1 = too conventional, 6 = desirably different, 11 = too different; and 1 = too common, 6 = perfectly unique, 11 = too unusual), adapted from Ma, Yang, and Mourali (2014). The average of the three items served as my proposed mediator for independents (α = .85). For group identification, they rated the degree of connectedness (1 = extremely weak, 7 = extremely strong), the degree of overlap in identities using Euler circles (1 = ◯◯, 7 = ◯), the degree of psychological closeness (1 = extremely far away, 7 = extremely close), and the degree of similarity (1 = completely different, 7 = exactly the same) between them and the reference group in the context of making decisions about donations. The average of these items served as my proposed mediator for interdependents (α = .93).

To rule out alternative explanations, I measured the perceived temporal impact of making donations using three 11-point scale items (1 = very short-term impact, 11 = very long-term impact; 1 = have an impact immediately, 11 = have an impact after a delay; 1 = have an impact in the moment, 11 = have an impact in the future). I also measured participants' perceived size of contribution to further rule out an alternative explanation that the group size may account for my effects (1 = a bigger individual impact relative to the cause, 7 = a bigger collective impact).

As a manipulation check for self-construal, participants answered the degree to which

seeing a donation appeal influenced them to focus on the self or others using an eight-item scale adapted from Hamilton and Biehal (2005). As a manipulation check for reference group, participants answered the degree to which they viewed the reference group as an important social identity (2 items: 1= not at all important to 7 = very important, 1 = not at all meaningful to 7 = very meaningful; α = .96) and in relation to the decision-making situation (2 items: 1 = fits very broadly to 7 = fits very specifically, 1 = very abstract description to 7 = very concrete description; α = .85). Finally, participants indicated the extent to which they were financially impacted by the pandemic (1 = not at all, 7 = very much) and then answered demographic questions.

Results

To test the hypothesis that independents and interdependents differ in which norms they follow, I conducted a 2 (self-construal) x 2 (reference group) ANOVA on donation amounts. Neither the main effect of self-construal nor the reference group was significant (*p*s > .40). As predicted, the interaction of self-construal and reference group on donation amounts was significant ($F(1, 398) = 8.09$, $p = .005$).

To examine the hypothesized joint effects of self-construal and reference group on donation amounts, I conducted planned contrasts between situational and social reference groups for each self-construal condition. For independents, descriptive norm information about the situational reference group (M = 9.13, SD = 7.95) compared to the social reference group (M = 7.06, SD = 7.47) was more persuasive in increasing participants' donation amounts ($F(1, 398) = 3.73$, $p = .054$). However, for interdependents, descriptive norms of the social reference group (M = 8.63, SD = 7.68) were more persuasive than those of the situational reference group (M = 6.41, SD = 7.15; $F(1, 398) = 4.38$, $p = .037$). These results provided additional support for H_1. To

test the robustness of the joint effects of self-construal and reference group on donation amounts, I ran three additional analyses: 1) controlling for ethnicity and the extent to which participants were financially impacted by the pandemic; 2) using the log-transformed donation amounts; and 3) running a binary logistic regression with donations recoded as a binary variable (0 = did not donate, 1 = donated). Substantive results remained the same across these analyses.

Mediating role of perceived distinctiveness for independents. A 2 x 2 ANOVA on perceived distinctiveness revealed that the main effect of self-construal was not significant (p = .11). The main effect of reference group was significant such that participants perceived greater distinctiveness in conforming to norms of the situational (M= 5.41, SD = 1.67) than social (M = 5.03, SD = 1.89) reference group ($F(1, 398) = 4.81$, $p = .029$). Importantly, the interaction effect was significant on perceived distinctiveness ($F(1, 398) = 4.74$, $p = .030$). Planned contrasts revealed that independents perceived greater distinctiveness in following norms of the situational (vs. social) reference group ($M_{situational} = 5.46$, SD = 1.64 vs. $M_{social} = 4.69$, SD = 1.74; $F(1, 398) = 9.46$, $p = .002$). However, for interdependents, the level of perceived distinctiveness did not differ between two reference groups ($M_{situational} = 5.36$, SD = 1.70 vs. $M_{social} = 5.36$, SD = 1.97; $F(1, 398) < .01$, $p = .990$).

Next, I tested a mediated moderation model using the PROCESS SPSS macro (model 8; Hayes 2013). The regression model included an independent variable of reference group (0 = situational, 1 = social), a moderator of self-construal (0 = independent, 1 = interdependent), a mediator of perceived distinctiveness, and a dependent variable of donation amounts. As predicted, a bootstrap analysis confirmed that the indirect effect was significant for independents (ß = -.49, SE = .22, 95% CI = [-.96, -.12]), but not for interdependents (ß = -.01, SE = .17, 95% CI = [-.37, .34]). Collectively, these results indicated that perceived distinctiveness mediated the

effect of reference group on donation amounts for independents and not for interdependents. These results supported H_2 that independents' greater conformity to norms of a situational (vs. social) reference group is driven by the perception that following the situational reference group is higher in distinctiveness.

Mediating role of group identification for interdependents. A 2 x 2 ANOVA on group identification revealed a nonsignificant main effect of self-construal ($p = .80$). The main effect of reference group was significant such that participants identified more with the social (M = 3.75, SD = 1.47) than situational (M = 3.20, SD = 1.51) reference group ($F(1, 398) = 13.74$, $p < .001$). Importantly, as predicted, the interaction effect on group identification was significant ($F(1, 398) = 10.74$, $p = .001$). Planned contrasts showed that interdependents identified more with the social (M = 4.01, SD = 1.43) than with the situational (M = 2.98, SD = 1.46) reference group ($F(1, 398) = 24.64$, $p < .001$). However, for independents, the level of group identification did not differ between two reference group conditions ($M_{social} = 3.49$, SD = 1.47 vs. $M_{situational} = 3.43$, SD = 1.53; $F(1, 398) = .09$, $p = .762$).

Next, I tested a mediated moderation model using the PROCESS SPSS macro (model 8; Hayes 2013). The regression model included an independent variable of reference group, a moderator of self-construal, a mediator of group identification, and a dependent variable of donation amounts. As predicted, a bootstrap analysis confirmed that the indirect effect was significant for interdependents ($\beta = 1.92$, $SE = .45$, 95% CI = [1.08, 2.87]), but not for independents ($\beta = .12$, $SE = .39$, 95% CI = [-.67, .86]). Collectively, these results showed that group identification mediates the effect of reference group on donation amounts for interdependents and not for independents. These results provided evidence supporting H_3 that interdependents' greater conformity to a social (vs. situational) reference group is driven by

strong identification with the social reference group.

Alternative mechanisms. I conducted ANOVA on measures I collected to rule out alternative mechanisms. For perceived temporal impact, although I had originally planned to use the average of three items as a variable, data showed low reliability among the three items (α = .45). Thus, I ran a 2 x 2 ANOVA for each item. The effects of self-construal, reference group, and the interaction were not significant for two of the measured items (*p*s > .30). For the measure of expected duration of the impact (1 = very short-term impact, 11 = very long-term impact), however, the interaction of self-construal and reference group was significant ($F(1, 398) = 6.21$, $p = .013$). Exploratory contrast analyses revealed that interdependents perceived that donating in accordance with descriptive norms of the social (vs. situational) reference group would have longer time impact ($M_{social} = 5.03$, SD = 3.10 vs. $M_{situational} = 4.19$, SD = 2.71; $F(1, 398) = 4.49$, $p = .035$). On the other hand, independents perceived that conforming to either group norms would have an equivalent impact on time ($M_{social} = 4.06$, SD = 2.67 vs. $M_{situational} = 4.63$, SD = 2.84; $F(1, 398) = 1.99$, $p = .160$). In addition to this measure of perceived temporal impact of conformity, I also found significant joint effects of self-construal and reference group on perceived size of the contribution ($F(1, 398) = 4.78$, $p = .029$). Exploratory contrast analyses revealed that interdependents perceived following norms of the social (vs. situational) reference group as being a part of a bigger collective cause ($M_{social} = 4.83$, SD = 1.63 vs. $M_{situational} = 4.24$, SD = 1.52; $F(1, 398) = 6.42$, $p = .012$). On the other hand, independents perceived the size of contribution to be equivalent when following either group's norms ($M_{social} = 4.31$, SD = 1.77 vs. $M_{situational} = 4.44$, SD = 1.77; $F(1, 398) = .32$, $p = .572$).

Given that the results revealed significant effects of reference group on these measures only for interdependents, I tested whether perceived duration of the impact and size of

contribution could better explain interdependents' prosocial behavior. I conducted a mediated moderation analysis using the PROCESS SPSS macro (model 8; Hayes 2013). The regression model included an independent variable of reference group, a moderator of self-construal, and a dependent variable of donation amounts. For the mediators, I simultaneously included group identification, perceived duration of the impact, and size of contribution. Results from a bootstrap analysis revealed that only group identification significantly mediated the effect of reference group on donation amounts for interdependents (ß = 1.30, SE = .43, 95% CI = [.58, 2.28]). Thus, these results provided further support for the role of group identification in driving interdependents' conformity to prosocial norms and ruled out alternative explanations.

Finally, manipulation checks showed that the manipulations worked as intended. Participants perceived the social reference group to represent a more important social identity (M_{social} = 4.67, SD = 1.78 vs. $M_{situational}$ = 2.86, SD = 1.86; $F(1, 400) = 99.12$, $p < .001$) and perceived the situational reference group to be more proximate to their decision-making situation ($M_{situational}$ = 4.39, SD = 1.68 vs. M_{social} = 4.04, SD = 1.65; $F(1, 400) = 4.40$, $p = .037$). Participants in the independent self-construal condition focused more on the self ($M_{independent}$ = 2.73, SD = 1.77 vs. $M_{interdependent}$ = 2.45, SD = 1.58; $F(1, 400) = 2.87$, $p = .091$), and those in the interdependent self-construal condition focused more on others ($M_{interdependent}$ = 5.62, SD = 1.32 vs. $M_{independent}$ = 5.33, SD = 1.57; $F(1, 400) = 3.99$, $p = .047$).

Discussion

In addition to replicating the findings of Studies 1 and 2 using the context of donating behavior, Study 4 provided support for my proposed mechanisms underlying the joint effects of self-construal and reference group on how persuaded people are by the descriptive norms. While perceived distinctiveness of behaving in accordance with descriptive norms mediated the effects

of situational reference group norms on independents' prosocial behavior, group identification with the reference group mediated the effects of social reference group norms on interdependents' prosocial behavior. Although I found significant joint effects of self-construal and reference group on perceived duration of donation impact and size of contribution, these alternative mechanisms did not mediate the joint effects on prosocial behavior. In the next study, I further test my hypotheses considering the moderating role of private versus public decision contexts on the effects of self-construal and descriptive norms of reference groups on prosocial behavior.

STUDY 5

In Study 5, I aimed to test the role of decision context (H_4 and H_5). In the previous three studies, participants made decisions about prosocial behavior in private in response to descriptive norms. However, it is likely that the persuasiveness of descriptive norms of situational versus social reference groups may undergo a shift in public settings in which people are influenced by others in their immediate environment. Specifically, a public decision context may make it less distinctive for independents to follow the norms of a situationally proximate group, whereas for interdependents, it may engender greater sensitivity and susceptibility to others in their immediate environment. Thus, consistent with my theorizing about the underlying mechanisms, I would expect that descriptive norms of a situational reference group are more persuasive to interdependents in public than in private, but that they will not influence independents to engage in greater prosocial behavior in public than in private. I further expect that descriptive norms of a social reference group are more influential in persuading independents to be prosocial in public

than in private, but that such difference will not occur among interdependents.

Method

Study 5 used a 2 (self-construal: independent, interdependent) x 3 (reference group: control, situational, social) x 2 (decision context: private, public) between-subjects design. As in Study 1, I operationalized self-construal to vary cross-culturally between the U.S. (N = 268) and South Korea (N = 230). Research has shown that people in South Korea are more interdependent than those in the U.S. (Hofstede 1980; Kim and Drolet 2003; Lalwani 2009; Singelis 1994). All materials were translated from Korean to English and back-translated into Korean by bilingual speakers who were blind to the hypotheses. The study was conducted in groups of six to ten people. Each group was randomly assigned to a public or private condition. Those who were assigned to the public decision context sat next to each other so that they could potentially observe each other's behavior. Those who were assigned to the private decision context sat one seat apart, and I blocked their views of one another using cubicles.

The cover story conveyed to the participants indicated that the study would examine the effects of childhood education on one's career development. As a token of appreciation for participating in the study, U.S. participants received $5 in five one-dollar bills, and Korean participants received 5,000 Korean Won (₩) in five 1,000-Won bills at the beginning of the experiment. At the time of data collection, ₩1,098 was equivalent to $1, and thus I planned to treat ₩1,000 and $1 bills as equivalent in the data analysis. As part of my cover story, participants answered filler questions (e.g., their favorite subject from elementary school, a subject in which they would most like to tutor an elementary school student, and their dream job). I did not include the responses to these questions in the data analysis.

Next, participants saw a donation envelope and were randomly assigned to read messages

that solicited donations of up to $5 (or ₩5,000) for children's education in third-world developing countries. The message included one of the three reference group conditions. Participants in the control condition read the following: "PLEASE MAKE A DONATION TO HELP CHILDREN'S EDUCATION. You can help children's education in developing countries by making a donation." In addition to this general appeal, those in the social (situational) reference group condition read that 73.8% of college students (study participants from the same computer lab) had indicated that they made donations in the past. After reading the message, participants took a moment to insert their donation amount in the envelope if they wished to do so. All participants were informed that any money that they did not donate would be theirs to keep. Participants in the private condition saw the word "anonymous" written on the envelope. In the public condition, participants had to write their name and the donation amount outside the envelope so that the amount would be visible to others. All participants sealed the envelope and left it next to the computer screen to be picked up by a research assistant. A research assistant picked up donation envelopes and left them visible at the front of the room. The donation amounts served as the main dependent variable. To make sure that participants found the reference group identities equally salient across conditions, I measured the extent to which participants were made were aware of the reference group identities (1 = not at all, 7 = very much). Manipulation check items for reference group were adapted from Study 2.

Pretest

In order to test my operationalization of the decision context, I conducted a pretest on a different set of U.S. participants (N = 46) drawn from the same sample population as the main study. Participants read a scenario about making a donation and were randomly assigned to a private or public decision context for handling the donation envelope. They were asked to

indicate the degree to which they agreed or disagreed with three statements to assess their situational self-awareness scale (α = .88; Govern and Marsch 2001). An ANOVA revealed participants in the public condition (M = 4.65, SD = 1.38) felt a greater sense of public self-awareness than those in the private condition (M = 3.53, SD = 1.53; $F(1, 44) = 14.25$, $p = .013$).

Results

I conducted an ANOVA to test the effects of decision context, self-construal, and reference group on donation amounts (scaled from 1 to 5). Since approximately ₩1,098 was equivalent to $1 at the time of data collection, I coded ₩1,000 as 1 so that I could easily compare it with $1. Decision context had a significant main effect on donation amounts ($F(1, 486) = 16.49$, $p < .001$). The public condition resulted in greater donation amounts (M = 2.78, SD = 2.15) than the private condition (M = 2.00, SD = 2.10). Importantly, the main effect was qualified by a significant three-way interaction ($F(2, 486) = 3.75$, $p = .024$). None of the remaining effects was significant (ps > .100). Analyses with standardized monetary amounts for each culture yielded results that were consistent with the reported findings.

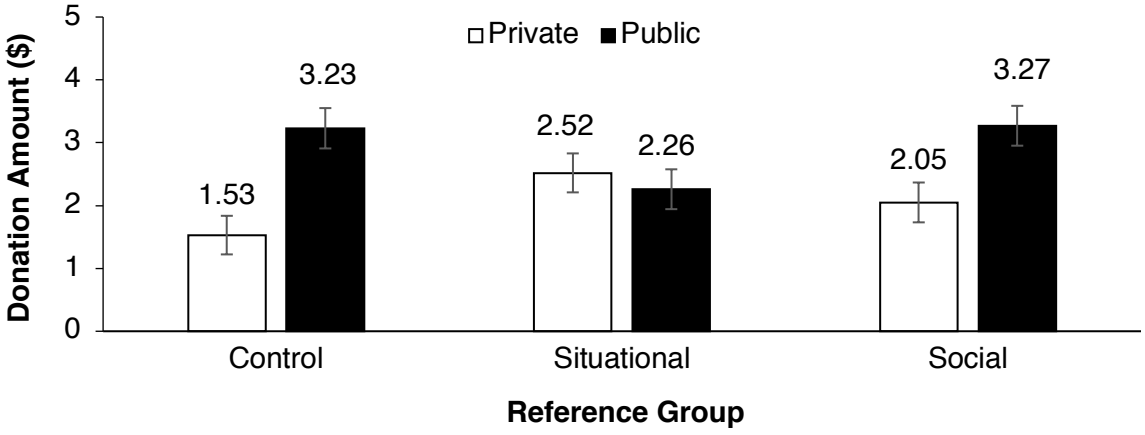
To decompose the three-way interactions, I first tested whether independents (interdependents) were more persuaded by descriptive norms of the situational (social) reference group (H_1) by running my analyses in the private decision context. In the private context, the two-way interaction was marginally significant ($F(2, 486) = 2.97$, $p = .052$). The simple effects of reference group were also marginally significant for both independents ($F(2, 486) = 2.39$, $p = .092$) and interdependents ($F(2, 486) = 2.86$, $p = .058$). Planned contrasts revealed that for independents, descriptive norms about the situational (vs. social) reference group were directionally more persuasive in increasing donation amounts ($M_{situational} = 2.52$, SD = 2.07 vs. $M_{social} = 2.05$, SD = 2.10; $p = .288$). Donation amounts were significantly higher in the

situational reference group than the control condition ($M_{situational}$ = 2.52, SD = 2.07 vs. $M_{control}$ = 1.53, SD = 1.91; $F(1, 486) = 4.79$, $p = .029$). However, the amounts did not differ between the social reference group and control conditions (M_{social} = 2.05, SD = 2.10 vs. $M_{control}$ = 1.53, SD = 1.91; $p = .288$). For interdependents, descriptive norms about the social (vs. situational) reference group were significantly more persuasive in increasing donation amounts (M_{social} = 2.64, SD = 2.13 vs. $M_{situational}$ = 1.55, SD = 2.02; $F(1, 486) = 4.92$, $p = .027$). Donation amounts were significantly higher in the social reference group than the control condition (M_{social} = 2.64, SD = 2.13 vs. $M_{control}$ = 1.70, SD = 2.20; $F(1, 486) = 3.61$, $p = .058$). However, they did not differ between the situational reference group and control conditions ($M_{situational}$ = 1.55, SD = 2.02 vs. $M_{control}$ = 1.70, SD = 2.20; $p = .758$). Together, these results were generally consistent with H_1.

Next, I tested whether the effects of descriptive norms on independents and interdependents' prosocial behavior is contingent on decision context (H_4 and H_5). I thus tested the interaction of reference group and decision context on donation amounts for each self-construal conditions. For independents, the interaction of reference group and decision context was significant ($F(2, 486) = 5.31$, $p = .005$; see Figure 3.2). As predicted, independents who saw the descriptive norms of the social reference group donated more in public (M = 3.27, SD = 2.04) than in private (M = 2.05, SD = 2.10; $F(1, 486) = 7.48$, $p = .006$). Those who were presented with the norms of the situational reference group donated similar amounts in public (M = 2.26, SD = 2.29 and private (M = 2.52, SD = 2.07; p = .556). These results supported H_4 and are consistent with the interpretation that changes in perceived distinctiveness of reference groups due to the heightened sensitivity to immediate surroundings in public may have played an important role for independents. Additional analysis revealed that independents in the control condition donated more in public than private (M_{public} = 3.23, SD = 1.89 vs. $M_{private}$ = 1.53, SD =

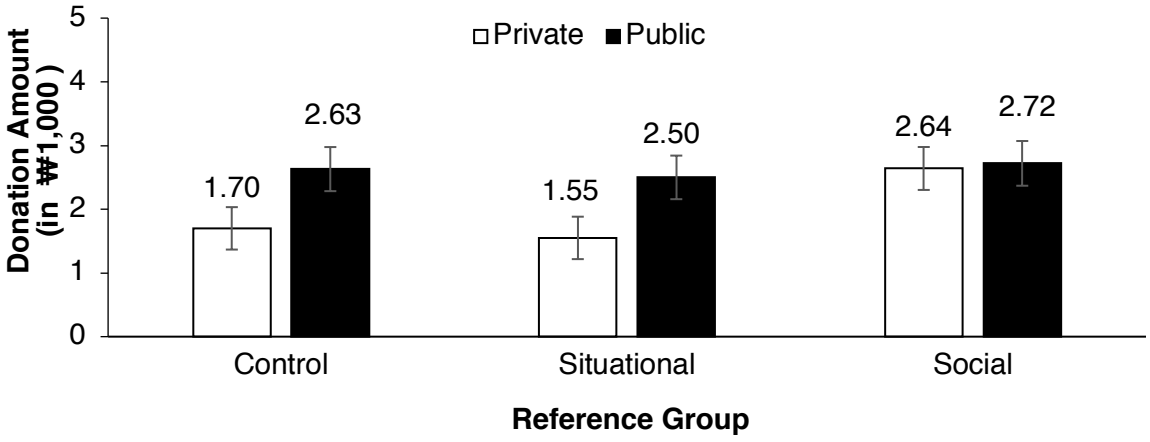
1.91; $F(1, 486) = 14.63$, $p < .001$).

Figure 3.2. Donation amounts by reference group and decision context for independents in Study 5. Error bars reflect ±1 standard error of the mean.



For interdependents, although the interaction of reference group and decision context was not significant ($p = .353$), I conducted planned contrast analyses to test H_5 (see Figure 3.3). As predicted, interdependents who saw the descriptive norms of the situational reference group donated more in public (M = 2.50, SD = 2.21) than private (M = 1.55, SD = 2.02; $F(1, 486) = 3.95$, $p = .048$). Those who viewed the norms of the social reference group donated similarly in public (M = 2.72, SD = 2.25) and private (M = 2.64, SD = 2.13; $p = .871$). These results are consistent with H_5 and an explanation that lower levels of group identification with the social reference group due to greater attentiveness to situational context in public may have played an important role for interdependents. Additional analysis revealed that interdependents in the control condition donated more in public (M = 2.63, SD = 2.16) than private (M = 1.70, SD = 2.20; $F(1, 486) = 3.69$, $p = .055$).

Figure 3.3. Donation amounts by reference group and decision context for interdependents in Study 5. Error bars reflect ±1 standard error of the mean.



To make sure that the results were not driven by different levels of salience of reference group identities, I conducted an additional ANOVA. Results showed no effect of reference group on the salience of group identities ($p = .457$). Manipulation checks revealed that people perceived college students to be most important, followed by helpful people and then by study participants ($M_{social} = 5.14$, SD = 1.36 vs. $M_{control} = 4.39$, SD = 1.76 vs. $M_{situational} = 2.42$, SD = 1.57; $F(2, 495) = 133.07$, $p < .001$).

Discussion

Results from Study 5 provided further support for the hypothesis that descriptive norms of situational and social reference groups are differentially persuasive for independents and interdependents. Whereas, in private, American students donated more when they learned how other study participants (i.e., the situational reference group) behaved, Korean students donated more when they learned how other college students (i.e., the social reference group) behaved. As might be expected, donation amounts were generally higher in public than private conditions for both independents and interdependents with two notable exceptions. When the descriptive norms for the situational reference group were presented in a public setting, independents did not donate more than they did in private. When interdependents viewed the descriptive norms for the

social reference group in public, they did not donate more than they did in private. These results serve to bolster the evidence in support of my account of the underlying processes (H_4 and H_5). The findings are consistent with the interpretation that the public (vs. private) decision context decreased the perceived distinctiveness of conforming to the situational reference group for independents and decreased identification with the social reference group for interdependents.

GENERAL DISCUSSION

The present research examined the effect of self-construal and descriptive norms of reference groups on prosocial behavior. Across five studies incorporating multiple methods, I provide converging evidence that consumers with an independent self-construal are more likely to be persuaded by descriptive norms about reference groups that are situationally relevant to the immediate decision rather than a social reference group. The more independent a consumer is, the more likely she may be to perceive distinctiveness in conforming to a situational (vs. social) reference group, and the greater her susceptibility to a situational reference group. Because perceived distinctiveness of a situational reference group decreases for independents in public, these patterns of independents' responses to descriptive norm information are more prevalent in private decision contexts. Compared to private decision contexts, independents are more likely to follow descriptive norms of social reference groups in public. In contrast, those with an interdependent self-construal are more likely to follow descriptive norms about a social (vs. situational) reference group that represents an important identity. The more interdependent a consumer is, the more likely she is to identify with a social (vs. situational) reference group, with greater conformity to the social reference group. Compared to private decision contexts, public

decision contexts increase interdependents' sensitivity and conformity to the group that is proximate to the situation at hand (i.e., situationally-relevant reference group) given their propensity for attunement to contextual surroundings and identification with people in their environment.

This research makes several contributions to the literature. First, it advances our understanding of normative influence. While the preponderance of evidence showed that a psychological affiliation with the reference group is necessary to trigger conformity to group norms (e.g., Harmeling et al. 2017; Turner 1991), previous research has also shown that norms of reference groups that are proximate to immediate surroundings can be persuasive regardless of a psychological affiliation (e.g., Goldstein, Cialdini, and Griskevicius 2008; Schultz, Khazian, and Zaleski 2008). I account for these mixed findings on normative influence by considering the role of self-construal. By doing so, I provide insights about the contexts in which a strong identification and affiliation with a group would matter and those in which perceptions of situational proximity would matter in increasing consumers' prosocial behavior.

Second, it contributes to the literature on self-construal. Although it is generally believed that interdependents are more likely to conform to group norms, it remains an important question whether different types of reference groups exert different levels of persuasion. Contrary to what one might expect, my findings suggest that merely referencing a group does not guarantee that interdependents will be susceptible to the normative influence of that group to engage in prosocial behavior. In public decision contexts, the situational reference group may exert a stronger normative influence than they would in private. In private decision contexts, descriptive norms of groups that represent socially important identities are effective in promoting prosocial behavior among interdependents. This suggests that previously documented relationships

between normative messages and persuasion in an independent culture (e.g., Goldstein, Cialdini, and Griskevicius (2008) may not generalize to other cultures.

Third, by comparing situational and social reference groups, my research conceptually and empirically distinguishes the persuasive appeals of primed versus chronic in-groups. Although previous research has considered chronic and situationally activated in-groups as substitutes (e.g., White and Dahl 2007), my findings reveal that when they are compared directly, they can have different levels of persuasive appeal for independents and interdependents. To the best of my knowledge, this is the first set of findings to uncover the differential effects of situationally relevant and socially meaningful groups on independents versus interdependents in the context of prosocial behavior. To deepen our understanding, future research is needed to examine the effects of socially meaningful groups that are also situationally relevant. In my studies, I intentionally treated situational and social reference groups as being distinct. However, there could exist reference groups that are both situationally and socially relevant. For example, had I used a situational reference group that also represented an important collective identity, such as college students or classmates in the behavioral lab, interdependents might have increased their prosocial behavior. Future research should more fully explicate the role of various dimensions and types of reference groups.

Managerial Contribution

Both non-profit and for-profit organizations use normative appeals to promote prosocial behavior in a variety of ways (e.g., encouraging environmental sustainability, soliciting donations, or donating a proportion of product sales to charities). my research suggests that optimal marketing strategies to increase persuasion and conformity may depend on the target market's cultural orientation. Based on my theorizing and empirical findings, effective

descriptive norm campaigns should strategically match the reference group with customers' self-construal. Thus, firms should correctly identify the target group of marketing campaigns that contain descriptive norm appeals. As my cross-cultural study (Study 5) suggests, firms may wish to consider using a situational reference group in independent cultures and a social reference group in interdependent cultures, as long as consumers are making decisions about prosocial behavior in private.

If the target group is not culturally homogeneous, there may be variability in how members of the target group construe the self. Indeed, even within an individualistic or collectivistic culture, people differ in their degree of interdependence and independence (Singelis 1994). However, firms that pursue mass marketing strategies that target a more diverse group of people can deliberately prime independence or interdependence through their use of descriptive norm information about a situational or social reference group, respectively. As Studies 1-3 demonstrate, situationally priming self-construal can make people adopt a more independent or interdependent orientation. In particular, using subtle verbal cues in advertising messages can be effective and easily employed by marketers to prime independence or interdependence (e.g., Ma, Yang, and Mourali 2014).

Finally, firms that target independents should be especially strategic about where they place their communications. My findings provide insights on how the situational contexts within which normative messages are placed have different levels of influence on independents versus interdependents. Because independents are more likely to follow reference groups that fit well with their immediate situational circumstances, marketers should place normative messages using the appropriate situational reference groups in the place where decision making occurs. For example, if marketers wish to increase donation amounts among independents by stating that

most shoppers at a mall donated to a cause, placing the message and soliciting donations at the mall would be effective. However, it is noteworthy that in such situations, providing a private decision context would enhance the effectiveness of the communications. For interdependents, the match between the reference group and the situational circumstances at the time of decision making (e.g., making a donation) is less critical, unless decisions are made in public. As interdependent consumers are readily responsive to descriptive norms about reference groups they identify with strongly, firms that target interdependents can be more flexible about where they deliver their messages.

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APPENDIX

STIMULI IN THE SITUATIONAL REFERENCE GROUP CONDITION IN STUDY 2

Notes: The signs on the left (right) were used for the independent (interdependent) self-construal condition. The signs used in the social reference group condition highlighted a social identity of university affiliates, and those used in the control condition contained generic environmental messages.

CHAPTER 4

Conclusion

Any decision making involves processing a variety of contextual information. I investigated contextual information surrounding time and group norms to develop a better understanding of the power of goal gradient effect and normative influences, respectively, in domains involving wellbeing. My dissertation work suggests that marketers should not only carefully decide which contextual information to provide to consumers but should also pay close attention to *how* they frame the information to consumers. Even when dates of precommitment and goal progress remain the same among consumers, framing the time lag between these two dates to appear shorter will increase consumers' motivation to complete a goal. When providing information regarding descriptive norms, framing the reference group as a commonly shared group identity and as a distinctively unique group to the given situation will increase interdependents' and independents' conformity, respectively, to prosocial norms.

Framing contextual information can act as a subtle yet powerful intervention that facilitates prosocial behavior, with implications to policy makers, charities, and business practitioners. Bounding a time lag between precommitment and goal progress can contract the perceived length of the lag and restore the goal gradient effect for a volunteering goal. Using social reference groups for interdependents and situational reference groups for independents can increase conformity to prosocial norms in relatively private decision settings. Future research should further test the robustness of the goal gradient effect in precommitment and examine the

complex role of the decision context in changing independents' and interdependents' conformity to descriptive norms.