Temporally Designing the Consumer Experience: Three Essays Examining the Influence of Time Architecture on Consumer Behavior

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Temporally Designing the Consumer Experience: Three Essays Examining the Influence of Time Architecture on Consumer Behavior

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How can the temporal aspects of the consumer experience be strategically constructed and communicated to improve consumer behavior and decision-making? This dissertation advocates for the explicit and systematic integration of time as a determining factor in consumer experiences, presenting three essays investigating different dimensions of *time architecture*, the temporal design of a consumer experience: temporal *sequencing* of planning prompt nudges (Essay 1), temporal *partitioning* of initial charitable contributions (Essay 2), and temporal *duration* of contemporary online promotions (Essay 3).

Essay 1 explores how the timing of planning nudge delivery impacts intervention effectiveness in tasks containing an optimal "early bird" deadline (i.e., after which benefits of task completion diminish). Results from three studies find that planning prompt nudge reminders delivered after the optimal deadline are significantly more effective than control reminders but offer little benefit when implemented before the optimal deadline. These findings call for 1) strategic temporal management of planning prompts and 2) increased research exploring the ideal timing of nudge delivery.

Essay 2 investigates how temporal aspects of giving perpetuate donor support. Consistent with an anchoring account, results from five studies demonstrate that prior donors who initially give a recurring time-dispersed gift (e.g., monthly $10 gift for 12 months) subsequently donate less than those who initially give a one-time lump-sum gift of the equivalent total amount (e.g.,

single $120 gift). Several approaches for offsetting recurring donors' later reduced giving are tested and implications for charities are discussed.

Essay 3 questions the degree to which contemporary instantiations of online time scarcity promotions (e.g., one-hour flash sales with countdown timers) can be presumed to operate in ways theoretically and empirically consistent with foundational demonstrations of time scarcity marketing tactics, which largely predate modern online retailing and predominantly involve offline contexts (e.g., printed newspaper ad). Results from 26 new studies find that present-day online time scarcity promotions may not be as effective as generally assumed, consistent with the argument that these promotions represent a novel theoretical and empirical phenomenon.

Together, these essays demonstrate that the temporal design of a consumer experience can promote or undermine traditionally accepted marketing practices, thereby warranting systematic investigation and proactive management.

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For my parents, Ed and Kathy,

with love

1.0 Introduction

"Explain time? Not without explaining existence.

Explain existence? Not without explaining time."

— John Archibald Wheeler (1986)

Time is a fundamental aspect of the human experience—inherently embedded within everything we do, think, and feel. Likewise, time is a fundamental feature of the consumer experience—malleable, yet completely inseparable from, all aspects of consumption. Consumer experiences do not occur in a temporal vacuum. Time represents not only an economic commodity for consumers to manage and allocate, but also an instrumental force shaping consumers' psychological experience (Jacoby, Szybillo, and Berning 1976). Time, however, remains largely unrecognized as an independent substantive content domain within marketing. Historically, time has been treated as incidental, rather than integral, to the consumer experience, with much of the prior work involving time focusing on its subjective perception (e.g., Gorn et al. 2004; Monga and Bagchi 2012; Siddiqui, May, and Monga 2014) or comparison to money (e.g., Macdonnell and White 2015; Mogilner and Aaker 2009; Okada and Hoch 2004).

This dissertation advocates for the explicit, systematic, and thoughtful integration of time as a determining factor in consumer experiences, seeking to answer the following overarching research question: How can institutions, firms, and policy-makers strategically construct and communicate temporal aspects of the consumer experience to maximize the value of traditional marketing practices? The current work is part of an ongoing program of research investigating how time can be strategically used to improve consumer behavior and decision-making. The three

essays presented in this dissertation examine *time architecture*, the temporal design of consumer experiences.

Time architecture involves the construction and communication of time-related aspects of consumer experiences. I conceptualize time architecture as consisting of six dimensions: sequencing, partitioning, duration, velocity, framing, and signaling (summarized in Table 1). Although time architecture may go unnoticed, it is never neutral. Each dimension characterizes a temporal feature innate to the consumer experience. Consequently, each dimension represents a potential mechanism by which marketers can shape consumer behavior. Sequencing refers to order and frequency associated with consumer experiences, such as serial arrangement, variety, or simultaneity. Positioning represents the temporal separation and categorization of aspects of consumer experiences, such as consumption delays, spacing, and temporal divisions. Duration involves the temporal length of an experience, including monitoring time elapsed and generating time pressure. Velocity captures aspects of an experience related to rate and intensity, such as speed and acceleration occurring over the course of a consumption episode. Framing broadly refers to the presentation and perception of temporal features of an experience, such as how to display timing elements or discuss temporal comparisons. Signaling represents the meaning and inference-making caused by and resulting from temporal features of an experience, such as lay theories and social-signaling value regarding the length of a consumer experience.

Table 1 Dimensions of Time Architecture

Dimension	Definition	Select Content Examples
Duration	length and constraint	temporal boundaries, deadlines, scheduling, time pressure/slack
Sequencing	ordering and frequency	serial arrangement, variety, repetition, simultaneity, synchrony
Partitioning	separation and categorization	temporal markers, interruptions, delays, spacing, mental accounting
Velocity	rate and intensity	acceleration/deceleration, inertia, speed, pacing, peak-end effects
Framing	presentation and perception	numerosity, time styles, opportunity costs, temporal construal, nostalgia
Signaling	meaning and inferences	symbolism, lay theories, self-signaling value, temporal inferences

Each essay in this dissertation touches on a different dimension of time architecture (i.e., sequencing, partitioning, and duration), examining how these temporal features can promote or undermine the influence of traditional marketing practices: temporal sequencing of planning prompt nudges (Essay 1), temporal partitioning of initial charitable contributions (Essay 2), and temporal duration of contemporary online promotions (Essay 3).

Essay 1 ("Prompts with Punch: Timing Planning Nudges for Maximum Effectiveness"), examines how the efficacy of planning prompt nudges depends on the timing of delivery relative to an optimal deadline. Although prompting plan making can offset procrastination and increase task completion for traditional terminal deadline tasks (e.g., those with a single "last chance" deadline), we know little about its effects for the many tasks that also contain an optimal deadline, after which benefits of task completion diminish (e.g., "early bird" deadline). This paper explores how the timing of planning prompt nudge delivery impacts intervention effectiveness in such cases. Results from three studies, including a consequential online lottery and a large-scale field experiment involving students filing the Free Application for Federal Student Aid (FAFSA), suggest that although planning nudges implemented before an optimal deadline appear to offer little benefit over simple reminders, these prompts are significantly more effective than control messages if delivered after the optimal deadline. These findings call for 1) strategic temporal management of planning prompt nudges and 2) increased research exploring the ideal timing of nudge delivery, as understanding how time-related decisions alter the efficacy of established behavioral interventions enriches both our theoretical and practical use of these tools.

Essay 2 ("Giving Again: Temporal Structure of Initial Contribution Impacts the Size of Donors' Subsequent Gift") investigates how temporally partitioning consumers' initial charitable contributions as a recurring time-dispersed gift (e.g., monthly $10 gift for 12 months) versus a

one-time lump-sum gift of the equivalent total amount (e.g., single $120 gift) impacts the size of subsequent support. Charities often approach prior donors with additional charitable requests. How should they do this? What factors influence prior donors' later generosity? Whereas much of the existing charitable giving literature focuses on the initiation of charitable giving, not its continuation, the current paper identifies time architecture of an initial donation experience (specifically, the temporal structure of donors' first contribution) as a critical factor shaping subsequent charitable support. Consistent with an anchoring account, but inconsistent with predictions based on hedonic editing or self-signaling, results from five studies demonstrate that when asked to make a second contribution to the same charity, prior donors whose initial charitable contribution was structured as a recurring time-dispersed gift subsequently give less than prior donors who initially donated the same amount structured as a one-time lump-sum gift. Several approaches for offsetting recurring donors' later reduced giving are tested, including explicitly cueing a more favorable anchor during the second appeal (Study 3), weakening the informational value of the unfavorable anchor (Study 4), and designing the initial donation experience to encourage lump-sum giving (Study 5A and 5B). Taken together, this work highlights the importance of examining how temporal aspects of giving perpetuate donor support and offers charities possible tools for maximizing donors' continued giving.

Essay 3 ("Time's Out: Examining the Effectiveness of Contemporary Online Time Scarcity Promotions") broadly examines the use and effectiveness of time scarcity tactics as they appear in contemporary online retail contexts. With foundational demonstrations of time scarcity marketing tactics (e.g., limited time offers) largely predating modern online retailing and predominantly involving offline marketing (e.g., newspaper ads), to what degree can contemporary instantiations of online time scarcity promotions (e.g., flash sales) be presumed to operate in theoretically and

empirically consistent ways? This paper presents a systematic approach to answering this question. First, we identify theoretically important differences between the contexts in which original time-scarcity theories were developed and the current marketplace where they are applied. Second, we conduct a retrospective analysis of offline empirical work, finding limited generalizable insight relevant to modern online time scarcity appeals. Finally, we report 26 new studies sampling from a range of products, timeframes, digital domains, formats, and indicators of product valuation. Both single-study analyses and single-paper meta-analyses suggest that although a few isolated studies find favorable online time scarcity effects for select measures, contemporary online time scarcity promotions primarily have negligible (or adverse) effects. Together, these findings suggest that present-day online time scarcity tactics may not be as effective as previous offline time scarcity research and pervasive marketplace usage might suggest, offering a grounded argument for treating online time scarcity promotions as a new theoretical and empirical phenomenon.

Together, these essays advance our understanding of time and consumer behavior, showing that without considering the time architecture of consumers' experience of marketing actions, marketers run the risk of profoundly miscalculating or overestimating their anticipated efficacy. Essay 1 demonstrates that the timing of planning prompt nudges can alter the intervention's effectiveness, suggesting evaluations of behavioral economic nudges should advance beyond questions of *whether* a tool is effective to assessments of *when* a tool may be maximally effective. Essay 2 identifies a previously unrecognized aspect of early donation experiences (i.e., temporal structure of donation payment) that can shape later patterns of giving, highlighting how temporal aspects of giving perpetuate donor support and offering charities possible tools for maximizing donors' continued giving. Essay 3 identifies several reasons to question the applicability of previous assumptions underlying offline time scarcity marketing tactics to the modern-day online

marketplace, modeling a novel approach to revisiting past theory and determining the robustness of a given effect across temporal and market changes. Together, these essays argue that the temporal design of a consumer experience can promote or undermine traditionally accepted marketing practices, thereby warranting systematic investigation and proactive management.

2.0 (Essay 1) Prompts with Punch: Timing Planning Nudges for Maximum Effectiveness

Embedding plan-making prompts in messages offers a light-touch way to nudge individuals toward task completion, especially when individuals face time-sensitive tasks (e.g., Mazar, Mochon, and Ariely 2018; Milkman et al. 2011; Nickerson and Rogers 2010). However, individuals often encounter tasks that involve a more complex deadline structure, in which taking action prior to an "optimal" deadline (the final opportunity to take maximally beneficial action) offers greater benefit than waiting to act until a later "terminal" deadline (the final opportunity to take any beneficial action). These optimal-deadline tasks are ripe for procrastination. We can put off taking action until after the optimal deadline passes, but such procrastination can have substantial costs. For example, though college students can access a larger pool of financial aid by submitting applications earlier, many students procrastinate, reducing the amount of aid they can access. Potential conference attendees can register before an early bird deadline to receive a discounted price, but many find themselves paying higher prices at or near the final registration deadline–despite intending, months in advance, to attend. Individuals who enter lotteries for schools or housing may benefit from entering sooner but may delay application processes – though they are aware of the needs they face and the declining availability. How can firms and policy makers use planning prompt nudges to encourage behavioral follow-through for tasks containing optimal deadlines? When will planning prompt nudges be more effective: before or after the optimal deadline?

Rationally, one might presume that nudging plan-making prior to the optimal deadline would most effectively reduce procrastination. Such early nudges would offer the greatest benefit of action as well as more time to make and enact a plan. However, we find that the benefit of

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planning prompts relative to control messages primarily emerges *after* an optimal deadline has passed. In both a controlled lottery experiment and a large-scale field study, we observe null or weak effects of planning nudges implemented prior to optimal deadlines, relative to identical control messages without planning prompts. By contrast, encouraging plan-making *after* the optimal deadline is more effective than control messages delivered at the same time.

In showing these effects, the present paper offers both theoretical and practical contributions. First, despite general acknowledgement that behavioral interventions are sensitive to timing (McBride, Emmons, and Lipkus 2003), research is only more recently beginning to identify specific circumstances that alter the effectiveness of planning prompt nudges (e.g., Bayuk, Janiszewski, and LeBoeuf 2010); to our knowledge, no prior work considers how the temporal structure of planning nudge delivery shapes decision making. Second, although planning nudges are widely recommended as a tool to facilitate behavioral task completion, firms and policy makers intending to use these behavioral interventions receive little guidance regarding when to implement them. Our findings indicate that organizations seeking to harness the power of planning prompts could benefit from managing the temporal context in which nudges operate. This may involve, for example, deliberately scheduling nudges to follow optimal deadlines or constructing task deadline structure to accommodate pre-existing timetables.

More broadly, this research raises questions about the time sensitivity of various behavioral economics tools, particularly those that involve tasks that include deadlines, temporal landmarks, or multi-part processes. We hope that in showing the importance of timing, we advance the evaluation of nudge interventions beyond questions of *whether* a tool is effective to assessments of *when* a tool may be maximally effective, and for what types of tasks.

2.1 Planning Prompt Nudges and Optimal Deadlines

Given a tendency for present-biased preferences, people often delay action (O'Donoghue and Rabin 1999), even for important and enjoyable tasks (Shu and Gneezy 2010). Although deadlines can help (e.g., Ariely and Wertenbroch 2002), they may not overcome procrastination for a host of reasons: the planning fallacy may lead us to be overconfident about our likelihood to complete a task (Buehler, Griffin, and Ross 1994; Kahneman and Tversky 1979); intertemporal discounting may lead us to focus on short-term gains, undermining the potential for long-term behavioral change (e.g., Kirby and Herrnstein 1995); multiple goals or plans may weaken our commitment to any given objective (Dalton and Spiller 2012); or the gain enjoyed from taking action may simply lack motivational power, as we have adapted to our current state (Frederick and Loewenstein 1999).

Planning, however, can offset procrastination and increase task completion. The benefits of creating concrete plans have been demonstrated in a broad range of domains, including positively influencing drug adherence (Brown, Sheeran, and Reuber 2009), smoking cessation (Armitage and Arden 2008), appointment attendance (Sheeran and Orbell 2000), safe driving (Brewster et al. 2016), and healthy food consumption (Armitage 2004). Perhaps most notably, Nickerson and Rogers (2010) showed that encouraging plan-making during a "get out the vote" voter-mobilization phone campaign increased voter turnout in the 2008 presidential election by 4.1 percentage points.

Since then, efforts have been made to use a similar planning mechanism to promote behavior enactment using low-cost, scalable interventions. These interventions, which we broadly refer to as *planning prompt nudges*, are light-touch tactics that simply promote plan-making. Planning prompt nudges include, for example, emails containing phrasing encouraging people to

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engage in planning-related actions (e.g., "make a plan," "planning ahead can help," "schedule time in your calendar"), as well as reminder postcards providing designated space for recipients to generate specific plans by writing down the precise date and time they will perform a behavior (e.g., "write your plan here: _____ (day of the week), _____(month) ___ (day) at _____(time)"). Another example includes prerecorded phone messages in outbound reminder calls that prompt the listener to interactively indicate a timeframe for engaging in a behavior (e.g., "if you plan on enrolling in the next 24 hours, press 1; if you plan on enrolling in the next week, press 2; if you plan on enrolling in the next month, press 3").

We know from prior research that planning prompt nudges tend to work. Exposure to planning prompts tends to increase the likelihood of task completion. Planning nudges implemented in field contexts, for example, have been shown to reduce credit card delinquency (Mazar et al. 2018), boost flu vaccination rates (Milkman et al. 2011), and increase preventative health screenings (Milkman et al. 2013). These studies have shown a positive impact on desired behavior between approximately .95 to 4.2 percentage points (see Table 2), improvements that are particularly impressive considering the minimal added cost required for implementation.

Notably, planning prompt nudges generally do not require people to actually engage in plan-making. In Nickerson and Rogers (2010), potential Pennsylvania voters actively responded to planning-related questions when speaking with a live agent on the phone (e.g., "where do you expect to be coming from when you go to the polls?"), allowing for the direct observation of plan-making behavior. Unlike that intervention, however, many planning prompts are more passively experienced, encountered as a unidirectional communication from a marketer, firm, or institution (e.g., embedded within a postcard), in which direct observation of plan-making behavior is often not possible or not measured. Indeed, much of the prior research on planning prompt nudges,

especially work with large-scale field experiments, focuses less on verifying actual engagement in planning behavior and more on assessing the intervention's efficacy with respect to the target outcome behavior (e.g., voted, received flu shot, completed health screening). In demonstrating a positive impact of planning prompts on target behavior without empirical evidence confirming planning behavior occurred, existing research suggests that merely presenting planning prompts may be sufficient for stimulating consumer action.

Table 2 (Essay 1) Summary of Effects from Select Studies since Nickerson and Rogers (2010) Using Planning Prompt Interventions

Article	DV	Intervention Mechanism	Impact
Nickerson and Rogers (2010) *Psychological Science*	Voter Turnout	Phone Call with Live Agent to Facilitate Plan Making	• Increased voter turnout in U.S. election by 4.1 percentage points
Milkman et al. (2011) *PNAS*	Flu Vaccination	Reminder Mailer with Planning Prompt	• Increased flu immunization rate by 4.2 percentage points
Milkman et al. (2013) *Preventive Medicine*	Colonoscopy Screening	Reminder Mailer with Planning Prompt	• Increased screenings by .95 percentage points
Brewster et al. (2016) *Journal of Experimental Psychology: Applied*	Speeding	Implementation Intention Task at End of Questionnaire	• Decreased instances of speeding in driving simulator by 16.73 percentage points
Anderson et al. (2018) *Political Psychology*	Voter Turnout	Planning Prompt Questions after Online Ad Exposure	• Increased self-reported turnout in Canadian election by 4.12 percentage points
Mazar et al. (2018) *Journal of Consumer Psychology*	Credit card Delinquency	Interactive Voice Response Call with Planning Prompt	• Increased likelihood of paying account current by 2.26 percentage points • Reduced time to cure account by .23 days

Despite the benefits of planning nudges documented in the literature, to date, extant empirical evidence has examined behavioral tasks involving a single, terminal deadline. *Terminal deadlines* (visualized in Figure 1, Panel A) represent the time after which action is no longer

possible or beneficial. With in-person voting, for example, the terminal deadline is when a polling location closes. After that time, individuals cannot cast a ballot in the current election. With marketing promotions, the terminal deadline is when the discount ends, after which consumers pay full price. Shoppers can only take advantage of one-day Cyber Monday deals, for example, by placing online orders before sales end at midnight. With commercial airline boarding, the terminal deadline is when the gate closes, after which passengers miss their flight. With free influenza vaccination workplace events, the terminal deadline is when the on-site clinic closes.

Many tasks, however, involve more complex deadline structures. Many tasks additionally contain an *optimal deadline* (Figure 1, Panel B). For tasks with benefits that diminish over time, optimal deadlines identify the time after which action is less beneficial. Consider, for example, conference registration fees. Often, the registration price increases as the conference date nears, with discounted pricing available if you enroll before an early registration deadline (e.g., 25% discount if register at least 3 months before the event date, 5% discount if register at least 1 day before the event date, full price if register on-site the day of the event). In such cases, the early registration deadline marks the end of the largest available discount and the final opportunity to register for the conference with maximum price savings, although individuals can still register (often with a discount) after the early registration period ends. As such, the early registration deadline represents an optimal deadline with respect to registration fees.

When defining optimal deadlines, it is noteworthy to emphasize that use of the term benefit is not synonymous with total utility. Rather than indicating the single ideal time for a consumer to act, an optimal deadline indicates the best opportunity for a consumer to obtain a specific marketer-defined advantage. With the conference registration scenario, for example, the focal advantage of enrolling before the early bird offer ends is price savings. Discount size represents the defining

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feature on which the registration deadlines are temporally structured for and communicated to potential registrants. To maximize this specific benefit (i.e., receive the largest discount), therefore, you must register before the early bird promotion–the optimal deadline–ends. You may personally experience greater total utility, however, by waiting to register until after the early bird offer expires, such as if you have not yet determined whether you can attend the conference. The "optimal" in optimal deadline, therefore, refers to a specific marketer-defined advantage, not to a global assessment of utility.

In contrast to a terminal deadline, which indicates the final opportunity to take *any* beneficial action, an optimal deadline indicates the final opportunity to take *maximally* beneficial action. Undergraduate students, for example, face both an optimal deadline and a terminal deadline when an instructor accepts, but penalizes, late assignments (e.g., 20% grade reduction per day). The stated due date represents the optimal deadline for assignment submission, offering students the opportunity to earn the highest possible grade. Students, however, can still benefit from submitting a late assignment and potentially receive partial credit if the late submission occurs before the terminal deadline, the date at which the grade penalty for late assignments leaves students with no opportunity to earn partial credit. Missing the optimal deadline, therefore, does not wholly eliminate the benefit of taking action.

Additional examples of optimal deadlines include marketers tempting consumers with extra savings or free gifts that exclusively occur during the first few hours of an online sale (e.g., "early-bird" promotions), after which the "regular" promotional discount applies; banks providing interest-free credit if balance is paid before a predetermined date, after which interest is imposed; and municipalities offering early payment discounts for property taxes and parking tickets paid weeks before their stated due date, after which the full amount is owed and late-payment penalties

can accrue. In each of these examples, it is preferable to act by the optimal deadline; however, action may still be taken after the optimal deadline, with diminished benefit.

A.

B.

C.

Implement Planning Prompt Nudge

? ?

Figure 1 (Essay 1) Example Deadline Structure and Planning Prompt Nudge Delivery

2.2 When to Implement Planning Prompts?

For tasks containing an optimal deadline, when should marketers implement planning prompt nudges (Figure 1, Panel C)? Are planning prompts more effective than simple reminders before or after the optimal deadline? Answering this question offers both theoretical and practical insights. Practically, this can improve firms' existing and future implementation of planning prompt nudges and identify potential timeframes during which alternative (non-planning)

interventions may effectively influence behavior. Theoretically, this can advance understanding of the temporal dynamics of planning prompts (as well as behavioral nudges, more generally) and reveal the relevance of investigating more complex consumer deadline structures—specifically, optimal deadlines.

As no prior work has characterized optimal deadlines, we next discuss the consumer experience that unfolds surrounding them. This description allows us to identify when—before or after the optimal deadline—the unique characteristics of planning prompts could make them particularly effective in motivating task completion.

2.2.1 Reminders With (vs. Without) Planning Prompts Before Optimal Deadline

Rationally, one might presume that delivering planning prompts prior to the optimal deadline would most effectively reduce procrastination. Simply, implementing planning nudges prior to an optimal deadline offers the greatest possible marketer-defined benefit. In addition, delivering planning prompt nudges before an optimal deadline provides extra time for individuals to create and enact a plan, thereby minimizing issues arising from underestimating the time required to complete a task (Buehler et al. 1994) and overestimating our ability to remember to complete a task (Ericson 2011). Moreover, research suggests that planning prompt nudges may be more effective when opportunities for enacting behavior are temporally restricted (Dholakia and Bagozzi 2003; Milkman et al. 2011), as happens when the optimal deadline creates a time-limited window of maximally beneficial opportunity. Also, planning prompts are argued to reduce forgetfulness (Gollwitzer and Sheeran 2006; Milkman et al. 2013), the consequences of which are more severe during the pre-optimal deadline period (when individuals face greater procrastination costs).

However, there are reasons to doubt that planning nudges will be more effective than simple reminders before an optimal deadline. First, planning prompt nudges may be unnecessary during this time. By highlighting the maximum benefit possible and increasing the salience of procrastination costs, the optimal deadline itself may increase one's motivation and commitment to enact the intended behavior (e.g., Ariely and Wertenbroch 2002; Bagozzi and Dholakia 1999; Gollwitzer 1999; Heath, Larrick, and Wu 1999; Locke and Latham 1990). Because strong initial intentions already exist, individuals may underestimate the benefits of plan making and implementation strategies (Koehler, White, and John 2011), thus failing to respond to planning cues. Alternatively, the motivating nature of optimal deadlines may increase the likelihood that individuals organically engage in plan-making behavior (to facilitate obtaining the maximum possible benefit of action). However, because planning prompt nudges are ineffective for those who have already generated plans (Nickerson and Rogers 2010), encouraging plan making prior to an optimal deadline is redundant (Rogers et al. 2015) and, consequently, no more beneficial than a simple reminder (without planning prompts).

The more complex nature of optimal deadline tasks relative to terminal deadline tasks may also reduce the effectiveness of planning prompts in the early phase. Optimal deadline tasks may involve multiple dates, information about pre- and post-deadline benefits, and guidance as to task completion. If planning prompt nudges administered prior to an optimal deadline draw attention to this greater complexity, they may also raise perceived task difficulty and undermine goal commitment (Dalton and Spiller 2012). Pre-optimal deadline delivery of planning prompts may also weaken behavioral intentions by encouraging individuals to construct failure contingencies. Because action can still be taken after the optimal deadline, planning nudges occurring during the pre-optimal deadline period may promote a specific type of plan-making—the development of

backup plans (Shin and Milkman 2016). Merely thinking through a backup plan can undermine motivation for and probability of successfully enacting behavioral intentions (Shin and Milkman 2016), suggesting that planning prompt nudges implemented before the optimal deadline may even lead to less task completion than simple reminders (without planning prompts) delivered at the same time.

2.2.2 Reminders With (vs. Without) Planning Prompts After Optimal Deadline

When the optimal deadline passes, the maximum benefit of action ends. Failing to complete behavioral intentions before the optimal deadline, therefore, means foregoing the most beneficial outcome. Consequently, motivation to complete the focal task is anticipated to decay following the passage of the optimal deadline, ultimately deteriorating the likelihood that people will act on their intentions (e.g., Bandura and Cervone 1986; Bandura and Simon 1977; Cochran and Tesser 1996; Soman and Cheema 2004). It is during this post-optimal deadline period that we expect planning prompts to reveal their value.

On one hand, it could be argued that both simple reminders and planning prompts would be more effective after an optimal deadline than before it, but their efficacy relative to each other would not differ. In this way, the optimal deadline constitutes a goal-based reference point, with positive outcomes representing "gains" and negative outcomes representing "losses" (Heath et al. 1999). Missing the optimal deadline equates to missing out on the best possible benefits of action (e.g., largest discount); any subsequent action, by comparison, is suboptimal in that it exclusively offers diminished benefits (e.g., smaller discount). Thus, failing to enact behavioral intentions prior to the optimal deadline would be coded as a loss. If this mechanism were at play, any post-optimal

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deadline message may increase loss salience, making planning prompts no more effective than simple reminders.

We propose, however, that the unique characteristics of planning prompts preserve motivation after an optimal deadline has passed to a greater extent than do simple reminders. First, although any reminder after missing optimal deadline provides negative goal performance feedback (Bandura and Cervone 1986; Garland 1985; Locke et al. 1981), planning prompts can increase task involvement and elaboration (Gollwitzer and Sheeran 2009), which may renew the motivation necessary for task completion. Furthermore, by increasing perceived goal attainability (Gollwitzer 1999; Gollwitzer and Oettingen 2011), planning can restore a sense of self-efficacy, bolstering motivation to complete the task (Bandura and Cervone 1986).

Motivation may also be bolstered if planning nudges trigger "fresh start" effects (Dai, Milkman, and Riis 2014, 2015) in a way that simple reminders do not. Planning nudges encourage concrete thinking (Gollwitzer 1999), which tends to increase perceived differences versus commonalities (Goodman and Malkoc 2012; Lamberton and Diehl 2013; Förster, Liberman, and Kuschel 2008; Malkoc, Zauberman, and Ulu 2005). Applied to the task completion process, this concrete thinking may highlight the difference between the pre- and post-optimal deadline periods. In doing so, planning nudges draw attention to the optimal deadline as a temporal landmark (Peetz and Epstude 2016; Shum 1998) to a greater degree than simple reminders. Such clear temporal landmarks can trigger a "fresh start effect," increasing goal commitment and pursuit (Dai et al. 2014, 2015). Therefore, in raising the profile of the optimal deadline as temporal marker, planning nudges may make the post-optimal deadline period a fresh start, enhancing motivation more than simple reminders.

Taken together, we predict that implementing planning prompt nudges after the optimal deadline will more effectively promote task completion than simple reminder messaging delivered at the same time. During the pre-optimal deadline period, however, planning nudges are likely undervalued as an implementation tool, dismissed as redundant, or regarded as (performance-eroding) backup-plans, suggesting that planning prompts may be minimally beneficial prior to the optimal deadline. Motivation is argued to decay after missing the optimal deadline and, consequently, the likelihood of task completion is reduced. However, during the post-optimal deadline period, we predict that planning nudges can offset this decrement, boosting task completion to a greater degree than messaging without plan-making prompts.

2.2.3 Overview

We test this prediction in three studies. Using a promotional discount scenario, Study 1 provides a preliminary examination of the temporal sensitivity of planning prompt nudges in a marketing context and tests several alternative mechanisms that may account for consumers' post-optimal deadline motivational boost. Study 2, which consists of a field experiment involving nudging online lottery enrollment behavior, tests our predictions within a behaviorally consequential, yet decontextualized, domain. Study 3, a field experiment that involves nudging prospective students to complete the Free Application for Federal Student Aid (FAFSA), offers a large-scale test of our predictions for a more effortful task within a far-reaching financial domain.

In each study, individuals receive a target marketing message before or after the optimal deadline, the content of which either includes or excludes planning prompts. These marketing communications specify the focal benefits of task completion, which diminish over time: the size of a promotional discount in Study 1, the likelihood of winning a lottery in Study 2, and the

accessibility of college financial aid in Study 3. The optimal deadline for each study, therefore, consists of the final opportunity for consumers to obtain the maximal benefit identified in the marketing message.

Results from all three studies suggest that including planning prompts in marketing messages effectively boosts consumer motivation more than messages that exclude planning prompts, when those messages are delivered after the optimal deadline. For messages received prior to the optimal deadline passing, however, including planning prompts did not boost motivation more than messages without planning prompts. We conclude with a discussion of possible implications for firms, policy makers, and individuals.

2.3 Study 1: Marketing Promotion

Study 1 provides a preliminary test of our prediction that planning prompts are more effective than simple reminders after the optimal deadline. Study 1 examines evidence of motivation decay in terms of reduction in the perceived benefit of taking action. Participants imagine receiving email marketing communications highlighting a promotional discount that decreases over time (from 45% to 25%). The optimal deadline, therefore, represents the final opportunity for consumers to obtain the largest discount. Participants imagine receiving two emails: the first email notifies consumers of the promotion before the sale starts and the second email reminds consumers of the promotion after the sale starts. We manipulated the presence of planning prompts within the content of these notifications, such that the emails either included or excluded phrasing that encouraged plan-making. We manipulated the timing of the second notification, such that the individuals received the second email during the promotional period

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before or after the optimal deadline (i.e., largest discount period). We predict a message content (including vs. excluding planning prompts) by message timing (before vs. after optimal deadline) interaction, such that consumers will perceive promotions with planning prompts as more beneficial than promotions without planning prompts to a greater degree after the optimal deadline.

If motivation decays after the optimal deadline passes, as we suggest, then consumers would be expected to perceive the promotion to be less beneficial after the optimal deadline (when the discount is smaller) compared to before the optimal deadline (when the discount is largest). We argue that reminders that include planning prompt content, compared to reminders without planning prompt content, will offset this post-optimal deadline motivation decay. The motivational boost from including (vs. excluding) planning prompt content in marketing reminders, however, is argued to be comparatively lower prior to the optimal deadline passing (i.e., when consumer motivation would likely be higher overall). Thus, we predict that, for communications delivered after the optimal deadline, messages containing planning prompts will increase the perceived benefit of the discount more than messages without planning prompts. We do not anticipate, however, a similar level of increase for communications delivered before the optimal deadline.

In addition to changes to perceived benefit of the target behavior (i.e., buying the promoted product at a discount), Study 1 examines other potential mechanisms that may account for consumers' post-optimal deadline motivational boost (i.e., enhancing self-efficacy or self-esteem, restoring a sense of control, amplifying fresh start effects) and one alternative explanation (i.e., perceived fairness of the promotional offer). Furthermore, Study 1 also explores implications for consumer planning behavior. This study examines whether the previously predicted interactive effect of message content and timing subsequently impacts the likelihood that consumers will

engage in planning actions intended to facilitate completion of target behavior, such as setting a reminder or scheduling time in a calendar.

2.3.1 Method

We tested our predictions using a 2 (timing: before optimal deadline, after optimal deadline) x 2 (message framing: planning, control) between-subjects design, pre-registered on Open Science Framework (OSF; https://osf.io/5rknw). Prior to analysis, and consistent with OSF pre-registration, we excluded participants who reported technical problems or identified as a non-native American English speaker (n = 19), resulting in a final analysis sample of 681 participants (55.51% female;[1] M_{age} = 37.39 yrs, SD_{age} = 10.90 yrs). Prior to data collection, individuals indicated whether they were considering getting a new phone in the next six months. Only those considering a new phone purchase continued with the study. This pre-registered exclusion criteria was used to enhance participant involvement in the fictional promotion scenario used in this study.

In this study, participants imagined receiving two emails from Apple promoting an online iphone sale. The promotion consisted of a two-week sale in which Apple offers a discount that decreases over time: 45% off during the first week of the promotion and 25% off during the second week of the promotion. Consumers buying an iphone before the end of the first week of the sale would receive the largest possible promotional discount (i.e., 45% off), whereas consumers buying an iphone during the second week would receive a smaller discount (i.e., 25% off). Thus, the end of the first week of the sale represents the optimal deadline; this is the point at which the most beneficial pricing to the consumer ends. After this optimal deadline, consumers can still receive

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[1] Participants could choose among three options: "male" (n = 301; 44.20%), "female" (n = 378; 55.51%), and "prefer not to say" (n = 2; .29%).

discounted pricing (although smaller in magnitude), meaning purchase during the post-optimal deadline period offers more beneficial pricing than not purchasing during the sale (see Figure 4).

All participants imagined receiving the first promotional email days before the two-week sale began. This first email displayed a generic calendar to visually depict the timing of the promotion. The first week of the calendar was blank, except to denote the current day marking when participants received the first email (i.e., "Today!"). We marked the second week of the calendar as the first week of the sale (i.e., "Week 1: 45% off"), and we marked the third week of the calendar as the second week of the sale (i.e., "Week 2: 25% off"). After viewing the first promotional email, participants imagined deciding to purchase a new iphone during this sale and wanting to make their purchase before the first week of the sale ends.

Participants then imagined several days passing. The sale starts, but the participant has not yet purchased the iphone. At this point, participants imagine receiving a second promotional email message from Apple reminding them about the ongoing iphone sale. This second email displayed a calendar nearly identical to that presented in the first email, with the current date altered to reflect the timing of the second email (see Appendix A for stimuli).

To manipulate message timing relative to the optimal deadline, this second promotional email was received two days before or two days after the end of the first week of the sale. Thus, participants randomly assigned to the before optimal deadline condition received their second email during the first week of the sale, meaning their maximum discount was 45%. Participants randomly assigned to the after optimal deadline condition, however, received their second email during the second week of the sale, meaning their maximum discount was 25%.

To manipulate message framing, in both emails the promotional offer were presented with or without planning content. Similar to prior planning prompt research promoting plan-making

and scheduling (e.g., Mazar et al. 2018; Milkman et al. 2011), the first and second emails viewed by participants randomly assigned to the planning message condition included prompts explicitly encouraging consumers to engage in planning behavior (e.g., "plan ahead," "make a plan," and "pick a time and put it in your calendar now"). By contrast, the first and second emails viewed by those randomly assigned to the control condition did not include such planning prompts.

After reviewing both the first and second promotional emails, participants then completed several close-ended rating scales within the scenario context (items provided in Appendix B). To assess perceived benefit of the promotional discount (that they could obtain given timing of the second email), participants responded to three items on close-ended, seven-point rating scales (e.g., "How would you rate the benefit of getting this discount?" 1 = Not at All Beneficial, 7 = Very Beneficial). Calculating the average of these items generates a single index of perceived benefit (α = .934). Participants then completed one item measuring likelihood to engage in planning-related behaviors, such as setting a phone reminder or scheduling time in one's calendar ("How likely would you be to engage in any planning behavior in order to make your purchase?" 1 = Not at All Likely, 7 = Very Likely).

Afterwards, participants completed items assessing potential motivational mechanisms of planning prompts. To measure the possible influence on self-esteem and self-efficacy, participants additionally completed the ten-item Rosenberg (1965) Self-Esteem (RSE) scale (α = .913) and a four-item self-efficacy scale (adapted from Judge, Locke, Durham, and Kluger 1998; α = .922). Each scale was computed into an average index prior to analysis. Participants also responded to a reduced-item measure of internal locus of control (used by Hoffman, Novak, and Schlosser 2003) containing five items that were averaged into a single index (α = .726). The "fresh start" effect suggests that consumer motivation increases as the psychological separation between one's current

self and one's past (imperfect) self grows (Dai et al. 2015). Thus, to assess potential fresh start effects, participants completed a three-item (mean-composite) index of psychological distance (adapted from Dai et al. 2015; $\alpha = .810$). In addition to these mechanisms, we also assessed the alternative account that differences in perceived fairness of the deal may be driving consumers' motivational response. Participants completed six items measuring perceived fairness of the deal (adapted from Darke and Dahl 2003), which were computed into an average index ($\alpha = .864$).

Participants then completed an item regarding current product usage ("Do you currently have an Apple iphone (any model/year)?" 1 = Yes, 0 = No) and liking of online shopping ("I like shopping online," 1 = Strongly Disagree, 7 = Strongly Agree) for inclusion as pre-registered covariates. Finally, participants reported any technical difficulties with the survey and provided basic demographic data (e.g., age, gender, household income).

2.3.2 Results

Consistent with our pre-registration, all analyses control for current product usage (1 = currently has iphone, 0 = does not have iphone) and liking of online shopping (mean-centered). All subsequent results reflect covariate-adjusted estimates.

2.3.2.1 Perceived Benefit

Conducting a linear regression analysis predicting perceived benefit as a function of message framing (1 = planning prompt reminder, 0 = control reminder) and timing (1 = after optimal deadline, 0 = before optimal deadline), controlling for current product usage (1 = currently has iphone, 0 = does not have iphone) and liking of online shopping (mean-centered), we found no main effect of message framing (b = .095, SE = .086, t = 1.11, $p = .266$; Table 3, model 2).

Promotional messages with planning prompts, relative those without planning prompts, did not increase perceptions of the promotion's benefit (5.50 vs. 5.40). We did, however, observe a main effect of timing relative to the optimal deadline (b = -1.603, SE = .086, t = -18.74, $p < .0001$; Table 3, model 2). Individuals perceived the promotional offer as less beneficial when they received the second marketing communication after (vs. before) the optimal deadline (4.64 vs. 6.25).

These were both qualified, however, by a marginal framing x timing interaction (b = .320, SE = .171, t = 1.87, $p = .062$; Table 3, model 4).[2] For email reminders received after the optimal deadline, the presence of planning prompts increased the perceived benefit of the promotional offer relative to reminders that did not include planning prompts (4.77 vs. 4.52; t = 2.11, $p = .035$; Figure 2). However, for email reminders received before the optimal deadline, including planning prompts in the reminder did not increase perceived benefit of the promotional offer compared to reminders without planning prompts (6.21 vs. 6.28; t = -.53, $p = .596$).

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[2] No interaction effects with demographics emerged (p's ≥ .204). Likewise, results showed no interaction effects with demographics for the behavioral outcomes in Studies 2 (p's ≥ .267) and 3 (p's ≥ .353). For brevity, this will not be discussed further.

Table 3 (Essay 1) Study 1: Regression Results Predicting Perceived Benefit

	Model 1	Model 2	Model 3	Model 4
	b/(SE)	b/(SE)	b/(SE)	b/(SE)
Timing of Prompt x Message Framing			.307~	.320~
			(.173)	(.171)
Message Framing	.117	.095	-.036	-.064
(1 = planning prompt, 0 = control)	(.087)	(.086)	(.122)	(.121)
Timing of Prompt	-1.618***	-1.603***	-1.770***	-1.761***
(1 = after optimal deadline, 0 = before optimal deadline)	(.087)	(.086)	(.122)	(.120)
Constant	6.197***	6.092***	6.272***	6.164***
	(.075)	(.086)	(.086)	(.095)
Controls	N	Y	N	Y
Number of Cases	681	681	681	681
R^2	.340	.362	.343	.365
Adj. R^2	.338	.358	.340	.360
F	174.491	95.744	117.741	77.579
df	2	4	3	5
p	< .000001	< .000001	< .000001	< .000001

Note. Significance values are indicated as follows: ~ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$. Control variables include the following: current product usage (1 = currently has iphone, 0 = does not have iphone) and mean-centered liking of online shopping.

2.3.2.2 Planning Likelihood

We conducted a linear regression analysis predicting self-reported planning likelihood as a function of message framing (1 = planning prompt reminder, 0 = control reminder) and timing (1 = after optimal deadline, 0 = before optimal deadline), controlling for previously stated covariates (i.e., current product usage, mean-centered liking of online shopping). Results show no main effect of message framing (b = .083, SE = .111, t = .75, $p = .452$; Web Appendix A Table 2, model 3), such that those receiving promotional email reminders with planning prompt content were no more likely to engage in planning behaviors, as compared to those receiving the reminders without planning prompts (5.72 vs. 5.64). However, we did observe a significant main effect of timing relative to the optimal deadline (b = -.692, SE = .111, t = -6.26, $p < .0001$; Web Appendix A Table 2, model 3). Individuals who received the promotional email reminders after the optimal deadline, compared to those who received the email reminders before the optimal deadline, were less likely to engage in planning behaviors (5.33 vs. 6.02).

These results are qualified, however, by a marginally significant interaction between message framing and timing relative to the optimal deadline (b = .370, SE = .221, t = 1.67, *p* = .095; Web Appendix A Table 2, model 5). For promotional emails messages after the optimal deadline, including planning prompt content in the reminder (vs. not including planning content in reminder) marginally increased the likelihood of engaging in planning behavior (5.46 vs. 5.20; t = 1.72, *p* = .087). No difference emerged, however, for email reminders sent before the optimal deadline (5.97 vs. 6.07; t = -.65, *p* = .519; see Figure 2).

Study 1: Perceived Benefit   Study 1: Self-Reported Planning Likelihood
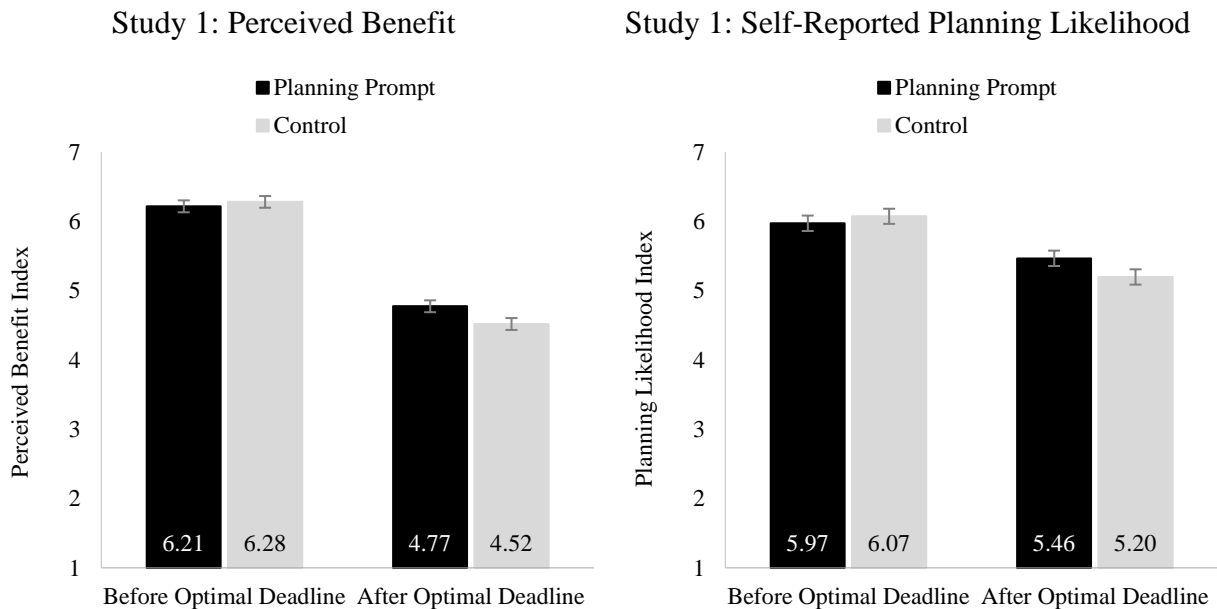
Note. Error bars represent standard errors. Estimates are adjusted for covariates (i.e., current product usage, mean-centered liking of online shopping)

Figure 2 (Essay 1) Study 1 Results Summary

2.3.2.3 Self-Efficacy

Repeating the same linear regression analysis with self-efficacy as the primary outcome variable found a significant main effect of message framing (b = .190, SE = .073, t = 2.59, *p* =

.010; Web Appendix A Table 3, model 3) and of timing relative to the optimal deadline. (b = -.252, SE = .073, t = -3.45, *p* = .001; Web Appendix A Table 3, model 3).

Those who received promotional email reminders with planning prompts reported greater self-efficacy compared to those who received reminders without planning prompts (5.95 vs. 5.76). In addition, those who received a reminder after the optimal deadline reported lower self-efficacy than those who received the reminder before the optimal deadline (5.73 vs. 5.98). Results show no interaction between message framing and timing relative to the optimal deadline (b = .067, SE = .147, t = .45, *p* = .649; Web Appendix A Table 3, model 5).

2.3.2.4 Self-Esteem

Results from a linear regression analysis with self-esteem as the outcome variable and message framing (1 = planning prompt reminder, 0 = control reminder), timing relative to the optimal deadline (1 = after optimal deadline, 0 = before optimal deadline), current product usage (1 = currently has iphone, 0 = does not have iphone), and liking of online shopping (mean-centered) as predictors found no main effect of message framing (b = .035, SE = .097, t = .36, *p* = .722; Web Appendix A Table 4, model 3) nor of timing relative to the optimal deadline (b = -.082, SE = .097, t = -.84, *p* = .399; Web Appendix A Table 4, model 3). Individuals receiving promotional email reminders with planning prompt content reported similar levels of self-esteem as those receiving reminders without planning prompt content (5.26 vs. 5.22). Likewise, individuals receiving a promotional email after the optimal deadline did not differ from those receiving the email reminder before the optimal deadline (5.20 vs. 5.28). Moreover, results showed no framing x timing interaction (b = -.118, SE = .194, t = -.61, *p* = .542; Web Appendix A Table 4, model 5).

2.3.2.5 Internal Locus of Control

We ran a linear regression analysis predicting internal locus of control as a function of message framing (1 = planning prompt reminder, 0 = control reminder) and timing relative to the optimal deadline (1 = after optimal deadline, 0 = before optimal deadline), controlling for the previously stated covariates (i.e., current product usage, mean-centered liking of online shopping). No main effect of timing emerged (b = .022, SE = .072, t = .30, p = .762; Web Appendix A Table 5, model 3), with those receiving a reminder after the optimal deadline reporting an internal locus of control to a similar degree as those receiving a reminder before the optimal deadline (5.21 vs. 5.19). However, results found a main effect of message framing (b = .157, SE = .072, t = 2.17, p = .030; Web Appendix A Table 5, model 3). Those who received messages with planning prompts reported a higher internal locus of control than those who received messages without planning prompts (5.28 vs. 5.12). No interactive effect between message framing and timing relative to the optimal deadline emerged (b = .068, SE = .145, t = .47, p = .641; Web Appendix A Table 5, model 5).

2.3.2.6 Psychological Distance

Running the same linear regression analysis to predict psychological distance revealed no main effect of message framing (b = .022, SE = .110, t = .20, p = .841; Web Appendix A Table 6, model 3), but did find a significant a main effect of timing relative to the optimal deadline (b = .508, SE = .110, t = 4.61, p < .0001; Web Appendix A Table 6, model 3). Although psychological distance did not differ between those exposed to a promotional email reminder with (vs. without) planning prompt content (3.59 vs. 3.57), those who received the email reminder after the optimal deadline reported greater psychological distance than those who received the reminder before the

optimal deadline (3.83 vs. 3.33). The framing x timing interaction was not significant (b = .219, SE = .220, t = 1.00, p = .320; Web Appendix A Table 6, model 5).

2.3.2.7 Deal Fairness

Conducting a linear regression analysis predicting deal fairness as a function of message framing (1 = planning prompt reminder, 0 = control reminder) and timing (1 = after optimal deadline, 0 = before optimal deadline), controlling for current product usage (1 = currently has iphone, 0 = does not have iphone) and liking of online shopping (mean-centered) revealed no main effect of message framing (b = -.009, SE = .089, t = -.11, p = .916; Web Appendix A Table 7, model 3), but a significant main effect of timing relative to the optimal deadline (b = -.640, SE = .089, t = -7.17, p < .0001; Web Appendix A Table 7, model 3). Consumers receiving a reminder with planning content perceived the deal as similarly fair compared to consumers receiving a reminder without planning content (5.15 vs. 5.16). However, consumers who received the promotional email reminder after the optimal deadline perceived the deal as less fair than those who received the reminder before the optimal deadline (4.83 vs. 5.47). No message framing x timing interaction effect emerged (b = .247, SE = .179, t = 1.38, p = .168; Web Appendix A Table 7, model 5).

2.3.2.8 Moderated Mediation

Using PROCESS 3.0 (Model 8; 10,000 bootstrap samples; Hayes 2018), we conducted a conditional process analysis to examine whether the interactive effect of message framing and timing on perceived benefit subsequently impacts likelihood of plan-making. Specifically, we tested whether the indirect effect of message framing (1 = planning prompt reminder, 0 = control reminder) on planning likelihood through perceived benefit is moderated by the timing of the

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message relative to the optimal deadline (1 = after optimal deadline, 0 = before optimal deadline; see Web Appendix A Table 8).

The index of moderated mediation was marginally significant (b = .181, SE = .099, CI_{90}[.025, .353], CI_{95}[-.005, .383]). For those who received the reminder before the optimal deadline, no indirect effect of message framing emerged (b = -.036, SE = .052, CI_{90}[-.123, .049], CI_{95}[-.139, .066]). By contrast, for those who received the reminder after the optimal deadline, results showed a positive indirect effect of message framing on planning likelihood through perceived benefit (b = .145, SE = .084, CI_{90}[.011, .288], CI_{95}[-.016, .316]; see Figure 3).

As a follow-up, we conducted the same moderated mediation analysis as a parallel multiple mediator model. Using PROCESS 3.0 (Model 8; 10,000 bootstrap samples; Hayes 2018), we tested whether the indirect effect of message framing (1 = planning prompt reminder, 0 = control reminder) on planning likelihood through each of the potential mediators (perceived benefit, self-efficacy, self-esteem, internal locus of control, psychological distance, and deal fairness) is moderated by timing of the message (1 = after optimal deadline, 0 = before optimal deadline; see Web Appendix A Table 9).

Consistent with prior results, the index of moderated mediation for perceived benefit was marginally significant (b = .144, SE = .080, CI_{90}[.020, .282], CI_{95}[-.004, .310]). However, no effects emerged through self-efficacy (b = .017, SE = .040, CI_{90}[-.046, .086], CI_{95}[-.059, .100]), self-esteem (b = .012, SE = .023, CI_{90}[-.022, .054], CI_{95}[-.031, .065]), internal locus of control (b = -.0002, SE = .010, CI_{90}[-.015, .016], CI_{95}[-.020, .021]), psychological distance (b = .019, SE = .022, CI_{90}[-.012, .059], CI_{95}[-.020, .070]), or deal fairness (b = .040, SE = .033, CI_{90}[-.006, .101], CI_{95}[-.014, .118]).
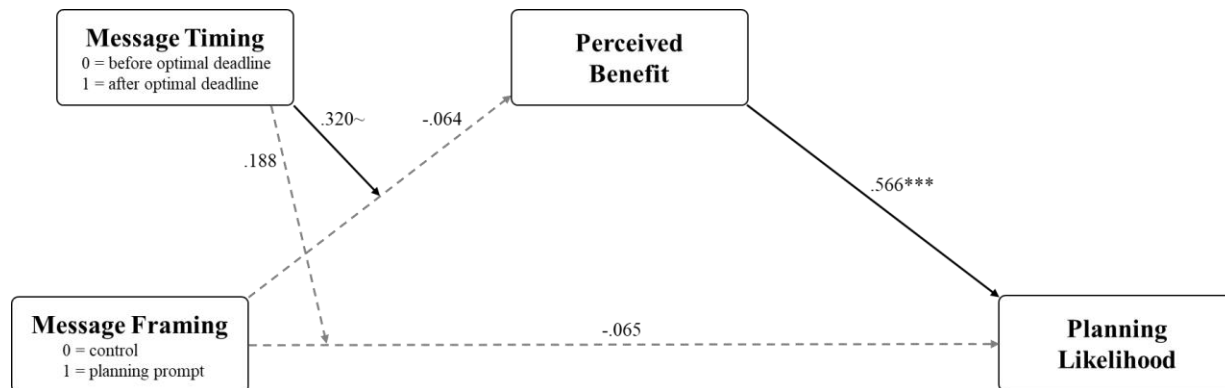
Note. Significance values are indicated as follows: ˜ *p* < .10, **p* < .05, ***p* < .01, ****p* <.001. Analysis conducted using PROCESS 3.0 (model 8; 10,000 bootstrapped samples) and with the following pre-registered covariates: current product usage and mean-centered liking of online shopping. Index of moderated mediation was marginally significant (b = .181, SE = .099, CI_{90}[.025, .353], CI_{95}[-.005, .383]), with a conditional indirect effect emerging after the optimal deadline (b = .145, SE = .084, CI_{90}[.011, .288], CI_{95}[-.016, .316], but not before the optimal deadline (b = -.036, SE = .052, CI_{90}[-.123, .049], CI_{95}[-.139, .066]). See Web Appendix A Table 8 for analysis details.

Figure 3 (Essay 1) Study 1: Moderated Mediation

2.3.3 Discussion

Study 1 provides preliminary evidence that the efficacy of planning prompts is temporally sensitive. For communications delivered after the optimal deadline (when consumers could obtain a reduced discount), marketing messaging with planning prompts increased consumer motivation more than marketing messaging without planning prompts, as evidenced by greater perceived benefit of engaging in the target behavior (i.e., buying the product at discounted price). However, for communications delivered before the optimal deadline (when consumers could obtain the largest possible discount), marketing messaging with planning prompts was no more effective than marketing messaging without planning prompts in boosting consumer motivation. In sum, the benefit of adding planning prompt phrasing to promotional messaging (vs. excluding it) only emerged after the optimal deadline had passed. No observable lift resulted from adding planning prompt phrasing (vs. excluding such phrasing) in pre-optimal deadline messaging.

The interactive effect, however, does not emerge for several alternative accounts (i.e., self-efficacy, self-esteem, control, psychological distance, deal fairness). This result suggests that

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planning prompts may preserve motivation after the optimal deadline passes more effectively by influencing how individuals perceive the target behavior more than how individuals perceive themselves. Study 1 results additionally show that this temporally sensitive effect subsequently impacts the likelihood that consumers will enact planning behaviors. Although the focus of this paper is not on the influence of engaging in plan-making—but rather the influence of promoting plan-making—this result suggests that planning prompts' influence on consumer motivation can translate into behavioral intentions. One potential issue, however, concerns the scenario-based design of Study 1. Although the sample consisted of consumers actively considering purchasing the focal product of the scenario, participants did not actually engage in the target behavior of the marketing messages (i.e., buying the product at a discount). To address this, in the next two studies, both field experiments, we examine whether this effect impacts real, consequential behavior.

2.4 Study 2: Lottery Enrollment

Whereas Study 1 examines changes to consumer motivation as evidenced by greater perceived benefit of engaging in target behavior, Study 2 and Study 3 examine changes to consumer motivation as indicated by increased enactment of target behavior: lottery enrollment (Study 2) and FAFSA submission (Study 3). Study 2 presents participants with the opportunity to enter a lottery in which the likelihood of winning decreases over time (visually depicted in Figure 4). The optimal deadline indicates the last chance to enroll with the greatest chance of winning, although participants can still win the lottery by enrolling after the optimal deadline (and prior to the terminal deadline, after which participants can no longer enroll). We manipulated both the presence of planning prompts (planning nudge vs. control) and the timing of messaging (before

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vs. after optimal deadline) within the lottery's description. We framed the next possible opportunity for participants to enroll in the lottery as occurring either before or after the optimal deadline (i.e., the entry period offering the greatest likelihood of winning). We anticipated an interactive effect of message content and message timing, with planning nudges increasing the likelihood of lottery enrollment compared to control messaging to a greater degree when participants believe they missed the optimal deadline.

Although the specific lottery design of Study 2 is decontextualized, note that individuals often face some kind of lottery when attempting to access critical resources, such as low-cost housing and school-choice preferences. For example, with the New York City Charter School system, applying earlier can allow parents to enter lotteries for a greater number of schools, since deadlines for each school can vary (New York City Charter School Center 2019).

Study 1 Design

A.

B.

Study 2 Design

A.

B.

Study 3 Design

A.

B.
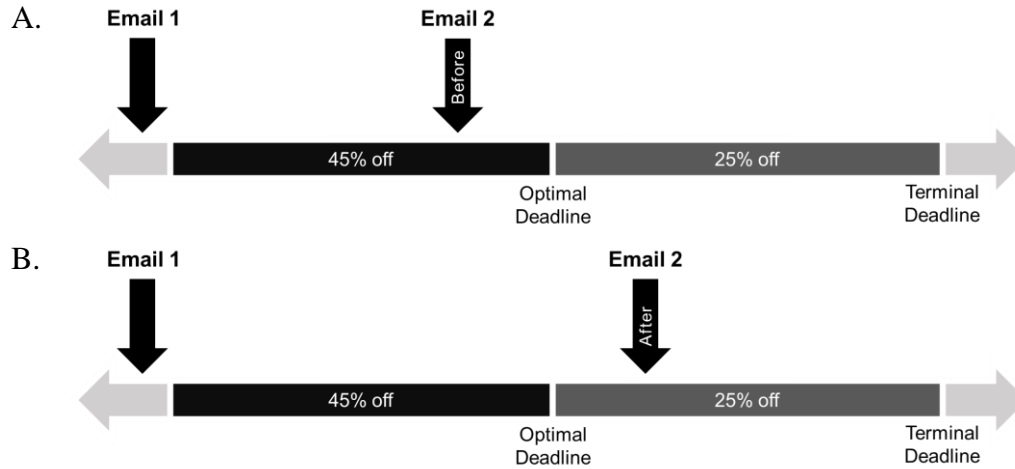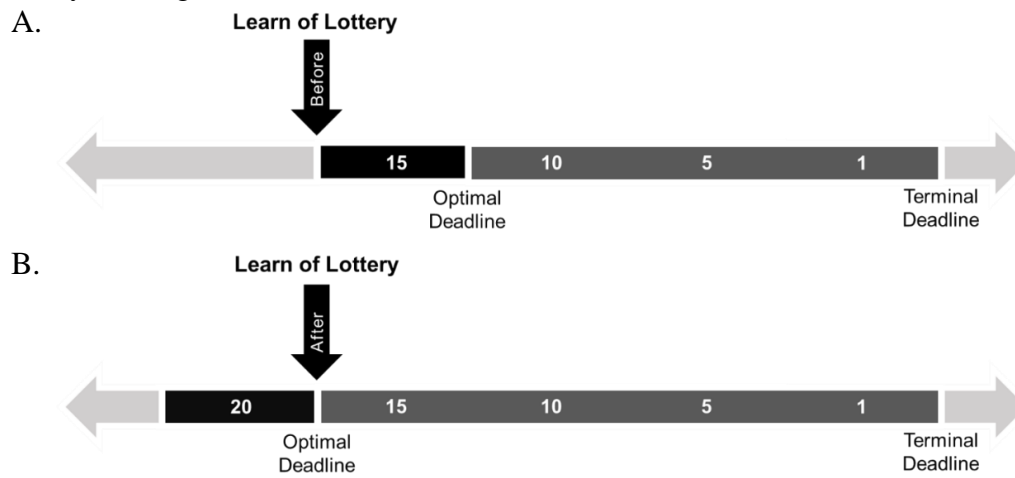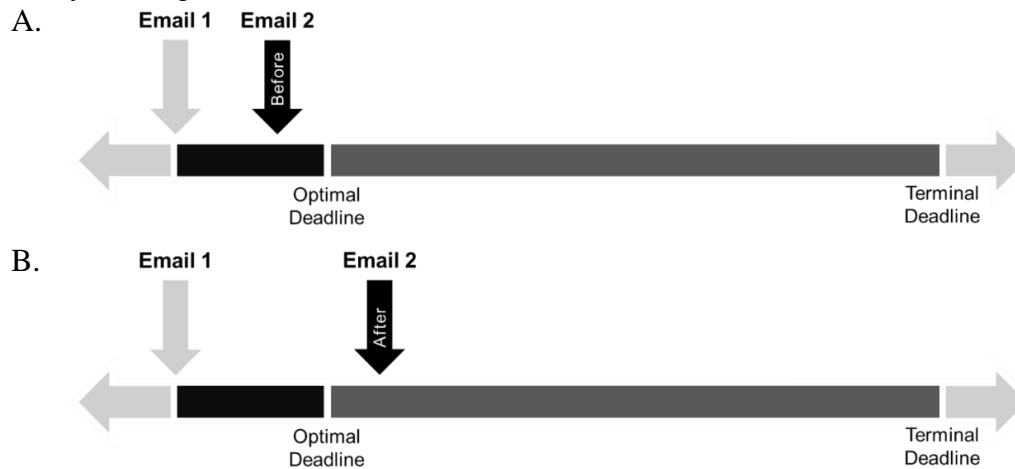
Note. For each study, Panel A displays the before optimal deadline condition and Panel B displays the after optimal deadline condition. Horizontal arrows represent passage of time, and vertical arrows represent when consumer messaging occurs. Darkest shading of the timeline (i.e., horizontal arrow) signifies when the benefits of action are greatest (e.g., largest discount, Study 1). Darkest shading of consumer messages (i.e., vertical arrows) signifies when message content was manipulated.

Figure 4 (Essay 1) Study Design

2.4.1 Method

We tested our predictions using a 2 (timing: before optimal deadline, after optimal deadline) x 2 (message framing: planning, control) x 2 (incentive size: $5, $20) between-subjects design, pre-registered on OSF (https://osf.io/8kjvf). The study sample consisted of MTurk workers (n = 920) who were compensated a nominal amount for participating. Despite requesting 1,400 participants, only 920 MTurkers completed the HIT before the final day of data collection. Because we decided *a priori* to restrict data collection to one week and wanted to avoid introducing any confounding influence of collecting data during a holiday, we did not extend recruitment beyond our planned data collection period of one week. Prior to analysis, and consistent with the OSF pre-registration, we excluded participants who reported technical problems or identified as a non-native American English speaker (n = 16), resulting in a final analysis sample of 904 participants (60.84% female; M_{age} = 39.66 yrs, SD_{age} = 12.11 yrs).

At the end of an unrelated study and after providing basic demographic data (e.g., age, gender, household income), participants learned that they were eligible for a bonus lottery. Each participant received an alphanumeric code and had eight days (starting the next day) to enroll in the lottery, which consisted of entering the code on a separate website between a specific timeframe. Participants could only enter the lottery once, but earlier enrollment earned participants extra entries. In four entry phases (each lasting two days), participants could receive 15, 10, 5, or 1 total lottery entries (see Appendix C for lottery schedule). Participants, however, could not enter the lottery until the following day, making the next opportunity identical for all participants (i.e., 15 total entries). Thus, it benefited the individual to enter the lottery early, but entering at any time was better than not entering at all.

The lottery was presented with or without planning prompts, depending on random assignment. Similar to Study 1, the planning prompts encouraged participants to purposefully engage in planning actions, such as setting aside time to enter the lottery (e.g., "Schedule time on your calendar") and making a plan (e.g., "Make a plan now to enter the lottery"). The control condition lottery, however, did not include these planning prompts and merely emphasized action execution (see Appendix D for stimuli).

We manipulated timing relative to the optimal deadline by shifting perceptions of when the optimal entry period occurs, holding constant all participants' specific action opportunity (i.e., enrolling the next day for 15 entries). Those randomly assigned to the before optimal deadline condition received no additional entry information, such that the next available entry opportunity (i.e., the next day) is perceived to offer the best opportunity to increase their chances of winning the lottery (i.e., with 15 total entries). Those randomly assigned to the after optimal deadline condition, however, learned that they had just missed the period in which they would have earned 20 total lottery entries. Thus, despite having the exact same entry opportunity as those in the before optimal deadline condition—everyone could earn 15 entries by enrolling the next day—those in the after optimal deadline condition believed that the best opportunity for lottery enrollment (i.e., with 20 total entries) had already passed.[3]

With respect to the lottery winnings, participants were randomly assigned to either the higher-stakes ($20) or lower-stakes ($5) lottery. The lottery's incentive size was explicitly presented to participants when they learned of the lottery. After data collection, we randomly selected one winner for both the $20 and $5 lotteries and distributed bonus payments. A pilot test

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[3] Immediately after learning of the lottery, participants completed two binary choice items assessing general loss aversion, unrelated to the lottery (i.e., a gain-framed and a loss-framed risk decision). Results did not show an interactive effect of planning prompts and message timing on participants' general loss aversion (Web Appendix C). Given the timing and generality of measurement, this result suggests that a broad gain-loss mindset shift may not occur immediately following the intervention.

using the same paradigm (n = 699 MTurk workers; detailed in Web Appendix B) demonstrated that the proposed moderating effect of prompt delivery timing did not depend on incentive size (t = -.81, p = .419). Consistent with these prior results, we do not anticipate that the moderating effect of prompt timing in the current study will depend on incentive size (i.e., no three-way interaction between planning prompt, prompt timing, and incentive size). Nevertheless, we retained the incentive size factor in the current experimental design for replication purposes and will test for the interactive effect of planning framing by prompt timing by incentive size on enrollment.

Note that the initial survey that manipulated lottery planning prompt framing and timing relative to the optimal deadline was administered over seven days. Consequently, we modified any dates displayed to participants, as appropriate. As previously stated, we decided a priori to restrict data collection to one week. Thus, all subsequent analyses control for the pre-registered covariates of day of survey administration (weekend vs. weekday) as well as participant-reported household income.

2.4.2 Results

Overall, 29.65% of participants enrolled in the lottery (n = 268). We first conducted a logistic regression predicting lottery enrollment (1 = enrolled, 0 = did not enroll), testing for a three-way interaction between lottery framing (1 = planning, 0 = control), timing relative to optimal deadline (1 = after, 0 = before), and incentive size (1 = $20, 0 = $5), controlling for participant income (mean-centered) and day of survey administration (1 = weekend, 0 = weekday). As anticipated based on pilot results, no significant three-way interaction between incentive size, timing, and message framing emerged (b = -.485, SE = .601, t = -.81, p = .419; Web Appendix C

Table 1, model 5). Thus, we controlled for incentive size in all subsequent analyses. In addition, all reported values reflect covariate-adjusted estimates.

Conducting a logistic regression predicting lottery enrollment, controlling for the previously described covariates (income, day of survey administration, and incentive size), we found no main effect of lottery framing (b = .155, SE = .147, t = 1.05, p = .292; Table 4, model 2), but a significant main effect of timing relative to the optimal deadline (b = -.443, SE = .148, t = -2.99, p = .003; Table 4, model 2). Lottery information with planning prompts, relative to information without planning prompts, did not increase the likelihood of entering the lottery (31.32% vs. 28.14%); however, participants were significantly less likely to enter the lottery after (vs. before) the optimal deadline had passed (25.02% vs. 34.13%).

These were both qualified, however, by a framing x timing interaction (b = .565, SE = .298, t = 1.90, p = .058; Table 4, model 4). For participants in the after optimal deadline condition (who believed the optimal deadline had passed), the inclusion of planning prompts significantly increased the likelihood of entering the lottery (29.55% vs. 20.84%; t = 2.11, p = .035; Figure 5), consistent with our predictions. For participants in the before optimal deadline condition (who believed the optimal deadline had not yet passed), however, exposure to planning prompts when learning of the lottery did not increase likelihood of enrollment (32.97% vs. 35.14%; t = -.49, p = .624).

Table 4 (Essay 1) Study 2: Binary Logistic Regression Results Predicting Likelihood of Lottery Enrollment

	Model 1	Model 2	Model 3	Model 4
	b/(SE)	b/(SE)	b/(SE)	b/(SE)
Timing of Prompt x Message Framing			.575~	.565~
			(.296)	(.298)
Message Framing	.151	.155	-.105	-.097
(1 = planning prompt, 0 = control)	(.147)	(.147)	(.198)	(.198)
Timing of Prompt	-.456**	-.443**	-.742***	-.725***
(1 = after optimal deadline, 0 = before optimal deadline)	(.147)	(.148)	(.210)	(.212)
Constant	-.723***	-.894***	-.602***	-.775***
	(.121)	(.149)	(.134)	(.160)
Controls	N	Y	N	Y
Number of Cases	904	904	904	904
Wald χ^2	10.659	15.88	14.441	19.499
df	2	5	3	6
p	.0048	.0072	.0024	.0034
Log Likelihood	-544.154	-541.543	-542.263	-539.734

Note. Significance values are indicated as follows: $^\sim p < .10$, $^*p < .05$, $^{**}p < .01$, $^{***}p < .001$. Control variables include the following: incentive size (1 = \$20, 0 = \$5), mean-centered income, day of week (1 = weekend, 0 = weekday).

2.4.3 Discussion

Study 2's results suggest that planning prompts delivered after, but not before, an optimal deadline, raise the likelihood of completing an economically-consequential task. Specifically, for those individuals who believed they had missed the maximally beneficial lottery enrollment period, exposure to planning prompts in the lottery's initial description increased the regression-adjusted lottery enrollment rate by 8.71 percentage points[4] as compared to the description excluding such planning prompts. By contrast, exposure to planning prompts in the lottery description prior to the optimal deadline offered no added benefit: the planning prompt intervention did not influence the likelihood of later enrollment.

These Study 2 results replicate those of a pilot test using the identical experimental paradigm with a different sample of MTurk workers (n = 699; 50.36% female; M_{age} = 38.04 yrs,

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[4] Unadjusted estimate increased 8.75 percentage points.

SD_{age} = 12.66 yrs; Web Appendix B). Conducting the same logistic regression analyses as Study 2, the pilot study similarly showed no three-way interaction with incentive size (b = -.485, SE = .601, t = -.81, p = .419) but a significant interaction between planning prompt framing and timing relative to the optimal deadline (b = .694, SE = .351, t = 1.98, p = .048). In this data, planning prompts (compared to reminders without planning prompts) also increased the likelihood of lottery enrollment after the optimal deadline (28.50% vs. 17.72%; t = 2.39, p = .017) but not before the optimal deadline (29.37% vs. 30.84%; t = -.30, p = .763). Results from a single-paper meta-analysis (McShane and Böckenholt 2017) using unadjusted values from both Study 2 and the Study 2 Pilot estimates the aggregate interactive effect to be positive and significant, at .1178 (SE = .0447, CI$_{95}$[.0303, .2054]; I^2 = 0%, CI$_{95}$[0%, 63%]): although no benefit of planning prompts emerges before the optimal deadline (-.0199, SE =.0329; CI$_{95}$[-.0844, .0445], after the optimal deadline planning prompts are estimated to significantly and positively impact enrollment relative to simple reminders delivered at the same time (.0979, SE = .0302; CI$_{95}$[.0387, .1572]).

An alternative explanation for the results of Study 2 and its pilot, however, could be that the optimal deadline manipulation unintentionally impacted understanding of the next enrollment opportunity, rather than shifting subjective perceptions of benefit of the next enrollment opportunity, as assumed. Results from a follow-up study, however, suggests this is likely not the case (Web Appendix D). An independent sample of Mturk participants (n = 865;[5] 52.60% female;[6] M_{age} = 38.93 yrs, SD_{age} = 12.30 yrs) imagined the same lottery paradigm from Study 2, in which the next action opportunity is objectively equally beneficial for everyone (15 total entries) but framed either as being the largest possible number of entries (before optimal deadline condition)

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[5] Consistent with Study 2, we excluded participants who reported technical problems or identified as a non-native American English speaker (n = 34), resulting in a final analysis sample of 865 participants.
[6] Participants could choose among three options: "male" (n = 402; 46.47%), "female" (n = 455; 52.60%), and "prefer not to say" (n = 8; .92%).

or the second-largest possible number of entries (after optimal deadline condition).[7] Consistent with the predicted manipulation effect, results show that participants in the after optimal deadline condition (compared to those in the before optimal deadline condition) perceived the next opportunity to enroll in the lottery as significantly less beneficial (6.29 vs. 6.63; b = -.345, SE = .064, t = -5.40, p < .0001; "How beneficial would it be for you to enroll in the lottery the next day [DATE]?" 1 = Not at All Beneficial, 7 = Very Beneficial), despite the opportunity being objectively equivalent (earning 15 total entries). In addition, this result does not seem to be due to systematic differences in participants' understanding of their next enrollment opportunity: those in the after optimal deadline condition and those in the before optimal deadline condition did not differ in their understanding of when they could next enroll in the lottery (89.18% vs. 90.45%; b = -.140, SE = .226, t = -.62, p = .536; "Because of when you learn of the lottery in the scenario ([TIME] on [DATE]), you can't enroll in the lottery until the next day [DATE];" 1 = True, 0 = False) nor of the objective outcome that could be gained from their next enrollment opportunity (85.47% vs. 85.18%; b = .023, SE = .192, t = .12, p = .905; "Because of when you learn about the lottery in this scenario ([TIME] on [DATE]), the greatest number of entries YOU could possibly get is 15;" 1 = True, 0 = False). Thus, this follow-up study suggests the manipulation used in Study 2 for timing relative to the optimal deadline was effective in shifting subjective perceptions of benefit, without unintentionally impacting understanding of the next enrollment opportunity.

Taken together, Study 2 suggests that the effectiveness of planning prompts can differ depending on whether the intervention occurs before or after an optimal deadline. Although Study 2 involved an economically consequential outcome and actual behavior, participants may have perceived the lottery as somewhat contrived given the broader context in which the lottery occurred

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[7] For the purpose of this manipulation check, the two additional factors from Study 2 (message framing and incentive size) are held constant in this study, such that all participants are exposed to the control messaging (without planning prompts) for a $5 lottery.

(e.g., following an unrelated study). Moreover, the act of enrolling in the lottery is relatively simplistic. We required participants to navigate a relatively minor procrastination obstacle by restricting the specific timeframe during which people could enroll, however, the action itself was straightforward. These factors may raise concerns regarding the generalizability of these effects to more complex or impactful tasks. Indeed, much of the most compelling prior work on planning prompts has been conducted using field experiments in inherently consequential domains that involve more complicated action, such as voting (Nickerson and Rogers 2010), preventative health (Milkman et al. 2011, 2013), and financial payments (Mazar et al. 2018). Although Study 2 participants did not know they were part of a research study, an essential characteristic of a robust field experiment (Morales, Amir, and Lee 2017), in Study 3 we test our predictions using a large-scale field study conducted in a naturally-occurring consequential context involving a more complex task: applying for student financial aid.

Study 2: Percentage of Participants Enrolling in Lottery

Study 3: Percentage of Students Submitting FAFSA
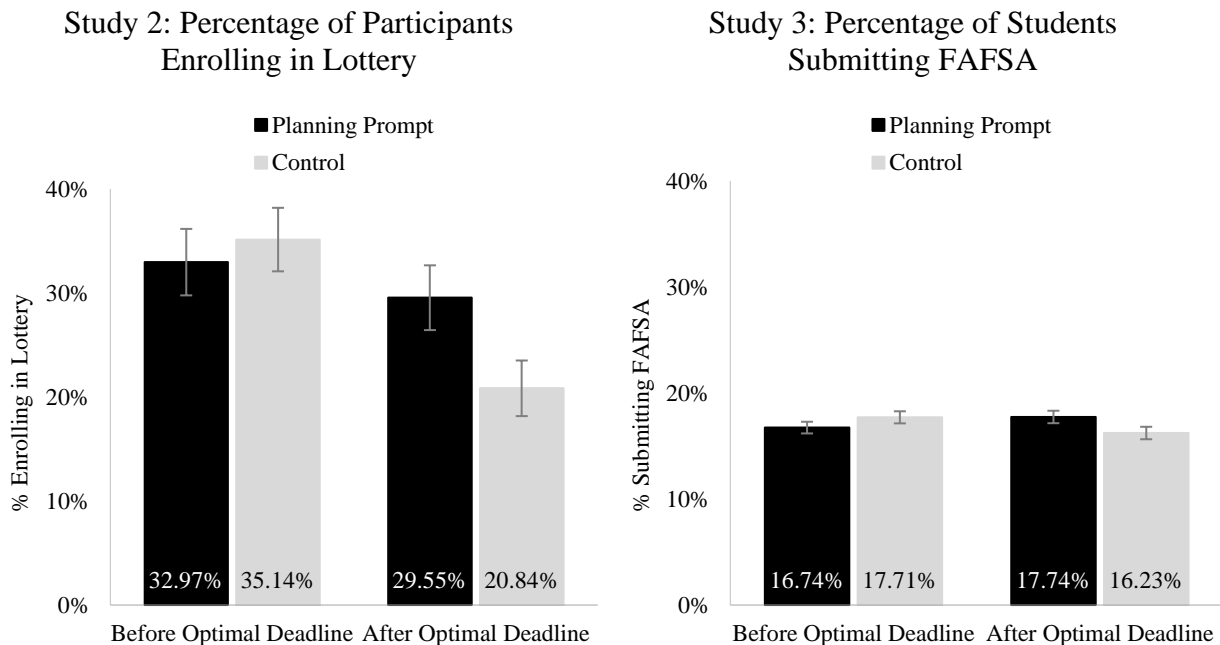
Note. Error bars represent standard errors. Estimates are adjusted for covariates. Study 2 control variables include the following: incentive size (1 = $20, 0 = $5), mean-centered income, day of week (1 = weekend, 0 = weekday).Study 3 control variables include the following: in-state status (1 = in-state, 0 = not in-state), gender (1 = female, 0 = not female), GPA, mean-centered income, and a separate indicator of missingness (1 = missing, 0 = not missing) for each covariate (i.e., in-state status missing, gender missing, GPA missing, income missing).

Figure 5 (Essay 1) Study 2 and Study 3: Binary Logistic Regression Results

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2.5 Study 3: Financial Aid

Study 3 aims to replicate the results of Study 2 in a large-scale field experiment involving university financial aid email communications and completion of the FAFSA. For many students, FAFSA completion is a critical step in the college-going process, as it is used to determine a student's eligibility for federal student aid (e.g., loans, grants, work-study programs), as well as state and institutional postsecondary financial aid. Further, many students who might benefit from FAFSA completion fail to submit this information: 36% of Pell-grant eligible students, in fact, lost an estimated $2.3 billion in financial aid as a result of unfiled FAFSA applications in 2016-2017 application cycle (Helhoski 2017). Although students can benefit from submitting the FAFSA any time during an application cycle, early submission increases access to more financial aid. Funds are awarded on a first-come first-served basis, meaning less aid is available for distribution as the application cycle progresses, with later FAFSA filers receiving less aid (McKinney and Novak 2015).

We partnered with a public higher education institution that processes approximately 30,000 freshman undergraduate applications per year. The university sent prospective students for the 2019-2020 academic year two university financial aid email communications encouraging submission of the FAFSA. The institution identified an "early bird" recommended FAFSA filing deadline as the optimal deadline, encouraging students to complete the FAFSA prior to this date to not miss out on available aid.

The timing and content of the first email was identical for all students. Sent on the first day students could file FAFSA (October 1, 2018), this email not only encouraged FAFSA submission, but also notified students of the optimal deadline for doing so (i.e., the university's "earliest bird" recommended FAFSA filing deadline of October 21, 2018). Unlike the first email, in the second

email we randomized the inclusion of planning prompts (i.e., planning prompts present vs. absent) and the timing of email delivery (i.e., sent before vs. after the optimal deadline; visually depicted in Figure 4). We predicted that the benefit of implementing planning prompts will be stronger when the optimal deadline has already passed. Specifically, we hypothesize that the timing of the second email relative to the optimal deadline will moderate the effectiveness of the planning prompts on both FAFSA completion and email engagement, with students receiving planning prompts more likely to open the second email and to federally file the FAFSA than those who do not receive planning prompts to a greater degree when those prompts are implemented after the optimal deadline.

2.5.1 Method

This field experiment followed a 2 (timing of second email: before optimal deadline, after optimal deadline) x 2 (message framing: planning prompt, no planning prompt) between-subjects design, pre-registered on OSF (https://osf.io/htxav). In October of 2018, the Office of Admissions and Financial Aid at a large public university sent two emails encouraging prospective undergraduate students to complete the FAFSA for the 2019-2020 academic year.

The first email was sent of October 1, 2018, which coincided with the first date students could file the FAFSA. The email reminded students that they could now complete their FAFSA and notified them of university-specific FAFSA resources (e.g., financial aid advisor). Importantly, this first email additionally informed students of an optimal deadline for FAFSA submission. Specifically, the email described the university's "earliest bird" recommended FAFSA filing deadline (October 21, 2018) using both text (e.g., "[UNIVERSITY] strongly recommends that you submit the FAFSA by Sunday, October 21, 2018") and visual (e.g., calendar

highlighting the recommended deadline) content (see Web Appendix E for email 1 stimuli). The optimal deadline was exclusively presented in the body of the first email and not through any other modality (e.g., email subject line, university website), meaning students who did not read the first email remained unaware of the optimal deadline. Consequently, prior to analysis, we retained only those students with a valid email address[8] who opened the first email (n = 12,592).

The second email contained the same basic FAFSA information as the first email, reminding all students of the recommended filing deadline and the resources available to assist with FAFSA submission. For those students randomly assigned to the planning prompt condition, however, the second email contained a slightly modified subject line and email body that additionally included planning prompts encouraging students to engage in planning behaviors (e.g., "Make a plan now," "Put a reminder in your phone or calendar," and "If you can't start today, then schedule a day when you will;" see Appendix E for email 2 stimuli).

Delivery of the second email and, therefore, the planning prompt occurred relative to the optimal filing deadline identified in the first email (October 21, 2018). Students received the second email either the Wednesday before (October 17, 2018) or the Wednesday after (October 24, 2018) the optimal deadline. The email's content and subject line reinforced whether students missed the optimal deadline, with statements noting whether they "haven't missed" (before condition) or "just missed" (after condition) the early bird deadline, a calendar visually highlighting the current date relative to the optimal deadline, and a table of deadlines using strikethrough text to denote those that have passed.

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[8] We removed those with invalid email addresses (n = 85).

2.5.2 Data

Student-level data for both of our primary outcomes of interest, opening email 2 and FAFSA submission, were provided by the university's office of admissions and financial aid in February 2019. In total, the data spans from October 1, 2018 until February 3, 2019. Originating from the university's CRM platform, data regarding student email behavior was coded as a binary indicator (1 = opened email, 0 = did not open email). FAFSA completion data included whether a student formally filed the FAFSA (1 = submitted FAFSA, 0 = did not submit FAFSA) and, if so, when they filed the FAFSA.[9] Using the date of FAFSA submission, we calculated the number of days that elapsed from the first opportunity to file FAFSA (October 1, 2018) until the student's FAFSA submission.

The university additionally provided demographic data about each student's state of residence, 5-digit zip code, gender, and high-school grade point average (GPA), from which we generated covariates for pre-registered inclusion in all subsequent analyses. In addition to student gender, which has been traditionally controlled for in work on FAFSA completion, we also controlled for high school GPA (e.g., McKinney and Novak 2015). We reasoned that prospective students with a higher GPA may perceive a greater likelihood of admission than those with a lower GPA and, consequently, influence their responsiveness to emails and recommendations from the institution, regardless of content. Prior to obtaining student information, the university converted all GPA scores to a four-point scale to allow for comparison across students with different GPA formats.

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[9] During the process of FAFSA filing, students select the postsecondary institutions to which they would like the federal government to provide their application. One potential limitation, therefore, is that if a student in our sample filed the FAFSA but did not report the data to our partnering institution, we would not observe it in this dataset.

Furthermore, we additionally controlled for factors associated with financial need that could impact FAFSA-related behaviors, including residency status and household income. Because non-resident students pay a higher tuition rate than in-state residents at this university, the different financial burden of college enrollment could plausibly influence engagement with and responsiveness to university financial aid email communications. Thus, using the student's residency information, we categorized students residing in the same U.S. state as our partnering institution (1 = in-state resident, 0 = not in-state resident). To control for household income, despite not having student-level income information, we employed a proxy measure based on the student's five-digit zip code. Specifically, we used median household income associated with the zip code of residence (U.S. Bureau of the Census 2017) to estimate income.

2.5.3 Results

All analyses control for each student's GPA (on a 4-point scale), in-state status (1 = in-state student, 0 = not in-state student), gender (1 = female, 0 = not female), and estimated income (mean-centered). To retain all student data, analyses additionally included a dummy-coded indicator of missingness for each covariate. All subsequent analyses reflect covariate-adjusted estimates.

2.5.3.1 Survival Analysis

Unlike traditional a logistic regression approach, which only models event incidence, a survival analysis approach additionally accounts for the time elapsed until an event of interest. In this case, the event of interest was FAFSA filing. With survival analysis, event timing is the dependent variable, which we calculated as the number of days that elapsed from the first

opportunity to file FAFSA (October 1, 2018) until the student's FAFSA submission. We right-censored data for those who did not file FAFSA during our observation period, coding time until filing as the maximum number of days observed in our dataset.

We conducted a Cox proportional hazard regression (Cox, 1972), with days until FAFSA submission as the dependent variable and timing of second email (1 = after optimal deadline, 0 = before optimal deadline), presence of planning prompts (1 = planning prompt, 0 = control), and their interaction as primary predictors, while accounting for the previously stated time-invariant covariates. Proportional hazard regression analysis estimates students' propensity to submit the FAFSA at a given point in time, conditional on "survival" in the sample up to that timepoint (i.e., having not already submitted FAFSA). Resulting hazard ratios greater than one indicate an increased risk of the event occurring (e.g., HR of 1.10 represents a 10% greater likelihood of FAFSA submission), whereas hazard ratios less than one indicate a decreased risk of the event occurring (e.g., HR of .80 represents a 20% lower likelihood of FAFSA submission).

In our analysis, we found no main effect of planning prompt (b = .020, SE = .043, t = .46, p = .648; Table 5, model 2); student exposure to planning prompts increased probability of submission by a non-significant 1.97% (HR= 1.0197). We also found no main effect of nudge timing (b = -.016, SE = .043, t = -.38, p = .706; Table 5, model 2); receiving the second email reminder after the optimal deadline reduced the probability of FAFSA submission by a non-significant 1.60% (HR = .9840).

Although no main effects emerged, we did observe a significant interaction between presence of planning nudge content and timing relative to the optimal deadline (b = .187, SE = .086, t = 2.18, p = .029; Table 5, model 4). Before the optimal deadline there was no significant difference in hazard rates between students who received planning prompts and those who did not

(t = -1.15, *p* = .249). Planning prompts had similar probability of FAFSA submission as no planning prompts, with a relative hazard ratio of .9341, indicating that students exposed to planning prompts had a non-significant 6.59% lower probability of FAFSA submission than those not exposed to planning prompts. After the optimal deadline, however, we found a marginal difference in hazard rates between planning prompts and no prompts (t = 1.92, *p* = .055). The hazard ratio was 1.1216, indicating planning prompts increased the probability of FAFSA submission by 12.16%. This suggests that when encouraging FAFSA completion after the optimal deadline has passed, including planning prompts in email messaging increases the probably of FAFSA completion. Before the optimal deadline has passed, however, the inclusion of planning prompts does not increase likelihood of FAFSA submission (Web Appendix F Table 2 and Web Appendix F Figure 1).

Additional analysis suggests that this pattern of results may be due to the ability of planning prompts to offset the reduced probability of submitting the FAFSA that occurs after students miss the optimal deadline. Examining the interaction, results show that students receiving the control message after the optimal deadline had a marginally significant 10.65% lower probability of submitting the FAFSA compared to students receiving the message before the optimal deadline (HR = .8937; t = -1.83, *p* = .067). By comparison, we found no significant difference in hazard rates between planning prompts elicited before versus after the optimal deadline (t = 1.25, *p* = .212). Students exposed to planning prompts after the optimal deadline had a non-significant 7.75% higher probability of filing the FAFSA than students exposed to planning prompts before the optimal deadline (HR = 1.0775). This suggests that reminding students to complete the FAFSA after the optimal deadline reduces the probability of task completion (relative to reminding

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students before the optimal deadline). Post-optimal deadline reminders that include planning prompts, however, don't reduce the probability of FAFSA completion.

Table 5 (Essay 1) Study 3: Survival Analysis (Cox Proportional Hazard)

	Model 1	Model 2	Model 3	Model 4
	b/(SE)	b/(SE)	b/(SE)	b/(SE)
Timing of Prompt x Message Framing			.175*	.187*
			(.086)	(.086)
Message Framing	.056	.020	-.026	-.068
(1 = planning prompt, 0 = control)	(.043)	(.043)	(.059)	(.059)
Timing of Prompt	-.089*	-.016	-0.179**	-.112~
(1 = after optimal deadline, 0 = before optimal deadline)	(.043)	(.043)	(.062)	(.061)
Includes Controls	N	Y	N	Y
Number of Cases	12592	12592	12592	12592
Wald χ^2	6.215	2284.527	9.894	2287.234
df	2	10	3	11
p	.0447	< .0001	.0195	< .0001
Log Likelihood	-20150.322	-18525.782	-20148.271	-18523.442

Note. Significance values are indicated as follows: ˜$p < .10$, *$p < .05$, **$p < .01$, ***$p < .001$. Control variables include the following: in-state status (1 = in-state, 0 = not in-state), gender (1 = female, 0 = not female), GPA, mean-centered income, and a separate indicator of missingness (1 = missing, 0 = not missing) for each covariate (i.e., in-state status missing, gender missing, GPA missing, income missing).

Table 6 (Essay 1) Study 3: Binary Logistic Regression Results Predicting Likelihood of Submitting FAFSA (Cumulatively by Week)

Week	Submission Date Range	Timing of Prompt x Message Framing	Message Framing 1 = planning prompt 0 = control	Timing of Prompt 1 = after optimal deadline 0 = before optimal deadline	Constant	Controls	Wald χ^2	p	Log Likelihood
		b (SE)	b (SE)	b (SE)	b (SE)				
1	10/01/18 – 10/07/18	.190 (.189)	-.005 (.127)	-.167 (.137)	-6.120*** (.711)	Y	596.673	< .0001	-1804.707
2	10/01/18 – 10/14/18	.121 (.149)	-.016 (.101)	-.142 (.108)	-6.432*** (.578)	Y	1123.344	< .0001	-2568.564
3	10/01/18 – 10/21/18	.130 (.131)	-.017 (.087)	-.241* (.094)	-6.287*** (.501)	Y	1658.65	< .0001	-3181.767
4	10/01/18 – 10/28/18	.132 (.123)	.009 (.084)	-.141 (.089)	-6.267*** (.466)	Y	2039.709	< .0001	-3487.615
5	10/01/18 – 11/04/18	.186 (.119)	-.021 (.081)	-.153~ (.086)	-6.287*** (.450)	Y	2298.32	< .0001	-3658.952
6	10/01/18 – 11/11/18	.246* (.117)	-.075 (.080)	-.185* (.084)	-6.257*** (.439)	Y	2511.133	< .0001	-3748.661
7	10/01/18 – 11/18/18	.265* (.115)	-.095 (.079)	-.203* (.083)	-6.155*** (.428)	Y	2676.757	< .0001	-3868.876
8	10/01/18 – 11/25/18	.264* (.115)	-.093 (.079)	-.168* (.082)	-6.298*** (.426)	Y	2817.135	< .0001	-3895.415
9	10/01/18 – 12/02/18	.275* (.114)	-.088 (.078)	-.168* (.082)	-6.139*** (.419)	Y	2908.394	< .0001	-3953.150
10	10/01/18 – 12/09/18	.320** (.113)	-.116 (.078)	-.193* (.081)	-6.009*** (.414)	Y	2962.281	< .0001	-3985.484
11	10/01/18 – 12/16/18	.286* (.113)	-.105 (.077)	-.168* (.081)	-6.028*** (.412)	Y	3039.314	< .0001	-4010.494
12	10/01/18 – 12/23/18	.284* (.112)	-.105 (.077)	-.163* (.081)	-6.083*** (.411)	Y	3072.571	< .0001	-4023.657
13	10/01/18 – 12/30/18	.260* (.112)	-.108 (.077)	-.143~ (.080)	-6.084*** (.410)	Y	3124.622	< .0001	-4032.148
14	10/01/18 – 01/06/19	.267* (.112)	-.111 (.077)	-.145~ (.080)	-6.089*** (.408)	Y	3184.855	< .0001	-4062.224
15	10/01/18 – 01/13/19	.260* (.112)	-.105 (.077)	-.146~ (.080)	-6.025*** (.406)	Y	3198.197	< .0001	-4081.337
16	10/01/18 – 01/20/19	.250* (.111)	-.099 (.077)	-.145~ (.080)	-6.078*** (.406)	Y	3234.963	< .0001	-4083.797
17	10/01/18 – 01/27/19	.246* (.111)	-.105 (.077)	-.139~ (.080)	-6.185*** (.406)	Y	3294.572	< .0001	-4087.458
18	10/01/18 – 02/03/19	.240* (.111)	-.093 (.076)	-.144~ (.080)	-6.200*** (.406)	Y	3311.905	< .0001	-4107.278

Note. Significance values are indicated as follows: ~p < .10, *p < .05, **p < .01, ***p < .001. Control variables include the following: in-state status (1 = in-state, 0 = not in-state), gender (1 = female, 0 = not female), GPA, mean-centered income, and a separate indicator of missingness (1 = missing, 0 = not missing) for each covariate (i.e., in-state status missing, gender missing, GPA missing, income missing). See Web Appendix G for details.

2.5.3.2 Week-by-Week Analysis

Supplementing the survival analysis, we also conducted a week-by-week analysis of FAFSA submission. For each of the 18 weeks of data collected, we coded the total number of people filing the FAFSA between the first possible submission day (October 1, 2018) and the end of week w (1 = submitted between October 1, 2018 and end of week w, 0 = did not submit between October 1, 2018 and end of week w). Recall that the first email was sent on the first day students could submit the FAFSA (October 1, 2018), representing the start of week 1. The optimal deadline occurred during week 3 (October 21, 2018). The second email was sent the Wednesday before the optimal deadline (October 17, 2018; week 3) or the Wednesday after the optimal deadline (October 24, 2018; week 4). See Table 6 for the specific dates associated with each week.

Conducting a series of logistic regression analyses predicting cumulative submission rates for each week (w) of the study timeframe (1 = submitted by end of week w, 0 = did not submit by end of week w) as a function of timing of second email (1 = after optimal deadline, 0 = before optimal deadline), presence of planning prompts (1 = planning prompt, 0 = control), and their interaction, controlling for the previously stated covariates. Because the information provided to students was identical prior to the receipt of the second email, we would not expect to observe any differences prior to the second email but would anticipate differences after the second email. Consistent with this anticipated pattern of results, findings show that a significant planning prompt by timing interaction does not emerge in weeks 1 through 5 (p ranges from .120 to .417) but does emerge in weeks 6 through 18 (p ranges from .005 to .036; see Tables 6 and 7). For each week from week 6 through week 18, which is after the optimal deadline, those students receiving planning prompt nudges were more likely to submit the FAFSA than those who did not receive

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planning prompt nudges. No difference in FAFSA submission emerged before the optimal

deadline (see Tables 6 and 7).

Table 7 (Essay 1) Study 3: FAFSA Submission (Cumulative by Week)

Week	Submission Date Range	Before Optimal Deadline		After Optimal Deadline	
		Control (n = 3,201)	Planning Prompt (n = 3,231)	Control (n = 3,081)	Planning Prompt (n = 3,079)
		% (n)	% (n)	% (n)	% (n)
1	10/01/18 – 10/07/18	4.15% (133)	4.27% (138)	3.31% (102)	4.12% (127)
2	10/01/18 – 10/14/18	7.19% (230)	7.30% (236)	5.91% (182)	6.79% (209)
3	10/01/18 – 10/21/18	10.56% (338)	10.74% (347)	8.11% (250)	9.32% (287)
4	10/01/18 – 10/28/18	12.00% (384)	12.44% (402)	10.03% (309)	11.69% (360)
5	10/01/18 – 11/04/18	13.31% (426)	13.53% (437)	11.07% (341)	13.09% (403)
6	10/01/18 – 11/11/18	14.43% (462)	14.14% (457)	11.78% (363)	13.93% (429)
7	10/01/18 – 11/18/18	15.50% (496)	15.01% (485)	12.56% (387)	14.78% (455)
8	10/01/18 – 11/25/18	15.78% (505)	15.32% (495)	13.11% (404)	15.39% (474)
9	10/01/18 – 12/02/18	16.21% (519)	15.82% (511)	13.50% (416)	15.98% (492)
10	10/01/18 – 12/09/18	16.65% (533)	15.97% (516)	13.66% (421)	16.34% (503)
11	10/01/18 – 12/16/18	16.87% (540)	16.31% (527)	14.09% (434)	16.56% (510)
12	10/01/18 – 12/23/18	16.99% (544)	16.43% (531)	14.25% (439)	16.73% (515)
13	10/01/18 – 12/30/18	17.15% (549)	16.56% (535)	14.57% (449)	16.79% (517)
14	10/01/18 – 01/06/19	17.46% (559)	16.84% (544)	14.83% (457)	17.12% (527)
15	10/01/18 – 01/13/19	17.59% (563)	17.02% (550)	14.93% (460)	17.21% (530)
16	10/01/18 – 01/20/19	17.68% (566)	17.18% (555)	15.03% (463)	17.28% (532)
17	10/01/18 – 01/27/19	17.87% (572)	17.30% (559)	15.25% (470)	17.41% (536)
18	10/01/18 – 02/03/19	17.99% (576)	17.55% (567)	15.32% (472)	17.54% (540)

Note. These estimates are not adjusted for covariates. The first email was sent in week 1 (October 1, 2018). The optimal deadline occurred in week 3 (October 21, 2018), with the before optimal deadline condition receiving the second email in week 3 (October 17, 2018) and the after optimal deadline condition receiving the second email in week 4 (October 24, 2018). A significant interaction between presence of planning nudge content and timing of delivery emerged in weeks 6 through 18.

2.5.3.3 Likelihood of FAFSA Submission

Overall, 17.11% of students completed the FAFSA and reported their FAFSA information

to the university (n = 2,155). We also conducted a logistic regression analysis predicting FAFSA

submission (1 = submitted, 0 = not submitted) as a function of timing of second email (1 = after

optimal deadline, 0 = before optimal deadline), presence of planning prompts (1 = planning

prompt, 0 = control), and their interaction, controlling for the previously stated covariates used in

prior analyses. No main effects emerged for planning framing (b = .020, SE = .055, t = .37, p = .714; Table 8, model 2), such that reminders with planning prompts (vs. control reminders) did not increase likelihood of FAFSA submission (17.22% vs. 17.01%). Similarly, we observed no main effect for timing (b = -.021, SE = .056, t = -.37, p = .709; Table 8, model 2), with students receiving the second email before the optimal deadline being equally likely to submit FAFSA as those receiving email after the optimal deadline (17.22% vs. 17.00%).

Results, however, showed a significant planning x timing interaction (b = .240, SE = .111, t = 2.16, p = .031; see Table 8, model 4). Prior to the optimal deadline, we found no difference in likelihood of FAFSA submission between students exposed to planning prompts and those who were not (16.74% vs. 17.71%; t = -1.22, p = .221). After the optimal deadline, however, those exposed to planning prompts were marginally more likely to submit the FAFSA (17.74% vs. 16.23%; t = 1.82, p = .069). This suggests that planning prompts deployed after, but not before, the optimal deadline increased FAFSA submission (Figure 5).

Analyzing the interaction by message content, we find that students receiving email reminders without planning prompts were marginally less likely to submit the FAFSA when the message was received after (versus before) the optimal filing deadline (t = -1.81, p = .071). Messages with planning prompts, however, did not exhibit this pattern. Students were equally likely to submit the FAFSA when planning prompts were sent before and after the optimal deadline (t = 1.24, p = .213), suggesting planning prompts mitigate the decrement in FAFSA submission rates that occurs for messaging sent after the optimal deadline.

2.5.3.4 Likelihood of Opening Email 2

Overall, 58.67% of students opened the second email (n = 7,388). Because the timing and planning prompt manipulations were embedded within the subject line of the second email, it is

possible that planning prompts may differentially impact open rates depending on the timing in which the email was deployed relative to the optimal deadline. To test this, we conducted a logistic regression predicting student opening of the second email (1 = opened, 0 = not opened) as a function of timing of second email (1 = after optimal deadline, 0 = before optimal deadline), presence of planning prompts (1 = planning prompt, 0 = control), and their interaction, controlling for the previously stated covariates.

We found a significant main effect of message timing (b = .126, SE = .037, t = 3.40, p = .001; Table 8, model 6), with students more likely to open the second email when it was delivered after (vs. before) the optimal deadline (60.17% vs. 57.23%). There was no significant main effect of planning prompt framing (b = -.058, SE = .037, t = -1.57, p = .117; Table 8, model 6); students exposed to planning prompts were similarly likely to open the second email as those not exposed to planning prompts (58.00% vs. 59.35%, respectively).

We found no significant timing x framing interaction (b = -.097, SE = .074, t = -1.32, p = .187; see Table 8, model 8), indicating that the inclusion (vs. exclusion) of planning prompt content similarly influenced the likelihood of opening the email when received before (57.11% vs. 57.35%) and after (58.92% vs. 61.42%) the optimal deadline.

Table 8 (Essay 1) Study 3: Binary Logistic Regression Results Predicting Likelihood of Submitting FAFSA and Likelihood of Opening Email 2

	Likelihood of Submitting FAFSA				Likelihood of Opening Email 2			
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)	b/(SE)
Timing of Prompt x Message Framing			.192*	.240*			-.094	-.097
			(.095)	(.111)			(.072)	(.074)
Message Framing	.060	.020	-.030	-.093	-.041	-.058	.005	-.010
(1 = planning prompt, 0 = control)	(.047)	(.055)	(.065)	(.076)	(.036)	(.037)	(.050)	(.051)
Timing of Prompt	-.095*	-.021	-.193**	-.144~	.101**	.126***	.148**	.174***
(1 = after optimal deadline, 0 = before optimal deadline)	(.047)	(.056)	(.068)	(.080)	(.036)	(.037)	(.051)	(.052)
Constant	-1.563***	-6.254***	-1.517***	-6.200***	.322***	-1.049**	.299***	-1.073**
	(.041)	(.405)	(.046)	(.406)	(.031)	(.336)	(.036)	(.337)
Controls	N	Y	N	Y	N	Y	N	Y
N	12592	12592	12592	12592	12592	12592	12592	12592
Wald χ^2	5.630	3307.222	9.74	3311.905	8.993	449.051	10.667	450.792
df	2	10	3	11	2	10	3	11
p	.0599	< .0001	.0209	< .0001	.0111	< .0001	.0137	< .0001
Log Likelihood	-5760.416	-4109.62	-5758.361	-4107.278	-8533.251	-8313.222	-8532.414	-8312.352

Note. Significance values are indicated as follows: ~p < .10, *p < .05, **p < .01, ***p < .001. Control variables include the following: in-state status (1 = in-state, 0 = not in-state), gender (1 = female, 0 = not female), GPA, mean-centered income, and a separate indicator of missingness (1 = missing, 0 = not missing) for each covariate (i.e., in-state status missing, gender missing, GPA missing, income missing).

2.5.4 Discussion

The results of this field experiment replicate those of Study 2, demonstrating that the effectiveness of planning prompt nudges on FAFSA submission differs depending on whether the nudge is delivered before or after an optimal deadline. No effect emerged for email open rates; however, for FAFSA submission, the interaction between planning prompt nudge and nudge timing repeatedly emerged. A basic binary logistic regression analysis found no influence of planning prompt nudge prior to the optimal deadline, although including (vs. excluding) planning prompts in the post-optimal deadline reminder increased the regression-adjusted FAFSA submission rate by 1.51 percentage points[10] after the optimal deadline. Survival analysis showed no influence of planning prompt nudges before the optimal deadline, but after the optimal deadline planning prompts increased the probability of FAFSA submission by 12.16%. A series of week-by-week logistic regression analysis found that the interactive effect does not emerge until after optimal deadline passes (in weeks 6 through 18), further demonstrating the robustness of the intervention.

Taken together, these findings demonstrate the differential efficacy of planning prompts depending on the timing of their delivery. It is particularly notable that these effects emerge within a naturally occurring behavioral context involving highly consequential implications. Moreover, the types of financial aid communications modified in this field study are distributed annually by the vast majority of US institutions, which suggests that the potential impact of implementing this intervention is not only meaningful, but also low-cost and easily scalable.

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[10] Unadjusted estimate increased 2.22 percentage points.

2.6 General Discussion

Planning prompts can effectively nudge people towards completion of tasks with a terminal "final" deadline (Mazar et al. 2018; Milkman et al. 2011; Nickerson and Rogers 2010). Many tasks, however, involve a more complex deadline structure that includes an optimal "early bird" deadline, indicating the final opportunity to take maximally beneficial action. For tasks containing an optimal deadline, when should firms and policy-makers nudge people to engage in plan-making behavior: before or after the optimal deadline?

This paper demonstrates that planning prompt nudges are more effective than simple reminders at prompting behavior enactment when delivered after an optimal deadline. Prior to an optimal deadline, however, reminders with planning prompts are no more effective than reminders without planning prompts. In all studies, the benefit of adding planning prompt phrasing to promotional messaging (vs. excluding it) only emerged after the optimal deadline had passed. No observable lift resulted from adding planning prompt phrasing (vs. excluding planning prompts) in pre-optimal deadline messaging. Results from one experiment and two field studies suggest that post-optimal deadline marketing messaging with planning prompts offsets motivation decay to a greater degree than marketing messaging without planning prompts delivered at the same time, as evidenced by greater perceived benefit of engaging in the target behavior (Study 1) and greater enactment of target behavior (Study 2 and Study 3). Study 1 provided preliminary evidence that the efficacy of planning prompts is temporally sensitive, showing that post-optimal deadline marketing messages containing planning prompts increased consumers' perceived benefit of the retailer's promotional discount offer. Study 2 showed that post-optimal deadline planning prompts boosted regression-adjusted lottery enrollment rate by 8.71 percentage points, whereas the same prompt deployed prior to the optimal deadline failed to impact enrollment behavior. In Study 3,

which modified the content of university financial aid email communications encouraging FAFSA submission, survival analysis demonstrated that exposure to post-optimal deadline planning prompts increased the probability of FAFSA submission by 12.16%. Notably, implementing a post-optimal deadline planning nudge was no more costly than implementing a simple reminder message before the optimal deadline, making these effects essentially costless for firms.

Our findings extend prior literature on planning prompt nudges, implementation intentions, goal pursuit, and deadlines. In contrast to prior work, which has primarily examined terminal deadlines, we investigate the efficacy of planning nudges in the context of an alternative, yet frequently encountered, task deadline structure—optimal deadlines. Our research not only identifies a previously unclassified type of deadline, but also suggests that decisions regarding when to implement a planning prompt nudge can dramatically alter the intervention's effectiveness. To date, however, research has generally ignored the temporal dynamics of nudges, including planning prompt nudges. This paper advances our understanding of how contextual features of the decision-making environment—in this case, temporal features—alter individual behavior.

This research also promotes a broader perspective shift for scholars studying nudges: moving from *eliciting* a nudge effect to *maximizing* a nudge's effectiveness. Our findings illuminate the potential behavioral benefit of considering not only whether planning prompt prompts are effective, but also when they are most effective. Our findings call for research that extends beyond a focus on nudge content and seriously considers the timing of nudge delivery. Along these lines, future research may consider additionally evaluating behavioral nudges with respect to their temporal effects (e.g., "does the effect emerge if delivered in different time periods?" "when does the nudge most effectively impact behavior?"). For firms and policy makers

intending to use such behavioral interventions, knowledge about their temporal sensitivity can meaningfully shape decisions about implementation timing. Thus, similar to Benartzi et al. (2017), this paper promotes an alternative approach to evaluating the effectiveness of behavioral interventions, one centered on the temporal sensitivity of a nudge's effectiveness.

Practically, our findings also indicate that firms and policy makers intending to implement nudges will benefit by using strategic, rather than standardized, timing when attempting to harness the power of planning prompts. For existing optimal-deadline tasks, this may involve intentionally scheduling planning prompt nudges to occur after the optimal deadline has passed, which is essentially costless because the firm intends to distribute message at some point anyways. For tasks lacking a clear optimal-deadline structure, the same effect may be achieved by generating a "phantom" optimal deadline like that used in Study 2 (e.g., notifying people of a fictional missed maximally beneficial opportunity and simultaneously encouraging planning behavior before the terminal deadline) or by specifying an "arbitrary" optimal deadline like that used in Study 3 (e.g., identifying a precise deadline to saliently demarcate a change in procrastination costs that may otherwise be ill-defined). In addition, firms can frame a task in multiple ways to shift deadline perceptions. Firms nudging people to "get a free flu shot at work" can frame the end of a one-day free clinic at work as a terminal deadline by emphasizing benefit of convenience (e.g., last opportunity to obtain a free on-site flu shot, after which you will not be able to receive influenza immunization for free at work) or as an optimal deadline by emphasizing benefit of vaccine efficacy (e.g., best opportunity to obtain a flu shot for maximum possible flu protection is at the clinic, after which you can still later get immunized although the delay means you will be protected for less of the flu season).

Despite the theoretical and practical contributions of this work, limitations exist that represent interesting directions for future research. The focus of this paper involves investigating whether the timing of planning prompt nudges relative to an optimal deadline influences the nudge's efficacy. Results indicate that the largest benefit of implementing planning prompt nudges (vs. simple reminders) occurs after, not before, an optimal deadline has passed, which is speculated to result from planning nudges offsetting motivation decay. Study 1 findings suggest motivation preservation occurring though altered perceptions of the target behavior (i.e., perceived benefit of discounted purchase) rather than perceptions of the self. Our field studies, however, do not provide granular insight regarding the specific psychological mechanisms underlying the motivation effect. Future research can formally and directly test for possible process mechanisms in additional field studies, potentially identifying alternative ways to achieve the same effect and offering additional intervention tools for firms and policy makers.

Although this paper focuses on planning nudges within the previously uninvestigated, yet commonly occurring, task structure of optimal deadlines, this task structure represents just one of many possible alternative temporal task structures that may shape nudge efficacy. The tasks studied in this paper, despite varying in effort and complexity (e.g., entering lottery code online in Study 2 vs. completing the FAFSA in Study 3), constitute single observed occurrences. It is unclear whether we would observe the same effect for behaviors that are regularly repeated. A recent large-scale field study found a null effect of planning prompt nudges on repeated exercise behavior (Carrera et al. 2018). For recurring tasks with a repeated optimal deadline structure (e.g., paying monthly utility bills), should planning nudges always be implemented after the optimal deadline? In providing insights regarding the implementation of planning nudges for tasks involving an

optimal deadline, the research also highlights the need to consider other temporal dimensions of task structures that could enhance or undermine traditional effectiveness.

In demonstrating the benefit of implementing planning prompt nudges after an optimal deadline, our findings raise additional questions regarding the timing of nudge delivery. The present research suggests that nudging plan-making during the post-optimal deadline time window can curb procrastination, but how short is this window of opportunity? Research on "teachable moments" (McBride et al. 2003) shows individuals can briefly exhibit increased sensitivity to health interventions (e.g., smoking cessation) following adverse health events (e.g., new medical diagnosis, ER visit; Keenan 2009; Williams et al. 2005). With planning prompts, how soon after the optimal deadline should we nudge plan-making? Will the intervention remain effective until a terminal deadline? Future research can delve deeper to explore temporal duration elements of this effect.

Finally, we suggest that other nudges merit temporal attention. For example, would social-norm nudges be similarly effective before and after an optimal deadline? Would default nudges demonstrate equivalent efficacy if the choice immediately followed a meaningful temporal marker or if the timing of presentation was altered? More broadly, our findings raise questions about the inherently assumed time insensitivity of other behavioral economics tools, opening the door for future research to reinvestigate nudge efficacy considering the temporal context in which they are used.

3.0 (Essay 2) Giving Again: Temporal Structure of Initial Contribution Impacts the Size of Donors' Subsequent Gift

A rich research investigating consumer charitable giving has focused on examining factors that stimulate initial contributions (especially from new donors), including persuasive strategies of charitable appeals (e.g., Botner, Mishra, and Mishra 2015; Kristofferson, White, and Peloza 2014; Macdonnell and White 2015; Savary, Goldsmith, and Dhar 2015; Smith, Faro, and Burson 2013), defaulted and suggested donation amounts (e.g., Croson and Shang 2008; Edwards and List 2014; Goswami and Urminsky 2016; Smith and Berger 1996), and donor characteristics (e.g., Lee, Winterich, and Ross 2014; Reed, Aquino, and Levy 2007; Winterich, Zhang, and Mittal 2012).

It is critical, however, to understand not only what motivates consumers' initial donation behavior, but also what influences prior donors' later giving. Very high donor and donation attrition rates, averaging above 50% for more than a decade (Levis, Miller, and Williams 2019), are particularly problematic considering that repeat donors typically provide a larger source of annual revenue for charities (Flannery and Harris 2010) and are generally regarded as more cost-effective compared to costs of acquiring new donors (Levis et al. 2019). To date, however, there is a limited understanding of such subsequent donations or the ways in which successive follow-up requests can be optimally designed.

In this paper, we explore how the temporal design of an initial donation experience, referred to as the "time architecture" of initial giving, can shape the size of future charitable support. Specifically, we consider two common temporal structures: a one-time lump sum donation (e.g., $120 single gift) and a recurring donation, in which the identical total contribution is temporally dispersed (e.g., $10 monthly gift for 12 months). We argue that, despite the financial equivalency

of these initial donations, the differences in their time architecture impact the size of later donor support. We find that prior donors whose first (i.e., "initial") contribution was made as a series of recurring gifts later made a smaller second (i.e., "subsequent") contribution to the same charity than prior donors who initially gave the same amount as a single lump-sum. When responding to a charity's next successive donation request, initial recurring donors gave less than initial one-time donors, despite being similarly likely as initial one-time donors to make the second donation. Consistent with an anchoring account, but inconsistent with predictions based on hedonic editing and self-signaling, findings from five studies demonstrate that initial charitable contributions made as recurring (vs. one-time) gifts reduced the size of prior donors' second donation in both simulated and consequential donation contexts. When presented with an additional (one-time) charitable request, those who made their initial contribution as a recurring gift subsequently gave a smaller amount than those initially donated the same amount as a single lump-sum gift. However, explicitly cueing donors to a larger anchor and reducing the informational value of the anchor both effectively attenuate the effect on subsequent giving.

The present research makes both important theoretical and practical contributions. This paper adds to the charitable giving literature by explicitly examining factors contributing to donor attrition and donation deflation. By contrast, most of the existing work has overwhelmingly focused on the initiation, not the continuation, of charitable giving. Furthermore, this paper identifies time architecture as an aspect of a consumer's initial donation experience that can influence long-term donor support. In doing so, this contributes to the development of a framework focusing on subsequent giving. For example, although some research suggests that displaying costs in a disaggregated format can increase compliance with initial charitable requests (e.g., Gourville

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1998, 2003), the present work demonstrates that actually making such disaggregated payments can negatively impact subsequent donation behavior.

In addition, the current research enhances understanding of the processes shaping continued charitable giving. In the present case, we find results consistent with anchoring and test simple strategies for weakening such anchoring effects. As such, this paper provides guidance regarding donation design. With results indicating that recurring donation structures can lead to less subsequent giving, charities may benefit from tailored appeals for prior recurring donors, such as reminding donors of their cumulative contribution to date when making a later donation request.

3.1 Time Architecture and Charitable Giving

3.1.1 Financially Equivalent ≠ Psychologically Equivalent

Frequently, when making donations (especially online) consumers can opt to make either a one-time, lump sum donation (e.g., a single gift of $120 today) or a recurring donation that automatically repeats at some regularly occurring interval (e.g., a monthly gift of $10 for 12 months). Although one-time and recurring donation formats differ with respect to their temporal structure, individuals cumulatively donate identical amounts over the same total time period (e.g., $120 over the course of the year). The time architecture of recurring donations, compared to one-time donations totaling the same amount, partitions charitable giving into a series of several smaller gifts.

We argue that despite the financial equivalency of these donation experiences, differences in their temporal design render them psychologically distinct. Extensive research has documented

such violations of rational behavior in the domain of money, including those resulting from differences in presentation format (e.g., Goldstein, Hershfield, and Benartzi 2016; Mishra, Mishra, and Nayakankuppam 2006; Raghubir and Srivastava 2002). For example, segregated (vs. aggregated) presentation of identical monetary amounts alters the perceived value consumers attach to different denominations (e.g., ten \$10 bills vs. one \$100 bill; Mishra et al. 2006) and the perceived adequacy of consumers' retirement savings (e.g., monthly annuity vs. lump sum; Goldstein et al. 2016). Past theory offers three possible mechanisms by which the temporal structure of an initial charitable contribution may impact the size of prior donors' next contribution to the same charity: anchoring, hedonic-editing, and self-signaling. We predict, and our findings support, an anchoring-based account.

3.1.2 Size of Prior Donors' Next Contribution

3.1.2.1 Anchoring Account: Recurring Donors < One-Time Donors

With anchoring effects, salient numeric values can systematically bias judgments and decisions in the direction of the anchor. In the context of the temporal structure of an initial charitable contribution, we suggest that recurring and one-time donation formats provide different anchors for prior donors, with recurring donors anchoring on a comparatively smaller amount than one-time donors (e.g., \$10 vs. \$120) and, consequently, skewing subsequent donation decisions in the direction of the anchor. Anchoring effects have been observed in a variety of consumer domains, including minimum credit card payments (Stewart 2009), online auctions (Kamins, Dreze, and Folkes 2004), calorie estimates (Chernev 2011), negotiation offers (Galinsky and Mussweiler 2001), price estimates (Nunes and Boatwright 2004), selling prices (Simonson and Drolet 2004), and willingness to pay (e.g., Critcher and Gilovich 2008; Gneezy, Gneezy, et al.

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2012; Jung, Perfecto, and Nelson 2016). In addition, research within the domain of charitable giving demonstrates that donation behavior can be impacted by referencing specific amounts during a charitable appeal (e.g., Croson and Shang 2008; Desmet and Feinberg 2003; Shang and Croson 2009; Smith and Berger 1996). For example, during an on-air public radio fundraising drive, mentioning another donor's contribution amount (i.e., "We had another member, they contributed $X. How much would you like to pledge today?") skewed how much callers gave in the direction of the anchor (Croson and Shang 2008; Shang and Croson 2009).

Multiple psychological mechanisms have been proposed to account for anchoring effects, including anchoring and adjustment (Tversky and Kahneman 1974), numeric priming (Wilson et al. 1996), selective accessibility (Chapman and Johnson 1994, 1999; Mussweiler and Strack 1999, 2000; Strack and Mussweiler 1997), and scale distortion theory (Frederick and Mochon 2012). Common across these theories is the idea that anchors provide information that subsequently impacts judgment and decisions (Turner and Schley 2016). Although scholars debate the specific process(es) by which anchoring occurs, the effects of anchoring are remarkably robust. Anchoring can occur even when anchor values are incidental to the environment and irrelevant to the evaluative judgment (e.g., Ariely, Loewenstein, and Prelec 2003; Critcher and Gilovich 2008; Wilson et al. 1996), with effects that can persist despite explicit forewarning and incentivized accuracy (Chapman and Johnson 2002; Epley and Gilovich 2005; Simmons, LeBoeuf, and Nelson 2010; Tversky and Kahneman 1974; Wilson et al., 1996).

With respect to charitable giving, we contend that donors are more likely to anchor on the salient numerical dollar amount associated with their initial donation experience rather than its rate of repetition. Numerosity research indicates that numerical values are, by default, more salient to consumers than accompanying units (Monga and Bagchi 2012). More broadly, work on temporal

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sequences finds that people generally neglect the duration of experiences with the most salient and most recent events disproportionally influencing judgment and recall (e.g., Ariely and Carmon 2000; Fredrickson and Kahneman 1993). Thus, recurring donors likely discount the frequency of their repeated donations and attend more to the numeric amount of each donation, suggesting recurring donors anchor on a smaller value than one-time donors.

Anchoring can also prime magnitude and activate a general sense of largeness or smallness (Adaval and Monroe 2002; Oppenheimer, LeBoeuf, and Brewer 2007), which has been shown to distort subjective evaluations of size (Oppenheimer et al. 2007). This suggests that, despite giving the same cumulative amount as one-time donors, recurring donors may perceive their total contribution as subjectively smaller than one-time donors as a result of anchoring on the smaller, separated values. It follows, therefore, that to the extent that prior donors anchor on the salient numeric value of their initial donation, future donation support will be skewed in the direction of that anchor. Consequently, we predict that initial contributions made in a recurring donation format will decrease the size of prior donors' next donation to the same charity relative to one-time lump sum initial contributions of the same total amount.

3.1.2.2 Hedonic Editing Account: Recurring Donors > One-Time Donors

The theory of hedonic editing (Thaler 1985; Thaler and Johnson 1990) would predict that, holding total giving amount constant, consumers should experience greater prosocial utility when their initial donation is broken into repeated installments. Such installments would represent segregated gains in prosocial utility (Thaler 1985), and boost happiness in the same way as seen in Morewedge et al.'s (2007) work, where consumers experience greater hedonic benefit when receiving $5 on each of five days rather than $25 on one day. By contrast, prospect theory predicts

that each additional dollar donated in a single occasion lump-sum should provide donors with less happiness (i.e., utility).

To the extent that contributions are experienced primarily as gains, temporally separated recurring payments would be expected to heighten consumers' donation happiness (relative to one-time lump-sum donations). Therefore, to the extent that happiness is an important "payoff" from donating, a recurring donation structure may encourage larger subsequent donations than the potentially less-satisfying lump-sum donation structure.

3.1.2.3 Self-Perception Account: Recurring Donors > One-Time Donors

Literature on self-perception theory (Bem 1972) and self-signaling (Bénabou and Tirole 2006; Bodner and Prelec 2003) suggests that people make inferences about themselves based on their behavior and decisions. Consumer choices are signals to the self, conveying information that can influence one's self-attributions and self-concept (e.g., Dhar and Wertenbroch 2012; Kahn and Dhar 2006). In fact, it has been proposed that self-signaling provides incremental utility independent of the outcome utility gained from consumption (Bodner and Prelec 2003).

Recent work shows that charitable giving serves as a self-signal of one's prosocial character that can increase subsequent donations (e.g., Gneezy, Imas, et al. 2012; Savary et al. 2015). However, the valence and strength of these signals are susceptible to contextual influences, such as content of persuasive appeals (Savary et al. 2015) and personal costliness of prosocial action (Gneezy, Imas, et al. 2012). It is possible, therefore, that the structure of one's charitable donation may similarly alter the self-signaling benefits of one's charitable donation. The greater frequency of recurring donations may amplify the salience of positive prosocial self-signals. As such, donors may interpret the greater frequency of their charitable donations as a stronger signal of their prosocial character or personal commitment to the charity, leading to increased subsequent

donor support. Based on this line of reasoning, it follows that, compared to a lump-sum charitable contribution format, a recurring donation format will increase future donation support, as a result of increased prosocial self-signaling.

3.1.3 Overview

In sum, when making subsequent donation decisions, consumers may be influenced by the anchor provided from their initial donation structure, the utility gained from the initial donation experience (e.g., experienced happiness), or its strength as an identity signal (e.g., perceived charity commitment). Fortunately, these various accounts lend themselves to different patterns of effects. If, as anticipated, an anchoring effect holds, we would expect to see recurring donors make smaller subsequent gifts than one-time donors. If a hedonic-editing or self-perception account holds, we would expect to see recurring donors making larger subsequent gifts than one-time donors (see Table 9 for summary). Across five studies, we observe a pattern consistent with an anchoring account, demonstrating reduced giving of recurring donors as well as moderating effects theoretically supporting the anchoring mechanism.

Table 9 (Essay 2) Summary of Predictions for Subsequent Donation Amount

Theoretical Account	Argument	Subsequent Donation Amount
Anchoring	The numeric anchor provided by the initial gift (e.g., objective size) will be smaller for prior donors who initially give in a recurring format (vs. one-time format), directionally skewing the size of donors' next contribution.	Recurring < One-Time
Hedonic Editing	The prosocial utility gained from the initial gift (e.g., happiness experienced) will be greater for prior donors who initially give in a recurring format (vs. one-time format).	Recurring > One-Time
Self-Signaling	The strength of the self-signal conveyed by the initial gift (e.g., perceived commitment to charitable cause) will be greater for prior donors who initially give in a recurring format (vs. one-time format).	Recurring > One-Time

Study 1 provides an initial evidence that making a one-time lump-sum donation (relative to recurring donations totaling the same amount) increases how much the donor gives on subsequent occasions, using an online donation scenario. Study 2 replicates this pattern of effects with a more consequential design, in which participants experience the temporal structure of their initial contribution and make real subsequent donations, additionally providing mediation evidence supporting the predicted anchoring account and ruling out alternative mechanisms.

Study 3 and Study 4 both offer additional process evidence, demonstrating that factors that weaken an anchor's influence also weaken the degree to which recurring donors scale back their giving (relative to one-time donors): providing a more favorable numeric anchor (Study 3) and reducing the informational value of the initial anchor (Study 4). Using a moderation-of-process design (Spencer, Zanna, and Fong 2005), Study 3 shows that reminding prior donors of their cumulative contribution to date when presenting a subsequent donation request attenuates the observed anchoring effect. Using a measurement-of-mediation design (Spencer et al. 2005), Study 4 finds a weaker anchoring effect for those less susceptible to the influence of external informational cues.

Studies 5A and 5B explore the implications of presenting a one-time temporal structure when the initial donation decision is made, to circumvent later reduced giving among recurring donors by setting a more favorable anchor at the outset. Results from Study 5A find that although restricting initial giving to a one-time format can increase the size of a donor's subsequent gift, consistent with an anchoring account, doing so can negatively impact the likelihood of making the initial donation. Study 5B suggests that jointly displaying both temporal structures during an initial donation request, however, can increase donors' preference for lump-sum giving, thus offering the potential to realize the boost from a one-time donation structure without adversely impacting initial donations.

3.2 Study 1: ASPCA Donation Scenario

Study 1 offers preliminary evidence that the temporal structure of an initial donation experience shapes subsequent giving in ways most consistent with an anchoring account. In this study, participants simulated making an online donation to the American Society for the Prevention of Cruelty for Animals (ASPCA), either as a time-dispersed recurring donation or as a one-time lump-sum donation of the equivalent total amount, and then estimated how much they would donate on a subsequent occasion and their likelihood of donating.

3.2.1 Method

This study, run in November 2018, preregistered using Open Science Framework (OSF; https://osf.io/ecy92/). MTurk participants (n = 344;[11] 50.0% male; M_{age} = 37.80, SD = 11.34), who were compensated ($.50) for their participation, imagined making an online charitable gift to the ASPCA and were randomly assigned to one of two temporal structure conditions: a one-time donation of $30 (*one-time*), or a recurring monthly donation of $5 for six months (*recurring*). Thus, all participants donated identical total amounts (i.e., $30).

When learning about their donation, all participants viewed a fictitious ASPCA webpage displaying their donation structure (see Appendix F for sample stimuli). To enhance participant involvement in the scenario study, participants then used a drop-down menu to select the amount that they had been assigned (i.e., $5, $10, $15, $20, $25, or $30) and their assigned frequency (i.e., one time, monthly for 3 months, monthly for 6 months, monthly for 9 months, monthly for 12 months, or yearly) of their donation, mimicking donor actions when making an online charitable gift. If this answer did not match their assigned condition, participants were asked to re-read the prior screen and only allowed to advance when they had answered correctly.

All participants then imagined being asked to make a subsequent donation to the same charity six months later and reported the likelihood (1 – "Not at Likely," 7 – "Very Likely") and the amount (US $, open response) they would donate, the order of which was counterbalanced across participants. Afterwards, participants completed a three-item index of charity perceptions for inclusion as a covariate ("ASPCA supports a good cause," "ASPCA is a great charity," "ASPCA makes a difference;" 1 – "Strongly Disagree," 7 – "Strongly Agree;" α = .94). Finally,

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[11] This is the final sample included in analysis after removing participants based on our pre-registered exclusion criteria (those who identified as non-native English speakers or who reported encountering technical problems; n = 8).

participants provided demographic information (e.g., income; for survey and stimuli see Web Appendix I).

3.2.2 Results

We included participants' income and charity perceptions as covariates in our analyses, as past research has shown that individual wealth (Kessler, Milkman, and Zhang 2019; List 2011; Piff et al. 2010; Smeets, Bauer, and Gneezy 2015) and attitudes related to the charity and its recipients can influence generosity (e.g., Bartlett and DeSteno 2006; Small, Loewenstein, and Slovic 2007; Winterich, Mittal, and Ross 2009). For the remainder of this paper, all subsequent results reflect covariate-adjusted estimates, and all analyses are robust to the exclusion of the covariates.

3.2.2.1 Subsequent Donation Amount

We conducted a regression analysis predicting prior donors' subsequent donation amount as a function of the temporal structure of their initial donation (0 = one-time donor, 1 = recurring donor), controlling for income and charity perceptions. We observe a significant main effect of initial donation structure (b = -13.907, SE = 1.093, t = -12.72, $p < .0001$), with recurring donors making a significantly smaller subsequent donation to the same charity (M = \$5.57) relative to one-time donors (M = \$19.47; see Figure 6).
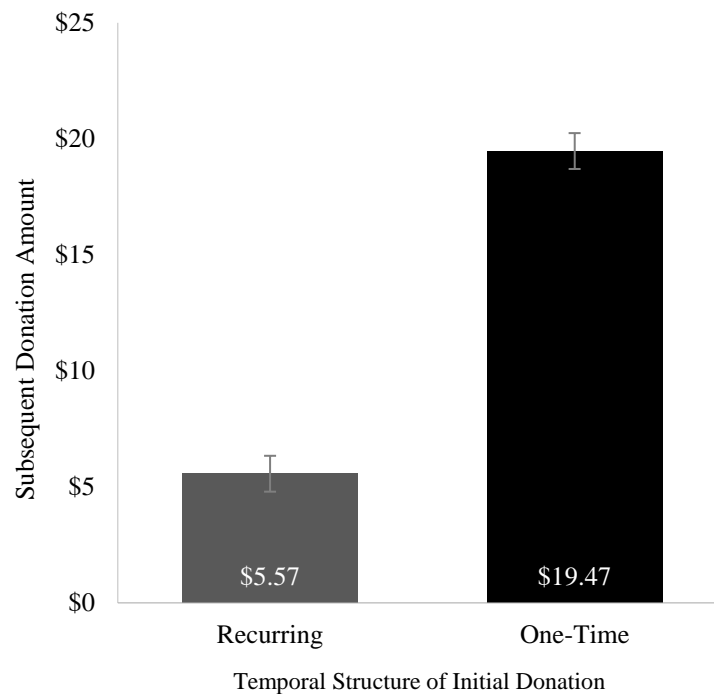
$$\text{Note. Error bars represent standard errors. Estimates are adjusted for covariates (income, charity perceptions).}$$

Note. Error bars represent standard errors. Estimates are adjusted for covariates (income, charity perceptions).

Figure 6 (Essay 2) Study 1: Subsequent Donation Amount

3.2.2.2 Self-Reported Subsequent Donation Likelihood

A linear regression analysis predicting prior donors' self-reported likelihood of making a subsequent donation as function of their initial donation structure (0 = one-time donor, 1 = recurring donor), controlling for income and charity perceptions, reveals a marginally significant main effect of initial donation structure (b = .329, SE = .178, t = 1.85, p = .066). Prior donors who made their initial donation as a recurring contribution weakly anticipated a tendency to be more likely to make a subsequent donation (M = 4.49) relative to one-time donors (M = 4.16).

3.2.2.3 Observed Likelihood of Making Subsequent Donation (Inferred from Amount)

We first created a binary indicator of whether prior donors made a subsequent donation based on the amount subsequently given. Those giving $0 were coded as 0 ("didn't make

subsequent donation") and those giving more than $0 were coded as 1 ("made subsequent donation"). In total, 80.23% of sample made a subsequent donation (n = 276). Results from a binary logistic regression with the indicator of additional giving as the dependent variable (0 = didn't make subsequent donation, 1 = made subsequent donation) and temporal structure of the initial donation as the primary predictor, controlling for income and charity perceptions, show no main effect of initial donation format (b = .118, SE = .298, t = .40, p = .691). Recurring donors (81.02%) and one-time donors (79.46%) were equally likely to make a subsequent donation.

3.2.3 Discussion

Study 1 finds a robust effect of the initial donation temporal structure on the size of prior donors' subsequent giving. Consistent with our predictions, recurring donors estimated they would give 71.39% less than one-time donors to the same charity six months later. This effect is consistent with an anchoring account, suggesting that individuals may be anchoring on the most accessible numerical value from their initial donation experience when making their subsequent donation decision, with individuals who initially gave smaller recurring amounts anchoring on a smaller value than individuals who initially gave an equivalent total amount as a larger one-time amount. Study 1 also showed weak evidence that recurring donors may believe they are more likely than one-time donors to make an additional contribution, although no difference in actual likelihood of additional giving emerged, suggesting that individuals may not anticipate the impact of donation structure on later giving.

3.3 Study 1: Follow-Up Studies

3.3.1 First Follow-Up Study

One alternative explanation for recurring donors' smaller subsequent giving, however, could be that the recurring donors assumed that the second donation would also be temporally segregated, despite being described as a one-time additional charitable gift. Thus, they may have reported subsequent donation amounts representing only a fraction of the true total additional amount they are willing to give (i.e., they reported the amount they would give per month or week, not the aggregate sum). A follow-up study, however, suggests this is likely not the case.

An independent sample of Mturk participants (n = 116;[12] 60.34% male; M_{age} = 34.85, SD = 11.40) were randomly assigned to the same Study 1 donation conditions (i.e., a one-time donation of $30 or a recurring monthly donation of $5 for six months) and imagined the identical online donation scenario (see Web Appendix I Follow-Up Study 1A for survey details). Unlike Study 1, after imagining making their initial donation, participants did not report their anticipated donation amount or likelihood to donate. Rather, participants imagined that six months after their initial gift they were asked to make a second, additional donation to the same charity. All participants imagined making a $5 donation in response to the additional charitable request. Participants then indicated how they interpreted their additional $5 donation ("Which of the following best represents how you interpreted the statement: You made a $5 donation."), either as a one-time donation (i.e., "This means I made a one-time donation of $5 (i.e., only at the time of the request)") or a recurring donation (i.e., "This means I made a repeating donation of $5 (e.g.,

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[12] This is the final sample included in analysis after removing those individuals who reported technical problems or being non-native English speakers (n = 2).

monthly for 6 months)"). Afterwards, participants completed the same three-item index of charity perceptions used in Study 1 ($\alpha = .92$) and provided basic demographic information (e.g., income).

Using the same exclusion criteria and covariates as in Study 1, we conducted a binary logistic regression regressing the perceived temporal structure of participants' additional \$5 donation (0 = one-time, 1 = recurring) on the temporal structure of participants' original donation (0 = one-time, 1 = recurring), controlling for income and charity perceptions. The temporal structure of the initial donation did not significantly predict the likelihood of participants interpreting their subsequent \$5 donation as a recurring gift (b = -.499, SE = .537, t = -.93, p = .352; see Figure 7). Recurring donors (12.28%) and one-time donors (18.70%) were similarly likely to perceive the second donation as temporally distributed. This suggests that systematic misinterpretation of the temporal structure of the additional charitable donation does not account for recurring donors' reduced giving in Study 1.

3.3.2 Second Follow-Up Study

Another alternative explanation is that recurring donations do not feel like a "gain" to the same degree as one-time donations. Charitable giving technically constitutes an economic loss. If individuals perceive donations as losses, prospect theory and hedonic editing predict that lump-sum contributions would create less pain than recurring time-dispersed giving. Therefore, the lump-sum giver, being less pained by their payment, may give more on a subsequent occasion than the recurring-payment giver, who experiences multiple acute losses over the course of their donation experience. We test this potential alternative account in a second follow-up study.

A separate sample of Mturk participants (n = 149;[13] 42.28% male; M_{age} = 37.54, SD = 12.21) imagined that they had completed a Mturk HIT six days ago (see Web Appendix I Follow-Up Study 1B for survey details). That HIT paid $10, and participants were told they donated a portion of their earnings to charity (i.e., ASPCA). The temporal structure of this donation was randomly assigned as being recurring or one-time. Participants learned that they either donated $1 every day for five business days (recurring condition) or donated $5 on the day they completed the HIT (one-time condition). Afterwards, participants completed four items measuring the degree to which their initial contribution felt like a gain versus a loss (e.g., "Right now, in this exact moment, to what extent does your donation to the ASPCA feel like a "loss" versus like a "gain"? sliding scale anchored at -5 = "Complete Loss," -2.5 = "More Loss than Gain" 0 = "Equally a Loss and a Gain," 2.5 = "More Gain than Loss," 5 = "Complete Gain"), which were averaged in a composite index of gain perceptions (α = .87). Finally, participants completed the same charity perceptions index (α = .94) and demographic information assessed in Study 1.

Employing the same exclusion criteria and covariates as in Study 1, we conducted a regression analysis predicting gain perceptions as a function of the temporal structure of participants' donation (0 = one-time, 1 = recurring), while controlling for income and charity perceptions. The temporal structure of the initial donation did not significantly predict the degree to which the donation was seen as a gain (b = .326, SE = .341, t = .95, p = .342); the "gain" felt by making a recurring donation (M = .88) was similar to that of making a one-time donation (M = .55; see Figure 7). Moreover, donations were not perceived as an economic loss. Both recurring donors (t = 4.10, p < .001) and one-time donors (t = 2.12, p = .036) perceived their donations more as a gain than a loss.

[13] This is the final sample included in analysis after removing those individuals who reported technical problems or being non-native English speakers (n = 4).

A. Temporal Structure of Subsequent Donation   B. Perceiving Donation as Gain (vs. Loss)
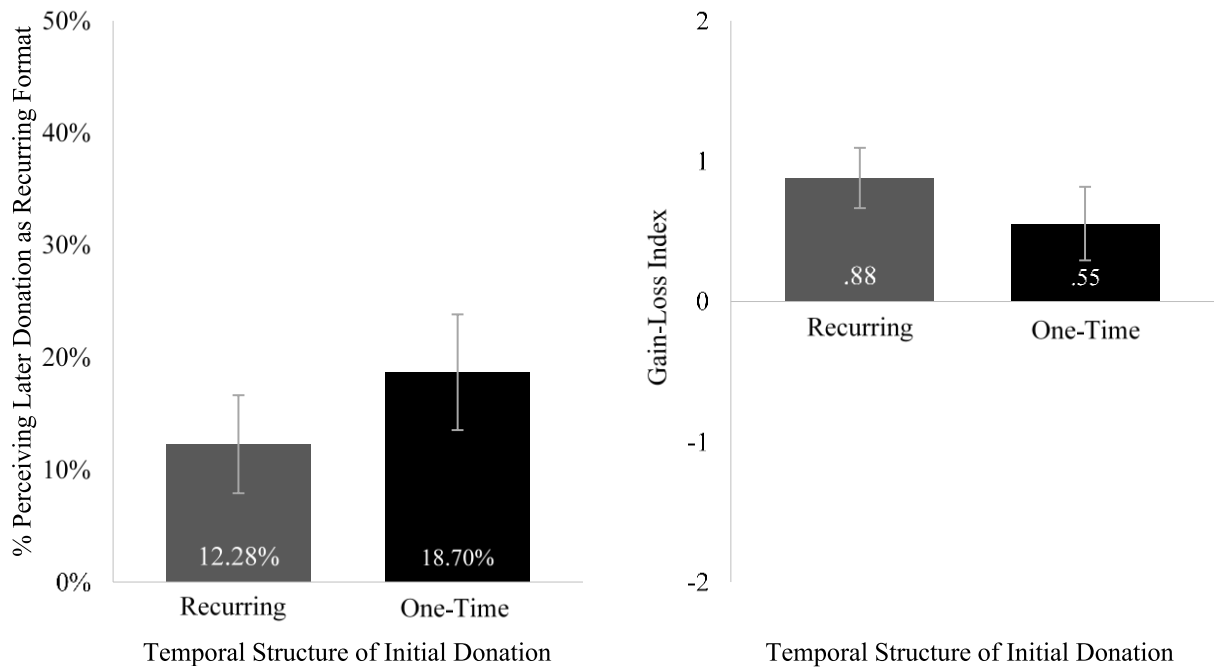
Note. Error bars represent standard errors. Estimates are adjusted for covariates (income, charity perceptions).

Figure 7 (Essay 2) Study 1: Follow-Up Studies

3.3.3 Discussion

These follow-up studies rule out two possible alternative explanations for recurring donors' reduced giving in Study 1: that recurring donors (1) misinterpreted the temporal structure of the subsequent donation as also being temporally dispersed or (2) experienced their donation as less of a gain than one-time donors. We find that the temporal structure of the initial donation does not differentially influence the likelihood of inferring that the subsequent donation is believed to be recurring nor the degree to which one's charitable giving feels like a gain.

These follow-up studies, however, do not address the concern that Study 1's donation experience was imagined. Despite the prevalence with which scenario-based experiments are used to study marketing phenomena, a recent examination of anchoring effects in the lab versus the field suggests that hypothetical payments may be more sensitive to anchors than real payments (Jung et al. 2016). Thus, a hypothetical donation experience could be stacking the deck in favor of results consistent with an anchoring mechanism. We directly address this weakness in Study 2 by introducing consequential donation behavior and having donors experience the temporal structure of their initial donation.

3.4 Study 2: Mediation Support for Anchoring Account

Unlike Study 1, participants in Study 2 experienced the temporal structure of their initial contribution and made real subsequent donations. Over a ten-day period, initial donations were made as either a lump-sum or recurring gift, after which participants were given the opportunity to make a second donation that was deducted from their payment. In addition, Study 2 sought to provide evidence of the anchoring process underlying the reduced subsequent giving of recurring donors while testing for potential alternative mechanisms. To do this, we measured constructs associated with each proposed mechanism: subjective magnitude of initial contribution (anchoring), experienced hedonic utility of initial contribution (prospect theory/hedonic editing), and altered self-inferences following the initial donation experience (self-signaling). If anchoring is driving effects, as predicted, we would expect that individuals who initially gave in a recurring donation structure would perceive their total charitable contribution as subjectively smaller than those who gave in a one-time donation structure, resulting in lower subsequent support. Moreover,

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we would not expect mediating effects through hedonic utility or self-inferences resulting from the initial donation experience. For the sake of realism, Study 2 also included a condition in which recurring donors received an email notification after each donation payment, mimicking online payment confirmation that charities sometimes send following each recurring gift, to see if such reminders altered our results.

3.4.1 Method

In June 2016, Mturk participants (n = 362;[14] 54.70% male; M_{age} = 36.15, SD = 12.00) learned that as a thank you for participation, donations would be made on their behalf to charity. Participants were randomly assigned to one of three donation conditions: a one-time $.50 donation made on the day of the survey; a recurring $.10 donation every other day for ten days, without email notifications; or a recurring $.10 donation every other day for ten days, with email notifications. Thus, all participants donated the identical total amount (i.e., $.50).

After learning how their donation was structured, participants selected which of three charities (i.e., Habitat for Humanity, ASPCA, Feeding America) would receive their donation, to increase participants' engagement with and attribution of the donation. All donations were actually made, in accordance with participants' charity selections.

To heighten the legitimacy of the donation and to boost participants' perceptions of the donation as coming from them (rather than the researchers), participants "approved" the donation (purportedly in accordance with institutional guidelines). Lump-sum donors selected a statement saying, "I am donating $.50 (today)," and recurring donors selected a statement saying, "I am

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[14] This is the entire initial sample included in analysis after removing those who reported technical problems or being non-native English speakers (n = 7).

donating $.10 every other day for the next ten days (starting today)." After confirming their

donation, participants provided basic demographic information (e.g., income).

Starting on the day of the survey, those in the recurring-with notifications condition

received an email following each of the five $.10 donations (i.e., every other day). All email

notifications were identical and simply notified the participant that their $.10 donation had been

made (see Figure 8 for donation and email schedule).

Temporal Structure of Initial Donation	Day 1	Day 2	Day 3	Day 4	Day 5	Day 6	Day 7	Day 8	Day 9	Day 10
Recurring (with notifications)	$.10 Email		$.10 Email		$.10 Email		$.10 Email		$.10 Email	Subsequent Donation Request
Recurring (no notifications)	$.10		$.10		$.10		$.10		$.10	Subsequent Donation Request
One-Time	$.50									Subsequent Donation Request

Figure 8 (Essay 2) Study 2: Email Notification and Donation Schedule

Ten days later, participants were invited via email to complete a follow-up survey,

reporting how happy they felt about their donation (1 – "Not at All Happy," 7 – "Very Happy"),

how committed they felt towards the charity (1 – "Not at All Committed," 7 – "Very Committed"),

and how large they perceived their donation to be ("How much did you donate?" 0 – "Very Little,"

100 – "Very Much"), the order of which was randomized. Participants were then given the

opportunity to donate a portion of their participation payment to the same charity on a sliding scale,

ranging from $.00 to $.50 (see Web Appendix J for time 1 survey and Web Appendix K for time

2 survey). In total, 79.28% of the original sample (n = 287; 56.45% male; M_{age} = 36.71, SD = 11.60) completed the follow-up survey, representing our final sample.[15]

3.4.2 Results

3.4.2.1 Subsequent Donation Amount

We first conducted a regression analysis predicting how much prior donors subsequently give as a function of the temporal structure of their initial donation (dummy-coded as recurring without notifications: 0 = one-time donor, 1 = recurring w/out notifications; and recurring with notifications: 0 = one-time donor, 1 = recurring w/notifications), while controlling for participant income. Results revealed a marginally significant direct effect of initial donation structure on subsequent donation amount (F(2,283) = 2.66, p = .072). Both recurring donors receiving notifications (M = $.09; b = -.044, SE = .021, t = -2.03; p = .043) and those not receiving notifications (M = $.09; b = -.042, SE = .021, t = -1.95; p = .052) gave less than one-time donors (M = $.13) to the same charity 10 days later (see Figure 9). Recurring donors with notifications donated a similar amount as recurring donors without notifications (b = -.002, SE = .022, t = -.09, p = .926).

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[15] Attrition did not vary by temporal structure of initial donation (χ^2 (2) = .86, p = .651). Recurring donors who received a reminder (76.73%; b = -.266, SE = .318, t = -.84, p = .403) and recurring donors who did not receive a reminder (80.65%; b = -.031, SE = .329, t = -.09, p =.926) were similarly likely to complete the follow-up survey as one-time donors (81.12%).
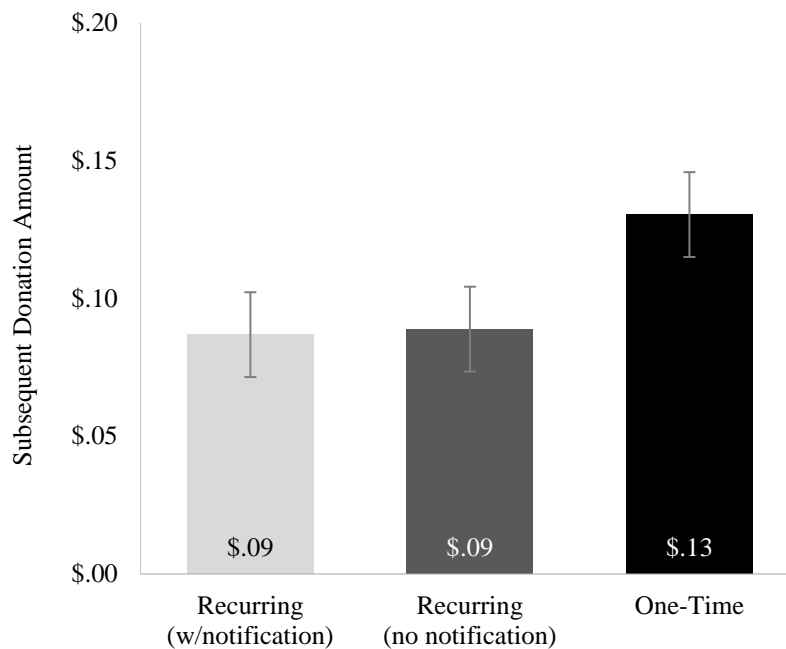
Note. Error bars represent standard errors. Estimates are adjusted for covariate (income).

Figure 9 (Essay 2) Study 2: Subsequent Donation Amount

3.4.2.2 Perceived Donation Size

Conducting a linear regression analysis using the same two dummy-coded indicators of temporal structure from the previous analysis to predict perceived subjective size of participants' initial donation, controlling for income, reveals a significant main effect of initial donation structure ($F(2, 283) = 5.99$, $p = .003$). Contrasts show that recurring donors who received a reminder ($M = 14.72$; $b = -10.433$, $SE = 3.177$, $t = -3.28$; $p = .001$) perceived their donation as significantly smaller than one-time lump-sum donors ($M = 25.15$). Similarly, recurring donors who did not receive a reminder ($M = 17.07$; $b = -8.080$, $SE = 3.153$, $t = -2.56$; $p = .011$) also perceived their donation as significantly smaller relative to one-time donors. No difference emerged with respect to perceived donation size between recurring donations with and without reminders ($b = -2.353$, $SE = 3.192$, $t = -.74$, $p = .462$). Thus, although all participants gave the

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same total amount for their initial donation (i.e., $.50), lump-sum donors perceived their initial charitable contributions as subjectively larger than recurring donors.

3.4.2.3 Experienced Happiness

A regression analysis predicting happiness experienced from donors' initial contribution as a function of temporal structure of the initial donation (again using the two dummy-coded indicators of temporal structure from the prior analyses), controlling for income, revealed no significant main effect of initial donation structure ($F(2,283) = .02$, $p = .984$). Neither recurring donors receiving notifications (M = 5.57; b = .022, SE = .190, t = .12 ; $p = .906$) nor recurring donors not receiving notifications (M = 5.54; b = -.012, SE = .189, t = -.06; $p = .951$) differed from one-time donors (M = 5.55) in the happiness they experienced from their initial donation. In addition, recurring donors with notifications and recurring donors without notifications did not differ in their experienced donation happiness (b = .034, SE = .191, t = .18, $p = .858$).

3.4.2.4 Charity Commitment

A linear regression analysis predicting donors' charity commitment as a function of temporal structure of the initial donation (using the same two dummy-coded indicators of temporal structure), while controlling for income, did not show a significant main effect of donation structure ($F(2,283) = 1.30$, $p = .275$). Results suggest that after making their initial donations, neither recurring donors receiving notifications (M = 4.53; b = -.397, SE = .335, t = -1.19; $p = .237$) nor recurring donors not receiving notifications (M = 5.05; b = .121, SE = .332, t = .37; $p = .715$) experienced different levels of charity commitment relative to one-time lump-sum donors (M = 4.92). Those making recurring donations with notifications and those making recurring

donations without notifications did not differ in their level of commitment (b = -519, SE = .337, t = -1.54, p = .124).

3.4.2.5 Mediation

Using PROCESS 3.0 (Model 4; 10,000 bootstrap samples; Hayes 2018), we ran a parallel multiple mediator model that simultaneously examined the indirect effect of the initial donation's temporal structure on the amount of subsequent charitable support through each of the proposed mediators (i.e., experienced happiness from the initial contribution, perceived size of the initial contribution, and perceptions of charity commitment). Donation structure was coded as a multi-categorical predictor, which generated two dummy variables: one comparing recurring donation without reminders to the one-time donation (0 = one-time donor, 1 = recurring w/out notifications) and the other comparing recurring donation with reminders to the one-time condition (0 = one-time donor, 1 = recurring w/notifications).

Results show a significant, negative indirect effect of donation structure on subsequent donation amount through the perceived size of the initial donation. Relative to one-time donors, recurring donors who received notifications perceived their initial charitable giving as smaller, resulting in a smaller second donation (b = -.012, SE = .007, CI_{95}[-.029, -.001]; see Figure 10 and Appendix G). Likewise, recurring donors who did not receive a notification also made a smaller second donation than one-time donors because they perceived their initial donation as smaller (b = -.009, SE = .006, CI_{95}[-.025, -.0004]).

No significant mediating effects, however, emerged through experienced happiness. The smaller subsequent donation of recurring donors who received notifications (b = .0003, SE = .003, CI_{95}[-.005, .006]) and recurring donors who did not receive notifications (b = -.0001, SE = .003, CI_{95}[-.006, .006]), relative to one-time donors, was not due to differences in experienced happiness

from the initial donation. In addition, results showed no indirect effect of donation structure through charity commitment. Recurring donors who received notifications (b = -.005, SE = .005, CI₉₅[-.015, .003]) and recurring donors who did not receive notifications (b = .002, SE = .004, CI₉₅[-.007, .011]) did not give less than one-time donors because of differences in inferred charity commitment. Thus, neither experienced happiness from the initial contribution nor perceptions of charity commitment account for the indirect effect of donation structure on the size of later donation support.
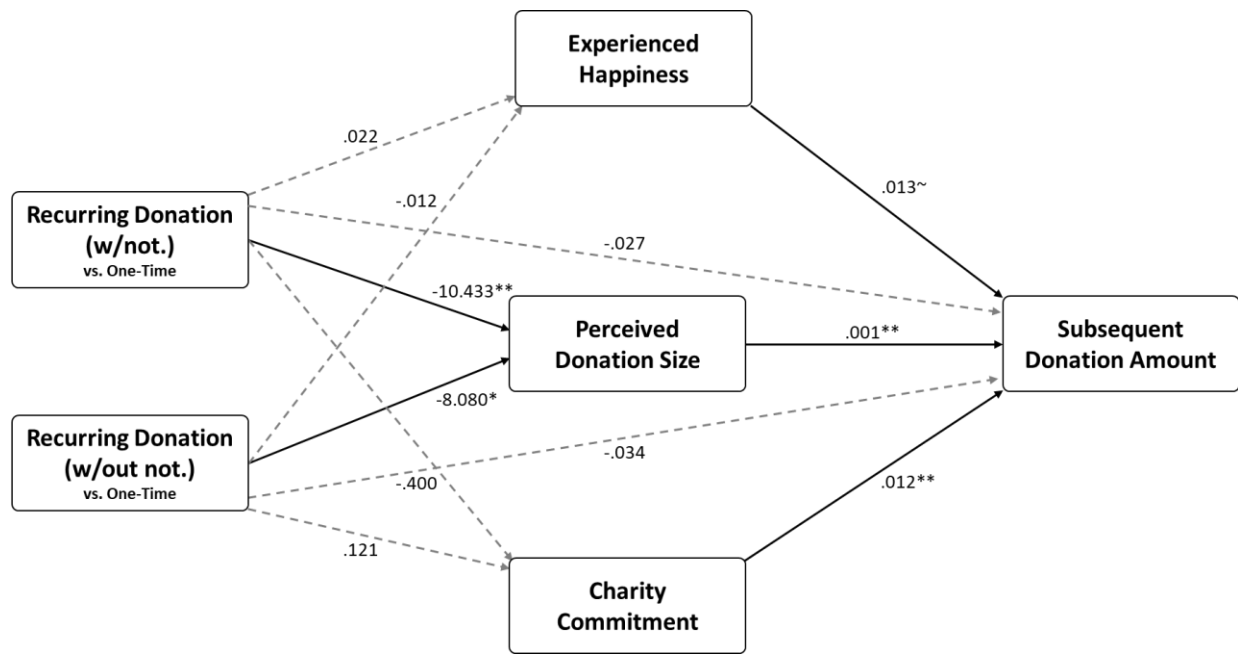
Note. Significance values are indicated as follows: ˜$p < .10$, *$p < .05$, **$p < .01$, ***$p < .001$. Conducted using PROCESS 3.0 (model 4; 10,000 bootstrapped samples), with a multicategorical IV (D_1: 0 = one-time donation, 1 = recurring donation with notification; D_2: 0 = one-time donation, 1 = recurring donation without notification) controlling for income. Indirect effects only emerged through perceived size of initial donation (D_1: b = -.012, SE = .007, CI₉₅[-.029, -.001]; D_2: b = -.009, SE = .006, CI₉₅[-.025, -.0004]).

Figure 10 (Essay 2) Study 2: Parallel Mediation Analysis

3.4.2.6 Observed Likelihood of Making Subsequent Donation (Inferred from Amount)

To test for differences in the likelihood of making a subsequent donation, we generated a binary indicator of additional giving based on the size of prior donors' additional contribution (0

= didn't make subsequent donation, 1 = made subsequent donation). In total, 54.01% of the final sample made a subsequent donation (n = 155). Results from a binary logistic regression analysis predicting whether prior donors made a subsequent donation as a function of the temporal structure of their initial donation (dummy-coded as recurring without notifications: 0 = one-time donor, 1 = recurring w/out notifications; and recurring with notifications: 0 = one-time donor, 1 = recurring w/notifications), while controlling for participant income, show no main effect of initial donation format ($\chi^2(2) = 2.86$, $p = .240$). This suggests that recurring donors who received notifications (47.31%), recurring donors who did not receive notifications (54.99%), and one-time donors (59.38%) were equally likely to make a second, subsequent donation.

3.4.3 Discussion

Despite giving the financially equivalent total donation amount, recurring donors perceived the subjective size of their initial contribution as significantly smaller than one-time donors, which subsequently reduced the size of their subsequent donation to the same charity. This mediating effect, however, did not emerge through either experienced happiness from the initial donation or self-perceptions of charity commitment. Taken together, these results are more consistent with an anchoring explanation than the proposed mechanisms derived from the hedonic editing and self-signaling literatures. Recurring donors appear to anchor on the amount of their repeated donation rather than on the cumulative amount they have donated to date, such that perceived magnitude of their total contribution is subjectively smaller than one-time donors, resulting in reduced future charitable support (but not reduced likelihood). Study 2, therefore, replicates the results of Study 1 using actual charitable contributions and provides mediation evidence of the psychological process underlying the observed anchoring effect. Moreover, Study 2 suggests that recurring

donation notifications do not exacerbate this effect, finding no evidence that recurring donors who received email reminders perceived the subjective size of their initial contribution or subsequently gave less than recurring donors who did not receive email reminders.

Although participants' subsequent, additional donation to the charity was deducted from their payment, one potential limitation of Study 2 is the initial donation was not voluntarily chosen or personally costly. Consequently, donors may have experienced less personal responsibility for the initial contribution, thereby weakening the potential hedonic benefits and self-signaling utility. There is some research suggesting that individuals' prosocial responses depend on whether donation "costs" are actually incurred, or independent from, participation compensation (e.g., Gneezy, Imas, et al. 2012). Although we do not anticipate that this substantively impacted the results given that the initial donations were made independent of participant payment for all conditions, this does represent a potential weakness—one that we address in Study 3 by presenting the initial donation as noncompulsory and personally costly.

3.5 Study 3: Shifting Attention to Alternative Anchor

Study 3 uses a moderation-of-process design (Spencer et al. 2005) to test for additional process evidence consistent with the anchoring account. If anchoring is driving previously observed effects, then factors that weaken the initial anchor's influence should also weaken the degree to which recurring donors scale back their giving relative to one-time donors. Study 3 tests whether shifting recurring donors' attention to a larger anchor—specifically, their cumulative contribution—moderates the effect.

If recurring donation structures reduce subsequent support because the prior donors are anchoring on a smaller value, it follows that anchoring on a comparably larger value should attenuate this effect. To test this, Study 3 manipulated the salience of donors' cumulative initial contribution, reminding participants of their total amount donated to date before making their subsequent donation. We argue that presenting the cumulative donation amount when donors are asked to make a second donation provides a new numeric anchor, such that recurring donors who would otherwise anchor on a smaller repeated donation amount will instead anchor on a larger value that is equivalent to that on which lump-sum donors are anchoring. When the cumulative donation amount is not presented, we anticipate that results will replicate the previous pattern of findings: one-time donors will subsequently give more than recurring donors. However, when the cumulative donation amount is presented, we predict that the difference in the size of subsequent donations between one-time donors and recurring donors will be attenuated. In sum, we hypothesize that presenting donors' cumulative contribution during a subsequent donation request will reduce the negative impact of a recurring donation structure (relative to a one-time lump sum donation structure) on the size of charitable support.

Study 3 also addresses potential methodological concerns from Study 2 regarding participants' initial donations. Both Study 2 and Study 3 constitute consequential designs, as participants experienced the temporal structure of their initial contribution and made real subsequent donations. Study 3 further enhances experimental realism, with participants actively choosing to make an initial donation by spending additional time completing a separate research task that directly results in a financial donation (the structure of which was randomly determined). Thus, the initial donations in Study 3 are both voluntary and consequential.

3.5.1 Method

The study was administered in February 2017 and the paradigm was similar to that of Study 2, with the notable exception of how initial donations were made. Following an unrelated study, Mturk participants learned that they could make a charitable contribution by completing an additional task. Those who volunteered (n = 255; 39.61% male; M_{age} = 41.20, SD = 13.84) spent approximately five minutes on the charity task, after which they learned how their donation was structured. Given same pattern of results emerged in Study 2 for both recurring donation conditions (with and without notifications), the current study did not include notifications for recurring donors. Thus, participants were randomly assigned to either the one-time lump-sum or the recurring-no notifications condition. Similar to Study 2, participants in the lump-sum condition read that a donation of $.50 would be made that day, whereas participants in the recurring donation condition read that a recurring donation of $.10 would be made every other day for ten days, starting that day. Thus, in total, all participants donated the same cumulative amount (i.e., $.50). After learning how their donation was structured, participants selected which of three charities would receive their donation (i.e., Habitat for Humanity, ASPCA, Feeding America) and "approved" their donation (see Web Appendix L for time 1 survey).

Ten days later, these volunteers were invited to complete a follow-up survey in which they were given the opportunity to make a subsequent donation to the same charity. Those randomly assigned to the cumulative anchor condition read, "To date, you have donated $.50 to [charity name]," whereas those in the no cumulative anchor condition were not presented with this information. All participants were then asked how much of their payment they would be willing to donate to the same charity (open response), which was deducted from their earnings (see Web Appendix M for time 2 survey). Participants then completed an attention check asking participants

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to identify the charity to which they made their initial donation. Only those who correctly answered this item were included in analyses (n = 193; 41.97% male; M_{age} = 41.25, SD = 13.64),[16] reflecting 75.69% of the original sample.[17]

3.5.2 Results

3.5.2.1 Subsequent Donation Amount

We conducted a regression analysis testing the influence of temporal structure of initial donation (0 = one-time donor, 1 = recurring), cumulative donation anchor (0 = not present, 1 = present), and their interactive effect, controlling for participants' income, on the size of participants' subsequent donations. Results show no main effect of providing a cumulative donation reminder (b = .008, SE = .024, t = .31, p = .756), nor a main effect of temporal structure of the initial donation (b = -.016, SE = .024, t = -.68, p = .497). These were qualified, however, by a marginal interaction between donation structure and cumulative anchor on the size of participants' subsequent donations (b = .083, SE = .048, t = 1.74, p = .084).

When the cumulative anchor was not presented, recurring donors made smaller subsequent donations (M = $.11) than one-time donors (M = $.17; b = -.058, SE = .034, t = -1.72, p = .088), replicating prior findings. However, reminding recurring donors of their cumulative charitable contribution mitigated this effect, with no difference emerging between the amount given by recurring donors (M = $.15) and that given by one-time donors (M = $.13; b = .025, SE = .034, t = .73, p = .465; see Figure 11). Thus, consistent with our predictions, reminding donors of their

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[16] This is the final sample included in analysis, after removing those who failed attention check (n = 10).
[17] Attrition did vary by temporal structure of initial donation (b = .645, SE = .301, t = 2.14, p = .032), with recurring donors (81.33%) more likely to complete the follow-up survey than one-time donors (69.89%).

total prior contribution when making an additional charitable request reduced the difference in subsequent donation size between lump-sum and recurring donors.

3.5.2.2 Observed Likelihood of Making Subsequent Donation (Inferred from Amount)

In total, 59.59% of participants in the final sample made an additional charitable contribution (n = 115). We first recoded the additional amount donated into a binary indicator of whether a donation was made (0 = didn't make subsequent donation, 1 = made subsequent donation). Using this indicator as the dependent variable, we conducted a binary logistic regression with temporal structure of initial donation (0 = one-time donor, 1 = recurring), cumulative donation reminder (0 = not present, 1 = present), and their interactive effect as predictors, controlling for participants' income. The interactive effect found for donation amount did not emerge for donation likelihood (b = .846, SE = .601, t = 1.41, p = .160). In addition, results show no main effect of initial donation format (b = -.190, SE = .298, t = -.64, p = .523), with recurring donors (57.54%) and one-time donors (62.07%) similarly likely to make a subsequent donation. There was also no main effect of cumulative reminder (b = -.376, SE = .300, t = -1.25, p = .210). Receiving a reminder (55.16%) did not increase likelihood to donate, compared to not receiving reminder (64.15%).
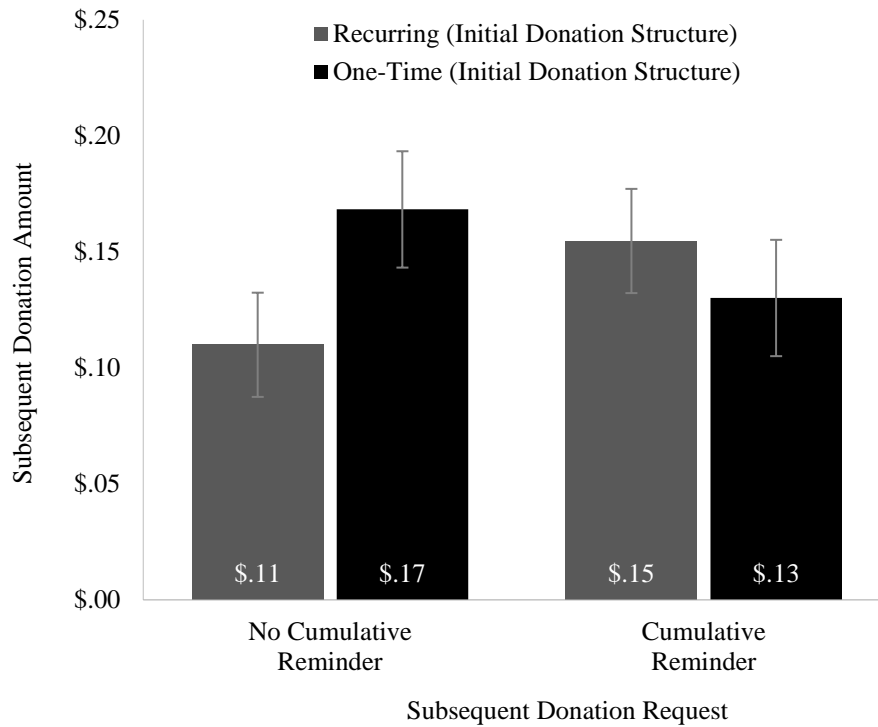
Figure 11 (Essay 2) Study 3: Impact of Displaying Cumulative Donation Amount on Subsequent Donation

Amount

3.5.3 Discussion

Study 3 shows that providing recurring donors with a larger anchor by reminding them of their cumulative contribution can mitigate the negative impact on subsequent charitable support. When no cumulative contribution reminder was presented, recurring donors gave 35.29% less than one-time donors, replicating the pattern of prior results. However, when presented with the cumulative contribution reminder at the time of the second donation request, recurring and one-time donors exhibited no difference in the size of their subsequent contributions. Consistent with the previous two studies, however, we found no evidence that the temporal structure of the initial charitable contribution influenced the likelihood of making a second donation. This suggests that

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initial donations made in a recurring format (vs. a one-time format) reduce the size, but not the likelihood, of a subsequent gift. Thus, this study not only further supports an anchoring account, it also identifies one possible method by which nonprofits can disrupt recurring donors' gift reduction in response to later charitable requests – reminding donors of the totality of their recent prior giving.

3.6 Study 4: Reduced Informational Value of Anchor

Study 4 sought to replicate the previous pattern of results in a different donation context (giving to religious congregation) and provide addition process evidence using a measurement-of-mediation design (Spencer et al. 2005). Unlike the prior studies, in which the cumulative initial donation amount was held constant across participants, participant-provided estimates of giving were used in Study 4 to calculate the total initial contribution amount. This allowed us to explore whether the objective size of the initial amount moderates the previously demonstrated effect of the temporal donation structure on the size of subsequent giving. Because the relative difference in perceived magnitude may be minimized with smaller charitable gifts, the reduced subsequent giving of recurring donors (vs. one-time donors) may attenuate when the total initial contribution amount is relatively small. To examine this possibility, Study 4 tests for the interactive effect of total initial donation amount and initial donation structure on the size of subsequent giving.

To test the proposed anchoring process, Study 4 examines whether recurring donors' reduced giving can be attenuated when individuals possess strong internal sources of alternate anchors. Prior research has demonstrated that susceptibility to anchoring effects diminishes when individuals engage in less inference-making, such as when the consumer is more knowledgeable

about the domain (Smith, Windschitl, and Bruchmann 2013) or when the consumption context provides less ambiguous information (Naylor, Lamberton, and Norton 2011). In such cases, individuals rely on internal anchors, making externally-provided anchors less informative. If anchoring is driving recurring donors' reduced giving, then we would expect to observe this effect weaken among those who derive little informational value from the anchor. In Study 4, which involves donating to a religious congregation, this would include those high in religiosity, whose giving is driven by internal norms and perceptions. Thus, Study 4 tests whether the previously described interactive effect of total initial donation amount and initial donation structure is conditional on religiosity, predicting that effects will be weaker among high-religiosity donors.

3.6.1 Method

Study 4 involved donating to a religious congregation. Historically, religious congregations have received the largest share of charitable giving in the U.S. (Giving USA 2017). Due to the nature of this scenario, the sample consisted only of individuals who reported giving money to a religious congregation. To determine eligibility, in April 2019 respondents first indicated their religious affiliation ("What is your present religion, if any? Are you Protestant, Roman Catholic, Mormon, Orthodox such as Greek or Russian Orthodox, Jewish, Muslim, Buddhist, Hindu, atheist, agnostic, something else, or nothing in particular?"); those who selected "atheist," "agnostic," or "nothing in particular" did not continue in this study. Respondents then estimated their monthly giving to their religious congregation ("Approximately how much do you give per month, on average, to your religious congregation (e.g., church, synagogue, or mosque)?"). Those who reported making no donation did not continue with this study.

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Eligible Mturk participants (n = 145;[18] 47.59% male; M_{age} = 41.21, SD = 13.55), compensated ($.50) for their participation, were randomly assigned to one of two temporal structure conditions: a one-time donation (*one-time*) or a recurring weekly donation (*recurring*). Using the participant's previously-provided monthly donation, we presented participants with a summary of their charitable giving over the past year estimated either in terms of the year (one-time condition) or the week (recurring condition): "This means you've donated about $[X] to your religious congregation [in total/each week] over the last year."

Participants then imagined attending a special congregation event in which a one-time offering is taken up to support a specific upcoming congregation project. We asked participants how much they would give (as a one-time donation amount) at this event. Along with this donation request, participants were reminded of their prior donation ("Given that you've donated $[X] to your religious congregation in total/each week] over the last year, how much would you donate at the congregation event?" Additional one-time donation amount $____; see Web Appendix N for survey and stimuli). We then assessed participants' religiosity using a 10-item index (Worthington et al. 2003) previously used to measure consumers' sense of chronic religiosity (Grewal, Wu, and Cutright 2019). Afterwards, participants indicated their household income and other basic demographic items.

3.6.2 Results

Prior to conducting primary analyses, we first tested whether the temporal structure of the initial donation had an unintended impact on self-reported religiosity. Results from a regression

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[18] This is the final sample included in analysis after removing those who identified as a non-native English speaker, who reported encountering technical problems, or indicated a subsequent donation amount greater than three standard deviations above the mean (n = 3).

analysis predicting religiosity as a function of initial donation structure (0 = one-time, 1 = recurring) controlling for size of initial donation amount and income, found no main effect of temporal structure (b = .110, SE = .166, t = .66, p = .508). Recurring donors (M = 3.511) and one-time donors (M = 3.401) reported similar levels of religiosity.
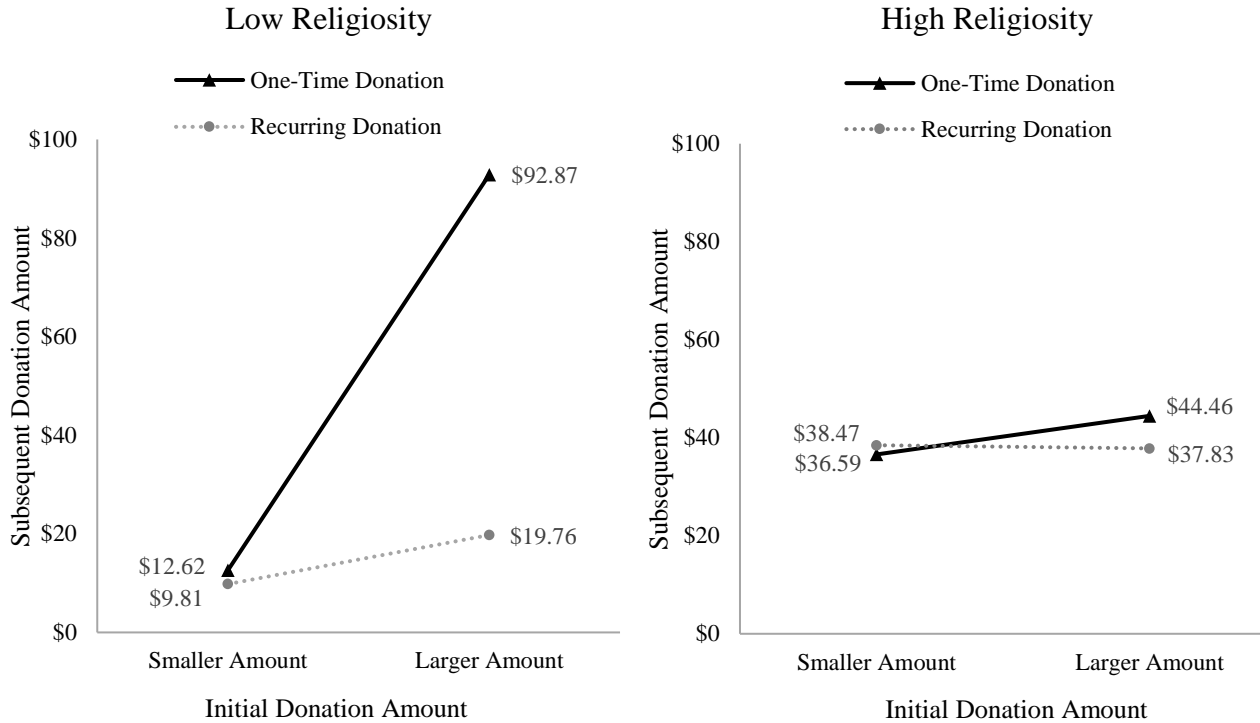
3.6.2.1 Subsequent Donation Amount

Using PROCESS 3.0 (model 3, 10,000 bootstrapped samples; Hayes 2018), we conducted a regression analysis predicting the subsequent donation amount as a function of the presented temporal structure of initial donation (0 = one-time, 1 = recurring), estimated monthly donation amount reported by participants, religiosity, and their interactive effects, while controlling for participant income. Results show a marginal main effect of the initial amount given (b = .049, SE = .029, t = 1.69, p = .093), a marginal main effect of donation structure (b = -13.067, SE = 7.309, t = -1.79, p = .076), and a marginal main effect of religiosity (b = 6.329, SE = 3.695, t = 1.71, p = .089). These results, however, are qualified by a significant three-way interaction between temporal structure of the initial donation, religiosity, and the size of the initial donation (b = .195, SE = .087, t = 2.24, p = .027; see Appendix H).

Spotlight analysis, using the 16th and 84th percentiles (Hayes 2018), indicates that the temporal structure x initial amount donated interaction is conditional on the donor's level of religiosity. For those low in religiosity, the two-way interaction between temporal structure and initial donation amount was significant and negative (b = -.554, p = .004; see Figure 12). As with prior studies, recurring donors gave less than one-time donors, and this difference increased with larger initial donation amounts. However, for donors high in religiosity, this interactive effect was not significant (b = -.067, p = .404), consistent with our prediction.

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3.6.2.2 Observed Likelihood of Making Subsequent Donation (Inferred from Amount)

In total, 93.79% of final sample (n = 136) made a subsequent donation. After recoding donation amount into a binary indicator (0 = didn't make subsequent donation, 1 = made subsequent donation), we conducted a binary logistic regression with temporal structure of initial donation (0 = one-time, 1 = recurring), estimated monthly donation amount reported by participants, religiosity, and their interactive effects as predictors, while controlling for participant income. The three-way interaction from the prior analysis did not emerge (b = .1243, SE = 19.295, t = .01, p = .995) and the set of two-way interactions did not converge. Results do, however, show a significant main effect of religiosity (b = 1.144, SE = .511, t = 2.24, p = .025), with more religious donors more likely to make a subsequent donation. In addition, we found a main effect of initial donation amount (b = -.010, SE = .003, t = -3.52, p < .001), suggesting larger initial contributions decrease the likelihood of making a subsequent donation. We observed no main effect of temporal structure of the initial donation (b = 1.054, SE = .905, t = 1.16, p = .244), with recurring donors (95.85%) and one-time donors (91.61%) equally likely to make a subsequent donation.

Low Religiosity

One-Time Donation
Recurring Donation

$100
$80
$60
$40
$20
$0

Subsequent Donation Amount

$92.87
$19.76
$12.62
$9.81

Smaller Amount   Larger Amount

Initial Donation Amount

High Religiosity

One-Time Donation
Recurring Donation

$100
$80
$60
$40
$20
$0

Subsequent Donation Amount

$44.46
$38.47
$36.59
$37.83

Smaller Amount   Larger Amount

Initial Donation Amount

Note. Conducted using PROCESS 3.0 (model 3; 10,000 bootstrapped samples), with 16th and 84th percentiles for spotlight analysis (Hayes 2018). Estimates represent predicted values, including covariate (income).

Figure 12 (Essay 2) Study 4: Interactive Effect of Religiosity, Initial Amount Given, and Temporal Structure

on Subsequent Amount Donated

3.6.3 Discussion

Study 4 shows that recurring donors give less than one-time donors to a greater degree as the total initial amount increases—an effect that only emerges among those lower in religiosity. No differences emerge between recurring and one-time donors, however, with respect to likelihood of making the second donation. These results are consistent with an anchoring account, which would predict the initial donation anchor to be less informative for higher religiosity donors. Thus, Study 4 replicates the anchoring effect observed in prior studies in an additional charitable giving context (i.e., giving to a religious congregation) using a different initial donation temporal structure manipulation (i.e., presenting annual contribution estimate in terms of a one-time gift or a recurring

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weekly gift). Moreover, Study 4 provides initial evidence that the observed anchoring effect may be more severe as the size of the initial donation anchor increases (i.e., the objective size of the initial contribution). This suggests that the temporal structure of a donor's initial contribution may more strongly impact the size of subsequent giving among the largest donors.

3.7 Studies 5A and 5B: Presenting Initial Contribution Temporal Structure Options

Previous experiments show that weakening the initial anchoring effect can subsequently reduce the degree to which recurring donors scale back their giving, such as when shifting attention to a more favorable anchor (Study 3) or when the informational value of the unfavorable anchor is low relative to internal norms (Study 4). It follows, therefore, that setting a more favorable anchor at the outset, such as by making initial donations in a one-time temporal format, should shortcut the effect on subsequent giving. Firms could do this, for example, by presenting the initial donation decision exclusively in terms of a one-time contribution (i.e., initial donors restricted to a single temporal structure) or jointly as both a one-time contribution and a recurring contribution (i.e., initial donors chose preferred initial temporal structure).

Although recommending a one-time temporal structure during the initial donation experience may effectively counteract recurring donors' reduced giving in a later donation request, it could also undermine total giving. Research on the "pennies-a-day" (PAD) pricing strategy (Gourville 1998, 2003) finds that temporally framing costs in a disaggregated format (e.g., daily expense vs. yearly expense) can increase consumer compliance, including compliance with donation solicitations. Individuals, therefore, may be more likely to make an initial donation to charity when the same total contribution amount is presented as a (temporally-dispersed) recurring

contribution versus a (temporally-aggregated) one-time contribution. If so, attempts to offset the reducing giving of recurring donors by recommending a one-time temporal structure when making the initial donation decision may have the unintended consequence of decreasing the rate of initial giving, raising questions about this intervention approach. Studies 5A and 5B test this PAD prediction and explore the possible net effects of an initial donation request's temporal structure.

We designed Studies 5A and 5B such that the decision of whether to make an initial donation is always voluntary, but the decision of how to temporally structure the initial donation is not. Using consequential giving paradigms, all individuals are presented with an initial charitable appeal and, later, a second donation request. In Study 5A, the initial donation request is presented either as a one-time contribution or a recurring contribution totaling the same cumulative amount, such that individuals are defaulted into a predetermined temporal structure and can only give in one way. In Study 5B, however, the initial donation request presents individuals with both a one-time contribution option and a recurring contribution option (totaling the same cumulative amount), such that individuals choose their preferred temporal structure.

Across both studies, we predict that when presented with an additional charitable appeal those donors who initially made a recurring contribution will give less that those donors who initially made a one-time contribution of the equivalent total amount, consistent with prior results. In addition, we also examine whether merely asking for an initial donation in a recurring format versus a one-time format impacts (a) the likelihood someone makes an initial charitable contribution and (b) the average cumulative total amount a person gives across both the initial and the subsequent donation requests. Taken together, these studies provide insight regarding how we can present the initial donation request to consumers in a way that may offset recurring donors' reduced giving, without undermining total giving. Based on the results, we conclude that attempts

to circumvent reduced subsequent giving by only providing a one-time donation option (to set a more favorable anchor at the outset) are not reliably effective. Charities wanting to intervene when requesting the initial donation, rather than when requesting the subsequent donation, may be better off jointly presenting both temporal structures to potential donors.

3.8 Study 5A: Present One Temporal Structure Option When Requesting Initial Donation

In addition to testing the primary prediction regarding recurring donors' reduced giving and exploring potential net effects, Study 5A additionally examines the implications of defaulting donors into a temporal structure for their initial contribution. As such, using a charitable giving paradigm in which both initial and subsequent donation decisions are voluntarily made, financially consequential, and temporally experienced, Study 5A tests both the robustness of the basic reduced giving effect demonstrated in previous studies and the viability of this possible intervention.

Study 5A also provides additional mediation testing of the possible psychological process underlying the observed anchoring effect. Results from Study 2 showed a mediating effect through perceived size of the initial contribution but not through either experienced happiness from the initial contribution or perceived commitment to the charity following the initial contribution. In Study 2, prior donors who initially gave in a one-time structure perceived their total charitable contribution as subjectively larger than those who gave in a recurring structure, shrinking subsequent support for recurring givers—results that are consistent with an anchoring account and, specifically, a magnitude priming mechanism. Using the same measures administered in Study 2, Study 5A examines whether the same pattern of results emerges when prior donors actively choose

and financially realize both initial and subsequent donation decisions. Consequently, Study 5A tests the robustness of these mediation findings.

In addition, Study 5A explores the possible moderating effect of mediator measurement timing. Study 2 measured mediators at the time of the second donation request, when a prior donor's psychological assessment of their initial contribution is argued to influence subsequent giving. One could argue, however, that measuring potential mediator variables closer in time to initial donation decision is more appropriate, given these mediators assess perceptions associated with the initial contribution. Although measurement at the time of the subsequent donation request is more theoretically consistent with our proposed process, Study 5A explores whether results depend on the timing of mediator measurement, with initial donors completing the three process measures either after the initial donation request or after the subsequent donation request.

3.8.1 Method

Similar to Study 2 and Study 3, the current study employed a two-part paradigm in which individuals responded to an initial donation request (included in an unrelated survey task) and then later respond to a subsequent donation request (included in an unrelated follow-up survey task). In this study, preregistered on OSF (https://osf.io/3kpr8) and conducted in February 2020, Mturk participants earned $1 for each survey completed (initial and follow-up), in addition to a base pay of $.25. To minimize attrition, participants began by reviewing the two-part task and payment structure and confirming their willingness to complete the follow-up survey. Only those indicating a willingness to complete the follow-up survey continued with the study.

Before starting an unrelated task, participants (n = 923;[19] 46.50% male;[20] M_{age} = 38.90, SD = 12.28)[21] learned of an opportunity to donate the $1 earned for completing the initial survey. We asked participants to make a donation to charity (see Web Appendix O for time 1 survey details). We presented the donation request as a binary choice (donate the equivalent of $1 or donate nothing), manipulating the temporal structure of donation option to represent either a one-time charitable gift or a recurring charitable gift. Whereas those randomly assigned to the one-time donation condition chose between "Yes, I will make a one-time donation of $1 today" and not donating, those randomly assigned to the recurring donation condition chose between "Yes, I will make a recurring daily donation of $.10 for ten days" and not donating. Those participants who chose to make an initial charitable contribution (n = 302;[22] 40.71% male;[23] M_{age} = 40.87, SD = 12.91)[24] then selected which of three charities would receive their donation (i.e., Habitat for Humanity, ASPCA, Feeding America) and the amount was deducted from their earnings. Donors reviewed a summary of their contribution and were thanked for their generosity. All participants then proceeded to an unrelated puzzle task and demographic items (e.g., income).

In this study, initial donors responded to items assessing potential process mechanisms. Specifically, donors completed the same three items from Study 2, indicating how happy they felt about their donation (1 – "Not at All Happy," 7 – "Very Happy"), how committed they felt towards the charity (1 – "Not at All Committed," 7 – "Very Committed"), and how large they perceived their donation to be ("How much did you donate?" 0 – "Very Little," 100 – "Very Much"), the order of which was randomized. Study 2 measured mediators at the time of the second donation

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[19] Number after employing exclusion criteria (reported technical problems or being non-native English speaker; n = 37).
[20] Participants could choose among three options: "male" (n = 405; 46.50%), "female" (n = 458; 52.58%), and "prefer not to say" (n = 8; .92%).
[21] Values reported reflect the subset of initial donors who completed demographic items (n = 871).
[22] Number included in analysis after removing those who reported technical problems or being non-native English speaker (n = 20).
[23] Participants could choose among three options: "male" (n = 114; 40.71%), "female" (n = 164; 58.57%), and "prefer not to say" (n = 2; .71%).
[24] Values reported reflect the subset of initial donors who completed demographic items (n = 280).

request, before responding to the additional charitable appeal. Unlike Study 2, however, Study 5A manipulated the timing of when these mediating were measured. Whereas some donors were randomly assigned to complete mediator items after the initial donation request (in the initial survey), other donors were randomly assigned to complete the mediator items after the subsequent donation request (in the follow-up survey). Thus, following the demographic items, those in the after initial donation request condition finished this survey by completing measures of experienced happiness, charity commitment, and perceived size of initial donation.

Twelve days later, all participants (both initial donors and initial non-donors) were invited via email to complete a follow-up survey, earning an additional $1 (see Web Appendix P for time 2 survey details). Prior to starting the unrelated follow-up survey task, however, participants responded to a new request for a one-time donation. Individuals who made an initial donation (n = 302)[25] were asked how much of their payment they would be willing to donate to the same charity that received their initial charitable contribution (US $, open response), which was deducted from their earnings. In total, 70.20% of initial donors (n = 212; 40.28% male;[26] M_{age} = 42.17, SD = 12.93)[27] completed the follow-up survey, representing our final analysis sample. Finally, all initial donors who were randomly assigned to complete mediator measures after the second donation request then responded to items assessing experienced happiness, charity commitment, and perceived size of their initial donation.[28]

Note that to explore the potential net effects of the temporal structure of an initial donation request we also offered those who did not initially give to charity the opportunity to make a one-time donation. These initial non-donors were asked how much of their payment for completing the

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[25] Number included in analysis after removing those who reported technical problems or being non-native English speaker (n = 20).
[26] Participants could choose among three options: "male" (n = 85; 40.28%), female" (n = 124; 58.77%), and "prefer not to say" (n = 2; .95%).
[27] Values reported reflect the subset of initial donors responding to the follow-up survey who completed demographic items (n = 211).
[28] Attrition did not vary by temporal structure of initial donation (b = -.273, SE = .255, t = -1.07, p = .285), with recurring donors (67.66%) just as likely as one-time donors (73.33%) to respond to the follow-up survey.

follow-up survey they would be willing to donate to charity (US $, open response). Those who opted to give selected a charity to receive their donation (i.e., Habitat for Humanity, ASPCA, or Feeding America), and the amount was deducted from their payment.

3.8.2 Results

We first test for the moderating effect of mediator measurement timing, when appropriate. Because not all participants completed demographic items, analysis sample is smaller when including (mean-centered) income as a control variable. However, exclusion of covariates does not substantively change observed pattern of results.

3.8.2.1 Subsequent Donation Amount

We first conducted a linear regression analysis predicting subsequent donation amount as a function of initial donation structure (0 = one-time donor, 1 = recurring donor) and timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request) and their interaction, controlling for (mean-centered) income. Results show no interaction between initial donation structure and timing of mediator measurement on subsequent amount donated (b = .010, SE = .100, t = .10, p = .923). Consequently, we control for timing of mediator measurement in the subsequent analysis.

When running a linear regression with subsequent donation amount as the dependent variable and initial donation structure (0 = one-time donor, 1 = recurring donor), (mean-centered) income, and timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request) as predictors, results show no main effect of mediator timing (b = .003, SE = .050, t = .07, p = .948). Donors who completed mediators after the first donation request

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(M = $.40) and donors who completed mediators after the second donation request (M = $.41) subsequently donated a similar amount. Results reveal, however, a significant negative main effect of initial donation structure (b = -.137, SE = .050, t = -2.76, p = .006). Recurring donors made a significantly smaller subsequent donation (M = $.34) than one-time donors (M = $.48), consistent with our prediction (see Figure 13).
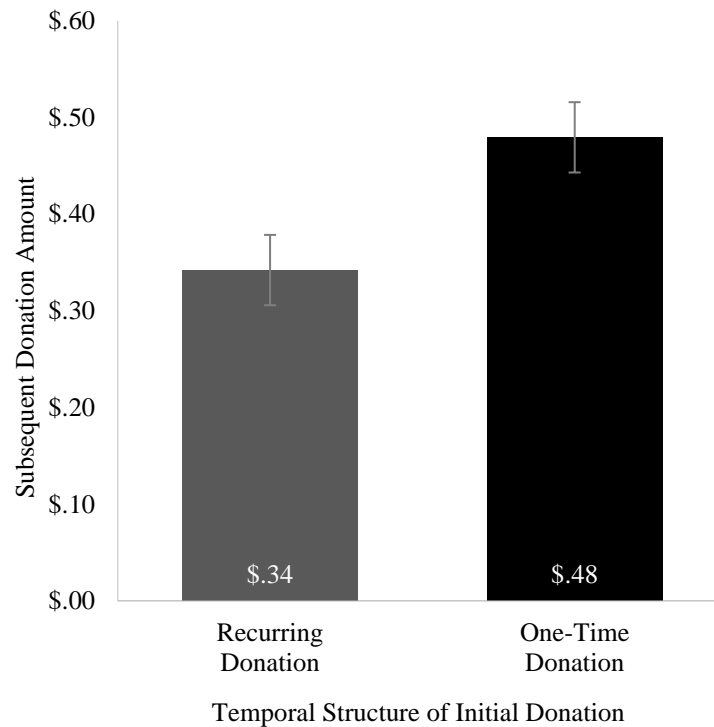
Note. Error bars represent standard errors. Estimates are adjusted for income and mediator timing.

Figure 13 (Essay 2) Study 5A: Subsequent Donation Amount

3.8.2.2 Perceived Donation Size

Results from a linear regression analysis predicting donors' perceived size of their initial contribution as a function of initial donation structure (0 = one-time donor, 1 = recurring donor), timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request), and their interaction, while controlling for (mean-centered) income, showed no

significant interaction effect (b = -2.054, SE = 9.325, t = -.22, p = .826). With no moderating influence, we control for timing of mediator measurement in the subsequent analysis.

We regressed perceived size of donors' initial donation on initial donation structure (0 = one-time donor, 1 = recurring donor), timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request), and (mean-centered) income. Results show a significant main effect for timing of mediator measurement (b = -9.989, SE = 4.623, t = -2.16, p = .032), such that donors perceived their initial contribution as larger when measured after the initial donation request (M = 39.080) compared to after the subsequent donation request (M = 29.091). No effect of initial donation structure emerged (b = 2.460, SE = 4.64, t = .53, p = .596). Contrary to predictions, recurring donors (M = 35.299) and one-time donors (M = 32.839) perceived their initial contribution to be similar in size.

3.8.2.3 Experienced Happiness

We conducted a linear regression analysis predicting happiness experienced from donors' initial contribution as a function of initial donation structure (0 = one-time donor, 1 = recurring donor), timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request), and their interaction, while controlling for (mean-centered) income. A marginal initial donation structure x timing of mediator measurement interaction emerged (b = -.617, SE = .348, t = -1.77, p = .078). Follow-up analyses indicate that when measured after the initial donation request, recurring donors reported experiencing greater happiness (M = 6.351) than one-time donors (M = 5.571; b = .675, SE = .243, t = 2.78, p = .006). However, when measuring happiness experienced after the second donation request, recurring donors (M = 5.629) and one-time donors (M = 5.571) did not differ in their reported happiness (b = .058, SE = .249, t = .24, p = .814).

3.8.2.4 Charity Commitment

Similar to the previous analyses, we first tested for an initial donation structure x timing of mediator measurement interaction. We regressed donors' reported commitment to the charity receiving their initial contribution on initial donation structure (0 = one-time donor, 1 = recurring donor), timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request), and their interaction, while controlling for (mean-centered) income. Results reveal no significant interactive effect (b = .310, SE = .577, t = .54, p = .591). Thus, we control for timing of mediator measurement in the subsequent analysis.

Running a linear regression analysis predicting donors' charity commitment as a function of initial donation structure (0 = one-time donor, 1 = recurring donor), timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request), and (mean-centered) income, we find a significant effect of timing of mediator measurement (b = -1.009, SE = .286, t = -3.52, p = .001). Donors reported greater commitment to the charity receiving their donation when assessed after the initial donation request (M = 7.165) compared to after the subsequent donation request (M = 6.157). A significant main effect also emerged for donation structure (b = .840, SE = .287, t = 2.92, p = .004), with recurring donors reporting greater charity commitment (M = 7.058) than one-time donors (M = 6.218).

3.8.2.5 Mediation

We first conducted a conditional process model using PROCESS 3.0 (Model 8; 10,000 bootstrap samples; Hayes 2018) to examine whether timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request) moderates the indirect effect of initial donation structure (0 = one-time donor, 1 = recurring donor) on subsequent donation amount through any of the potential mediators (i.e., perceived donation size, experienced happiness,

charity commitment), while controlling for (mean-centered) income. The index of moderated mediation was not significant through any of the mediators: perceived donation size (b = -.001, SE = .008, CI$_{95}$[-.019, .016]), experienced happiness (b = -.031, SE = .027, CI$_{95}$[-.098, .005]), and charity commitment (b = .005, SE = .013, CI$_{95}$[-.020, .036]). Thus, we controlled for timing of mediator measurement in the subsequent analysis.

We ran a parallel multiple mediator model using PROCESS 3.0 (Model 4; 10,000 bootstrap samples; Hayes 2018) to simultaneously examine the indirect effect of the initial donation's temporal structure (0 = one-time donor, 1 = recurring donor) on the amount of subsequent charitable support through each of the proposed mediators (i.e., perceived donation size, experienced happiness, charity commitment), controlling for (mean-centered) income and timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request). Consistent with prior results from Study 2, no indirect effects emerged through experienced happiness (b = .018, SE = .014, CI$_{95}$[-.003, .049]) or charity commitment (b = .013, SE = .014, CI$_{95}$[-.012, .044]; see Figure 14). Unlike prior results, however, no indirect effect emerged through perceived donation size (b = .001, SE = .004, CI$_{95}$[-.007, .011]; see Appendix I).
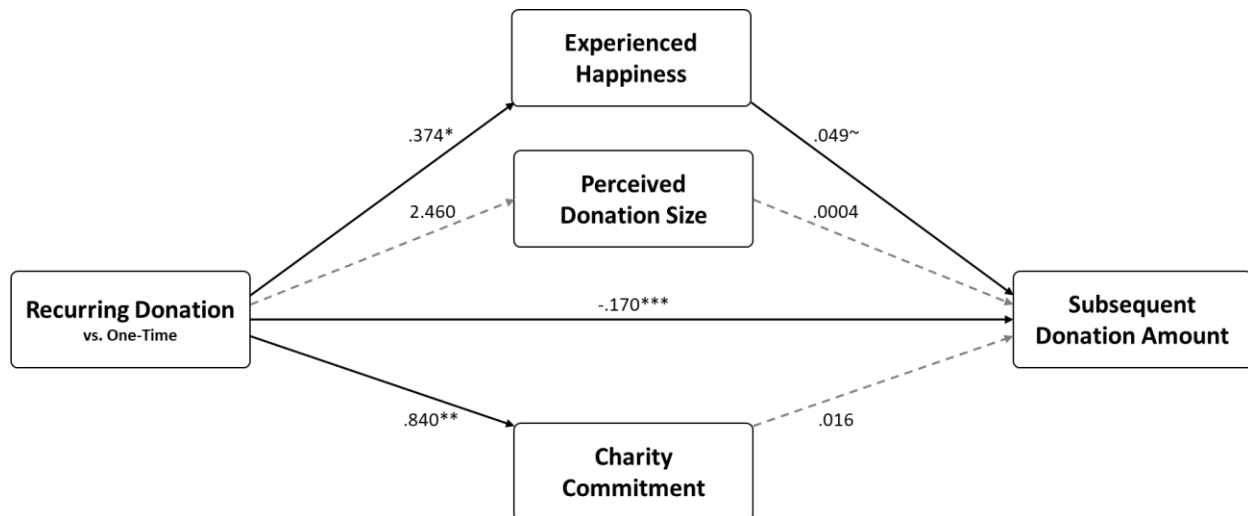
Experienced Happiness

.374*   .049~

Perceived Donation Size

2.460   .0004

Recurring Donation vs. One-Time   -.170***   **Subsequent Donation Amount**

.840**   .016

Charity Commitment

Note. Significance values are indicated as follows: ~ $p < .10$, *$p < .05$, **$p < .01$, ***$p < .001$. Conducted using PROCESS 3.0 (model 4; 10,000 bootstrapped samples), controlling for (mean-centered) income and timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request). No indirect effect emerged through any of the mediators: perceived donation size (b = .001, SE = .004, CI$_{95}$[-.007, .011], experienced happiness (b = .018, SE = .014, CI$_{95}$[-.003, .049]), and charity commitment (b = .013, SE = .014, CI$_{95}$[-.012, .044]).

Figure 14 (Essay 2) Study 5A: Parallel Mediation Analysis

3.8.2.6 Observed Likelihood of Making Subsequent Donation (Inferred from Amount)

To explore whether the temporal format of the initial donation impacted the likelihood of donors making a subsequent donation, we generated an indicator variable to identify whether initial donors made a subsequent donation (0 = size of subsequent donation equals \$0, 1 = size of subsequent donation exceeds \$0). Of the initial donors who responded to the follow-up survey, 82.08% (n = 174) made an additional donation.

We first conducted a binary logistic regression predicting likelihood of making a subsequent donation (1 = made subsequent donation, 0 = did not make subsequent donation) as a function of initial donation structure (0 = one-time donor, 1 = recurring donor), timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request), and their interaction, controlling for (mean-centered) income. Results show no interaction between initial donation structure and timing of mediator measurement on subsequent amount donated (b = .142,

SE = .743, t = .19, p = .848). Thus, we control for timing of mediator measurement in the subsequent analysis.

We then conducted a binary logistic regression analysis with the subsequent donation decision indicator (1 = made additional donation, 0 = did not make additional donation) as the dependent variable and initial donation structure (0 = one-time donor, 1 = recurring donor), timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request), and (mean-centered) income as predictors. No effect of timing of mediator measurement emerged (b = -.568, SE = .368, t = -1.54, p = .123), with donors completing the mediator items after the initial donation request (86.04%) similarly likely to give as those completing mediators after the subsequent donation request (77.83%). In addition, results reveal no effect of initial donation structure (b = -.019, SE = .365, t = -.05, p = .957), such that recurring donors (81.86%) and one-time donors (82.14%) were equally likely to make an additional donation.

3.8.2.7 Likelihood of Making Initial Donation

To examine whether the temporal format presented to potential donors shaped the likelihood of making an initial donation, this analysis includes all participants who were initially asked to donate (n = 923),[29] of which 32.72% (n = 302) chose to give. We conducted a binary logistic regression predicting likelihood to make initial donation (1 = made initial donation, 0 = did not make initial donation) as a function of the temporal structure presented with the donation request (0 = one-time donation, 1 = recurring donation), controlling for (mean-centered) income.[30] Given that only those who made an initial donation were asked to complete the mediator items, timing of mediator measurement was not included as a predictor in this analysis. Results show a

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[29] Number after employing exclusion criteria (reported technical problems or being non-native English speaker; n = 37).
[30] Mean-centering was re-computed for this analysis.

marginally significant positive effect of initial donation structure (b = .282, SE = .146, t = 1.94, p = .052). Consistent with the pennies-a-day effect, individuals were more marginally likely to donate when the initial donation request was presented as a recurring donation structure (35.25%) than as a one-time donation structure (29.11%).

3.8.2.8 Average Total Donation Amount

For all participants who were initially asked to donate (n = 923),[31] the net total donation was computed as the sum of the initial donation amount (equivalent to $1 or $0, depending on initial donation decision) and the subsequent donation amount (ranging from $0 to $1). To test the net effect of restricting individuals to the recurring or one-time temporal format, we conducted a regression analysis predicting net total donation amount as a function of the initial donation structure presented (0 = one-time donation, 1 = recurring donation), controlling for (mean-centered) income.[32] Similar to the previous analysis, timing of mediator measurement was not included because only initial donors were asked to complete mediator items.

No effect of initial donation structure emerged (b = .042, SE = .043, t = .98, p = .326), such that those initially presented with a recurring donation format option (M = $.46) gave a similar net amount as those initially presented with a one-time donation format option (M = $.42). This suggests that presenting an initial donation request in a way that forces those who donate to give in a one-time format decreased the average net donation amount $.04 per person, although this difference was not statistically significant.

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[31] Number after employing exclusion criteria (reported technical problems or being non-native English speaker; n = 37).
[32] Mean-centering was re-computed for this analysis.

3.8.3 Discussion

Study 5A finds that donors who initially gave a recurring contribution subsequently gave a smaller amount than donors who initially gave a one-time contribution, consistent with prior results suggesting an anchoring effect. Although temporal structure of the initial donation influenced the amount prior donors additionally gave to the same charity, the initial temporal structure did not influence the likelihood of prior donors making a subsequent donation. The current study, therefore, further demonstrates the robustness of the basic effect of reduced giving among recurring donors and provides additional empirical evidence consistent with the anchoring account.

In Study 5A, no mediating effects emerged through experienced happiness or charity commitment, as predicted. We did not, however, find anticipated mediation results through perceived size. Recurring and one-time donors perceived the subjective size of their initial contribution as similar in magnitude. This contrasts the results of Study 2, which found perceived size of initial donation mediating the influence of initial donation structure on subsequent donation amount. Moreover, Study 5A showed that timing of mediator measurement does not substantively impact results. Across all analyses, we generally observed no moderating influence of mediator measurement timing.

Both methodological and theoretical explanations may account for the inconsistent mediating effect of perceived size. Methodologically, Study 2 and Study 5A differ in several ways. Most notably, prior donors in Study 5A actively choose and financially realize both initial and subsequent donation decisions. Additional differences include increased frequency of recurring temporal structure (i.e., donation made every day vs. every other day), larger donation amount (i.e., donors give up to $1 vs. $.50 each time), and reversed sequencing of initial donation request

(appeal presented before vs. after unrelated task). Theoretically, the mixed evidence could also indicate that psychological mechanisms other than subjective evaluations of size may underly the observed anchoring effect. Multiple psychological processes have been used to explain anchoring effects, such as scale distortion theory (Frederick and Mochon 2012) and selective accessibility (Strack and Mussweiler 1997), among others. Thus, although all studies consistently show a pattern of reduced giving among recurring donors, suggesting consumers anchor on the salient numeric value associated with their initial contribution, it is possible the specific mechanism(s) driving this observed anchoring effect may be multiply determined.

Study 5A also examined the viability of restricting the initial donation option to a one-time giving structure as a way to offset the reducing giving of recurring donors. When presenting the initial donation request, charities can offer lump-sum giving in one of two ways: allowing one-time giving as the only donation option or allowing both temporal formats as options. As shown in Study 5A, the first method may reduce giving by undermining the "pennies-a-day" effect (Gourville 1998, 2003). Although we observed no adverse impact on average total funds raised per person, individuals presented with the recurring format (vs. one-time format) were more likely to make an initial donation, consistent with "pennies-a-day" predictions (Gourville 1998, 2003). Presenting an initial donation request that restricts gifts to a one-time giving structure, therefore, can potentially backfire for charities. Findings from the next study, however, suggest that the second method may be more promising.

3.9 Study 5B: Present Both Temporal Structure Options When Requesting Initial Donation

Whereas Study 5A considers the implications of limiting the temporal structure of initial giving to a single option, Study 5B considers the implications of offering both temporal structures as options for initial giving. In the current study, individuals choose their preferred temporal format when making their initial donation decision (i.e., a one-time gift or recurring gift totaling the same amount). The previous study suggests that requiring an initial donation be made as a lump-sum gift may have the unintended effect of reducing the rate of initial giving. Study 5B examines whether presenting both temporal structure options simultaneously similarly suppresses the likelihood of initial giving.

Furthermore, Study 5B explores the potential impact of the size of the initial donation request of potential donors. Results from Study 4 suggest that larger initial donations can enhance the previously observed anchoring effect of reduced giving among recurring donors. Study 5B examines the robustness of this result when the initial donation amount requested is randomly determined, rather than retrospectively self-reported by prior donors.

Unfortunately, Study 5B could not be completed as planned due to the COVID-19 pandemic. Although data collection ended prematurely (during the time between the initial and subsequent donation requests) and precluded observation of donors' subsequent donation behavior, we were still able to observe participants' initial donation decisions.

3.9.1 Method

Study 5B uses a two-part charitable appeal paradigm similar to that of Study 5A. People first respond to a survey containing an initial donation request and then later respond to a separate

follow-up survey containing a second donation request. Participants earn $1 for each survey completed in addition to a base pay of $.25 and donations are deducted from their payment.

In March 2020, participants first reviewed the task and payment structure, and only those indicating a willingness to complete the follow-up survey continued with the study. After completing an unrelated task and standard demographic items (e.g., income), participants (n = 1,055;[33] 43.03% male;[34] M_{age} = 39.56, SD = 12.74) were presented with the initial donation request asking them to donate to charity (see Web Appendix Q for survey details). Consistent with our preregistration (https://osf.io/h6cm5), people made their donation decision by choosing among three simultaneously presented options: making a one-time donation ("Yes, I will make a one-time donation of $X today"), making a recurring donation ("Yes, I will make a recurring daily donation of $X for ten days"), or making no donation ("No, I don't want to help"). Cumulatively, both the one-time and recurring donation options gave a financially equivalent total amount. The size of that amount, however, varied between participants. Participants were randomly assigned to view a charitable appeal requesting an initial total donation amount between $.10 and $1.00, presented in increments of $.10. The initial donation decision was consequential. For those who opted to give (n = 319;[35] 42.63% male;[36] M_{age} = 42.19, SD = 13.37), the amount was deducted from their earnings and given to their selected charity recipient (i.e., Habitat for Humanity, ASPCA, Feeding America). Donors were thanked for their generosity and all participants were reminded to complete the follow-up survey.

We planned to administer the follow-up survey twelve days after completing the initial survey, with participants first responding to an additional (one-time) donation request and then

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[33] Number after employing exclusion criteria (reported technical problems or being non-native English speaker; n = 49).
[34] Participants could choose among three options: "male" (n = 454; 43.03%), "female" (n = 591; 56.02%), and "prefer not to say" (n = 10; .95%).
[35] Number included in analysis after removing those who reported technical problems or being non-native English speaker (n = 16).
[36] Participants could choose among three options: "male" (n = 136; 42.63%), "female" (n = 180; 56.43%), and "prefer not to say" (n = 3; .94%).

completing the same mediation items from Study 5A. However, due to disruptions resulting from the COVID-19 pandemic the follow-up survey was not administered. Thus, neither the key dependent variable (i.e., the dollar amount prior donors give to the same charity in response to an additional one-time charitable appeal) nor process measures (i.e., experienced happiness, charity commitment, perceived donation size) were assessed. We could examine patterns of initial giving but could not test predictions regarding the impact of initial donation temporal structure on prior donors' subsequent donation amount.

3.9.2 Results

We conducted a multinomial logistic regression, with initial donation decision as a three-level categorial dependent variable (chose not to give, chose to make recurring donation, chose to make one-time donation). Making a recurring donation was used as the reference group. Initial donation amount ($.10, $.20, $.30, $.40, $.50, $.60, $.70, $.80, $.90, $1.0; coded as a continuous predictor) was included as the primary predictor, controlling for (mean-centered) income.

The fully estimated model was significant ($\chi^2(4) = 49.76$, $p < .0001$, pseudo $R^2 = .032$). Examination of the intercept effect ($\chi^2(2) = 46.39$, $p < .0001$) suggests that individuals were more likely to not donate than to make a recurring donation (b = 2.093, SE = .308, t = 6.79, $p < .001$, OR = 8.108) and were more likely to make a one-time donation than to make a recurring donation (b = 2.005, SE = .318, t = 6.31, $p < .001$, OR = 7.425). The analysis additionally revealed a main effect of initial donation amount requested ($\chi^2(2) = 46.67$, $p < .0001$). Increasing the size of the initial donation request increased the probability of choosing not to donate compared to making a recurring initial donation (b = 1.260, SE = .541, t = 2.33, $p = .020$, OR = 3.527). However, increasing the size of the initial donation request did not impact the probability making a one-time

initial donation compared to a recurring initial donation (b = -.474, SE = .567, t = -.84, *p* = .404, OR = .622; see Figure 15).
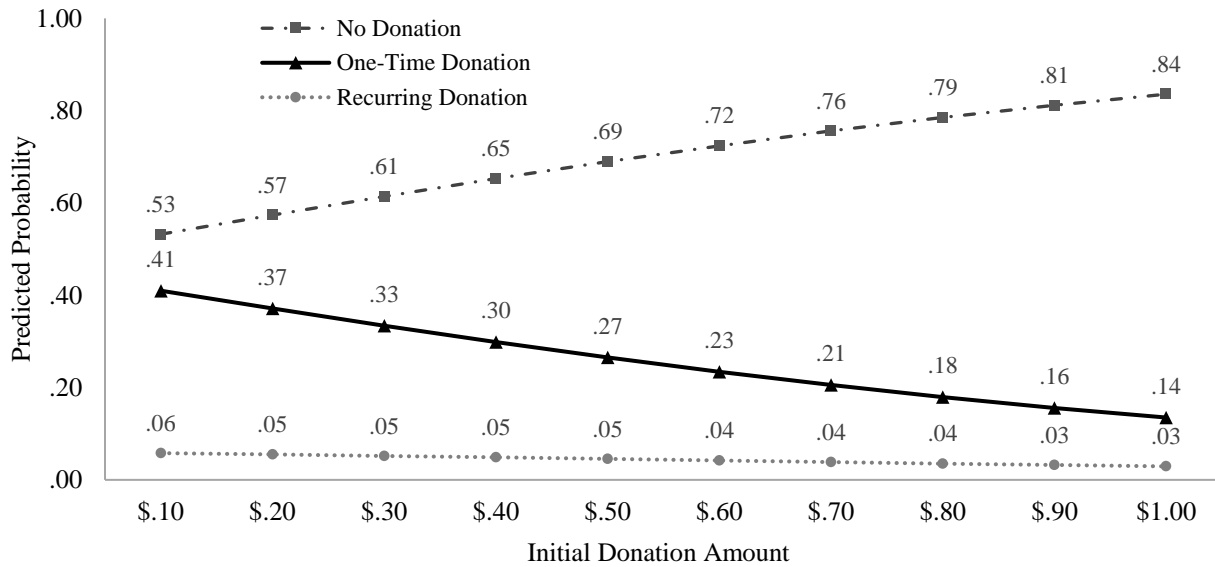
Note. Estimates represent predicted values, including covariate (income).

Figure 15 (Essay 2) Study 5B: Initial Donation Decision

3.9.3 Discussion

Study 5B examined implication of presenting both a recurring donation structure and a one-time donation structure simultaneously when making the initial donation decision. Although we could not test our primary predictions regarding additional giving due to disruptions resulting from the COVID-19 pandemic, observations of individuals' initial donation decisions allowed us to explore the impact of jointly presenting a one-time and recurring temporal structure on initial donation likelihood.

Results show people were more likely to opt to give in a one-time format than a recurring format and that larger donation requests, despite generally increasing the likelihood of not making

an initial donation, did not impact the likelihood of selecting a one-time versus a recurring structure. Not only is this finding inconsistent with the traditional PAD effect, which would predict increased donation likelihood for temporally-dispersed structures, it also suggests that jointly displaying both temporal structures during an initial donation request has the potential to decrease the likelihood of donors choosing a recurring format, which our prior studies have shown can reduce subsequent giving. Thus, although restricting the temporal structure of the initial donation may not be a reliably effective intervention for offsetting recurring donors' smaller subsequent giving (Study 5A), charities that desire to employ interventions that target the initial donation experience may be more successful at encouraging one-time donations by jointly presenting both temporal structures.

Because the follow-up portion of Study 5B was not conducted and subsequent donation decisions were not made, we could not directly test predictions regarding the impact of initial donation structure on prior donors' subsequent donation amount. Having replicated recurring donors' reduced subsequent giving in prior studies, however, allows us to use previous effects to estimate possible net donation implications in the current context. We computed the net total donation as the sum of the initial donation amount and the additional donation amount, which was estimated using covariate-adjusted average values obtained in Study 5A (which, of all studies conducted in this paper, employs a donation paradigm most similar to Study 5B with respect to the voluntary and financially consequential nature of initial donation decisions). Results using the approximated net total amount found that initial donors who chose the one-time temporal format gave a larger net amount, on average, than initial donors who chose the recurring temporal format (see Web Appendix R for analysis details). Although additional experimental investigation is needed, this estimation suggests that presenting an initial donation request in a way that allows

initial donors to choose a lump-sum gift as their preferred temporal format may boost the average total amount of charitable funds raised per person.

3.10 General Discussion

Most of the research on charitable giving has focused on what spurs initial contributions. However, charities are continuously challenged by high donor and gift attrition rates (Levis et al. 2019), making it critical to understand not only what motivates consumers' donation behavior, but also what sustains it. In the current paper, we consider how marketers can shape consumers' initial donation experience to increase the magnitude of future charitable gifts, examining whether structuring one's initial contribution as a one-time donation (e.g., $120 single gift) or a recurring donation (e.g., $10 monthly gift for 12 months) is superior in promoting future charitable support. Drawing on prior anchoring research, we predicted that temporally structuring an initial donation experience as a series of recurring gifts (vs. a one-time lump-sum gift) results in a smaller subsequent donation. We argue that consumers anchor on the salient numeric value associated with their initial donation, which subsequently skews the size of their next donation.

The current research demonstrates that the temporal structure of consumers' initial charitable contributions, despite being financially equivalent, can shape donation perceptions in ways that impact future donation support. Across six studies, we find that recurring donations reduce the size of later giving, consistent with an anchoring account. Specifically, when prior donors encounter a later one-time charitable request, those who made their initial contribution as a recurring gift subsequently gave a smaller amount than those who made their initial contribution as a single lump-sum gift. This result replicated with both consequential (Study 2, Study 3, Study

5A) and hypothetical (Study 1, Study 4) donations, with different types of charitable giving (e.g., nonprofit organizations, religious congregations), with varied initial donation amounts (e.g., $.50, $30), and with different donation timeframes (e.g., 12 days, 6 months).

This pattern of reduced giving among recurring donors is most consistent with an anchoring explanation. Several studies explored the specific psychological processes underlying the observed anchoring effect. Study 2 and Study 5A examined whether temporal structure impacts subsequent giving by skewing the perceived size of donors' initial charitable contributions. In Study 2, but not Study 5A, recurring donors perceived their initial contribution as subjectively smaller than one-time donors, resulting in smaller subsequent giving. Although these studies offer mixed mediation evidence regarding distorted perceptions of subjective size, both rule out competing psychological accounts, finding no indirect effect through either experienced happiness or charity commitment.

Study 3 and Study 4 offer additional processes evidence, testing whether factors that weaken the effects of an anchor subsequently reduce the degree to which recurring donors scale back their giving. In Study 3, when asked to make an additional donation, recurring donors provided with a new (larger) numeric anchor equivalent to that of lump-sum donors subsequently gave a similar amount. In Study 4, recurring donors' reduced subsequent giving to their religious congregation was attenuated when the anchor provided less informational value (i.e., for those higher in religiosity).

Studies 5A and 5B explored the implications of recommending a one-time temporal structure when the initial donation decision is made, to circumvent later reduced giving among recurring donors by setting a more favorable anchor at the outset. Results from Study 5A find that presenting an initial donation request in a way that compels donors to make a one-time gift increases the size of their subsequent gift, consistent with the anchoring account. However,

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limiting initial giving to a one-time format has the potential to decrease the likelihood of making an initial donation to charity (Study 5A). Thus, restricting the temporal structure of the initial donation may not be a reliably effective intervention for offsetting recurring donors' smaller subsequent giving. Study 5B suggests, however, that charities that nevertheless desire to target the initial donation request may be more successful at encouraging one-time donations by simultaneously presenting both temporal structures.

The observed anchoring effect, however, seems to be limited to the size of prior donors' subsequent giving. Across all relevant studies, we find that although temporal structure of the initial donation influenced the amount prior donors additionally gave to the same charity, the temporal structure did not influence the likelihood of prior donors making an additional donation. Thus, compared to one-time lump-sum donors, recurring donors subsequently give less but are no less likely to give (see Table 10 for results summary).

Table 10 (Essay 2) Summary of Results for Donation Amount and Donation Likelihood

Study	Initial Donation		Subsequent Donation	
	Amount	Likelihood	Amount	Likelihood
1	–	–	Recurring < One-Time	ns
2	–	–	Recurring (no notification) < One-Time Recurring (with notification) < One-Time	ns
3	–	–	[No Cumulative Reminder] Recurring < One-Time [Cumulative Reminder] ns	ns
4	ns*	–	[Low Religiosity] Recurring < One-Time [High Religiosity] ns	ns
5A	–	Recurring > One-Time	Recurring < One-Time	ns
5B	ns	Recurring < One-Time	–	–

Note. Likelihood of making subsequent donation was inferred from subsequent amount donated, generating an indicator variable as the (0 = size of subsequent donation equals \$0, 1 = size of subsequent donation exceeds \$0).
* No difference was expected, as temporal structure manipulation had not yet occurred (i.e., result suggests balanced giving between conditions).

This paper also tests different methods by which charities can disrupt recurring donors' gift reduction in response to later charitable requests. When appealing to prior donors for a new one-time contribution, including a reminder of their cumulative contribution to date may offer charities a low-cost, scalable intervention to effectively offset reducing giving (Study 3). Attempts to set a more favorable anchor at the outset by only allowing initial donors the lump-sum format option, however, have the potential to decrease the likelihood of making an initial donation (Study 5A).

3.10.1 Theoretical Implications

The present research extends prior work on charitable giving in several ways. Our findings demonstrate that the "time architecture" of a consumer's initial donation experience impacts subsequent donation behavior. In doing so, we not only identify a previously unrecognized aspect of charitable giving that can influence long-term donor support, but also highlight how the structure of early donation experiences can shape later patterns of giving. In addition, this research also contributes to the charitable giving literature by extending our understanding of factors contributing to donor and donation attrition. To date, charitable giving research has largely focused on factors motivating consumers' initial donation decisions. Despite the considerable insights gained from such investigations, there remains a limited understanding of when and why donors scale back and abandon their contributions. The present paper highlights the role of initial donation experiences (and, specifically, their temporal structure) in perpetuating donor support.

This focus on the continuation, not the initiation, of charitable giving differentiates the current work from similar research exploring the "pennies-a-day" (PAD) effect (e.g., Gourville 1998, 2003). Studies of the PAD effect in the domain of charitable giving focus exclusively on the initial instantiation of donation behavior, whereas the present research investigates the

consequences of that initial donation behavior. Furthermore, the PAD studies examine a framing effect, namely how successful disaggregated pricing formats are in persuading consumers to make a donation; they do not involve effects associated with making payments in disaggregated pricing structures. Conversely, the current research studies the impact of experiencing different donation payment structures. Thus, the current research compliments prior PAD findings by demonstrating the downstream consequences of making initial charitable contributions in PAD-type segregated donation structures.

More broadly, these results also contribute to the growing literature on information architecture and the subjective value of money. Research is increasingly demonstrating that the way in which money is presented can distort its perceived value. For example, consumers exhibit a "bias for the whole" and attach greater value to money in the form of a whole (e.g., $100 bill) versus parts (e.g., ten $10 bills; Mishra et al. 2006). In addition, consumer spending is influenced by the "denomination effect," wherein consumers are less likely to spend an equivalent sum of money represented in larger denominations (Raghubir and Srivastava 2002). Recent work has also shown that consumers' perceptions of the adequacy of their retirement savings and, consequently, subsequent saving decisions are influenced by the mode in which their savings are presented (lump sum or monthly amount; Goldstein et al. 2016). We extend this research into the domain of charitable giving, showing that the format in which a person's prior donation is structured shapes the size of future support. Thus, although we limit the current research to the context of charitable giving, the effects demonstrated could have broader implications for how consumers manage their spending and saving decisions.

3.10.2 Managerial Implications

Charitable organizations generally regard recurring donors as more valuable than one-time donors. Benefits include more cost-effective and more predictable fundraising, as well as the possibility for a larger lifetime return (especially when donors forget to cancel the renewal of their recurring gift). The current research does not dismiss the potential benefits of recurring giving nor recommend that charities eliminate recurring donations. Rather, the current research highlights a potential downside of a recurring temporal structure—reduced future support—and illustrates the importance of systematically understanding donor behavior beyond initial giving. Although some research suggests that displaying costs in a disaggregated format can increase compliance with initial charitable requests (e.g., Gourville 1998, 2003), we demonstrate that actually making such disaggregated payments can negatively impact subsequent donation behavior. This presents a curious possibility for charities: that recurring donations may have the ironic effect of increasing the rate of initial donations and then decreasing the magnitude of future charitable contributions. Results from Study 5B offer initial support for this possibility. This study finds that presenting an initial donation request in a way that compels donors to make a lump-sum gift not only increases the size of donors' subsequent donations, it can also reduce likelihood of making an initial donation to charity.

The current research suggests that charities may want to 1) reconsider the blanket strategy of encouraging all donors to make recurring gifts and 2) utilize different methods when appealing for additional donations from prior recurring givers. In the current research, we identify a very simple and costless adjustment to donation appeals may help charities boost giving of existing recurring donors. Because recurring donors anchor on the comparatively smaller numeric value of associated with their initial contribution, relative to one-time donors, recurring donors

subsequently give less when faced with a subsequent donation request from the same charity. Reminding prior donors of the cumulative amount they have given to date, however, effectively eliminates the negative impact on subsequent charitable support by providing recurring donors with a new and more favorable (i.e., larger) anchor.

Such interventions and individualized marketing strategies will likely become increasingly important as online giving continues to rise. Because consumers are typically encouraged to become recurring donors when making online donations, understanding the implications of donation structures on subsequent support is increasingly relevant with the growing proliferation of online charitable giving. Online giving has repeatedly outpaced the growth of overall charitable giving for several years. In 2019, for example, online giving accounted for 8.7% of all fundraising, representing a 6.8% growth in online giving (MacLaughlin et al. 2020). By comparison, overall giving grew by just 1.0% in 2019 (MacLaughlin et al. 2020).

3.10.3 Limitations and Future Research

The current paper focuses on an under-researched aspect of the donor lifecycle–what influences continued charitable support. Although our findings extend understanding of the potential downstream effects of initial charitable giving, we do not examine how long these effects last or whether they depend on how the initial charitable appeal was made. As we have suggested, temporally dispersed donations may initially increase the likelihood that a person gives to charity but may subsequently prompt a scaling back of charitable support the next time the charity requests a donation. Therefore, to better understand the net-effect of one-time versus recurring initial contributions, we recommend that future research simultaneously consider how donation structure influences both the initiation *and* continuation of charitable contributions.

Another limitation of the current paper that represents an opportunity for future research involves the initial donation decision. Because we wanted to empirically demonstrate the implications of the structure of the initial donation on subsequent donor support, we enacted greater methodological control regarding the initial contribution (e.g., holding the amount constant, randomly assigning people to a donation structure). However, it is possible that the reasons for selecting a particular temporal structure (one-time vs. recurring) may moderate our observed effects. For example, people may purposefully choose to make recurring monthly donations because a lump sum payment is not financially feasible or because they want to strategically manage the "warm glow" they experience from giving, consistent with prior work on hedonic editing (Thaler 1985). The current research does not address the granular impact of such reasons. Future research might explore the moderating influence of the motivations driving donors' selection of a particular temporal structure.

In addition, the current research exclusively examines the effect of initial donation structure on subsequent contributions to the same charity. Future research could test whether these effects extend to donation requests from other charities (that did not receive the initial donation). Our findings indicate that anchoring accounts for the negative impact of recurring donations on future support. If the anchoring is priming magnitude among recurring donors, then as long as that construct is activated it may reduce donations for charities other than the one initially supported. Future research might explore this idea and, more broadly, the scope of anchoring effects observed in the current research.

4.0 (Essay 3) Time's Out: Examining the Effectiveness of Contemporary Online Time Scarcity Promotions

Consistent with Cialdini's (1984) canonical argument that "when our freedom to have something is limited, the item becomes less available, and we experience an increased desire for it" (188), time scarcity marketing tactics are a mainstay in promotional advertising. Historical content analyses have revealed that "time-oriented appeals have been used as primary appeals with generally increasing frequency since the late 1800s" (Gross and Sheth 1989, 79) and that "scarcity appeals are clearly dominated by the use of the limited time technique" (Howard, Shu, and Kerin 2007, 23), with over one in six offline retail advertisements containing time scarcity promotions (Howard et al. 2007). Widespread use of offline time scarcity promotions among marketing practitioners has garnered increased empirical attention among marketing scholars, creating a related body of work exploring the nuances of these offline tactics (e.g., Abendroth and Diehl 2006; Brannon and Brock 2001; Kristofferson et al. 2017). Since the exponential growth of ecommerce, however, time scarcity promotions have additionally proliferated within online retailing and digital marketing, often in the form of flash sales and exploding deals (e.g., "HURRY! 40% off FLASH SALE ending in: 06 hours: 38 minutes: 52 seconds"), recurring online daily deals (e.g., Amazon's "Gold Box Deal of the Day," Best Buy's "Daily Doorbuster Deals"), online-exclusive offers (e.g., Cyber Monday, Prime Day), and entire websites focused on such promotions (e.g., Groupon.com, 1sale.com, GILT.com). Simply applying marketing's past theory and findings about offline time scarcity promotions to the current online retail context, one would predict contemporary online time scarcity tactics to increase consumers valuation of the promoted product.

The foundational demonstrations of time scarcity's effectiveness, however, were largely conducted prior to the rise of online retailing. When Cialdini's book *Influence*, which popularized the use of time scarcity promotions, was published in 1984, all consumer shopping was offline: the World Wide Web had not publicly launched and only 8% of U.S. households had a computer (U.S. Bureau of the Census 1988). The first completely online transaction did not occur until 1994 Fessenden 2015; Lewis 1994) and large-scale ecommerce adoption took even longer. Modern-day offline retailing is almost unrecognizable when compared to offline retailing of the past, especially with consumers often interacting with multiple retail channels before purchase occurs. Should we expect that prior time scarcity results in offline retail contexts will generalize to a fundamentally different marketplace? Firms, themselves, are questioning the use of such tactics. For example, Disney recently scrapped its iconic "Disney Vault," which only allowed consumers to purchase a film within a limited timeframe before being 'locked away' for years. After years of generating "scarcely scarce scarcity" (Felton 2011) following the growth of online retailing and digital delivery, the vault is being dismantled and made continuously available via Disney's new "Disney+" streaming service (Rahmanan 2019). In 2010, Marvel Comics similarly abandoned a vault-style online time scarcity promotion on its digital app platform after users refused to accept the marketing practice (Felton 2011), expressing that "in terms of digital media, scarcity is a drawback, not a selling point" (Brothers 2010).

This paper broadly examines the use and effectiveness of time scarcity tactics as they appear in contemporary online retail context, exploring the degree to which present-day instantiations of online time scarcity promotions (e.g., flash sales) theoretically and empirically operate in ways consistent with predictions originating from prior work that predominantly consists of offline time scarcity tactics (e.g., newspaper ads) and that historically predates modern

online retailing. Therefore, the focus of this paper is not on comparing the relative efficacy of contemporary online tactics versus contemporary offline tactics, nor is the focus on testing the reproducibility of prior offline time scarcity promotion experiments. Rather, the paper focuses on comparing widely accepted expectations regarding online time scarcity promotions to modern marketplace realities, asking whether there are reasons to question such assumptions.

Defining online time scarcity promotions as *marketing tactics used in the online environment designed to impose salient temporal restrictions explicitly related to the promotional offer's acquisition availability*, we undertake this exploration in a four-part process. First, we examine the assumptions underlying offline time scarcity theory in the face of market changes resulting from the internet that have occurred since foundational work in this area was developed— namely, shifts in (1) the *contexts* in which time scarcity effects are desired (e.g., from "bricks" to "clicks"), (2) the *methods* by which time scarcity is intended to be implemented (e.g., from static timeframes to dynamic timers), and (3) the *consumer experience* of target individuals (e.g., from non-existent to proficient understanding of online time scarcity tactics; from limited to excessive information; and from onerous to trivial search costs). Although it is impossible to construct a present-day test of online time scarcity tactics that entirely excludes these factors (or to reverse time to obtain completely naïve consumers), these changes raise questions about the comprehensive applicability of previously established assumptions about time scarcity promotions for the present online environment.

Second, we conduct a retrospective analysis of past marketing research that most closely matches our focal phenomenon. We find that, empirically, prior work on offline time scarcity promotions tells conflicting stories, depicting positive, conditional, and null effects. Typically, meta-analysis can help resolve such inconsistencies. However, not only do several study

characteristics of past work preclude meta-analytic comparison, the vast majority examine offline time scarcity promotions that are explicitly designed for non-digital distribution and print media, such as newspaper inserts (e.g., Howard and Kerin 2006), physical coupons (e.g., Cheema and Patrick 2008), flyers (e.g., Inman, Peter, and Raghubir 1997), and print ads (e.g., Aggarwal, Jun, and Huh 2011; McQuilken et al. 2015). Only in the last decade have a handful of studies expressly investigated online time scarcity promotions (e.g., Coulter and Roggeveen 2012; Eisenbeiss et al. 2015), although most have focused on daily deal websites that have cooled in popularity (Baker 2013; Sheppard 2017) and, by their nature, lack a control condition where no time scarcity is present. Thus, we conclude that direct examination of time scarcity appeals in their modern online form remains too sparse to offer generalizable recommendations.

Consequently, we conduct new empirical tests of online time scarcity promotions. Twenty-six empirical studies with over 35,000 participants examine whether contemporary online time scarcity promotions increase product valuation in ways consistent with prior offline theory and findings. This portfolio of studies was designed to reflect the range of contemporary online time scarcity promotions, using a variety of different product types (e.g., tablet computer, air purifier, information access, snacks), price points (e.g., average market value ranging from approximately $5 to $3,500), digital domains (e.g., online retail websites, email marketing messages, social media) and implementation formats (e.g., dynamic countdown timers, which represent the prototypical modern instantiation of online time scarcity tactics). We also employ multiple indicators to assess consumer value, such as monetary and subjective willingness to pay (WTP), self-reported purchase intentions, actual digital behavioral engagement, perceived product desirability, and purchase decisions. Moreover, individual studies capture several other consumer perceptions (e.g., persuasion knowledge activation, attitude toward the retailer, website

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perceptions) or manipulate specific factors (e.g., risk reduction, system 1 vs. system 2 mindset, elaboration) to explore potential explanations for patterns observed in the data.

Finally, we synthesize our compendium of new empirical findings. Results from both single-study analyses and single-paper meta-analyses (McShane and Böckenholt 2017) demonstrate that, despite a few single-study instances where positive effects emerge for select measures, contemporary online time scarcity promotions more often have null or negative effects on marketing-relevant measures that would be presumed to benefit if past offline theory held. Single-paper meta-analytic results also show that plausible alternative explanations for these findings, such as persuasion knowledge activation and negative attitudes toward the retailer, do not fully account for our results. Although much remains to be learned, this evidence suggests that contemporary online time scarcity tactics are not as universally effective as previous offline time scarcity research and pervasive marketplace usage may suggest, thus providing an empirically-grounded call for new theory development in this area.

This article represents a novel approach to revisiting past theory that contributes to a new understanding of the current consumer and marketplace. First, this article provides new empirical evidence that directly tests online time scarcity promotions. Findings indicate that contemporary online time scarcity promotions can negligibly or adversely influence valuation of goods or opportunities. These results suggest weakened relevance of previous offline time scarcity theory and findings for contemporary online marketing and calls for renewed consideration of the translation of offline theory to online contexts.

A second contribution concerns the paper's general approach. This article is not intended to be a literature review nor a meta-analysis of prior work. Rather, we model a generalizable paradigm for considering the relevance of past marketing theory to current marketing contexts.

The framework, which we refer to as the *ARTS* paradigm (*Assumption, Retrospection, Testing, Synthesis*), involves evaluating the appropriateness of prior theory's assumptions in the present market (*Assumption*), analyzing the relevance of past research (*Retrospection*), conducting new empirical tests for the present phenomenon (*Testing*), and synthesizing new findings using aggregating methodology (*Synthesis*). Thus, the *ARTS* process offers a roadmap for determining whether given theories or effects are robust across temporal and market changes, particularly when the permanency and prevalence of such changes renders perfect duplication of prior circumstances impossible and, consequently, untestable. Moreover, the *ARTS* framework provides a systematic way to theoretically and empirically reexamine established marketing phenomena, without casting aspersion on the rigor or credibility of past work.

This article additionally responds to repeated calls for a means to publish rigorously obtained null results (Abadie 2018; Angrist et al. 2017; Doshi et al. 2013; Landis et al. 2014; Rothstein and Bushman 2012; van Assen et al. 2014). Although, historically, the selective reporting of findings and aversion to null effects has been pervasive (e.g., Bakker, van Dijk, and Wicherts 2012; Fanelli 2012; Ferguson and Heene 2012; Franco, Malhotra, and Simonovits 2014; Giner-Sorolla 2012; Ioannidis et al. 2014; Ziliak and McCloskey 2008), the publication of null results has become increasingly common and important in a wide-range of basic and applied fields, such as education (e.g., Angrist et al. 2017; Oreopoulos et al. 2018), economics (e.g., Bhattacharya, Garber, and Goldhaber-Fiebert 2015; Bronchetti et al. 2011; Camerer et al. 2016; Carrera et al. 2018), and oncology (e.g., Begley and Ellis 2012). By transparently presenting our observed null effects, this work informs the direction of future research and industry practices, calling for critical re-evaluation and ongoing testing of online time scarcity promotions. Consequently, this article is intended to stimulate work that generates new theory regarding online time scarcity promotions

and identifies whether specific circumstances exist under which these tactics are robustly effective. We discuss ideas toward this end in the General Discussion.

4.1 Assumptions: Are the Assumptions Underlying Past Research Appropriate for the Contemporary Marketplace?

Cialdini's (1984) publication of *Influence* popularized scarcity as powerful means of consumer influence. Cialdini suggested that consumers use scarcity as a heuristic cue for inferring value. In addition to proposing the effectiveness of "limited-number" tactics that place quantity restrictions on an offer, Cialdini also speculated on the efficacy of "deadline" tactics that place time limits on an offer, noting that "people frequently find themselves doing what they wouldn't particularly care to do simply because the time to do so is shrinking" (Cialdini 1984, 181). Despite relying primarily on anecdotal examples (e.g., the "Great *Poseidon* Auction" among TV networks, his brother's college car sales, and his desire to tour a Mormon temple) and on previously existing empirical evidence of a basic quantity-based scarcity effect (e.g., Worchel, Lee, and Adewole 1975, cookie studies), the book contributed to the widespread use of time scarcity promotions among marketing practitioners and increased empirical attention among marketing scholars.

To support this assertion, we must adopt two major assumptions. First, it is necessary to assume that time scarcity promotions increase the perceived value of the promoted product. This assumption originates from classic research seeking to understand the influence of scarcity on perceived value, starting with commodity theory (Brock 1968). Commodity theory proposed that commodities are perceived to be more valuable as their availability decreases (Brock 1968). Meta-analytic results generally support commodity theory's claims with offline quantity-based

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restriction (Lynn 1991), yet reversals have been noted (e.g., Shippee, Mowen and Gregory 1981). Although researchers debate the mechanisms underlying the association between scarcity and increased value (e.g., need for uniqueness, Fromkin and Snyder 1980; reactance, Brehm 1966; Clee and Wicklund 1980; Worchel et al. 1975; lay theories, Lynn 1992; value inferences, Cialdini 1984; Ditto and Jemmott 1989; elaborative processing, Brock and Brannon 1992; arousal, Zhu and Ratner 2015), most agree that a basic scarcity effect is robust.

The second assumption required to support Cialdini's (1984) contention in the present-day marketplace is that the influence of time scarcity promotions emerges independent of the retail context in which the tactics are employed. Scarcity is generally regarded as a "universal" economic and marketing principle, such that "any commodity will be valued to the extent that it is unavailable" (Brock 1968, 246). In marketing, quantity-based scarcity effects have been reported for a wide range of products, including cookies (Worchel et al. 1975), recipe books (Verhallen and Robben 1994), wine (van Herpen, Pieters, and Zeelenberg 2009), and automobiles (Cachon, Gallino, and Olivares 2019). Research has also demonstrated quantity-based scarcity effects in a wide range of consumption situations, including when experiencing financial constraint (Sharma and Alter 2012), viewing real-time inventory information (Cui, Zhang, and Bassamboo 2019), and inferring market circumstances for product shortage (e.g., high demand, Gierl and Huettl 2010). The diversity of these quantity-based scarcity findings contributed to further strengthening of the assumption that scarcity principles are broadly generalizable and invariant to the method or medium of implementation.

We question whether these assumptions remain applicable for present-day online time scarcity promotions. Introduction of the internet and subsequent growth in e-commerce radically transformed (1) the contexts in which time scarcity tactics are meant to be applied, (2) the methods

by which time scarcity tactics are intended to be implemented, and (3) the experience of consumers expected to favorably respond to these tactics. Under such circumstances, even the most well-designed and rigorous research may not uniformly translate to the present.

First, the internet has transformed the contexts in which time scarcity tactics are meant to be applied. If parallels exist between modern marketing retail contexts and those from over 30 years ago, one might preserve the assumption that time scarcity promotions favorably enhance value of the promoted product irrespective of the retail environment in which the promotions are implemented. However, such parallels may be quite weak, if they exist at all. When Cialdini's book was published in 1984, only 8% of U.S. households had a computer and 0% had internet (U.S. Bureau of the Census 1988). It would be seven years until the World Wide Web publicly launched in 1991, and even longer until the first exclusively online transaction reportedly occurred in 1994 (for Sting's "Ten Summoner's Tales" CD; Fessenden 2015; Lewis 1994). By contrast, 76% of U.S. adults have made and online purchase in the past year (NPR Marist Poll 2018) and global e-commerce sales are estimated to exceed $4.4 trillion (16% of all retail sales) by 2021 (eMarketer 2017).

The internet prompted unprecedented changes that have profoundly transformed the retail context, with major theoretical implications. For one, the internet increased information availability and accessibility to consumers, dramatically reducing search costs (e.g., Alba et al. 1997; Bakos 1997, Brynjolfsson and Smith 2000, Hoffman and Novak 1996; Lynch and Ariely 2000). This prompted the emergence of new decision aids, such as price comparison websites (Häubl and Trifts 2000) and, more broadly, the adoption of the internet as a decision support tool (Lamberton and Stephen 2016). In addition, the internet increased product selection and variety for consumers (Brynjolfsson, Hu, and Smith 2003), contributing to the marketplace's shift to "the

long tail" (Anderson 2004), marked by increasing the proportion of sales generated by niche products and reducing the share from a few best-sellers (Brynjolfsson, Hu, and Simester 2011). Moreover, the internet introduced a new form of physical separation between consumers and products, characterized by limited sensory information (Alba et al. 1997; Degeratu, Rangaswamy, and Wu 2000; Saini and Lynch 2016).

Second, the internet has transformed the methods by which time scarcity tactics are implemented. In contrast to traditional tactics previously used for offline print advertisements and brick-and-mortar retailers, such as static coupons or flyers, contemporary online time scarcity marketing tactics are more dynamic, precise, and brief. Each of these changes introduces new cognitive and affective processes that may alter their effects. For example, across multiple digital marketing channels (e.g., email, social media, online retail product posting), consumers have been repeatedly exposed to "flash sales" that emphasize an exceedingly short promotional window (e.g., 12-hour online retail fashion sale). Some retailers regularly offer online "deal-of-the-day" promotions that provide time-limited discounts on specific items for a 24-hour period (e.g., Amazon's "Gold Box Deal of the Day," Home Depot's "Special Buy of the Day," and Best Buy's "Daily Doorbuster Deals"). Often accompanying flash offerings is a timer counting down the time remaining in the promotion intended to increase consumers' urgency to act (e.g., "sale ends in 3 hours: 12 minutes: 48 seconds!"). In fact, the popularity of such promotions has sparked the emergence of multiple online retailers with business models that rely heavily or exclusively on platforms of online daily flash sales (e.g., Groupon.com).

Third, the internet has dramatically transformed the retail experience of consumers. Online, consumers experience greater ease to obtain product price information and engage in cross-store comparisons (e.g., Lynch and Ariely 2000), which may reduce the likelihood that a given online

time scarcity promotion provides the most salient information possible about the value of a good or the availability of a given price. In addition, the internet has made it easier for consumers to share their information about and experiences with online time scarcity promotions. This has prompted greater production of and reliance on consumer-generated evaluative content and word of mouth (e.g., customer ratings, online reviews, blog posts, product reveal videos) as means of evaluating a product's value (Lamberton and Stephen 2016). Online time scarcity tactics, therefore, may have little influence on product valuation in the presence of information provided alternative sources that are more heavily weighted by the consumer.

Whereas consumers' understanding of online tactics in general, and online time scarcity promotions specifically, was virtually non-existent in the decade after *Influence* was published, consumers' exposure to exploding deals and countdown clocks is now widespread. Frequent exposure is likely to generate persuasion knowledge (Friestad and Wright 1994) that can increase consumer suspicion and skepticism surrounding the offer and the retailer (Campbell and Kirmani 2000) or promote unfavorable inferences regarding the retailer and the retail environment. In addition, the competitive orientation elicited by such scarcity cues (Kristofferson et al. 2017; Roux, Goldsmith, and Bonezzi 2015), which is often directed at other consumers (e.g., Aggarwal et al. 2011), may shift to be directed at the retailer in online retail contexts, where the retailer (rather than other consumers) represents the source of the restriction.

Thus, the emergence of the internet has radically altered the context, implementation, and experience of time scarcity promotions. These changes provide theoretical grounds on which to question the relevance of prior theory related to online time scarcity promotions. If, however, time scarcity promotions examined in prior research have consistently raised product valuation relative to cases in which they were not present, then we may have less reason to doubt that such online

transformations have undermined their effectiveness despite the contextual changes described above. We next examine past academic research for such evidence.

4.2 Retrospection: To What Degree is Related Past Empirical Evidence Conclusive and Generalizable?

We first conducted a search for terms such as "time scarcity," and "time scarcity promotions" in the peer-reviewed marketing literature, examining the citation lists of resulting papers to find related work. Given our definition of online time scarcity promotions as *marketing tactics used in the online environment designed to impose salient temporal restrictions explicitly related to the promotional offer's acquisition availability*, we excluded studies restricting decision time not explicitly related to a promotional offer, such as limiting the time participants had to complete their shopping (Chowdhury, Ratneshwar, and Mohanty 2009, Study 1) or to review a print advertisement (Krishnan, Dutta, and Jha 2013, Studies 2 and 3). Additionally, we excluded studies for which the time restriction was unrelated to the acquisition availability of the promotional offer (e.g., exclusively related to offer redemption), such as imposing "blackout dates" that restrict when participants could use a fictional flight deal (Sinha, Chandran, and Srinivasan 1999, Study 2) or manipulating how long after a purchase that a promotional incentive would be dispersed (Roehm and Roehm 2011, Study 1).

Table 11 displays coding and details for each identified study. The first two columns identify the article and study, respectively. The next three columns describe the product stimuli (Column 3), final sample (Column 4), and empirical context (Column 5) used in each study. Columns 6 and 7 provide information about study conditions, detailing specifics related to the time

scarcity promotion manipulation or measurement in Column 6 (e.g., phrasing used in time scarcity, quantity scarcity, and control conditions) and identifying any additional study design conditions in Column 7 (e.g., low vs. high discount size offered).

Column 8 indicates whether acquisition of the promotion occurs in an online retail context, regardless of whether the context was fictional or real (e.g., simulated retail website, fictional social media post, Groupon.com data); thus, studies conducted using an online survey platform (e.g., Qualtrics) were not coded as investigating an online promotion. Column 9 lists the primary outcome variable(s), the results of which are summarized in Columns 10 and 11. For each outcome, Column 10 displays the overall nature of the time scarcity effect (relative to the control condition), which is coded either as positive ("+"), negative ("–"), null ("="), or conditional ("~"). Subscripts indicate when testing of the time scarcity promotion condition versus the promotion control condition is not applicable (i.e., "NA_a" denotes that a control condition is absent; "NA_b" denotes that not direct test was reported). Additional details regarding the observed time scarcity effect are provided in Column 11, including specific contrasts when the effect is conditional. In total, 34 empirical studies from 19 published articles comprise Table 11.

Table 11 (Essay 3) Summary of Existing Empirical Evidence for Time Scarcity Promotions

(1) Article	(2) Study	(3) Product	(4) Final Analyzed Sample	(5) Context	(6) TS Manipulation/Measurement TS = time scarcity, C = control, TC = time control, QS = quantity scarcity	(7) Other Factor(s)	(8) Online TS Promo	(9) Outcome Variable	(10) TS Promo Effect	(11) Effect Details
Lee et al. 2018	1	Sunglasses	150 (students)	Viewed printout of fictional website with fashion promotion	TS: "Hurry, limited time offer" QS: "Hurry, only a few items left" C: no restriction in ad	Reversibility: low; high	Yes	Purchase Intentions	~	R_H: ↑ R_L: TS = C
								WTP ($)	~	R_H: ↑ R_L: TS = C
								Falsity Inference	NA_b	
Kristofferson et al. 2017	7	iPhone 6	152 (students)	Viewed fictional ad; played 2 competitive games: first-person shooter; word search	TS: "Sale lasts for 1 day only" TC: "Sale lasts for 30+ days" QS: "only 3 available" QC: "3,000 available" C: no restriction in ad	—	No	Aggression (# of shots fired)	=	TS = C TC = C
								Nonaggression (# of words found)	=	TS = C TC = C
Shen 2016	1	Soft drink	200 (students)	Viewed print ad for fictional new soft drink	TS-short: "Special Introductory Offer. One week only!" TS-long: "Special Introductory Offer. Six months only!"	Argument: strong; weak	No	Product Evaluation	NA_a	
								Evaluation Confidence	NA_a	
								Thought Listing Valence (of Product Attributes)	NA_a	
								Perceived Availability	NA_a	
								Message Content	NA_a	
Weathers et al. 2015	1	Inflatable massage chair	190 (students)	Saw product description and typical price; viewed fictional print ad	TS-short: "1 Day Sale!" TS-long: "30 Day Sale!"	Discount Size: 5% (small); 50% (large)	No	Purchase Intentions	NA_a	
								Retailer Credibility	NA_a	
	2	Waterproof phone case	226 (online panel)	(same as Study 1)	(same as Study 1)	Discount Size: 10% (small); 50% (large)	No	Purchase Intentions	NA_a	
								Retailer Credibility	NA_a	
								Product Quality	NA_a	
								Retailer Opportunism	NA_a	
McQuilken et al. 2015	1	2-year Telecom Contract	180 (online panel)	Viewed fictional print ad promoting telecom services	TS: "Bundle and save! Hurry! Save 15%. Offer ends [DATE]." C: "Bundle and save!"	Bundling: no bundle; 2-item bundle; 3-item bundle	No	Promotion Value (rating)	~	B_3: ↓ B_2: ↑$_2$ B_0: TS = C
								Perceived Risk	~	B_3: ↑ B_2: TS = C B_0: TS = C
								Perceived Confusion	=	TS = C
	2	Sony PlayStation 4	206 (online panel)	Viewed fictional print ad promoting videogame products	(same as Study 1)	(same as Study 1)	No	Promotion Value (rating)	=	TS = C
								Perceived Risk	=	TS = C
								Perceived Confusion	=	TS = C
Eisenbeiss, et al. 2015	1	Dinner; Bicycle tune-up	126 (students)	Imagined finding a "Daily Deal" promotion for consumer experience	TS-short: "available only 1 day" TS-long: "available only 7 days"	Discount Size: 50% off; 70% off Product: hedonic; utilitarian	Yes	Deal Attractiveness	NA_a	
	2	Groupon deal	5,698 (Groupon shoppers)	Analysis of Groupon transaction data (Sept. 2009 - July 2011)	*Coded TS: (inverse of) promotion duration	Product Category: hedonic; utilitarian (*coded)	Yes	Unit Sales	NA_a	

(1) Article	(2) Study	(3) Product	(4) Final Analyzed Sample	(5) Context	(6) TS Manipulation/Measurement TS = time scarcity, C = control, TC = time control, QS = quantity scarcity	(7) Other Factor(s)	(8) Online TS Promo	(9) Outcome Variable	(10) TS Promo Effect	(11) Effect Details
Jang et al. 2015	1	Watch; yogurt	184 (students)	Imagined possible release of new limited-edition items (only scarcity groups); all viewed fictional ad	TS: "only available for [XX] days" (yogurt: 15, watch: 30) QS: "only [XX] available" (yogurt: 5,000, watch: 500) C: no restrictions in ad	Conspicuous Product: watch (yes); yogurt (no) (*no manipulated for C)	No	Purchase Intentions Product Value (rating) Brand Attitude	NA_b NA_b NA_b	
	2	Watch; yogurt	216 (Mturk Workers)	Imagined possible release of new limited-edition items, then viewed fictional ad	TS: (same as Study 1) QS: (same as Study 1)	Conspicuous Product: (same as Study 1) Need for Uniqueness: low; high (*measured)	No	Purchase Intentions Product Value (rating) Brand Attitude WOM Intentions	NA_a NA_a NA_a NA_a	
Krishnan et al. 2013	1	DVD player	141 (students)	Completed booklet with printed ad and survey	TS: "Weekend Sale Only" C: "Weekend Sale"	Reference Price: plausible; exaggerated	No	Promotion Value (rating) Shopping Intentions Attitude Toward Deal	NA_b NA_b NA_b	
Coulter and Roggeveen 2012	1	Groupon deal	109 (Groupon deals; 2 US markets)	Analysis of Groupon promotion data (Spring 2011)	*Time until expiration, measured 4 times daily after deal "tipped" TS-short: last two measurements (TS_3, TS_4) TS-long: first two measurement (TS_1, TS_2)	Prior Buyer Number (*measured)	Yes	% change of Buyers	NA_a	
	2	Groupon restaurant deal	121 (online panel)	Imagined finding fictional Groupon deal	TS-short: "3 minutes 57 seconds" TS-long: "2 days, 13 hours, 9 minutes, 57 seconds"	Prior Buyer Number: small; large Purch. Limit: yes; no	Yes	Purchase Intentions Promotion Value (rating) Anticipated Regret	NA_a NA_a NA_a	
Aggarwal et al. 2011	1	Watch	123 (students)	Viewed print ad for fictional sales event	TS: "For six days only" QS: "First 100 customers only" C: no restriction in ad	—	No	Purchase Intentions Consumer Competition	NA_b NA_b	
	2	Laptop	207 (students)	Imagined shopping for laptop (favorite brand), viewed fictional print ad	TS: "Limited Time Only" QS: "Limited Quantities Only" C: no restriction in ad	Brand Concept: symbolic; functional	No	Purchase Intentions Consumer Competition	NA_b NA_b	
Cheema and Patrick 2008	1	Deal at Favorite Store	131 (students)	Primed mindset; imagined receiving coupon	*coupon had one-week redemption window TS-expansive: "anytime between noon and 4pm" TS-restrictive: "only between noon and 4pm"	Mindset: implemental; deliberative Discount Size: 30% (low); 60% (high)	No	Feasibility of Use Time Interval Precision Deal Attractiveness	NA_a NA_a NA_a	
	1_{post}	Deal at Favorite Store	55 (students)	(same as Study 1)	(same as Study 1)	—	No	Deal Rarity Deal Exclusivity Deal Specialness	NA_a NA_a NA_a	
	2	Deal at Favorite Store	168 (consumers near university)	Imagined receiving coupon; completed BIF	(same as Study 1)	Mindset: implemental; deliberative (*measured)	No	Feasibility of Use Time Interval Precision Usage Likelihood	NA_a NA_a NA_a	
	3	Coffee shop coupon	222 (students)	Completed BIF; received coupon for nearby local coffee shop	TS-expansive: "anytime over the next seven days" TS-restrictive: "only over the next seven days"	(same as Study 2)	No	Coupon Redemption	NA_a	

(1) Article	(2) Study	(3) Product	(4) Final Analyzed Sample	(5) Context	(6) TS Manipulation/Measurement TS = time scarcity, C = control, TC = time control, QS = quantity scarcity	(7) Other Factor(s)	(8) Online TS Promo	(9) Outcome Variable	(10) TS Promo Effect	(11) Effect Details
Suri et al. 2007	1	Cancun travel package	49 (students)	Imagined purchasing travel package after reviewing booklet of available packages	TS: "available once in the summer" C: "available every weekend throughout summer"	Price: $728 (low); $998 (high)	No	Product Value (rating)	~	P_H: ↑ P_L: ↓
								Monetary Sacrifice	~	P_H: ↓ P_L: ↑
								Product Quality	~	P_H: ↑ P_L: ↓
	2	Cancun travel package	108 (students)	(same as Study 1)	(same as Study 1)	Price: (same as Study 1) Info Processing Motivation: low (M_L); high (M_H)	No	Product Value (rating)	~	$M_H = P_H$: ↑ P_L: ↓ $M_L = P_H$: ↓ P_L: ↑
								Monetary Sacrifice	~	$M_H = P_H$: P_L: ↑ TS = C $M_L = P_H$: ↑ P_L: ↓
								Product Quality	~	$M_H = P_H$: ↑ P_L: ↓ $M_L = P_H$: ↓ P_L: ↑
								Heuristic Thoughts	~	M_H: ↑ M_L: ↓
								Elaborated Thoughts	~	M_H: ↓ M_L: ↑
								Recall Accuracy	~	M_H: ↓ M_L: ↑
								Thought Valence	=	TS = C
Devlin et al. 2007	1	TV	90 (adults)	Viewed promotional advertisement as part of shopping task in online experiment	TS: "For one week only!!!" C: no time restriction in ad	Discount Size: "…Save £40!" (low); "…Save £190!" (high); "…Save!" (C)	No	Purchase Intentions	=	TS = C
								Promotion Value (rating)	=	TS = C
								Search Intentions	=	TS = C
Howard and Kerin 2006	1	Bedroom set; Sofa	160 (furniture outlet shoppers)	intercepted as entering store; viewed set of fictional print ads	TS: "Three Days Only! Now $_." Ref. Price: "Now $_. Regularly $_." TS + Ref. Price: "Three Days Only! Now $_. Regularly $_." C: "Now $_."	Involvement: low; high Product: bedroom; sofa	No	Attitude Toward Advertised Price	NA_b	
	2	Bedroom set; Sofa	240 (furniture outlet shoppers)	(same as Study 1)	TS: (same as Study 1) Ref. Price: (same as Study 1) TS + Ref. Price: (same as Study 1) C: (same as Study 1) Discount: "Now $_. Regularly $_. 40% Off." TS + Ref. Price + Discount: "Three Days Only! Now $_. Regularly $_. 40% Off."	(same as Study 1)	No	Attitude Toward Advertised Price	~	I_H: NA_b I_L: ↑$_1$
								Shopping Intentions	~	I_H: NA_b I_L: ↑$_1$
	3	Bedroom set; Sofa	240 (furniture outlet shoppers)	(same as Study 1)	TS: (same as Study 1) Ref. Price: (same as Study 1) TS + Ref. Price: (same as Study 1) Sale: "Now $_. Regularly $_. Sale." TS + Sale: Three Days Only! Now $_. Regularly $_. Sale." TS + Ref. Price + Sale: "Three Days Only! Now $_. Regularly $_. Sale."	(same as Study 1)	No	Attitude Toward Advertised Price	NA_a	
								Shopping Intentions	NA_a	

(1) Article	(2) Study	(3) Product	(4) Final Analyzed Sample	(5) Context	(6) TS Manipulation/Measurement TS = time scarcity, C = control, TC = time control, QS = quantity scarcity	(7) Other Factor(s)	(8) Online TS Promo	(9) Outcome Variable	(10) TS Promo Effect	(11) Effect Details
Abendroth and Diehl 2006	2	Concert t-shirt	313 (students)	Imagined attending concert by favorite band; sketched t-shirt design; imagined available for purchase	TS: "…the shirt is officially licensed merchandise and is only available at the concert"	Purchase: bought; did not buy Utility: low; high	No	Short-Term Regret	NA_a	
								Changes in Regret	NA_a	
	3	Concert CD	92 (students)	Imagined attending concert by favorite band; live recording available for purchase	TS: "…will only be available at the concert" C: "…will also be available on the band's website and at the Virgin Megastore…"	Purchase: (same as Study 1)	No	Short-Term Regret	NA_b	
								Changes in Regret	=	TS = C
								Product Value ($)	=	TS = C
								Psych Reactance	=	TS = C
Aggarwal and Vaidyanathan 2003	1	Ketchup; Peanut Butter	not reported (Nielsen panelists; moderate-heavy users; 1 city)	Secondary data analysis of purchase behavior using A.C. Nielsen Scanner Panel Data	*Coded TS: Store coupons TS: In-store price specials C: Manufacturers' coupons	—	No	Purch. Acceleration: Store Coupon (K)	NA_a	
								In-Store Specials (K)	NA_a	
								Manuf. Coupon (K)	NA_a	
								Store Coupon (PB)	NA_a	
								In-Store Specials (PB)	NA_a	
								Manuf. Coupon (PB)	NA_a	
	2	Home stereo system	102 (students)	Viewed fictional print ad in experiment questionnaire booklet	TS: "10 Hours Only Super Sale" C: "10th Anniversary Super Sale"	—	No	Search Intentions	−	↓₁
								Willingness to Buy	+	↑₁
								Attitude Toward Deal	+	↑₁
Brannon and Brock 2001	3	Cinnamon twist	305 (drive-thru customers)	Encouraged to order cinnamon twist before providing food order (during off-peak hours)	TS-short: "Would you like a cinnamon twist made with our special recipe today only?" TS- long: "Would you like a cinnamon twist made with our usual recipe for this year?"	Argument: strong; weak	No	Product Purchase	NA_a	
Inman et al. 1997	3	batteries; cassette; electric toothbrush	182 (students)	Imagined needing to buy a product, viewed fictional print ad for supermarket promotion	TS: "Restricted Offer. Only Available for a Limited Time [Expires _]" QS: "Restricted Offer. Maximum Purchase Allowed: One (1) per Customer" C: no restrictions given	Discount Size: 5% (low); 20% (high)	No	Deal Evaluation	~	D_H: ↑ D_L: ↓
								Purchase Intentions	~	D_H: ↑ D_L: ↓
	4	batteries; cassette; electric toothbrush	128 (students)	Viewed 2 fictional print ads for same product, each offering different discount promotion	TS: (same as Study 3) QS: (same as Study 3) C: (same as Study 3) Precondition: "Restricted Offer. Only Available with a Minimum Purchase of $25"	*within-subjects Discount Size: 5% (low); 50% (high)	No	Purchase Intentions	~	D_H: ↑ D_L: TS = C
Inman and McAlister 1994	1	Spaghetti sauce (7 varieties)	260 (coupon drops)	Analysis of coupon redemption in A.C. Nielsen Scanner Panel Data	*Measured TS: Time until expiration of coupon	—	No	Weekly Coupon Redemption Rate	NA_a	

Note. Consistent with our previously stated conceptualization of online time scarcity promotions, this table only includes empirical studies examining time restriction that was explicitly related to the acquisition availability of a promotional offer. Column 8 ("Online TS Promo") indicates whether acquisition of the promotion occurs in an online retail context (actual or simulated). In Column 10, the nature of the time scarcity effect, relative to the control condition, is indicated as follows: positive ("+"), negative ("−"), null ("="), conditional ("~"), not applicable due to no control condition ("NA_a"), and not applicable due to no direct test of TS effect ("NA_b"). In Column 11, the subscript "₁" indicates a one-tailed test, and subscript "₂" indicates a marginal effect. All conditions were manipulated between subjects, unless otherwise indicated.

4.2.1 Inconsistent Results

Several characteristics of prior research weaken our ability to conclude that online time scarcity promotions will reliably raise consumers' value of the promoted product. First, prior empirical work on time scarcity promotions offers inconsistent results, demonstrating positive, conditional, and null effects (see Table 11). For example, Aggarwal and Vaidyanathan (2003; Study 2) found that exposure to time scarcity print promotions, compared to control promotions, can increase willingness to buy the advertised product, enhance favorability of deal, and reduce intentions to search for a better deal. By contrast, additional work suggests that such positive effects are conditional on other factors. Inman et al. (1997), for example, found that time scarcity print promotions increase purchase intentions at higher discount levels (20%, 50%), but exhibit negative (Study 3) or null (Study 4) effects at low discount levels (5%). Consequently, Inman et al. (1997) suggested that "the effect of [time] restrictions is not uniform" (77).

Howard and Kerin (2006) also report a conditional time scarcity promotion effect, dependent on consumer involvement with the promoted product. In Study 2, time scarcity print advertisements reviewed by furniture outlet customers increased store shopping intentions and favorability of product price attitudes, relative to ads without time scarcity, but only for low-involvement shoppers (for whom the ad was not personally relevant). This finding suggests that time scarcity promotions do not positively influence consumers who are actively shopping for the product or service promoted in the advertisement. As an additional example of conditional effects, Suri, Kohli, and Monroe (2007) found that when individuals were highly motivated to process the promotional offer's message, time scarcity promotions (relative to a control condition) enhanced a higher-priced promoted product's subjective value and reduced a lower-priced product's subjective value. This pattern reversed, however, for individuals with low processing motivation:

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exposure to time scarcity promotions lowered the subjective value of a higher-priced product and increased the subjective value of a lower-priced product (Study 2).

And yet, as another alternative to positive or conditional effects, we also observe instances of prior empirical work reporting a null effect of time scarcity promotions. In examining the impact of limited purchase opportunities on regret, Abendroth and Diehl (2006, Study 3) manipulate the temporal availability of a concert CD but find no influence on its estimated monetary value. McQuilken et al. (2015) find that time scarcity enhanced a promoted product's value only when the fictional print advertisement involved a 3-item telecom contract (Study 1); in all other cases, there was no effect of the time scarcity tactic on consumers' subjective valuation of the advertised offer (Studies 1 and 2). Devlin et al. (2007) reported no significant impact time scarcity promotions on subjective value of the promoted deal, regardless of the size of the discount offered, contrary to the findings of Inman et al. (1997).

4.2.2 Limited Direct Comparability

Although meta-analysis can often provide insight for resolving such inconsistencies, there are several factors that severely limit direct comparability across studies, making a traditional meta-analytic approach inappropriate. One issue concerns the lack of a promotional control condition: only 50% of studies detailed in Table 11 have a promotional control condition to which we can directly compare a time scarcity promotion effect. Rather, many studies make comparisons among different marketing tactics or promotion formats (e.g., time scarcity vs. quantity scarcity promotions). For example, Cheema and Patrick (2008, Studies 1-3) compare whether expansive ("anytime between") or restrictive framing ("only between") time restriction is better for coupon promotion attractiveness and redemption. Eisenbeiss et al. (2015, Study 1) examine whether

people perceive shorter ("available only 1 day") or longer ("available only 7 days") daily deal promotions as more attractive. Similarly, Weathers, Swain, and Makienko (2015, Studies 1 and 2) investigate whether shorter ("1 Day Sale!") or longer ("30 Day Sale!) time scarcity print promotions prompt greater purchase intentions. Brannon and Brock (2001, Study 3) examine cinnamon twist purchases at a Mexican drive-thru to compare whether employees' use of shorter timeframes ("would you like a cinnamon twist made with our special recipe today only?") or longer timeframes ("would you like a cinnamon twist made with our usual recipe for this year?") in time scarcity messaging generates more sales.

Some studies, despite including a promotional control condition, lack direct statistical testing relative to the time scarcity condition. Jang et al. (2015), for example, jointly test whether scarcity promotions (combining both quantity and time scarcity promotions) increase the perceived value of the promoted product relative to a control condition, but do not report testing the effect of time scarcity promotions in isolation. Similarly, Lee, Oh, and Jung (2018) combine quantity scarcity and time scarcity promotion conditions when testing against control condition but do not test the distinct time scarcity promotion effect. As another example, when assessing shopping intentions, Krishnan et al. (2013) report a significant interaction between the presence of a time scarcity tactic (i.e., present vs. absent) and the promotion's reference price (i.e., plausible vs. exaggerated) but do not provide simple effects comparing time scarcity promotion to the control condition for each reference price; rather, they report the simple effects comparing plausible and exaggerated reference prices for each promotion type (time scarcity and control).

An additional factor limiting the direct comparability across studies is dissimilar outcome measurement. Two of the 34 total studies in Table 11 measured objective financial valuation of the promoted product in monetary terms (i.e., WTP $ amount, product $ value). By contrast, nine

studies measured subjective valuation (of product or promotion) and 11 studies assessed self-reported purchase intentions.

4.2.3 Designed for Offline Retail

Even if all prior studies were perfectly comparable, conclusions drawn from any meta-analysis conducted on the previous work would rely almost entirely on time scarcity promotions intended for offline retail contexts, making the applicability of past empirical evidence to the present-day marketplace questionable. The bulk of work in this area examines offline time scarcity promotions explicitly designed for non-digital distribution and print media, such as newspaper inserts (e.g., Howard and Kerin 2006), physical coupons (e.g., Cheema and Patrick 2008), supermarket flyers (e.g., Inman et al. 1997), and print advertisements (e.g., Aggarwal et al. 2011; McQuilken et al. 2015). Several studies used ad copies physically printed as part of a paper-and-pencil questionnaire (e.g., Aggarwal and Vaidyanathan 2003; Inman et al. 1997; Krishnan et al. 2013). Other studies, despite presenting stimuli using online survey software (e.g., Qualtrics), displayed time scarcity promotions that were explicitly stated to be for print media or dissemination via non-digital marketing (e.g., Aggarwal et al. 2011; McQuilken et al. 2015).

Direct examination of time scarcity appeals in their modern online form is sparse: only a handful of studies in the past decade (14.71% of studies in Table 11) have expressly investigated online time scarcity promotions (e.g., Coulter and Roggeveen 2012; Eisenbeiss et al. 2015). Given that online time scarcity promotions often display remaining promotion time in digitally dynamic ways, it is unclear how prior, primarily offline, work can be directly comparable.

Taken together, this retrospective review suggests that we lack a coherent body of research that replicates consistent, practically meaningful effects of time scarcity marketing tactics that are

relevant for contemporary online settings. Little past work appears to consistently or directly address online time scarcity promotions. This, coupled with the transformative changes to the marketing context, promotion implementation, and consumer experience, weakens the theoretical relevance and empirical generalizability of earlier research examining traditional offline time scarcity promotions. However, if new empirical evidence investigating online time scarcity promotions in the present-day marketing context converges with patterns and predictions of research conducted in the preceding marketing context, then the field can have greater confidence that initial theory regarding time scarcity promotions extends to current online implementation of these tactics in the modern marketplace of contemporary consumers. To test this possibility, we next conduct a series of new empirical studies that addresses the issues that limit the relevance of prior work: directly examining time scarcity promotions in their current online form, consistently measuring product valuation using retailer-applicable metrics, and regularly including promotional control conditions. In doing so, we generate a new body of evidence that is generalizable to current marketing contexts and consumers, informative for retailers, and directly tests for the influence of online promotions with and without time scarcity.

4.3 Testing: What New Empirical Investigations Reflect the Contemporary Manifestation of this Phenomenon?

We conducted 26 studies directly testing the effect of online time scarcity promotions across a range of consequential and attitudinal outcomes, promotional stimuli, and digital contexts. Appendix J presents each study's online time scarcity manipulation, Web Appendix W summarizes each study's separate design and findings, and Web Appendix X provides additional

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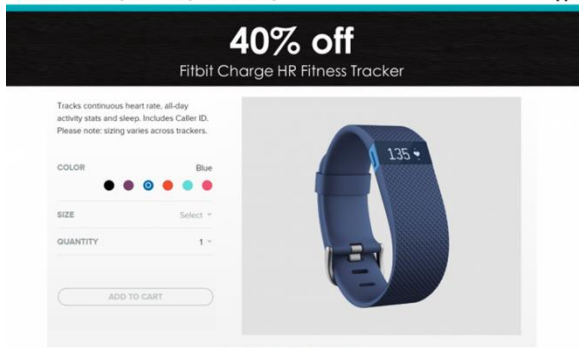
analysis information. We further refer the reader to OSF (https://osf.io/epsdg/) for access to all stimuli, data, and survey materials.

Note. Example online time scarcity promotions are from a varied subset of studies: Panel A (Studies 1 and 14), Panel B (Study 7), Panel C (Study 11), and Panel D (Studies 4 and 5). All online time scarcity promotion manipulations consisted of the presence (vs. absence) of a countdown timer; all other website, product, and promotional content was identical across time scarcity and control conditions. All countdown timers were dynamic, with time remaining actively decreasing upon viewing the online promotion. Panels A and D display categorical time manipulations (i.e., a specific pre-determined amount of time); Panels B and C display continuous time manipulations (i.e., a randomly selected amount of time within a pre-determined range). Studies that directly assessed WTP amount ($) did not present product price (Panels A, B, and D).

Figure 16 (Essay 3) Example Online Time Scarcity Promotion Manipulations

Our goal was to sample broadly across possible operationalizations and typical contexts of time scarcity promotions. The basic paradigm used involves participants imagining wanting to purchase a product or having the real opportunity to do so; encountering a deal for the item when

shopping online or via an email; viewing the promotional offer (with or without online time scarcity); and subsequently reporting their willingness to pay (WTP) and/or observing other indicators of product valuation. To reflect the range of contemporary online time scarcity promotions, we adapted this paradigm across studies, using a variety of different product types (e.g., tablet computer, air purifier, web access, snacks), price points (e.g., approximately $5 to $3,500), digital domains (e.g., online retail websites, email marketing messages, social media), and implementation formats (e.g., dynamic countdown timers, static Facebook ad). Furthermore, we also assess various indicators of consumer value, including monetary and subjective WTP, self-reported purchase intentions, actual digital engagement, perceived product desirability, and purchase decisions (see Web Appendix Z for individual items used in each study). Note that in the set of studies, we include four consequential experiments – one study with a real online boutique using Facebook ads to activate time scarcity perceptions, one with a real service provider using email communications and an actual promotion, one in a lab setting involving real choice of snacks, and one involving MTurkers and a time-sensitive HIT.

For those studies using an active countdown timer embedded within the promotion to mimic contemporary instantiations of online time scarcity marketing tactics (see Figure 16 for explicated screenshots of typical experimental conditions and Appendix J for a full list of stimuli), the specific amount of time displayed to consumers was manipulated in one of two ways. Categorical online time scarcity promotion manipulations displayed a specific pre-determined amount of time: the amount of time remaining was pre-programmed such that random assignment determined whether the countdown represented a shorter or longer time period (e.g., Study 16). Continuous online time scarcity promotion manipulations, however, displayed a randomly selected amount of time within a pre-determined range of time (e.g., Study 12). Using these techniques for

manipulating remaining promotion time minimizes the possibility that any specific time displayed drives observed effects.

Numerous indicators of consumer valuation were also assessed: WTP, online purchase behavior, reported purchase intentions, product quality perceptions, and product desirability perceptions. Some studies measured WTP as an objective financial amount (e.g., "How much would you be willing to pay for this product?" sliding scale anchored at $0 and $[approximately twice listed price]). Others assessed WTP as a subjective rating relative to a mean price (e.g., "Average [product] from this brand have been seen selling on this website for approximately $[approximately twice listed price]. How much would you be willing to pay for [product]? 1–Much Less than $X, 2–Less than $X, 3–A Little Less than $X, 4–Exactly $X, 5–A Little More than $X, 6–More than $X, 7–Much More than $X).

To measure purchase intentions, participants reported likelihood of buying the promoted product on a 7-point response scale (e.g., "How likely are you to buy this tablet right now?" 1–Very Unlikely, 7–Very Likely). In total, eight studies assessed purchase intentions. Online purchase behavior was measured as whether individuals exposed to an online promotional offer decided to pay for the promoted product. Consumers in the two studies made consequential purchase decisions in simulated retail contexts, meaning choice to buy was honored and individuals received their purchase (i.e., website access, study 24; snacks, study 26). We also identified product quality and desirability perceptions as possible alternative indicators of increased valuation. Participants evaluated the promoted product, rating quality in eight studies (e.g., "How would you rate the quality of this product?" 1–Very Poor Quality, 7–Very High Quality) and desirability in six studies (e.g., "How desirable is this product?" 1–Not at All Desirable, 7–Extremely Desirable). Select studies assessed online engagement behavior as the

primary outcome of interest, such as time searching online for better deal (Study 2), accepting

MTurk HIT (Study 22), opening email (Study 23), social media engagement (Study 25).

To enhance understanding of the psychological effects of exposure to online time scarcity

promotions, some studies additionally measured consumers' persuasion knowledge activation,

retailer competition activation, mood, or psychological reactance. Over time, consumers are

argued to develop persuasion knowledge that enables them to recognize influence tactics more

readily and cope with persuasion attempts (Friestad and Wright 1994). In retail contexts,

persuasion knowledge can shape the inferences consumers make about a retailer's motives, with

shoppers perceiving the use of certain marketing tactics as evidence the retailer is attempting to

manipulate them (e.g., Campbell and Kirmani 2000). It is possible, therefore, that using online

time scarcity promotion tactics may activate consumers' persuasion knowledge, subsequently

increasing suspicion of the retailer's motives (Campbell and Kirmani 2000) and inclination to

regard the retailer as an adversary trying to maximize their own gain. Exploring this possibility,

individuals in three studies completed a five-item mean-composite index of persuasion knowledge

(e.g., "I'm skeptical about how good this deal is;" 1–Strongly Disagree, 7–Strongly Agree; α's

range from .62 to .78 across 3 studies). The scale incorporated items adapted from previous

research assessing inferences of motives and manipulative intent (Campbell 1995; Campbell and

Kirmani 2000), as no single commonly accepted measure of persuasion knowledge has been

developed (Ham, Nelson, and Das 2015).

In six studies, we examined the impact of online time scarcity promotions on consumers'

perception of competition with the retailer. Prior research suggests that a sense of competition can

be evoked by scarcity cues. For example, consumers exposed to generalized resource scarcity cues

adopt a diffuse competitive orientation (Kristofferson et al. 2017; Roux et al. 2015) and print

advertisements using quantity scarcity promotion tactics can increase perceptions of competition with other consumers, which subsequently increased reported purchase intentions (Aggarwal et al. 2011). Following this logic, it is possible that online time scarcity promotion tactics may also engender a competitive orientation, one directed at the primary source of the temporal restriction–the retailer. To investigate this possibility, participants completed six items capturing the degree to which they felt a sense of competition with the retailer (e.g., "Purchasing the product feels like a contest: me vs. the retailer;" 1–Strongly Disagree, 7–Strongly Agree), which were averaged to form a composite index of perceiving the retailer as an opponent (α's range from .82 to .90 across 6 studies; see Web Appendix Z for all items).

Multiple studies additionally examined retail website perceptions, general product scarcity perceptions, and time scarcity, enabling examination of these factors as possible alternative explanations. In 18 studies, individuals evaluated the retail website itself. Specifically, participants indicated their agreement that the website is "professional," "easy to understand," and "visually attractive" on a 5-point response scale (1–Strongly Disagree, 5–Strongly Agree). The mean of these three items provides a composite index of retail website perceptions (α's range from .64 to .85 across 18 studies). To assess general scarcity perceptions, participants in 16 studies reported how scarce they perceived the promoted product to be (e.g., "How scarce is this product?") measured on a 7-point response scale (1–Not at All Scarce, 7–Extremely Scarce). To assess time scarcity perceptions, participants in 11 studies indicated how much time remained to purchase the promoted product (e.g., "How much time is left to purchase this product?" 1–Not Long at All, 7–Extremely Long). Consequently, each item represents an opportunity to confirm that participants specifically experienced time scarcity.

Critically, all studies included an online promotion control condition, which enabled direct testing of the effect of online time scarcity promotions. Select studies manipulated additional factors to test boundary conditions or potential explanatory mechanisms of online time scarcity promotions, such as risk reduction tactics (Study 4, Study 5), enhanced elaboration (Study 6, Study 20), and holiday retail context (Study 3).

4.4 Synthesis: What Does Synthesizing New Empirical Findings Reveal About Effect's Robustness Across Temporal and Market Changes?

Each study was first analyzed in isolation, the results of which we summarize in Web Appendix W. In short, we do not observe a reliable convergence of positive online time scarcity promotion results as would be predicted by offline time scarcity theory. For example, among the 17 studies measuring willingness to pay (as a dollar amount), one study finds that online time scarcity promotions increase consumer WTP amount (Study 7), one study finds a conditional negative effect (Study 16), and 15 studies find no effect on the amount consumers are willing to pay (Studies 3, 4, 5, 6, 8, 12, 13, 14, 15, 17, 18, 19, 20, 21, 24).

Though individual studies do not yield clear effects, online time scarcity promotions can take many forms and firms may vary in their interest in specific product valuation outcomes measured. As such, although any given study may deviate from effects seen in offline settings, single-paper meta-analyses on comparable studies allows us to see if certain combinations of experimental factors do, in fact, yield positive effects of time scarcity on various retailer-relevant outcomes (McShane and Böckenholt 2017). By reporting every study conducted, determining inclusion criteria *a priori* (i.e., based on construct measurement), and pre-registering multiple

studies, we seek to avoid the methodological pitfalls of internal meta-analyses that can inflate likelihood of obtaining a false positive result (Vosgerau et al. 2019).

4.4.1 Study Identification

We began our SPM analysis by identifying the different ways in which our 26 studies measure consumer valuation responses to online time scarcity promotions. WTP was assessed using both objective and subjective responses. Although both measures are believed to measure the same underlying construct, each elicits a different type of valuation response: one involving a subjective comparison and the other an objective dollar amount. Because it is not definitively known that participants use both scales in the same way, we follow recommendations of McShane and Böckenholt (2017) and conduct separate SPM analyses for each WTP measure, identifying 17 eligible studies for inclusion in the WTP (amount) SPM and three eligible studies for inclusion in the WTP (subjective) SPM.

Because alternative indicators of consumer valuation, including online purchase behavior, reported purchase intentions, product quality perceptions, and product desirability perceptions, were also similarly assessed in multiple studies, we could analyze these constructs using SPM methodology. Several studies additionally examined persuasion knowledge activation, retailer competition activation, retail website perceptions, general product scarcity perceptions, and time remaining in promotion, which enabled SPM testing.

4.4.2 Factor Retention

For each SPM analysis, we totaled the number of experimental factors and factor levels across those studies measuring the outcome of interest. Each study contained an online time scarcity promotion experimental factor, manipulated between subjects in all but two studies (Studies 9 and 10, which employed a within-subjects design). To specifically examine the influence of online time scarcity relative to no online time scarcity, we aggregated across different time scarcity durations (e.g., shorter, longer), if present, within individual experiments.

When factors appeared exclusively in a single or small subset of studies (e.g., risk reduction; Study 5), only the factor level corresponding to the control condition in unmoderated studies was retained (McShane and Böckenholt 2017); in moderated studies, the factor level serving as a control-equivalent condition was retained for comparison (e.g., control elaboration condition in Studies 6 and 20; no risk reduction condition in Studies 4 and 5). Because Study 3 was the only three-factor study and revealed no main effect of product recipient (self vs. other), we treated the conditions of purchasing for the self or other person as separate studies (i.e., Studies 3A and 3B, respectively), enabling us to retain the data from all experimental tests of online time scarcity promotions in Study 3. When studies were identified as eligible for inclusion in multiple SPM analyses, we treated each equivalently across all SPM analyses unless otherwise stated. The resulting number of experimental tests of the effect of online time scarcity, therefore, did not always equal the number of studies eligible for inclusion in the SPM analysis.

The final sample of experimental tests of the effect of online time scarcity promotions on WTP used in SPM analyses totaled 18 for WTP dollar amount (Studies 3A, 3B, 4, 5, 6, 7, 8, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 24) and three for subjective WTP ratings (Studies 1, 4, 5). With respect to alternative indicators of consumer valuation, the final sample of experimental tests used

in SPM analyses totaled nine for purchase intentions (Studies 1, 3A, 3B, 4, 5, 6, 11, 14, 15), two

for online purchase behavior (Studies 24, 26), six for product desirability (Studies 1, 4, 5, 7, 8, 15),

and nine for product quality (Studies 1, 3A, 3B, 4, 5, 6, 7, 8, 15). With respect to psychological

consequences of online time scarcity promotions, the number of experimental tests included in

SPM analyses totaled four for persuasion knowledge activation (Studies 3A, 3B, 9, 10) and seven

for adversarial retailer perceptions (Studies 3A, 3B, 4, 5, 6, 7, 8). Examining possible alternative

explanations, the final sample of experimental tests used in SPM analyses totaled 17 for general

scarcity perceptions (Studies 1, 3A, 3B, 4, 5, 6, 7, 8, 11, 14, 15, 16, 17, 18, 19, 20, 21), 12 for time

scarcity perceptions (Studies 3A, 3B, 6, 7, 8, 11, 16, 17, 18, 19, 20, 21), and 19 for retail website

perceptions (Studies 1, 3A, 3B, 4, 5, 6, 7, 8, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21). All summary

statistics were standardized within study prior to SPM analysis, unless otherwise stated (see Web

Appendix Y for summary statistics).

Table 12 (Essay 3) SPM Results Summary

Outcome	n (studies)	SPM Est. [95% CI]	I^2 Est. [95% CI]	TS Impact
Willingness to Pay (WTP) Results				
WTP (Dollar Amount)	18	-.0388 [-.1029, .0253]	0% [0%, 26%]	*null*
WTP (Dollar Amount)[1]	13	.0274 [-.0482, .1029]	0% [0%, 6%]	*null*
WTP (Dollar Amount)	10	–	0% [0%, 13%]	–
Shorter Online Time Scarcity Promotion	–	-.1449 [-.2463, -.0434]	–	↓
Longer Online Time Scarcity Promotion	–	-.0725 [-.1738, .0288]	–	*null*
WTP (Subjective Rating)	3	-.3054 [-.5040, -.1070]	0% [0%, 43%]	↓
Alternative Indicators of Consumer Valuation				
Purchase Likelihood	9	.0130 [-.0903, .1163]	6% [0%, 54%]	*null*
Online Purchase[2]	2	-.0695 [-.1716, .0325]	0% [0%, 89%]	*null*
Product Desirability	6	-.0529 [-.1797, .0739]	7% [0%, 63%]	*null*
Product Quality	9	-.0270 [-.1397, .0856]	35% [0%, 64%]	*null*
Psychological Consequences				
Persuasion Knowledge Activation	4	.0588 [-.0747, .1923]	27% [0%, 68%]	*null*
Perceiving Retailer as Opponent	7	.1391 [-.0092, .2873]	0% [0%, 37%]	*null*
Alternative Explanations				
Perceived General Scarcity of Promoted Item	17	.1036 [.0329, .1743]	0% [0%, 21%]	↑
Perceived Time Remaining to Purchase Item	12	-.9008 [-.9951, -.8066]	40% [2%, 64%]	↓
Retail Website Perceptions	19	-.0727 [-.1331, -.0123]	7% [0%, 37%]	↓

[1] As a robustness check, five studies plausibly containing non-equivalent comparison conditions (Studies 16, 17, 18, 19, and 21) were excluded.
[2] Because outcome of interest was a proportion, values were not standardized prior to conducting SPM analysis.
Note. I^2 quantifies degree of heterogeneity, with values 25%, 50%, and 75% describe low, moderate, and high, respectively (Higgins et al. 2003).

4.4.3 Willingness to Pay (WTP) Results

4.4.3.1 WTP Amount ($)

The classic time scarcity effect predicts that marketing promotions including online time scarcity will significantly increase consumers' valuation of the promoted product–in this case, the standardized WTP amount ($). As seen in Figure 17, however, SPM results of 18 experiments (Studies 3A, 3B, 4, 5, 6, 7, 8, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 24) estimated the effect to be nonsignificant, at -.0388 (CI$_{95}$[-.1029, .0253] with a confidence interval that includes zero (see Table 12 for I^2 heterogeneity estimates for each SPM analysis).

Despite this, one might question the degree to which the specific factor levels of control-comparable conditions in studies with moderators (e.g., control elaboration condition; Studies 6)

functioned similarly to the control condition in studies without moderators. Thus, as a robustness check, we re-ran this analysis after dropping moderated studies lacking an identical comparable control condition (n = 5).[37] An SPM of the remaining 13 experiments (Studies 3A, 3B, 4, 5, 6, 7, 8, 12, 13, 14, 15, 20, 24) estimated the effect at .0274 (CI_{95}[-.0482, .1029] with a confidence interval including zero, again indicating a non-significant difference in how much consumers are willing to pay for products promoted with (vs. without) online time scarcity tactics.

4.4.3.2 WTP Subjective Rating

We also tested whether exposure to online marketing promotions with time scarcity increased subjective willingness to pay for the promoted product. SPM of three studies (Studies 1, 4, 5) estimates the effect to be significant, at -.3054 (CI_{95}[-.5040, -.1070]), with a confidence interval that does not include zero. This finding indicates that online time scarcity promotions have a significant *negative* effect on subjective WTP ratings, contrary to the standard time scarcity marketing prediction. Thus, neither objective nor subjective WTP ratings demonstrate support for a value-enhancing influence of online time scarcity promotions.

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[37] Dropped Studies 16, 17, 18, 19, and 21.
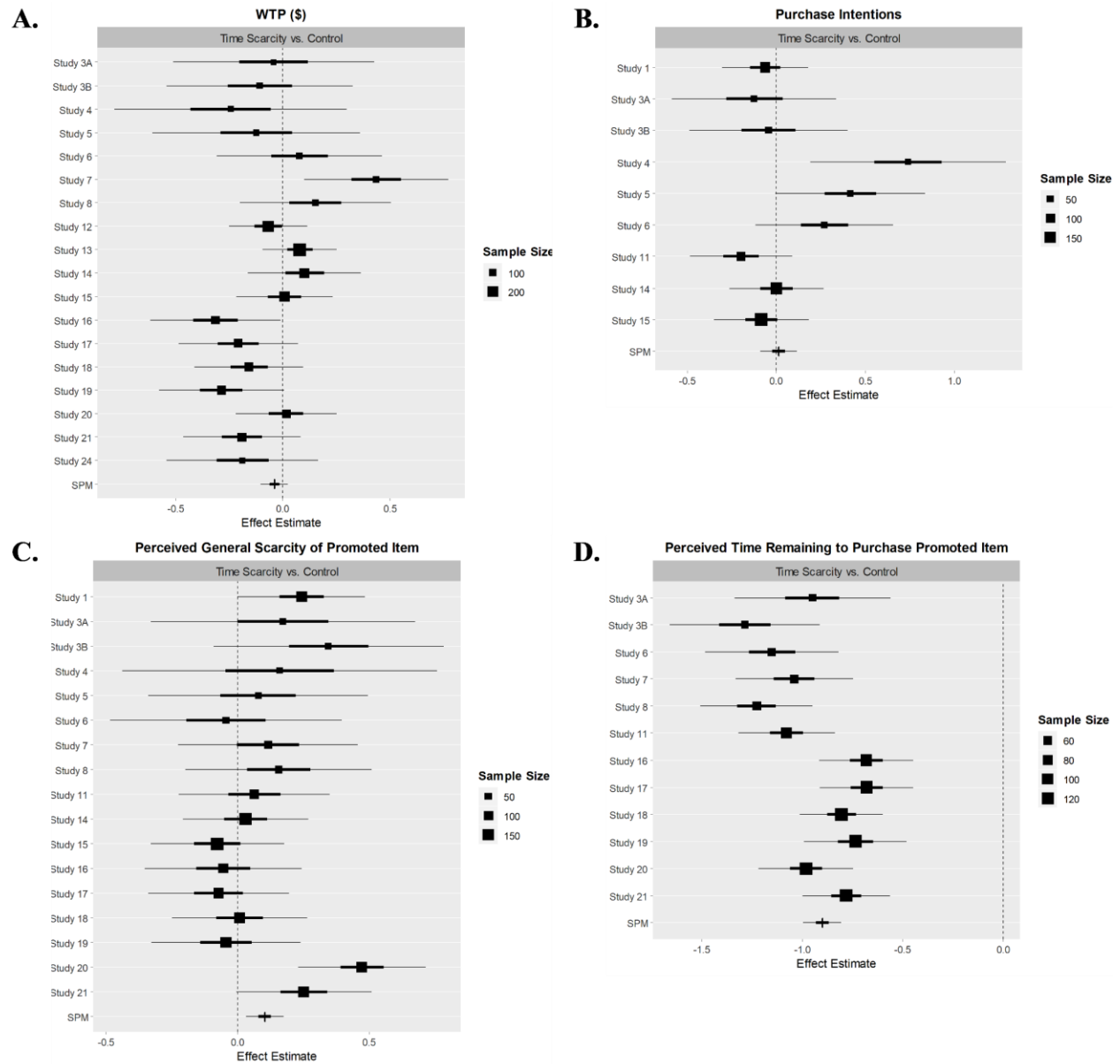
Note. Single-study effect estimates are represented by squares; Single-Paper Meta-Analysis (SPM) estimate is represented by vertical bars. Thick and thin lines represent the 50% and 95% confidence intervals, respectively. Square size reflects the average sample size per condition in each study. The SPM suggests the effect does not attain statistical significance for WTP $ amount (Panel A) nor purchase intentions (Panel B). The SPM suggests a positive effect on general scarcity perceptions (Panel C) and a negative effect on perceived amount of time remaining in the promotion (Panel D).

Figure 17 (Essay 3) SPM Results

4.4.3.3 Exploratory Analysis: Amount of Time Remaining

We also conducted an exploratory SPM analysis testing for the distinct effects of shorter versus longer online time scarcity promotions, relative to promotions without online time scarcity. Studies that could be discretely categorized as using either shorter or longer online time scarcity tactics were included in the analysis. An SPM of the 10 experiments that enabled such comparisons (Studies 4, 5, 14, 15, 16, 17, 18, 19, 20, 21) shows that online promotions with time scarcity do not significantly increase consumers' WTP dollar amount for the promoted product, regardless of whether time remaining in the promotion is shorter or longer: the effect of shorter online time scarcity promotions was negative and significant, at -.1449 (CI$_{95}$[-.2463, -.0434]) with a confidence interval that does not include zero; and the effect of longer online time scarcity promotions was non-significant, estimated to be -.0725 (CI$_{95}$[-.1738, .0288]) with a confidence interval that includes zero.

Taken together, we observe no significant positive effect (and in some cases, observe a negative effect) of online time scarcity promotions on WTP, regardless of how WTP was assessed (i.e., as an objective financial amount or a subjective relative rating) and how much time remained before the promotional offer expired (i.e., shorter or longer timeframe).

4.4.4 Alternative Indicators of Consumer Valuation

4.4.4.1 Purchase Intentions

Some work suggests there are circumstances under which WTP may be particularly difficult to move (e.g., when consumers possess well-established internal reference prices, Monroe 1973; Urbany, Bearden, and Weilbaker 1988). Consequently, we explored the potential impact of online time scarcity promotions on alternative indicators of valuation, including one of the most

commonly used in prior time scarcity promotion research: purchase intentions. SPM analysis of nine experimental tests (Studies 1, 3A, 3B, 4, 5, 6, 11, 14, 15) estimated the effect of online time scarcity promotions on purchase intentions to be positive but not significant, at .0130 (CI$_{95}$[-.0903, .1163]) with a confidence interval that includes zero.

4.4.4.2 Real Online Purchasing

The traditional time scarcity promotion hypothesis proposes that exposure to online time scarcity marketing tactics should increase online purchase of the promoted product. SPM results from two studies measuring the proportion of consumers who spent money to make a consequential online purchase (Studies 24 and 26) estimated the effect of online time scarcity promotions on actual purchase likelihood to be negative, but non-significant, at -.0695 (CI$_{95}$[-.1716, .0325] with a confidence interval that includes zero.[38]

4.4.4.3 Product Perceptions

An SPM analysis with six experiments (Studies 1, 4, 5, 7, 8, 15) estimates the effect of online time scarcity promotions on product desirability ratings to be non-significant, at -.0529 (CI$_{95}$[-.1797, .0739]) with a confidence interval that includes zero. An SPM analysis with nine experiments (Studies 1, 3A, 3B, 4, 5, 6, 7, 8, 15) estimates the effect of online time scarcity promotions on product quality ratings to also be non-significant, at -.0270 (CI$_{95}$[-.1397, .0856]) with a confidence interval that includes zero. Thus, the presence of online time scarcity tactics does not seem to impact evaluations of the product's quality or desirability, as would have been suggested by Lynn (1991) based on commodity theory (Brock 1968).

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[38] Because the outcome of interest was a proportion, we did not standardize values before conducting the SPM analysis.

4.4.5 Psychological Consequences

4.4.5.1 Persuasion Knowledge Activation

In our assessment of the changes that may undermine the robustness of an online time scarcity effect in the present market, we argued that perhaps increased exposure to exploding deals led consumers to develop persuasion knowledge about such tactics, which is activated by additional exposure to online time scarcity promotions. To test this, we considered whether our online time scarcity promotions activated persuasion knowledge, as measured by the 5-item index described earlier. An SPM analysis of four experiments (Studies 3A, 3B, 9, 10) estimates the effect to be non-significant, at .0588 (CI_{95}[-.0747, .1923]) with a confidence interval that includes zero. This suggests, therefore, that consumers' persuasion knowledge was not consistently triggered by exposure to online time scarcity promotions.

4.4.5.2 Perceiving Retailer as Opponent

Drawing on prior research demonstrating scarcity can prompt a competitive orientation among consumers (Aggarwal et al. 2011; Kristofferson et al. 2017; Roux et al. 2015), we examined whether online time scarcity promotions evoke a competitive orientation directed at the retailer, the source of the promotion time restriction. SPM analysis with seven experiments measuring the 6-item index of competitive orientation towards the retailer described earlier (Studies 3A, 3B, 4, 5, 6, 7, 8) estimated the effect to be non-significant, at .1391 (CI_{95}[-.0092, .2873]) with a confidence interval that includes zero. This finding suggests that online time scarcity promotions do not impact competitive perceptions of the retailer.

4.4.5.3 Failure to Induce Scarcity

It is possible that accessibility to other information about product availability prevented consumers from perceiving the promoted products as scarce. Results, however, suggest our manipulations effectively reduced perceived product availability. An SPM analysis of 17 experiments (Studies 1, 3A, 3B, 4, 5, 6, 7, 8, 11, 14, 15, 16, 17, 18, 19, 20, 21) estimated the effect of online time scarcity promotions on general scarcity perceptions to be positive and significant, at .1036 (CI_{95}[.0329, .1743]) with a confidence interval that does not include zero. This indicates that consumers perceive products promoted with online time scarcity tactics to be scarcer than those promoted without online time scarcity tactics.

It is also possible that our online time scarcity manipulations did not generate a perception of less time remaining in the promotion. This could occur, for example, if consumers were skeptical about the veracity of the time limit displayed. An SPM of 12 experiments (Studies 3A, 3B, 6, 7, 8, 11, 16, 17, 18, 19, 20, 21), however, estimated the effect of online time scarcity promotions on perceived time remaining to purchase the promoted product as significant and negative, at -.9008 (CI_{95}[-.9951, -.8066]) with a confidence interval that does not include zero. This indicates that consumers reliably perceived the amount of time left to purchase the promoted product as significantly shorter when accompanied by an online time scarcity tactic, meaning the online promotion effectively evoked time scarcity perceptions.

4.4.5.4 Website Perceptions

It is also possible that online time scarcity promotion effects did not emerge because such tactics altered consumer perceptions of the retailer's website. Using the three-item website perceptions index described earlier, an SPM of 19 experiments (Studies 1, 3A, 3B, 4, 5, 6, 7, 8, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21) estimated the effect to be significant and negative, at -

.0727 (CI$_{95}$[-.1331, -.0123]) with a confidence interval that does not include zero. This suggests that, indeed, consumers' value perceptions can spillover to the online retail context; the SPM results indicate individuals perceive online retail environments with online time scarcity promotions less, rather than more, favorably (in terms of visual attractiveness, ease of understanding, and professionalism) than those running the identical promotion without an online time scarcity tactic. Importantly, across conditions, the websites were identical with the exception of the time scarcity marketing intervention.

4.4.6 Discussion

Taken together, both isolated single-study analyses and meta-analyses suggests that online time scarcity effects fail to emerge in the present market in ways consistent with predictions that would be expected based on theory related to offline time scarcity promotions. Though we reliably manipulate perceived time scarcity, findings related to multiple stimuli, consumer populations, behavioral outcomes, and attitudinal measures consistently demonstrate no significant increases in financial WTP amount, nor in subjective WTP ratings. Somewhat surprisingly, individuals' willingness to pay amount even decreased for products promoted with exceedingly short timeframes (i.e., less than one hour).

SPM results also revealed no increase in actual online purchase behavior or in self-reported purchased intentions–one of the most commonly used metrics of value in prior tests of time scarcity promotions. Furthermore, online time scarcity offers did not favorably enhance perceived value of the promoted product (i.e., quality or desirability) or of the retail environment (i.e., website perceptions); in fact, consumers devalued the e-commerce website of those retailers employing online time scarcity promotional tactics.

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We also gain some preliminary insight into the psychological effects of exposure to online time scarcity. Results do not indicate that exposure to online time scarcity promotions lowered inferred product quality, activated consumers' persuasion knowledge, or consistently elicited a competitive orientation towards the retailer. It appears, however, that despite holding all other elements of the websites and products constant within each study, consumers view websites using online time scarcity tactics as less attractive, understandable, and professional than those without online time scarcity promotions. This finding may suggest that such tactics have come to be associated with less-appealing retailers, a finding we discuss later.

4.5 General Discussion

This article revisits a marketing tool widely implemented and generally accepted as effective: online time scarcity promotions. Applying the *ARTS* paradigm (*Assumption, Retrospection, Testing, Synthesis*) for revisiting established marketing phenomena, we identify several reasons to question the relevance of past theory and findings to novel instantiations of online time scarcity promotions. First, we evaluated the applicability of prior offline theory to the present online marketplace. The emergence of the internet and growth in online retailing fundamentally altered the contexts in which time scarcity tactics are meant to be applied, the methods by which time scarcity tactics are implemented, and the experience of consumers targeted by time scarcity tactics. Foundational theories and demonstrations of time scarcity promotions, however, largely predate these radical market changes, reducing the suitability of applying previous assumptions underlying offline tactics to the modern-day online marketplace. Under such transformative conditions, even the most rigorous research warrants reevaluation.

We then conducted a retrospective analysis of prior empirical work, finding that existing findings offer limited insight that is generalizable to contemporary online time scarcity promotions. Results reveal inconsistent evidence supporting the efficacy of offline time scarcity promotions, demonstrating a combination of positive, conditional, and null effects. In addition, nearly all studies examine offline time scarcity promotions explicitly designed for non-digital distribution. With prior work providing inconclusive evidence and sparse direct examination of time scarcity appeals in their modern online form, extrapolating prior offline research to contemporary online retailing seems misguided without additional robust empirical testing.

Consequently, we then generated a new body of empirical evidence investigating present-day online time scarcity promotions. The series of 26 new studies addressed several issues that limit the generalizability and relevance of prior work (e.g., examining time scarcity promotions in their contemporary online form, regularly including a promotion control condition to which online time scarcity tactics can be compared). The newly conducted studies used a variety of product stimuli, promotional timeframes, digital formats, behavioral outcomes, and attitudinal measures that more aptly reflect contemporary online time scarcity promotions and enhance the empirical generalizability of results. Consequently, the field can have greater confidence in assessing whether initial theory regarding time scarcity promotions extends to current online implementation of these tactics in the modern marketplace with contemporary consumers.

Synthesizing both single-study and single-paper meta-analytic results from this new empirical evidence, we find little to suggest that online time scarcity effects emerge in the present market in ways consistent with predictions derived from theory regarding offline time scarcity promotions. Despite a small set of single-study isolated instances for which favorable time scarcity effects emerge for select measures, contemporary instantiations of online time scarcity promotions

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more often have negligible or adverse effects. Indeed, SPM analyses show that exposure to online time scarcity promotions fail to positively impact any indicator of consumers' valuation of the promoted product: financial WTP amount, subjective WTP ratings, actual online purchase behavior, self-reported purchase intentions, nor perceptions of the promoted product's desirability or quality. Moreover, several possible alternative accounts (e.g., failure to induce scarcity perceptions, persuasion knowledge activation, competitive orientation towards the retailer) do not account for these findings. Taken together, these findings show that present-day online time scarcity tactics are not as universally effective as previous offline time scarcity research and marketplace usage would suggest, thereby providing a strong foundation on which to develop new theory and recommendations regarding the use of online time scarcity tactics.

4.5.1 Practical and Managerial Implications

If offline time scarcity theory does not hold in cases of typical present-day online time scarcity promotions, what implications arise? First, our results imply that marketers should systematically question and judiciously revise existing marketing practices regarding online time scarcity promotions. The radical innovation of the internet and the growth of online retailing (e.g., e-commerce, m-commerce) has transformed both consumers' exposure to and marketers' implementation of online time scarcity tactics. This article suggests that such changes have not only dramatically shifted the marketing landscape, but also the assumptions on which time scarcity promotions are based. Thus, the amount of evidence currently supporting the efficacy of online time scarcity promotions does not seem to justify marketers' preponderant usage. Consequently, practitioners working in online retail or digital marketing domains may benefit from reducing online time scarcity promotion prevalence and executing the tactic more discerningly. Failure to

update existing practices may jeopardize marketers' estimates of the marketing promotion's efficacy, thereby forfeiting prospective benefits of a successful campaign.

Second, our work implies that marketers should carefully attend to the ways in which practice applies past theory. Prudent marketers would refrain from the use of online time scarcity promotions unless tactics have been repeatedly demonstrated using multiple operationalizations, robustly supported by meta-analyses that can directly compare their effects, and consistently replicated among new consumer samples and retail contexts. In fact, if alternative promotional tactics meet these criteria, we would be comfortable recommending that they be used before online time scarcity promotions. For example, recent work has shown the durability of extremeness aversion as a driver of consumer behavior (Neumann, Böckenholt, and Sinha 2016) and the robust effects of defaults in consumer settings (Jachimowitz et al. 2019). Our results, however, suggest that online time scarcity promotions may constitute a much weaker marketing tool. Thus, retailers could benefit from tactics with a stronger evidentiary base, at least until a new theory for and more robust evidence supporting online time scarcity promotions emerges.

In addition to providing specific insights regarding contemporary online time scarcity promotions, this article also makes a methodological contribution: providing a simple paradigm for reconsidering established marketing phenomena. This *Assumption, Retrospection, Testing, Synthesis* method (i.e., *ARTS* paradigm) is driven by theory, in contrast to a "forensic statistics" approach employed to identify dubiously replicable effects. We have no reason to doubt that any prior research on time scarcity promotions was not executed using the highest standards of precision or transparency; indeed, the work contains numerous convincing field studies, identifies important moderators, and provides much-needed experimental support for Cialdini's (1984) original assertions. Applying the *ARTS* paradigm, however, we can systematically reconsider the

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relevance of past theory for the present-day marketplace, thus contributing to our understanding of both offline and online time scarcity: building on, rather than dismantling, past knowledge.

4.5.2 Next Steps: Toward a Theory of Online Time Scarcity Promotions

This research also implies that researchers have an opportunity for impactful theoretical development, either augmenting prior offline scarcity theory to accommodate online operationalizations and experiences or developing a distinct theory that treats these tactics as a fundamentally new phenomenon. Our empirical work offers preliminary ideas about the way such a theory might take shape. First, though we assessed some psychological factors that might explain the absence or reversal of online time scarcity effects (e.g., persuasion knowledge, retailer-oriented competition), much work remains to be done to pinpoint consumer psychological processes that account for muted efficacy of online time scarcity promotions.

Despite experiments using multiple product stimuli, dependent measures, and online promotions, the current results do not identify the specific conditions under which online time scarcity promotions might be effective. As one possible route for inquiry, we note that studies did not systematically vary the depth of this discount, nor did we offer free product or samples for a limited time. If time scarcity is used to promote online free trial, rather than purchase, it is likely to have much more positive effects, as such offers may be unique to one retailer and involve very little risk for the consumer. Future research may consider such boundary conditions, in part, as a means of developing new theory for this tactic.

Our results also suggest that consumers' perceptions of the online retail context itself should be integrated into any new theory of online time scarcity promotions. Consumers viewed websites using online time scarcity marketing tactics less favorably (i.e., less attractive,

understandable, and professional) than the websites of retailers offering the identical online promotion without time scarcity. However, neither website perceptions nor a failure to induce a sense of general or time-specific scarcity seems to account for the observed inefficacy of online time scarcity promotions across our new empirical investigations. Although the SPM methodology does not allow for aggregate tests of mediation, we separately tested for the mediating effect of website perceptions for each single-study instance in which online time scarcity promotions impacted consumers' willingness to pay.[39] In each case, single-study mediation results showed no mediating influence of website perceptions.[40] Thus, although website perceptions do not explain the failure of online time scarcity promotions to reliably enhance the promoted product's value, the unfavorable perceptions captured can help to identify consequences of using these tactics or instances when they may be undermined.

In addition, broad consideration of the unique features of the online retail context can also offer new avenues for understanding what circumstances could enhance the effectiveness of online time scarcity promotions. As previously discussed, for example, one of the biggest implications of online retailing involves dramatically lowered search costs, in both online and offline retailing, which may have reduced the efficacy of online time scarcity promotions. If so, it follows that online time scarcity tactics may be more effective (vs. online control promotions) when consumers experience greater online search frictions. We conducted preliminary studies exploring the idea that increasing online search costs may increase consumers' proneness to online time scarcity promotions. The first study (https://osf.io/y8adn) adapted the fitness tracker promotion paradigm from Studies 7 and 8, varying search costs by manipulating whether individuals expected that

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[39] Study 1 (WTP subjective rating), Study 7 (WTP $ amount), and Study 16 (WTP $ amount).
[40] Mediation analyses (PROCESS, model 4; 10,000 bootstrap samples; Hayes 2018) found no indirect effect through website perceptions occurring for Study 1 (-.005, SE = .009, CI$_{95}$[-.025, .011]), Study 7 (1.879, SE = 1.913, CI$_{95}$[-1.482, 6.183]), or Study 16 (-1.108, SE = .680, CI$_{95}$[-2.548, .091]). All results were robust to the exclusion of covariates.

searching online would be faster or slower than normal. Participants (n = 577 MTurkers) viewed a promotional offer (with or without time scarcity) and then indicated how likely they would be to take the deal without searching other online retailers for a better price and the amount they would be willing to pay for the promoted product. No interactive effects, however, emerged for either outcome.[41] The second study (https://osf.io/y2afz) employed an incentive-compatible and behaviorally consequential design involving new type of wireless earbuds. Participants (n = 295 students) saw an online promotion (with or without time scarcity) and then selected whether to buy the item from that online retailer at the promoted price or search other online retailers' prices for the same product. To manipulate search costs, participants learned that they would have access to either a price comparison aggregator website (easier search) or would have to search each retailer website individually (harder search). Again, however, results showed no moderating effect of search costs.[42] Although these studies represent only two of many ways online search costs can be experimentally operationalized—and do not reflect a comprehensive test of the search cost prediction—these preliminary findings nevertheless highlight that increased online search costs may not uniformly enhance susceptibility to online time scarcity promotions.

Relatedly, it is possible that the market-level changes instigated by the introduction of the internet has shifted when in the consumer decision-making process online time scarcity tactics are more likely to be effective. Rather than directly influencing purchase likelihood or product valuation, for example, online time scarcity promotions may be increasing the likelihood that the promoted product is included in a consumer's consideration set. We conducted a preliminary study

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[41] Results from a linear regression predicting participants' reported likelihood of taking the deal without searching as a function of online promotion type (control, time scarcity with countdown timer, time scarcity without countdown timer), search costs (high, low), and their interaction, controlling for pre-registered covariates, showed no significant promotion type x search cost interaction ($F(2,567) = .57$, $p = .566$). Conducting same analysis on WTP similarly showed no interactive effect ($F(2,567) = .09$, $p = .911$).

[42] Results from a binary logistic regression predicting participants' likelihood of taking the deal without searching (1 = purchased with promotion, 0 = searched for better price) as a function of online promotion type (control, time scarcity with countdown timer, time scarcity without countdown timer), search costs (high, low), and their interaction, controlling for pre-registered covariates, showed no significant promotion type x search cost interaction ($\chi^2(2) = 1.25$, $p = .536$).

testing the prediction that online time scarcity promotions will be more effective than online control promotions in increasing consideration of (but not willingness to pay for) the promoted product. In this study (https://osf.io/zmrf4), participants (n = 422 MTurkers) imagined shopping for a new desk chair and examined an online retailer's assortment. Holding the discount constant across all presented products, one item in the assortment was randomly identified as the focal product (promoted with or without time scarcity). With limited product information provided, individuals clicked all items for which they desired more detail and then reported their willingness to pay for the focal promoted product. Consideration was operationalized as whether a person requested additional information about the focal promoted product (0 = did not consider product, 1 = did consider product). No difference in willingness to pay emerged for those exposed to the online time scarcity promotion and the control promotion, consistent with the pattern of results summarized in this paper. Exposure to online time scarcity promotions, however, did not increase the likelihood of consumers including the promoted product in their consideration set.[43] Although this preliminarily result suggests that the observed weakened effect of online time scarcity promotions on consumer WTP may not correspond to observed increases in consumer consideration, future research can test whether such effects emerged with alternative manifestations of online consideration.

We recognize that this paper's emphasis on time scarcity tactics precludes empirical assertions regarding the efficacy of quantity online scarcity promotions or combination (time and quantity) online scarcity promotions. Prior research has suggested that scarcity promotions employ temporal restrictions more frequently than quantity restrictions (Howard et al. 2007). Anecdotally,

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[43] Results from a binary logistic regression predicting participants' likelihood of including the focal promoted product in their consideration set (1 = selected, 0 = not selected) as a function of online promotion type (control, time scarcity with countdown timer), controlling for pre-registered covariates, showed no significant effect of promotion type ($\chi^2(1) = .37$, $p = .541$). Conducting a similar linear regression analysis to predict WTP also showed no main effect of promotion type (F(1,415) = .01, $p = .906$).

however, the simultaneous implementation of both time and quantity scarcity appears to be increasingly adopted by retailers (e.g., "23% claimed, Ends in 37 minutes: 03 seconds"), with limited empirical research. Although our work does not directly test the effectiveness of blending multiple scarcity marketing tactics, future research examining the nature of such effects can further inform new theory regarding influence of online time scarcity promotions.

We encourage future research to examine these questions and to undertake broader theoretical inquiry regarding online time scarcity promotions. Researchers exploring time scarcity tactics can feel emboldened by our work to undertake efforts to articulate a truly new theory of online time scarcity promotions, which likely integrates aspects of both past findings and contemporary phenomena. We believe that the present work provides a strong theoretical and empirical basis to warrant such investments of time, thought, and enthusiasm.

Appendix A (Essay 1) Study 1: Stimuli

Survey Page	Content Presented
Page 1	Imagine the following scenario:
	You receive an email on Wednesday from Apple. The email tells you about an upcoming iphone sale that starts on Sunday and runs for two weeks.
	You are really excited because Apple rarely offers discounts!
Page 2	Look at part of the email (displayed below) and consider the discount Apple is offering.
	(You may be asked about details of the email later in the HIT)
	[EMAIL 1 CONTENT]
Page 3	You don't want to miss out on these savings, and decide to purchase a new iphone online from Apple during their two-week sale.
	However, Apple's sale hasn't started yet, so you have to remember to make the purchase sometime in the two weeks after the sale starts.
	You'd like to buy your iphone before the first week of the sale ends, because that's when you'd get the largest discount possible.
	[EMAIL 1 CONTENT]
Page 4	Days pass, and you go about your normal routine.
Page 5	It is now **[EMAIL 2 DAY]**, and you haven't taken advantage of the discount offer.
	When checking your email, you read a NEW message from Apple.
	The email reminds you about Apple's ongoing iphone sale. Look at part of the email (displayed below)
	(You may be asked about details of the email later in the HIT)
	[EMAIL 2 CONTENT]
Page 6	**[EMAIL 2 SUMMARY]**
	[EMAIL 2 CONTENT]
	INSTRUCTIONS: On the next few pages, complete the items as if you were in this scenario.

Content	Condition	Text or Visual Specific to Condition
Email 1 Content	Planning Prompt	

Email 1 Content	Control		

Email 2 Day	Before Optimal Deadline	Friday
	After Optimal Deadline	Monday

Email 2 Content	Planning Prompt & Before Optimal Deadline	
	Control & Before Optimal Deadline	
	Planning Prompt & After Optimal Deadline	

Email 2 Content	Control & After Optimal Deadline	

TWO-WEEK SALE!
Don't miss this special offer!

| Sun | Mon | Tues | Wed | Thurs | Fri | Sat |

Week 1: **45% off**

Today! Week 2: **25% off**

Email 2 Summary	Planning Prompt & Before Optimal Deadline	SUMMARY: In this scenario, Apple offers a two-week sale: 45% off during week 1 and 25% off during week 2. You receive an email during the first week of the promotion that reminds you to make a plan and take advantage of the 45% off discount available.
	Control & Before Optimal Deadline	SUMMARY: In this scenario, Apple offers a two-week sale: 45% off during week 1 and 25% off during week 2. You receive an email during the first week of the promotion that reminds you to take advantage of the 45% off discount available.
	Planning Prompt & After Optimal Deadline	SUMMARY: In this scenario, Apple offers a two-week sale: 45% off during week 1 and 25% off during week 2. You receive an email during the second week of the promotion that reminds you to make a plan and take advantage of the 25% off discount available.
	Control & After Optimal Deadline	SUMMARY: In this scenario, Apple offers a two-week sale: 45% off during week 1 and 25% off during week 2. You receive an email during the second week of the promotion that reminds you to take advantage of the 25% off discount available.

Appendix B (Essay 1) Study 1: Item and Response Scales

Index of Perceived Benefit
(α = .934)
1. Discount I would get is a great deal. (1 = Strongly Disagree, 7 = Strongly Agree)
2. Taking advantage of the discount would feel like a big gain. (1 = Strongly Disagree, 7 = Strongly Agree)
3. How would you rate the benefit of getting this discount? (1 = Not Beneficial at All, 7 = Very Beneficial)

Likelihood to Engage in Planning Behavior
1. Your day is incredibly busy when you receive this email. To take the time to take advantage of this discount, you need to plan to do it. For example, you might set a reminder, pick a time and place to make the online purchase, or schedule time in your calendar. How likely would you be to engage in any planning behavior in order to make your purchase? (1 = Not at All Likely, 7 = Very Likely)

Rosenberg Self-Esteem (RSE) Scale
(Rosenberg 1965; α = .913)
1. I feel that I am a person of worth, at least on an equal plane with others. (1 = Strongly Disagree, 7 = Strongly Agree)
2. I feel that I have a number of good qualities. (1 = Strongly Disagree, 7 = Strongly Agree)
3. All in all, I am inclined to feel that I am a failure. (1 = Strongly Disagree, 7 = Strongly Agree) *reverse coded*
4. I am able to do things as well as most other people. (1 = Strongly Disagree, 7 = Strongly Agree)
5. I feel I do not have much to be proud of. (1 = Strongly Disagree, 7 = Strongly Agree) *reverse coded*
6. I take a positive attitude toward myself. (1 = Strongly Disagree, 7 = Strongly Agree)
7. On the whole, I am satisfied with myself. (1 = Strongly Disagree, 7 = Strongly Agree)
8. I wish I could have more respect for myself. (1 = Strongly Disagree, 7 = Strongly Agree) *reverse coded*
9. I certainly feel useless at times. (1 = Strongly Disagree, 7 = Strongly Agree) *reverse coded*
10. At times I think I am no good at all. (1 = Strongly Disagree, 7 = Strongly Agree) *reverse coded*

Self-Efficacy

(adapted from Judge, Locke, Durham, and Kluger 1998; α = .922)
1. I feel confident in my ability to follow-through with what I intend to do. (1 = Not at All True of Me, 7 = Very True of Me)
2. I am capable of following-through with what I intend to do. (1 = Not at All True of Me, 7 = Very True of Me)
3. I am able to follow-through with what I intend to do. (1 = Not at All True of Me, 7 = Very True of Me)
4. I feel able to meet the challenge of following-through with what I intend to do. (1 = Not at All True of Me, 7 = Very True of Me)

Internal Locus of Control (Reduced-Item Scale)
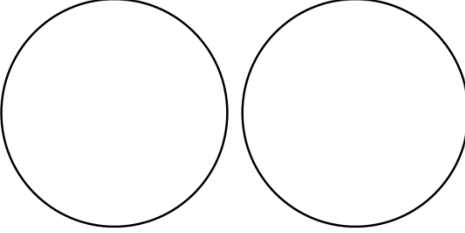
(used by Hoffman, Novak, and Schlosser 2003; α = .726)
1. How many friends I have depends on how nice a person I am. (1 = Strongly Disagree, 7 = Strongly Agree)
2. My life is determined by my own actions. (1 = Strongly Disagree, 7 = Strongly Agree)
3. I can pretty much determine what will happen in my life. (1 = Strongly Disagree, 7 = Strongly Agree)
4. When I make plans, I am almost certain to make them work. (1 = Strongly Disagree, 7 = Strongly Agree)
5. When I get what I want, it's usually because I worked hard for it. (1 = Strongly Disagree, 7 = Strongly Agree)
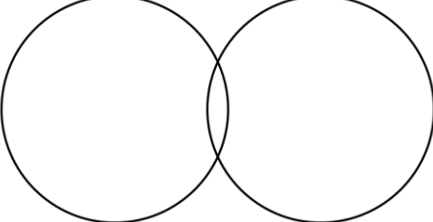
Psychological Distance
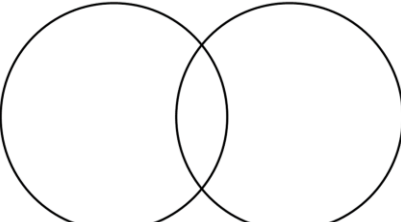
(adapted from Dai, Milkman, and Riis 2015; α = .810)
1. How far would you feel today (being reminded of the sale) from your past self (when first learning about the sale)? Please select the one diagram out of the seven below that best reflects your feelings. No overlap means "extremely far away" and complete overlap means "extremely close." *reverse coded*
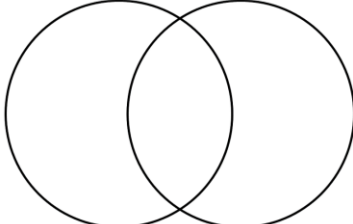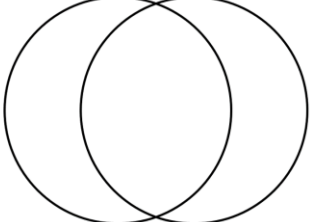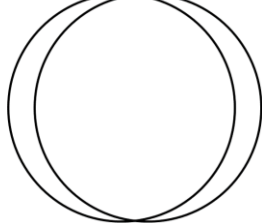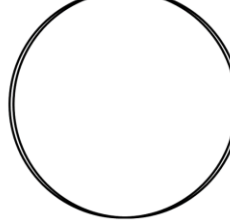
○
Your <u>Past</u> Self

(when you first learned
about the sale)

You <u>Today</u>

(being reminded about
the sale)

○
Your <u>Past</u> Self

(when you first learned
about the sale)

You <u>Today</u>

(being reminded about
the sale)

○
Your <u>Past</u> Self

(when you first learned
about the sale)

You <u>Today</u>

(being reminded about
the sale)

○
Your <u>Past</u> Self

(when you first learned
about the sale)

You <u>Today</u>

(being reminded about
the sale)

○
Your <u>Past</u> Self

(when you first learned
about the sale)

You <u>Today</u>

(being reminded about
the sale)

○
Your <u>Past</u> Self

(when you first learned
about the sale)

You <u>Today</u>

(being reminded about
the sale)

○
Your <u>Past</u> Self

(when you first learned
about the sale)

You <u>Today</u>

(being reminded about
the sale)

2. To what extent would you now (being reminded of the sale) feel <u>*distant*</u> from your past self (when first learning about the sale)? (1 = Extremely Close, 7 = Extremely Far Away)
3. To what extent would you now (being reminded of the sale) feel <u>*different*</u> from your past self (when first learning about the sale)? (1 = Exactly the Same, 7 = Completely Different)

Perceived Fairness
(adapted from Darke and Dahl 2003; α = .864)
1. Given the discount available to me, the price I'd pay for the iphone is **fair**. (-3 = Disagree, 3 = Agree)
2. Given the discount available to me, the price I'd pay for the iphone is **questionable**. (-3 = Disagree, 3 = Agree) *reverse coded*
3. Given the discount available to me, the price I'd pay for the iphone is **justified**. (-3 = Disagree, 3 = Agree)
4. Given the discount available to me, the price I'd pay for the iphone is **honest**. (-3 = Disagree, 3 = Agree)
5. Given the discount available to me, the price I'd pay for the iphone is **unfair**. (-3 = Disagree, 3 = Agree) *reverse coded*
6. Given the discount available to me, the price I'd pay for the iphone is **a "rip-off."** (-3 = Disagree, 3 = Agree) *reverse coded*

Appendix C (Essay 1) Study 2: Lottery Schedule

Wave	End Date	Launch Date	Number of Lottery Entries for Each Date																Lottery
			6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	
1	11/15	11/7 (W) @ 1:30pm EST	20	20	15	15	10	10	5	5	1	1							11/26
2	11/16	11/8 (TH) @ 1:30pm EST		20	20	15	15	10	10	5	5	1	1						11/26
3	11/17	11/9 (F) @ 1:30pm EST			20	20	15	15	10	10	5	5	1	1					11/26
4	11/18	11/10 (SA) @ 1:30pm EST				20	20	15	15	10	10	5	5	1	1				11/26
5	11/19	11/11 (SU) @ 1:30pm EST					20	20	15	15	10	10	5	5	1	1			11/26
6	11/20	11/12 (M) @ 1:30pm EST						20	20	15	15	10	10	5	5	1	1		11/26
7	11/21	11/13 (T) @ 1:30pm EST							20	20	15	15	10	10	5	5	1	1	11/26

Note. The survey informing individuals of the lottery was launched at 1:30 pm EST each day of the week for seven days. The entry website was open daily (from 9:00 am until 1:00 pm EST) throughout each wave's eight-day lottery entry window.

Appendix D (Essay 1) Study 2: Stimuli

Day 1: survey launches at 1:30 pm (EST), participant gets code
Day 2: lottery opens (first day participant can enter)
Days 2 & 3: 15 entries
Days 4 & 5: 10 entries
Days 6 & 7: 5 entries
Days 8 & 9: 1 entry
Day 9: lottery closes (last day participant can enter)
Day 11: drawing for each of the incentive winners; winners emailed

Survey Page	Content Presented
Page 1	Congratulations! You are eligible to participate in a lottery to win a $5 [$20] bonus!!! (details on next page)
Page 2	*[TAGLINE]* What do I win? $$$$$! A **$5 [$20]** MTurk bonus! What do I have to do? Enter a code! A qualtrics survey (titled "BONUS LOTTERY ENTRY") is available online (link provided below). Go to this qualtrics link, where you will complete a few brief survey items about yourself, and then enter the code below. The survey will be active between 9am and 1pm (EST) every day **[*START DATE*]** through **[*END DATE*]**. By entering your code during this time, you will be entered to win! LOTTERY ENTRY CODE: [personalized code] *this is not your HIT code for today's HIT* LOTTERY ENTRY LINK: [lottery entry website link] Do I only get one entry? No! Entering earlier gets you EXTRA entries! You can only enter your code once but entering earlier gets you extra lottery entries. Here's the schedule: *[LOTTERY SCHEDULE 1]* Who can enter the lottery? Only MTurkers who complete this HIT. The lottery is only open to MTurkers who participated in the HIT you just completed. After the lottery closes, one entry will be randomly selected as the winner of the **$5 [$20]** bonus.
Page 3	*[TAGLINE]* LOTTERY ENTRY CODE: [personalized code] *this is not your HIT code for today's HIT* LOTTERY ENTRY LINK: [lottery entry website link] *[PAGE 2 CONTENT]* REMEMBER: *[LOTTERY SCHEDULE 2]*
Page 4	*[TAGLINE]* INSTRUCTIONS: STEP 1: Copy your personal entry code: [personalized code] STEP 2: Copy this link: [lottery entry website link] STEP 3: *[STEP 3]*, from 9am - 1pm (EST) one time from **[*START DATE*]** to **[*END DATE*]**. STEP 4: Check your email on 11/26. A winner (hopefully, you!) will be randomly drawn on 11/26 and will be notified via email through MTurk on the same day.
Page 5	*[TAGLINE]* Here is a final summary of the critical lottery info. **BONUS: [$5/$20]** **ENTRY CODE:** [personalized code] **LINK:** [lottery entry website link] *[LOTTERY SCHEDULE 2]* Entry window closes at 1:00PM (EST) on **[*END DATE*]**. Winner will be randomly drawn and notified on 11/26.

Content	Condition	Text Specific to Condition
Tagline	Planning Prompt	Your calendar could get crowded--Make a plan to enter early to lock down your chance to win!
	Control	Enter early to lock down your chance to win!
Start Date* & End Date*	Before Optimal Deadline	Start Date: 11/8; End Date: 11/15
	After Optimal Deadline	Start Date: 11/6 End Date: 11/15

Content	Condition		
Lottery Schedule 1*	Before Optimal Deadline	Enter Code Between:	Total # of Entries:
		11/8 and 11/9	15 entries
		11/10 and 11/11	10 entries
		11/12 and 11/13	5 entries
		11/14 and 11/15	1 entry
	After Optimal Deadline	Enter Code Between:	Total # of Entries:
		~~11/6 and 11/7~~	~~20 entries~~
		11/8 and 11/9	15 entries
		11/10 and 11/11	10 entries
		11/12 and 11/13	5 entries
		11/14 and 11/15	1 entry

Content	Condition	Text Specific to Condition
Page 2 Content	Planning Prompt	The busier you are, the more important it is to plan to enter your code early! The entry window opens soon!
		Participants can enter their code as early as *[START DATE]*. It's a busy time of year, so plan on setting aside time to do this!
		Entries can only be made during a specific period of time. This means that prioritizing entering your code early could help increase your chances of winning. Make a plan now to enter the lottery early! Schedule time on your calendar now to do this tomorrow between 9am and 1pm EST!
	Control	The entry window opens soon!
		Participants can enter their code as early as *[START DATE]*.
		Entries can only be made during a specific period of time. Entering your code early could help increase your chances of winning.
Lottery Schedule 2*	Before Optimal Deadline	Enter between 9AM and 1PM (EST) on 11/8 and 11/9 = 15 chances to win [$5/$20] Enter between 9AM and 1PM (EST) on 11/10 and 11/11 = 10 chances to win [$5/$20] Enter between 9AM and 1PM (EST) on 11/12 and 11/13 = 5 chances to win [$5/$20] Enter between 9AM and 1PM (EST) on 11/14 and 11/15 = 1 chance to win [$5/$20]
	After Optimal Deadline	~~Enter between 9AM and 1PM (EST) on 11/6 and 11/7 = 20 chances to win [$5/$20]~~ Enter between 9AM and 1PM (EST) on 11/8 and 11/9 = 15 chances to win [$5/$20] Enter between 9AM and 1PM (EST) on 11/10 and 11/11 = 10 chances to win [$5/$20] Enter between 9AM and 1PM (EST) on 11/12 and 11/13 = 5 chances to win [$5/$20] Enter between 9AM and 1PM (EST) on 11/14 and 11/15 = 1 chance to win [$5/$20]
Step 3	Planning Prompt	Put a reminder in your phone or your calendar right now to enter the lottery
	Control	Enter the lottery

* Specific dates presented depended on the day of administration. The dates provided in this table correspond to those presented on the first administration day (November 7, 2018). The code entry survey was deactivated outside of 9am EST and 1pm EST each day.

Appendix E (Essay 1) Study 3: Email 2 Stimuli

BEFORE OPTIMAL DEADLINE CONDITIONS

Control Condition

Subject Line: Haven't filed FAFSA? You HAVEN'T missed the "earliest bird" window.

You Haven't Missed the Early Bird Submission Window!
File FAFSA ASAP!

You haven't missed [UNIVERSITY]'s "Earliest Bird" FAFSA filing recommendation of 10/21; so submit soon and you'll still be earlier than most!

A year from now, you'll be thinking about classes and a new academic year – you'll also want to be confident you can pay your tuition bills.

REMEMBER: Some funding is limited and distributed on a first-come, first-served basis. So start your FAFSA early! Although [UNIVERSITY] **recommends submitting FAFSA by October 21st (in 4 days)**, submitting as soon as possible still can help ensure that you do not miss out on available aid.

Start your FAFSA right away!

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

Critical Deadlines:
10/1/18   First day you can file the FAFSA
10/21/18   **Last day you can meet [UNIVERSITY]'s "earliest bird" FAFSA filing recommendation**
3/1/19   Last day you can file the FAFSA for full financial aid
6/30/19   Last day you can file the FAFSA

- If you haven't already, Click to get your FSA ID. You'll need this to start the FAFSA.
- Collect these things so you're ready. (SIMPLIFIED! You may be able to instantly upload tax return information into your FAFSA using the IRS Data Retrieval Tool!)
- **File your FAFSA by (Sunday) 10/21!**

START YOUR FAFSA TODAY by clicking here.

And when you have questions, [UNIVERSITY] is always here to help. Contact us to speak with a financial aid advisor: [EMAIL CONTACT] or [PHONE CONTACT].

Planning Prompt Condition

Subject Line: Haven't filed FAFSA? You HAVEN'T missed the "earliest bird" window. Plan to file ASAP!

You Haven't Missed the Early Bird Submission Window!
Make a Plan to File FAFSA ASAP!

You haven't missed [UNIVERSITY]'s "Earliest Bird" FAFSA filing recommendation of 10/21; so submit soon and you'll still be earlier than most!

If you made a plan to start FAFSA today, stick with it!
If you can't start today, then schedule a day when you will.

A year from now, you'll be thinking about classes and a new academic year – you'll also want to be confident you can pay your tuition bills. **Planning ahead can help.**

REMEMBER: Some funding is limited and distributed on a first-come, first-served basis. So start your FAFSA early! Although [UNIVERSITY] **recommends submitting FAFSA by October 21st (in 4 days)**, submitting as soon as possible still can help ensure that you do not miss out on available aid.

Make a plan now to start your FAFSA right away!
Write it in your calendar, put a reminder in your phone, or set an alarm.

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

Critical Deadlines:
10/1/18   First day you can file the FAFSA
10/21/18   **Last day you can meet [UNIVERSITY]'s "earliest bird" FAFSA filing recommendation**
3/1/19   Last day you can file the FAFSA for full financial aid
6/30/19   Last day you can file the FAFSA

- ✓ If you haven't already, Click to get your FSA ID. You'll need this to start the FAFSA.
- ✓ Collect these things so you're ready. (SIMPLIFIED! You may be able to instantly upload tax return information into your FAFSA using the IRS Data Retrieval Tool!)
- ✓ **Put a reminder in your phone or on your calendar right now to file your FAFSA by (Sunday) 10/21!**

START YOUR FAFSA TODAY by clicking here. Plan to get started soon.

And when you have questions, [UNIVERSITY] is always here to help. Contact us to speak with a financial aid advisor: [EMAIL CONTACT] or [PHONE CONTACT].

AFTER OPTIMAL DEADLINE CONDITIONS

Control Condition

Planning Prompt Condition

Subject Line: Haven't filed FAFSA? You JUST missed the "earliest bird" window. Plan to file ASAP!

Subject Line: Haven't filed FAFSA? You JUST missed the "earliest bird" window.

You *Just* Missed the Early Bird Submission Window!
File FAFSA ASAP!

You *just* missed [UNIVERSITY]'s "Earliest Bird" FAFSA filing recommendation of 10/21; but submit soon and you'll still be earlier than most!

A year from now, you'll be thinking about classes and a new academic year – you'll also want to be confident you can pay your tuition bills.

REMEMBER: Some funding is limited and distributed on a first-come, first-served basis. So start your FAFSA early! Although [UNIVERSITY] **recommends submitting FAFSA by October 21st (3 days ago)**, submitting as soon as possible still can help ensure that you do not miss out on available aid.

Start your FAFSA right away!

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 Deadline!	22	23	24 Today!	25	26	27
28	29	30	31			

Critical Deadlines:
10/1/18   First day you can file the FAFSA
10/21/18   **Last day you can meet [UNIVERSITY]'s "earliest bird" FAFSA filing recommendation**
3/1/19   Last day you can file the FAFSA for full financial aid
6/30/19   Last day you can file the FAFSA

- If you haven't already, <u>Click to get your FSA ID</u>. You'll need this to start the FAFSA.
- Collect <u>these things</u> so you're ready. (SIMPLIFIED! You may be able to instantly upload tax return information into your FAFSA using the IRS Data Retrieval Tool!)
- **File your FAFSA as soon as possible!**

START YOUR FAFSA TODAY <u>by clicking here.</u>

And when you have questions, [UNIVERSITY] is always here to help. Contact us to speak with a financial aid advisor: [EMAIL CONTACT] or [PHONE CONTACT].

You *Just* Missed the Early Bird Submission Window!
Make a Plan to File FAFSA ASAP!

You *just* missed [UNIVERSITY]'s "Earliest Bird" FAFSA filing recommendation of 10/21; but submit soon and you'll still be earlier than most!

If you made a plan to start FAFSA today, stick with it!
If you can't start today, then schedule a day when you will.

A year from now, you'll be thinking about classes and a new academic year – you'll also want to be confident you can pay your tuition bills. **Planning ahead can help.**

REMEMBER: Some funding is limited and distributed on a first-come, first-served basis. So start your FAFSA early! Although [UNIVERSITY] **recommends submitting FAFSA by October 21st (3 days ago)**, submitting as soon as possible still can help ensure that you do not miss out on available aid.

Make a plan now to start your FAFSA right away!
Write it in your calendar, put a reminder in your phone, or set an alarm.

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 Deadline!	22	23	24 Today!	25	26	27
28	29	30	31			

3/1/19   Last day you can file the FAFSA for full financial aid
6/30/19   Last day you can file the FAFSA

- If you haven't already, <u>Click to get your FSA ID</u>. You'll need this to start the FAFSA.
- Collect these things so you're ready. (SIMPLIFIED! You may be able
- ✓ If you haven't already, <u>Click to get your FSA ID</u>. You'll need this to start the FAFSA.
- ✓ Collect <u>these things</u> so you're ready. (SIMPLIFIED! You may be able to instantly upload tax return information into your FAFSA using the IRS Data Retrieval Tool!)
- ✓ **Put a reminder in your phone or on your calendar right now to file your FAFSA as soon as possible!**

START YOUR FAFSA TODAY <u>by clicking here.</u> Plan to get started soon.

And when you have questions, [UNIVERSITY] is always here to help. Contact us to speak with a financial aid advisor: [EMAIL CONTACT] or [PHONE CONTACT].

Appendix F (Essay 2) Study 1: Sample Online Donation Stimuli

Recurring Donation Condition

One-Time Donation Condition

Appendix G (Essay 2) Study 2: Parallel Mediation Results

	Consequent											
	M_1 (Perceived Size)			M_2 (Experienced Happiness)			M_3 (Charity Commitment)			Y (Donation Amount)		
Antecedent	Coeff.	SE	p	Coeff.	SE	p	Coeff.	SE	p	Coeff.	SE	p
D_1 (Recurring w/not.)	-10.4327	3.1766	.0012	.0224	.1900	.9061	-.3973	.3349	.2365	-.0268	.0206	.1947
D_2 (Recurring w/out not.)	-8.0797	3.1533	.0109	-.0117	.1886	.9507	.1214	.3324	.7152	-.0335	.0203	.1009
M_1 (Perceived Size)	−	−	−	−	−	−	−	−	−	.0012	.0004	.0031
M_2 (Experienced Happiness)	−	−	−	−	−	−	−	−	−	.0126	.0075	.0924
M_3 (Charity Commitment)	−	−	−	−	−	−	−	−	−	.0122	.0043	.0051
C_1 (Income)	$< .0001$	$< .0001$	.2699	$< .0001$	$< .0001$	.1343	$< .0001$	$< .0001$	.5712	$< .0001$	$< .0001$	.2592
Constant	23.2730	2.7492	$< .0001$	5.3947	.1644	$< .0001$	5.0258	.2898	$< .0001$	-.0415	.0385	.2815
	$R^2 = .0438$			$R^2 = .0080$			$R^2 = .0101$			$R^2 = .1449$		
	$F(3,283) = 4.3169$, $p = .0054$			$F(3,283) = .7616$, $p = .5164$			$F(3,283) = .9663$, $p = .4090$			$F(6,280) = 7.9075$, $p < .0001$		

Note. Conducted using PROCESS 3.0 (model 4; 10,000 bootstrapped samples) with a multicategorical IV (one-time donation, recurring donation with notification, and recurring donation without notification).

Appendix H (Essay 2) Study 4: Moderation Results

	Coeff.	SE	t	p
X (Recurring Donation)	-1.9298	28.4184	-.0679	.9460
W (Initial Amount)	1.1107	.2838	3.9145	.0001
Z (Religiosity)	10.7309	5.7906	1.8532	.0660
XW (Recurring Donation x Initial Amount)	-.9623	.3629	-2.6520	.0090
XZ (Recurring Donation x Religiosity)	.9009	8.0333	.1121	-.9623
WZ (Initial Amount x Religiosity)	-.2280	.0705	-3.2357	.0015
XWZ (Recurring Donation x Initial Amount x Religiosity)	.1946	.0868	2.2422	.0266
C_1 (Income)	< .0001	< .0001	-.2832	.7775
Constant	-12.1420	20.9188	-.5804	.5626

$$R^2 = .1932$$

$$F(8,136) = 4.0711, \; p = .0002$$
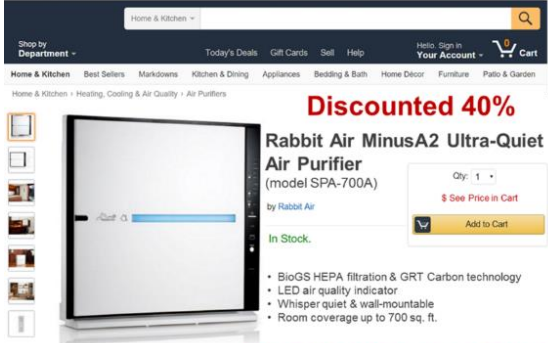
Note. Conducted using PROCESS 3.0 (model 3; 10,000 bootstrapped samples).

Appendix I (Essay 2) Study 5A: Parallel Mediation Results

Antecedent	M$_1$ (Perceived Size)			M$_2$ (Experienced Happiness)			M$_3$ (Charity Commitment)			Y (Donation Amount)		
	Coeff.	SE	p	Coeff.	SE	p	Coeff.	SE	p	Coeff.	SE	p
X (Recurring Donation)	2.4596	4.6381	.5965	.3737	.1745	.0334	.8402	.2874	.0038	-.1697	.4961	.0008
M$_1$ (Perceived Size)	–	–	–	–	–	–	–	–	–	.0004	.0007	.6268
M$_2$ (Experienced Happiness)	–	–	–	–	–	–	–	–	–	.0490	.0257	.0574
M$_3$ (Charity Commitment)	–	–	–	–	–	–	–	–	–	.0158	.1563	.3139
C$_1$ (Income)	.00002	.00003	.5044	-.000001	.000001	.4383	-.000001	.000002	.6897	< .0001	< .0001	.0990
C$_1$ (Mediator Timing)	-9.9893	4.6226	.0318	-.4357	.1739	.0130	-1.0085	.2864	.0005	.0441	.0502	.3800
Constant	37.6232	3.9678	< .0001	5.8312	.1493	< .0001	6.7205	.2458	< .0001	.0690	.1211	.5693
	$R^2 = .0250$			$R^2 = .0482$			$R^2 = .0858$			$R^2 = .1028$		
	$F_{(3,207)} = 1.7711,\ p = .1538$			$F_{(3,207)} = 3.4909,\ p = .0166$			$F_{(3,207)} = 6.4775,\ p = .0003$			$F_{(6,204)} = 3.8972,\ p = .0010$		

Note. Conducted using PROCESS 3.0 (model 4; 10,000 bootstrapped samples). Control variables include (mean-centered) income and timing of mediator measurement (0 = after initial donation request, 1 = after subsequent donation request).

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Appendix J (Essay 3) Online Time Scarcity Promotion Manipulations

Study 1	Study 2
Countdown timer started at 1 hour: 26 minutes: 40 seconds	*Countdown timer started at 1 hour: 26 minutes: 40 seconds*
Study 3	Study 4, 5
Countdown timer started at a randomly selected time between 8 minutes: 40 seconds & 1 hour: 18 minutes: 0 seconds	*Countdown timer started at 1 hour: 26 minutes: 40 seconds*

Study 6	Study 7
Countdown timer started at a randomly selected time between 8 minutes: 40 seconds & 1 hour: 18 minutes: 0 seconds	*Countdown timer started at a randomly selected time between 8 minutes: 40 seconds & 1 hour: 18 minutes: 0 seconds*

Study 8	Study 9, 10
Countdown timer started at a randomly selected time between 8 minutes: 40 seconds & 1 hour: 18 minutes: 0 seconds	*Countdown timer started at a randomly selected time between 2 minutes: 0 seconds & 2 hours: 0 minutes: 0 seconds*

Study 11	Study 12, 13
Countdown timer started at a randomly selected time between 8 minutes: 40 seconds & 1 hour: 18 minutes: 0 seconds	*Countdown timer started at a randomly selected time between 8 minutes: 0 seconds & 24 hours: 0 minutes: 0 seconds*

Study 14 0 days 1 hour 26 minutes 28 seconds ...time left until this deal expires! *Countdown timer started at 8 minutes: 0 seconds or 1 hour: 26 minutes: 40 seconds*	**Study 15** *Countdown timer started at 12 minutes: 0 seconds, 48 minutes: 0 seconds, 3 hours: 12 minutes: 0 seconds, or 15 hours: 12 minutes: 0 seconds*
Study 16, 17, 18, 19, 20, and 21 *Countdown timer started at 8 minutes: 40 seconds or 23 hours: 8 minutes: 40 seconds*	**Study 22** MTurk HIT Title content varied between conditions. *Title read "Only 45 minutes left to participate in this consumer behavior study! Click now!"*

Study 23	Study 24
Time is Running Out: **Create a Holiday Gift to Last a** **Lifetime!** Everyone can sing - especially when it comes to holiday carols and Christmas classics! This year you can give the gift of music by recording your own professional, party-ready production of your favorite holiday hits. Whether you prefer to sing solo or with the family, there's no better way to capture a memory to share with relatives or generations to come. **Come With Your Talent, Leave with a Radio-Ready** **Production** Our Holiday Recording Packages include a variety of recording options including audio or video, as well as media types, such as flash drive, CD, and even DVD (video only). You bring the talent, we'll take care of all the rest, including copyright, recording artist fees, and instrumental backtracks. (*Applies to songs included on our song list only.*) Visit our website to learn more and schedule your Holiday Recording Package, plus discover why Lone Star School of Music is the fastest growing music school in Austin. **Limited Time Offer:** **Use this Link for 10% off the Holiday Hits Package of your Choice**	

Study 25	Study 26
	 Countdown timer started at 20 seconds or 3 hours: 24 minutes: 20 seconds

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