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OVERVIEW

Consumers' lives are filled with choices between new and old. For example, consumers regularly decide between exciting new dishes vs. their go-to orders, the latest blockbusters vs. their favorite films, and strange new travel destinations vs. their tried-and-true vacation spots. In my dissertation research, I explore how consumers navigate this tradeoff to better understand what consumers prefer, why their preferences change, and how different consumption contexts can influence their decisions.

Decades of marketing research have examined factors that drive novelty seeking vs. familiarity seeking. The current research attempts to build on this literature in two ways. The first is by exploring how broad mental frameworks consumers use to navigate the world—specifically social and temporal frameworks—organize consumers' novelty and familiarity preferences. The second is by exploring how consumers prefer novelty and familiarity to coexist within experiences.

In Chapter 1 (published at the Journal of Personality and Social Psychology), I investigate how social context shapes novelty and familiarity preferences. Consumers frequently make decisions with other people, around other people, and using information that comes from other people. Whether and how others are involved can fundamentally shift the decision making process because sociality (i.e., the social/nonsocial nature of a target) is a fundamental, organizing principle consumers use to understand the world. This chapter examines how merely knowing that information has come from a social source (as opposed to a non-social or less socially-salient source), pushes consumers toward preferring novelty. Participants were presented with a minimalist and incentive-compatible "treasure hunting" paradigm in which they could choose between known (i.e., familiar) and unknown (i.e., novel) monetary payouts. I find

that participants explored unknown payouts (and forwent known payouts) more often when the known payouts had been uncovered by a social agent, rather than a non-social agent (e.g., a computer). I find that this behavior is driven by an inherent tendency to adopt a "we"-perspective (i.e., a group-level perspective), as opposed to a "me"- perspective (i.e., an individual-level perspective), when exploring with other humans. This chapter is joint work with Ayelet Fishbach, Shoham Choshen-Hillel, and Yanping Tu.

In Chapter 2 (published at the Journal of Personality and Social Psychology), I turn to a different ever-present mental framework that consumers use to navigate the world: temporal context. Consumers invariably experience life through the lens of time and sequence. They are perpetually starting, finishing, or somewhere in the middle of an experience or set of experiences. In this chapter, I explore how preferences for novelty and familiarity shift in the context of endings. Consumers generally aim to experience ending on a high note. Novel experiences could potentially match the occasion, given their inherently exciting nature (a possibility that is consistent with the popular notion of the "bucket list"). And yet, I instead find the opposite: Perceived endings shift consumer preferences toward familiar experiences and away from novel ones, even when the endings are only temporary. I find that this shift is driven by an increased desire to have meaningful experiences, even at the cost of excitement. It turns out the "high" notes are meaningful ones, and familiar experiences are a better match for ending occasions. This is joint work with Ed O'Brien.

In Chapter 3, I expand on the ideas in Chapter 2 by studying temporal context at a more granular, within-experience level. Existing research on novelty and familiarity seeking generally characterizes experiences as distinctly novel or familiar—and yet, many experiences are composed of both qualities. Experiences that unfold over time (e.g., narratives) often consist of a

series of novel and familiar moments in sequence, and by examining experiences at the within-experience level, we may begin to unpack consumer preferences for how novel and familiar elements should be arranged in sequence to be most favorable. This chapter focuses on one particular arrangement: the "callback." My findings show that consumers find experiences more favorable when the end of those experiences reintroduce a familiar element from earlier in the same experience while also imbuing them new insight. Further, I find that this effect is driven by a sense of "narrative guidance"—the idea that consumers value feeling that the creator of an experience has constructed the experience in a way that guides them through the narrative to interpret it in the most enjoyable and satisfying way.

Across these three chapters, this work demonstrates how the lenses of social context and time and sequence shape the ubiquitous decisions people make as they trade off novelty and familiarity.

Chapter 1

Social Exploration: When People Deviate from Options Explored by Others

Abstract

People often face choices between known options and unknown ones. Our research documents a social-exploration effect: People are more likely to explore unknown options when they learn about known options from other people's experiences. Across four studies (N = 2,333), we used an incentive-compatible paradigm where participants chose between known and unknown options (e.g., cash bonuses). We found higher exploration rates (i.e., choosing of unknown options) when information about known options came from other people, compared with an unidentified source (Experiment 1a) or a computer (Studies 1b-4). We theorize that the social-exploration effect results from people's tendency to intuitively adopt a group-level perspective with other people: a "we"-perspective. Thus, in social contexts, people explore more to diversify their experience as a group. Supporting this account, we find the effect attenuates in exploration of losses, where people do not wish to adopt a group-level perspective of others' losses (Experiment 2). Furthermore, the effect is obtained only if others have experienced the outcome; not when they only revealed its content (Experiment 3). Finally, the social-exploration effect generalizes to everyday choices, such as choosing a movie to watch (Experiment 4). Taken together, these findings highlight the social aspect of individual exploration decisions and offer practical implications for how to encourage exploration.

On your next night out, will you choose a restaurant you have heard of before or will you explore a new and unknown one? And if watching a movie instead, will you choose a movie a colleague or a website had mentioned, or explore one you have not yet heard of? Individuals often face decisions between capitalizing on options they know something about and exploring options that they know nothing about. Choosing known options means having a reliable and predictable experience, but choosing unknown options makes no such guarantees. Unknown options offer novelty and the potential to provide an even better experience than a known option while also carrying the risk of providing a worse one.

This research asks whether the information source matters for exploration. Holding constant the information about known options, we hypothesize that emphasizing that this information was obtained from social sources (i.e., other people's experiences) increases the motivation to explore as compared to no such mentioning. We dub this the social-exploration effect. We posit that this effect is driven by an intuitive tendency to take a group-level perspective (or "we"-perspective) when exploring an environment with others, even when no group membership is made explicit. This we-perspective makes people gravitate towards exploring new options to diversify their experiences as a group.

Exploration Behavior

What drives individuals to explore their environment? Whether a hummingbird chooses to keep drinking nectar from a flower or move to a new one, or whether a stock trader decides to keep her money invested in a particular stock or invest in a new one, exploration decisions may appear relatively simple. And yet, these choices represent complex tradeoffs between the benefits of certainty and the costs of uncertainty (Krebs, Kacelnik, & Taylor, 1978; Lee, Zhang, Munro, & Steyvers, 2011). Three main categories of factors have been found to influence the tendency to

explore (Melhorn et al., 2015). The first category includes environmental factors, such as the variation of the options (Cohen, McClure, & Yu, 2007; Lejarraga, Hertwig, & Gonzalez, 2012), the prevalence of rewards (Teodorescu & Erev, 2014a), and the number of decisions one expects to make (Wilson, Geana, White, Ludvig, & Cohen, 2014). A second category includes individual factors (Teodorescu & Erev, 2014b), such as the tendency to maximize (vs. satisfice; Schwartz et al., 2004), the desire for novelty (Hirschman, 1980), the desire for variety (Kahn, 1995) and curiosity (Golman, Loewenstein, Molnar, & Saccardo. 2019; Hsee & Ruan, 2016; Loewenstein, 1994). Most relevant to the current research is the third category: social factors that influence exploration.

Though relatively little research has examined social factors in exploration decisions, the influence of others in exploration is pervasive. Social influence may depend on the relationships with others, which vary between competitive, collaborative and neutral. In competitive contexts, multiple parties vie for the same resources, which increases the costs of exploration and thus inhibits it. For example, players in a competitive sampling game spend less time sampling balls from different urns before making a final choice, so they explore fewer possible payouts. Learning that another player can choose an urn and thus remove it from the choice set reduces exploration compared with choosing alone (Phillips, Hertwig, Kareev, & Avrahami, 2014). In collaborative contexts, multiple parties use group-level exploration strategies like role specialization, where different subdivisions of a group separately pursue exploration and exploitation to optimize group outcomes. For example, many bird species naturally organize themselves into subgroups of "producers" who explore new food sources and "scroungers," who secure those findings (Giraldeau & Lefebvre, 1986). Similarly, human organizations designate

certain subgroups to experiment with novel tactics and simultaneously designate others to leverage existing ones (Gupta, Smith, & Shalley, 2006).

Intuitive We-Perspective

People tend to use the word "we" when referring to others. Authors use the author's "we" as ambassadors of collective wisdom, doctors use "we" when referring to their patients (e.g., "How are we feeling today?" means "How are you feeling today?"), and waiters use the "royal" we when asking, "What are we in the mood for?" This tendency entails adopting an inclusive perspective that incorporates social others into one's own group by switching from a me-perspective (or "me-and-you"-perspective) to a we-perspective (Agnew, Van Lange, Rusbult, & Langston, 1998; Fitzsimons & Kay, 2004). This tendency is driven by an inherent motivation to share psychological states with others, which serves the adaptive purpose of facilitating coordination among group members (Tomasello, Carpenter, Call, Behne, & Moll, 2005; Tu & Fishbach, 2015; Tu, Shaw & Fishbach, 2016).

A we-perspective is similar to vicarious experience. Vicarious experience happens, for example, when merely watching a spider crawl on a stranger's body makes people shiver (Keysers et al., 2004), and when observing a stranger receive painful pinpricks activates the same pain-related brain regions as receiving pinpricks for oneself (Morrison, Lloyd, Di Pellegrino, & Roberts, 2004). Like vicarious experience, a we-perspective implies that people spontaneously expand their perspectives to integrate other people's experiences with their own. Yet, rather than seeing their own and others' experiences as identical, when adopting a we-perspective, individuals perceive both sets of experiences as components of a collective, group-level whole. For example, football fans use phrases like "we won" and "we got beat" after merely watching the victories or defeats of their team and do so more, in fact, in victory than

defeat (i.e., “cutting off reflected failure”; Snyder, Lassegard, & Ford, 1986). Similarly, Americans who have never left Earth regularly claim “we landed on the moon” ever since Neil Armstrong and his team stepped foot there (Lepore, 2019).

Specifically, a we-perspective does not imply confusion between one’s own and other people’s personal experiences (i.e., “inclusion of other in the self”; Aron, Aron, & Smollan, 1992; Aron, Aron, Tudor, & Nelson, 1991). For example, at no point do Americans misremember themselves as stepping foot on the moon. Instead, a we-perspective allows people to recognize and take into account differences between group members’ individual experiences to coordinate their behavior in complementary ways for the sake of the collective. That is, a we-perspective supports the pursuit of shared goals by effectively dividing labor within a group. For example, romantic couples spontaneously divide responsibilities by assigning one partner to manage finances on behalf of the couple (Ward & Lynch, 2018; see also Wegner, Raymond, & Erber, 1991).

While most of the evidence on we-perspective is in regard to close relationships, sharing others’ experiences at a group level does not require interpersonal connections or even apriori group associations. On the other hand, people are not likely to grant all social beings the in-group status necessary for group-level perspective-taking. When individuals are motivated not to affiliate with a given social other, such as when this person is known to be an out-group member, one should not expect a we-perspective. Thus, while taking a we-perspective is not limited to close relationships and extends to social groups of all sizes and degrees of familiarity, is not extended to disliked others.

The Social-Exploration Effect

The tendency to treat others, even strangers, as part of one's group and switch to a we-perspective may have implications for exploration decisions that individuals make for themselves. Specifically, a we-perspective may make the options that have not been experienced by others seem like opportunities to provide beneficial diversity to one's experience "as a group." In contrast, options that have been experienced by others may seem redundant for the group experience. Importantly, a given choice option can seem redundant at the group level because someone else tried it, but non-redundant at the individual level because it is still novel to the individual.

Typically, people value diversity in outcomes (Choi, Kim, Choi, & Yi, 2006). Variety seeking can occur for a number of interconnected reasons, such as to seek stimulation, to avoid repetition, to satisfy curiosity and to slow hedonic adaptation (Fishbach, Ratner, & Zhang, 2011; Hirschman, 1980; Kahn, 1995; McAlister & Pessemier, 1982; O'Brien, 2019; Raju, 1980; Ratner, Kahn, & Kahneman, 1999; Read & Loewenstein, 1995; Sheldon, Boehm, & Lyubomirsky, 2013; Simonson, 1990). While the tendency to explore is multidetermined, the social-exploration effect specifically refers to exploration that is driven by the social source of information. It predicts that people will explore more when the information comes from a social or more socially-salient source (e.g., other people), as opposed to a nonsocial or less socially-salient source (e.g., computers). For example, consider a person reading an article about another person's experience traveling to an exciting destination. Certainly, merely reading this article will elicit a desire to travel there; independently, the social-exploration effect would predict that the same article would encourage more exploration of alternative travel destinations when the

author is clearly a social entity (e.g., "I give this destination a rating of 5/5 stars") as opposed to a nonsocial entity (e.g., "This website gives this destination a rating of 5/5 stars").

Stated formally, our main prediction is that when information about a known option comes from another person, people will be more likely to explore unknown options than when the information source is not mentioned or is not social. We posit that, because information from social sources increases awareness of those sources' experiences, people will be more inclined to seek out alternative experiences for themselves than when information comes from nonsocial sources (e.g., mere information, computers, etc.).

Our next predictions stem from the hypothesis that a we-perspective underlies the social-exploration effect. We predict that the social-exploration effect will attenuate in the loss (vs. gain) domain. While people are motivated to adopt a we-perspective to share in other people's positive experiences (i.e., to "bask in reflected glory"; Cialdini et al., 1976), they are also motivated to protect themselves from negative experiences and will thus avoid sharing in others' losses (i.e., to "cut off reflected failure"; Snyder et al. 1986). In our experimental context, people should therefore be less likely to adopt a we-perspective in the loss domain than in the gain domain, which will cause the social-exploration effect to attenuate for losses.

Our third prediction is that the social-exploration effect should attenuate when the social source does not experience the outcomes associated with the information they provide. Choices and their associated outcomes are separable. While some choices lead to their intended outcomes, others lead to unintended ones (e.g., purchasing a product only to find out it is not in stock or voting for a political candidate who does not win their election) or even to no outcomes at all (e.g., hypothetical choices). Consequently, individuals respond to choices and experienced outcomes in distinct ways. For example, individuals diverge less from others' product choices

(i.e., stated preferences) than others' product experiences (i.e., purchases; Tu & Fishbach, 2015). The we-perspective account suggests that people explore in order to diversify their experiences as a group and thus avoid repeating others' individual experiences. It follows that, if the information source makes a choice but does not have a corresponding experience to share, people should not be more inclined to explore than if they received mere information alone (e.g., from a website, a book).

Correspondingly, if the information source was assigned to experience a certain option, which they did not choose, people should be more inclined to explore other options than if they received the same information from a nonsocial source (i.e., the social-exploration effect will hold). This comparison helps separate our model from uniqueness seeking. Indeed, another reason people might choose differently is to express their unique identity. While people generally conform to others' choices (Surowiecki, 2005), at times they purposely deviate to express themselves as unique individuals (Ariely & Levav, 2000; Berger & Heath, 2007; Tian, Bearden, & Hunter, 2001). This tendency to choose differently as a form of identity signaling is more likely when choices are public and when they involve expressions of personal taste. Accordingly, we test the social-exploration effect in private choices using monetary rewards (and further discuss their diverging predictions in the General Discussion). We further predict that people will deviate more from others' experiences but not from their choices which were not followed by experiences, although deviating from both similarly conveys unique identities.

The Present Research

To test the social-exploration hypothesis, we developed a treasure-hunt paradigm (used in all studies except Experiment 4). This minimalistic and incentive-compatible computerized paradigm involves choosing one out of four treasure sites, each containing an unknown monetary

reward within a given range (e.g., between 1 and 40 cents). The amount of monetary reward in each site is fixed and unknown. Before making their selection, participants learn the contents of a single site (e.g., Site B always returns 20 cents). They then decide whether to choose this known site (in which case, they would earn 20 cents) or explore another site with an unknown amount. Critical to our main hypothesis, this paradigm allows us to manipulate participants' awareness that information about a known site either comes from another person or from a mere information source. In the social-information condition, the information source is always another participant who chose that site and received the associated reward. In the mere-information condition, the information source is either not mentioned or is a computer simulation that randomly reveals a site.

In the interest of capturing social exploration, we designed the treasure-hunt paradigm to provide a context in which exploration is attractive to some extent, so we can test whether it is even more attractive in social contexts. We incorporated several elements to this end. First, we instilled curiosity about the contents of unknown sites by presenting them as rewards. Second, we reminded choosers of the unexplored options; we presented clear exploration opportunities alongside clear exploitation opportunities. Third, we reduced participants' social pressure to conform and controlled for beliefs that social others seek information from them or derive hedonic utility from their choices. We did this by ensuring anonymity, by restricting communications between participants and information sources, and by making clear that participants' outcomes were independent from information sources' outcomes.

We designed our minimalistic paradigm to simulate realistic group exploration contexts. In such contexts, individuals often explore the environment with others who are neither explicitly similar nor dissimilar to themselves; they merely share a common goal of exploring some

environment (e.g., exploring a specific product category with anonymous consumers). Classic work on the minimal group paradigm suggests that people readily form groups with individuals who are only tentatively connected to them (Tajfel, Billig, Bundy & Flament, 1971). Testing our hypothesis in a minimalistic context allowed us to leverage the tendency to form minimal groups (in this case with individuals who merely share an exploration task) and manipulate only the key variable—sociality—while holding all other qualities of the information sources constant.

In what follows, we report four studies, which are summarized in Table 1.1. Across these studies, we targeted a minimum sample of 50 participants per experimental condition. We anticipated that we would find medium-to-large effects[1]. Power analyses conducted in G*Power (Faul, Erdfelder, Lang, & Buchner, 2007) based on respective sample sizes and target alpha level (α = .05) revealed that power was sufficient across all studies (i.e., ≥ .80) to detect a medium-to-large effect. The studies in this paper incorporate online participants recruited from Amazon Mechanical Turk (MTurk), Prolific and various universities in China. All materials and data are publicly archived (https://osf.io/zps5x/?view_only=c58e3a5bc30242ac80d6548b04b9cadb).

Experiment 1a: The Social-Exploration Effect

Experiment 1 tested the social-exploration effect. Participants played an incentive-compatible, computerized treasure-hunt game in which they had to choose one of four "treasure sites," each containing a monetary reward. They learned that the amount offered by each site varied between 1 and 40 cents. They further learned about the amount in one of the sites: between 5 and 35 cents in increments of 5 cents (seven conditions). In the social-information condition, participants learned that information about the known site came from a previous

[1] We calculated an estimate of effect size using the two revealed monetary amount conditions in Experiment 1 (i.e., 20 and 25 cents) that are used in subsequent studies. This yielded a medium-to-large effect (d = 0.76).

player; the mere-information condition did not mention the information provider. We predicted that more participants would choose one of the three unknown sites (i.e., explore) in the social- than in the mere-information condition, regardless of the size of the known amount.

Method

Participants. We opened the experiment to 420 U.S.-based participants on MTurk in return for a $0.30 fixed payment plus a variable performance-based bonus. In this and all subsequent MTurk studies, participants were invited to participate as long as their approval rating was at or above 95%. MTurk returned 427 responses (M_{age} = 36.58, SD = 12.76, 54% women; one participant reported "four" and was excluded from calculating age mean). We report participant attrition in all studies in the SOM (Zhou & Fishbach, 2016).

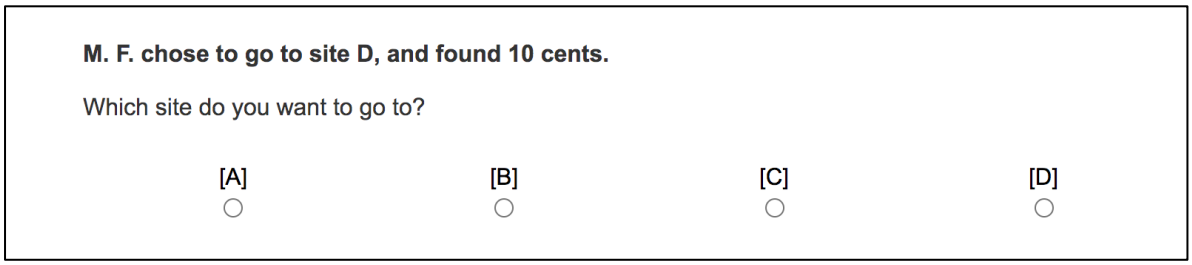
Procedure. This experiment used a 2 (social information vs. mere information) between-participants design. We also manipulated the reward offered by the known site (5, 10, 15, 20, 25, 30, and 35 cents) between-participants for conceptual replication with a decreasing incentive to explore. Participants read that they would play a treasure-hunt game in which they would choose one of four available sites: Site A, B, C, or D (represented as four boxes on the screen). Each site contained some unknown amount between 1 and 40 cents that they would receive as a bonus. They next answered a series of comprehension questions about the game and could proceed only after answering correctly.

Participants in the mere-information conditions then read, "It is known that site D provides 5 [10] [15] [20] [25] [30] or [35] cents. Which site do you want to go to?" In the social-information conditions, participants read, "Our computer program has paired you up with another participant, M. F., who will be your treasure hunting partner, while you were reading the game instructions. M. F. will choose a site to visit first. When (s)he returns, we will inform you

of what (s)he finds. You will choose a site to visit next. When you return, we will inform M. F. of what you find. M. F. will receive the money (s)he finds as a bonus; you will receive the money you find as a bonus. That is, the two of you will not share the money." Next, participants saw a waiting screen while M. F. ostensibly made their selection. After 14 seconds, participants read, "M. F. chose to go to site D, and found 5 [10] [15] [20] [25] [30] or [35] cents. Which site do you want to go to?" (see Figure 1.1). Notably, participants were assured that the amount in each site remained constant, meaning each time the site was selected, the same amount would be earned.

Figure 1.1

An example choice presented to participants in the social information condition in Experiment 1



Next, if participants chose the known site, they earned the corresponding amount in their revealed amount condition; if they chose a different site (i.e., explored), they earned 8 cents. As an attention check, participants next reported whether the site they chose was the same or different from the known site and reported the amount they received. We also asked participants to reflect on the rationale of their choice ("What are the reasons that underlie your treasure site choice?").

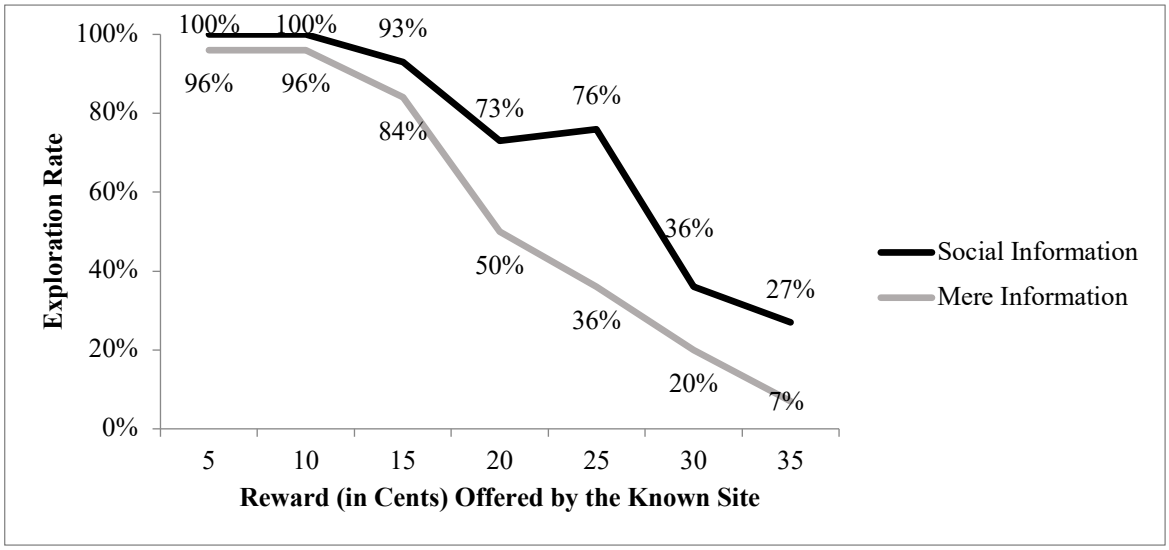
Results and Discussion

Our dependent variable was whether participants explored an unknown site. In support of the hypothesis, across reward amounts, participants were more likely to explore an unknown site in the social-information (153/213, 72%) than in the mere-information conditions (118/214,

55%), $\chi^2(1, N = 427) = 12.83$, $p < .001$ (see Figure 1.2). The odds ratio was 2.08 (see Table 1.1), suggesting a small-to-medium effect size across all seven levels of known rewards.

Figure 1.2

Exploration rate as a function of information source and known reward amounts



Note. Participants in Experiment 1a explored an unknown site more often after learning the reward offered by a known site from a person than from mere information, regardless of the amount of reward offered.

For exploratory purposes, we analyzed the effect of the reward amount revealed in the known site. We conducted a logistic regression with exploration as the dependent variable and information source, revealed reward amount (ordinal variable) and their interaction term as predictors. This analysis did not reveal a significant interaction between revealed reward amount and information source, $\beta = -0.03$, $SE = 0.04$, $Wald(1) = 0.57$, $p = .449$, odds ratio = 0.97. It yielded a main effect: The exploration rate decreased as the known reward increased, $\beta = -0.18$, $SE = 0.03$, $Wald(1) = 47.38$, $p < .001$, odds ratio = 0.83. This finding is to be expected because larger rewards are closer to the optimal outcome and thus decrease the potential benefits of further exploration (i.e., the satisficing principle; Simon, 1956).

Responses to the open-ended rationale question mainly fell into four categories (see Table 3 in the SOM), the most common of which was, "desire to maximize earnings" (147/650, 23%)[2]. Only 16 participants in the social-information condition mentioned reasons related to taking a we-perspective (16/315 or 5%). These results suggest that people are largely unaware of the we-perspective mechanism.

Results from Experiment 1 support the social-exploration hypothesis: Participants explored more when they were made aware that the information source was a person. Our theory posits that the tendency to adopt a we-perspective with other people underlies the effect, yet several alternative explanations are possible. First, the information in the mere-information condition may have appeared to be a recommendation by the experimenter, which could have encouraged participants to choose the known option more often. To eliminate this possibility, we adapted the mere-information condition in all subsequent studies, unless otherwise noted, to have a computer simulation ostensibly select the known option at random. Another possible explanation for the effect is that participants were trying to reciprocate the previous player by collecting new information to share with them. We tested this possibility by making participants' decision private and unknown to the previous player in subsequent Studies 2–4, thus eliminating the possibility to reciprocate information. In these subsequent studies, we also remove the information that the previous player explored, leaving it possible that that person did not explore. We find no evidence that it matters whether participants expected to reciprocate information or thought the first person explored.

[2] Here and in subsequent studies, the total number of responses exceeds the total number of participants because some participants listed multiple reasons for their choice.

Experiment 1b: Generalizing to a Payout-Maximizing Frame

The experimental setting in Experiment 1a was explicitly referred to as a game, which might have encouraged participants to take a more "playful" approach than they would have if they were trying to maximize their earnings, potentially more so in the social-information condition. Experiment 1b tested whether the social-exploration effect would generalize to a non-game context that emphasized earning as much money as possible. We used a similar paradigm as in Experiment 1a, except we manipulated the framing of the task, describing it either as a "treasure-hunt game" (as in Experiment 1a) or as a "financial task" involving investment opportunities. The financial-framing instructions emphasized that the participant's goal was to maximize their earnings. We expected to replicate the social-exploration effect in the financial-decision conditions.

Method

Participants. We opened 240 HITs to predominantly U.S.-based participants on MTurk and provided a $0.20 fixed payment plus a variable performance-based bonus. MTurk collected 251 responses (M_{age} = 37.34, SD = 11.36, 56% women; one person did not report their age).

Procedure. This experiment used a 2 (social information vs. mere information) × 2 (task framing: treasure-hunt game vs. financial-decision task) between-participants design. Based on the results of Experiment 1a, we revealed either 20 cents or 25 cents, between-participants. We used these different amounts as a conceptual replication. We report the data after collapsing across these amounts.

The basic procedure was similar to Experiment 1a. To manipulate game framing, participants in the treasure-hunt-game conditions read, *"*This is an exploration game, which allows you to discover prizes in a new environment. There are multiple treasure sites, each

containing a unique reward. Consider how you might select among your potential options to pick the most satisfying outcome." Participants in the financial-decision-task conditions read, "In this task, you will make a financial decision that will lead to actual payment. Consider how you use financial information in the real world to accumulate money. Think about how to make the best judgments and decisions to earn the most worthwhile payout." To further emphasize the game versus task nature of the decision, we used the following terms throughout the experiment: "treasure hunting" ["financial decision making"], "sites" ["investment opportunities" (or just "investments")], "finding" ["earning"], "going to" ["investing in"], and the "game" ["task"], respectively.

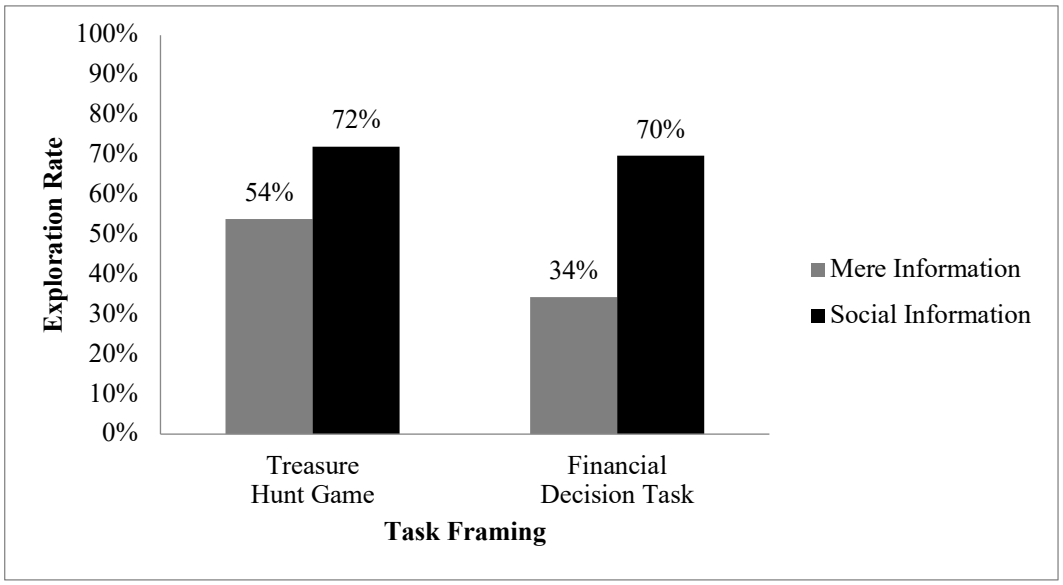
In the social-information conditions, participants received information about one site ostensibly based on what another participant found (as in Experiment 1a). In the mere-information conditions, participants learned the ostensible result of a real-time computer simulation, which allowed both conditions to be equally dynamic while preserving differences in perceived sociality. Across all conditions, participants waited a few seconds to learn the rewards provided by a single site (20 or 25 cents). Then, participants chose a site for themselves. If they chose the known site, they earned the corresponding amount in their condition; if they chose a different site, they earned 23 cents.

Results and Discussion

Our dependent variable was exploration rate: whether participants explored an unknown site or not. In support of the social-exploration hypothesis, participants were more likely to explore in the social-information (88/124, 71%) than in the mere-information conditions (56/127, 44%), $\chi^2(1, N = 251) = 18.53$, $p < .001$ (see Figure 1.3). The odds ratio was 3.10, suggesting a medium-to-large effect size.

Figure 1.3

Exploration rate as a function of information source and task framing



Note. Participants in Experiment 1b explored unknown sites more when the information source of a known option was a person (the social-information condition) than when it was a computer simulation (the mere-information condition) regardless of whether the decision was described as a treasure-hunt game or a financial-decision task.

We also found a marginal effect of task framing: The exploration rate was marginally lower in the financial-decision-task-framing conditions (66/127, 52%) than in the treasure-hunt-game-framing conditions (78/124, 63%), $\chi^2(1, N = 251) = 3.07$, $p = .08$; odds ratio = 1.57. This pattern is reasonable given that activating the goal of maximizing earnings may decrease risk tolerance and exploration involves inherently higher risk than not exploring.

We found no evidence that task framing moderated the social-exploration effect. A logistic regression with exploration as the dependent variable and information source, task framing and the interaction of these two terms as the independent variables did not yield a significant interaction, $\beta = 0.69$, $SE = 0.54$, $Wald(1) = 1.66$, $p = .197$. Instead, we found the social-exploration effect within each task-framing condition: for the treasure-hunt game (mere

information: 34/63, 54% vs. social information: 44/61, 72%), $\chi^2(1, N = 124) = 4.38$, $p = .036$; odds ratio = 2.21; for the financial-decision task (mere information: 22/64, 34% vs. social information: 44/63, 70%), $\chi^2(1, N = 127) = 16.00$, $p < .001$; odds ratio = 4.42.

Taken together, these results suggest that our participants were not exploring more in social contexts because they felt encouraged to treat their choice as a game, prioritizing enjoyment over earnings.

Experiment 1c: Replicating the Social-Exploration Effect in China

Experiments 1a and 1b were run using predominantly U.S. samples and thus established the social-exploration effect in an individualistic population where people tend to value personal freedom and independence from group pressures, relative to collectivistic populations as in East Asian cultures where people tend to value interdependence and group harmony (Kim & Markus, 1999). The purpose of Experiment 1c was to explore the generalizability of the effect in a more collectivistic population, specifically, in China.

Method

Participants. We published the experiment to a pool of registered participants from multiple universities in China in return for a 1 RMB (14 cents) fixed payment plus a variable performance-based bonus. We pre-determined the sample size to be at least 200 participants. We posted the experiment for 1 day and returned 264 participants (70% women; age information was not collected).

Procedure. This experiment used a similar experimental setup as Experiment 1a, except that payment was in Yuan. Participants learned that the rewards in the sites could vary between 0.1–2.8 RMB (approximately 1–40 cents), and we sampled stimuli by randomly assigning

revealed amounts to be either 1.75 RMB (25 cents) or 2.1 RMB (30 cents) between-participants. We report the data after collapsing across these amounts.

Results and Discussion

Our dependent variable was exploration rate. In support of the social-exploration hypothesis, participants were more likely to explore in the social-information condition (48/135, 36%) than in the mere-information condition (17/129, 13%), $\chi^2(1, N = 264) = 17.80$, $p < .001$. The odds ratio was 3.64, suggesting a medium-to-large effect size.

We also asked participants to explain the reasons for their choice with an open-ended text response. Responses to the open-ended rationale question mainly fell into 7 categories, the most common of which was that participants, "considered the known amount" (137/475, 29%). Only 16 participants mentioned reasons related to the influence of the teammate (16/264, 6%; see Table 4 in the SOM). This, like the open responses in Experiment 1a, suggests that people are largely unaware of the we-perspective mechanism.

The results of this experiment replicate the social-exploration effect among a Chinese sample, suggesting that the effect also exists among more collectivistic cultures. While there are lower absolute levels of exploration among the Chinese sample compared with the U.S. sample in Experiment 1b (possibly because we revealed somewhat larger monetary rewards), the effect size of the social-exploration effect was, in fact, larger in Experiment 1c.

Experiment 2: Moderation by Loss Frame

To investigate whether group-level perspective-taking underlies the social-exploration effect, in Experiment 2, we introduced a condition in which people made choices in the loss domain. We hypothesized that the social-exploration effect would attenuate within the loss domain. Because people are less likely to adopt a we-perspective with "losers" (i.e., to "cut off

reflected failure"; Snyder et al., 1986), participants would explore more in social- than in mere-information contexts when the options represent gains, but would not when they represent losses. Before reporting the main experiment, we report a pretest in which we explored whether, within our paradigm, people are more inclined to switch to a we-perspective in the domain of gains than losses.

Pretest: More We-Perspective-Taking for Gains than Losses

To test whether people see others' gains as collective gains more than they see others' losses as collective losses, we conducted a pretest using a 2-condition (framing: gain vs. loss) between-participants design within the social condition. We opened 100 HITs to predominantly U.S.-based participants on MTurk and provided a $0.20 fixed payment plus a variable performance-based bonus. MTurk collected 100 responses (M_{age} = 35.56, SD = 10.21; 35% women).

We used a similar paradigm as Experiment 1a, except that in the loss condition, participants were given an endowment of 120 cents and instead of choosing among four "treasure" sites, they chose among "land mine" sites that would cause them to lose instead of gain money. Also, participants did not read that their choice would be shared with the first player.

Sites earned [lost] between 1 and 119 cents. All participants played the game ostensibly with another player ("M. F.") who also gained [lost] money from their choice. As in the previous studies, participants learned about the contents of a site from the other player's choice. To make the experience more vivid, we presented colorful, relevant background images (green dollar bills or burning dollar bills), a visual aid (a pie chart depicting the known site's contents relative to

total possible earnings) and sound (applause or a siren, which we verified participants could hear).

The revealed amount in the gain condition was 60, 66 or 75 cents (randomly determined) and in the loss condition was 24, 30 or 36 cents (randomly determined). Because experiences of losses are generally 2-2.5 times more intense than experiences of gains (Tversky & Kahneman, 1991), we chose these loss amounts to match the intensity of the gain experiences.

Crucially, upon learning about the other player's gain [loss], participants indicated whether they agreed with a statement about group-level perspective-taking, adapted from Cialdini et al. (1976): "M. F.'s earning/losing [××] cents feels like a gain/loss for M. F. and me as a team" (1 = *completely disagree*, 7 = *completely agree*). To echo the cover story, participants also made a choice for themselves afterwards. Participants' choices were not analyzed.

Supporting our hypothesis, participants more readily adopted a we-perspective when the other player gained money ($M = 4.17$, $SD = 1.81$) than when they lost money ($M = 3.06$, $SD = 1.88$), $t(98) = 2.99$, $p = .003$; Cohen's $d = 0.60$.

We then conducted the main experiment, which used a similar procedure, except that we further manipulated the information source (social information vs. mere information) and did not have participants answer the group-perspective question (which would not be applicable in the mere-information condition). Participants played three rounds of the game. We predicted that the social-exploration effect would be larger in the gain- than in the loss-framing condition.

Method

Participants. We opened 200 HITs to predominantly U.S.-based participants on MTurk and provided a $0.20 fixed payment plus a variable performance-based bonus. MTurk returned 202 responses ($M_{age} = 37.48$, $SD = 11.11$, 55% women).

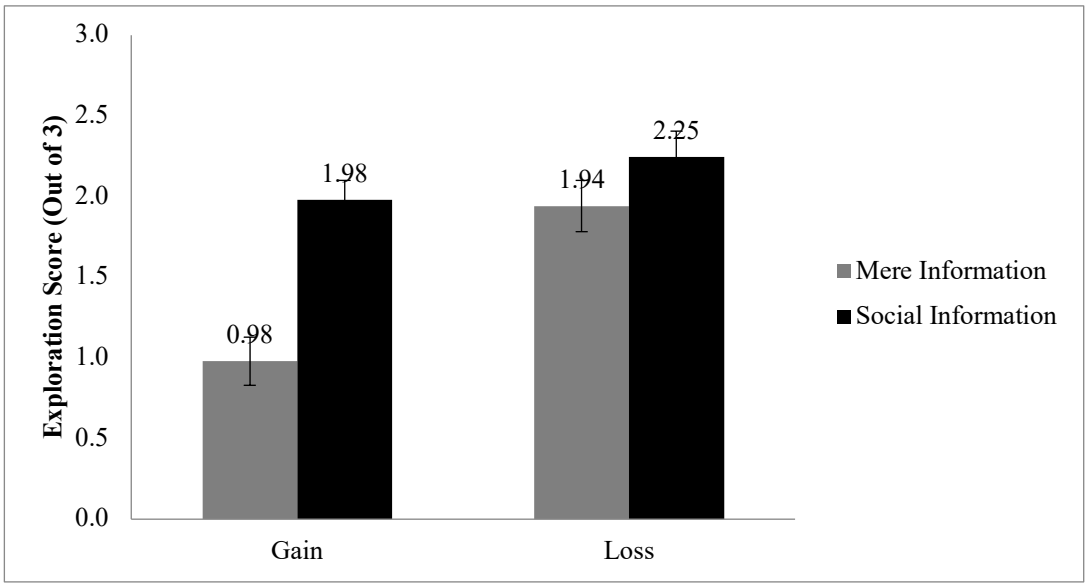
Procedure. This experiment used a 2 (source: social information vs. mere information) × 2 (framing: gain vs. loss) between-participants design. We used a procedure similar to the one in the pretest, except that (1) in the mere-information conditions, participants learned information from a computer simulation, (2) participants did not answer the group-perspective-taking question, and (3) participants learned they would play three independent rounds of the game in which the site contents would change in each round and their payment would be determined by one of their choices at the end. The order of the revealed amounts (60, 66, or 75 cents in the gain conditions; 24, 30, or 36 in the loss conditions) was counterbalanced across rounds. Before starting the game, participants in the social-information conditions learned that the other player was randomly assigned to choose first in all three rounds. We report the data after collapsing across these amounts.

Results and Discussion

We calculated participants' exploration score as the number of times they chose an unknown site across the three rounds (between zero and three). In support of our hypothesis, a 2 (source: social information vs. mere information) × 2 (frame: gain vs. loss) ANOVA yielded an interaction effect, $F(1,198) = 5.60$, $p = .019$, $\eta_p^2 = 0.03$ (see Figure 1.4). Within the gain frame, participants explored more in the social-information ($M = 1.98$, $SD = 1.13$) than in the mere-information condition ($M = 0.98$, $SD = 1.10$), $F(1,198) = 22.43$, $p < .001$; $\eta_p^2 = 0.10$ (the social-exploration effect). Within the loss frame, however, exploration was similar in the social-information ($M = 2.25$, $SD = 0.88$) and mere-information ($M = 1.94$, $SD = 1.07$) conditions, $F(1,198) = 2.20$, $p = .140$, $\eta_p^2 = 0.01$.

Figure 1.4

Exploration score as a function of information source and gain/loss frame



Note. Participants in Experiment 2 explored more in the social-information than in the mere-information condition, but only in the gain domain. The social-exploration effect was attenuated in the loss domain. (Error bars represent +/-1 standard error.)

This analysis also yielded a main effect of information source, $F(1,198) = 19.63$, $p < .001$, $\eta_p^2 = 0.09$, such that participants chose to explore more in the social-information ($M = 2.12$, $SD = 1.01$) than in the mere-information conditions ($M = 1.47$, $SD = 1.18$). In addition, we found a main effect of framing, $F(1,198) = 17.36$, $p < .001$, $\eta_p^2 = 0.08$, whereby participants chose to explore more when options were framed as losses ($M = 2.10$, $SD = 0.98$) than gains ($M = 1.49$, $SD = 1.22$). This main effect is consistent with the general tendency to seek more risk in loss than gain domains (Kahneman & Tversky, 1979), although, given the amounts that could be earned from revealed sites differed for gain- and loss-framing conditions, it should be interpreted with caution. Further, given this main effect, we cannot draw conclusions from comparisons between the mere-information conditions, or between the social-information conditions. We

therefore limit our hypothesis testing and conclusions to comparisons within each frame, which confirm the social-exploration effect only for gains.

The results of Experiment 2 are consistent with the proposed group-level perspective-taking account of the social-exploration effect. First, we found in the pretest that it is more intuitive to people to consider others' gains as the group's gain than to consider others' losses as the group's losses. Second, the effect was replicated in the gain domain. In the loss domain, where people are less likely to intuit the other person's outcomes as a collective outcome, the effect was attenuated. Additionally, across studies, we varied the number of (independent) rounds participants played (See Table 2 in the SOM). In doing so, we gathered evidence for the generalizability of the social-exploration effect.

Experiment 3: Social Exploration in Response to Others' Realized Outcomes

We predict that people are more likely to explore in social settings because they want to diversify their experience as a group rather than repeat a group member's experience. Two hypotheses follow. First, if the other person only made a choice—but did not experience its corresponding outcome—then people who observe this choice should not explore more than people who receive the information from a mere information source (i.e., because their group had not experienced this option yet, they ought to still try it out). Second, if the other person only experienced the outcome—but did not choose it—then people who observe this should explore more than people who receive the information from a mere information source (i.e., because an option had already been experienced by the group, they ought to choose a different option). Together, if these hypotheses are confirmed, we can conclude that people explore more in social situations not because they are trying to make different choices than others (e.g., to signal their uniqueness), but because they are trying to have new experiences at the group level.

To test the first hypothesis, in Experiment 3 we created a new "choice-without-experience" social condition, where the other player revealed the contents of a treasure site without being able to earn anything from it (i.e., without experiencing the outcome). We predict that in this condition, exploration rates should be lower than in the standard social-information condition (and more similar to the mere-information condition).

To test the second hypothesis, in Experiment 3 we added the "experience-without-choice" social condition. Here, the other player was randomly assigned a treasure site and could not choose for themselves. The other player did, however, obtain the reward from that site. Thus, we predict that in this condition, exploration rates should be higher than in the mere-information condition (and similar to the social-information condition) because participants will want to experience a new option.

Yet, before reporting the main experiment, we report another pretest. This pretest explored whether people are more inclined to switch to a we-perspective in a social context than a mere-information context.

Pretest: More We-Perspective-Taking When Receiving Information from a Person

To test whether people are more inclined to adopt a group-level perspective when receiving information from another person versus a computer simulation, we first ran a pretest using a 2-condition (mere information vs. social information) within-participants design.

We opened the survey to 80 predominantly U.S.- and U.K-based participants on Prolific and provided a £0.40 (equivalent to 50 cents) fixed payment. Prolific returned 81 participants (M_{age} = 30.64, SD = 9.09; 48% women). Participants read a generic description of the treasure-hunt task. In this task, the four treasure sites were said to provide different amounts of monetary rewards but no range was mentioned.

Participants were asked to consider playing two rounds of the game, one where a computer simulation randomly revealed a site and the participant learned what amount of money could be gained there, and one where another participant randomly revealed a site and the participant learned what could be gained there.

To test group-level perspective-taking, participants rated their agreement with the following four statements, presented in random order (1 = *not at all*, 7 = *very much*): "In this game, how natural it is to think of the computer simulation and yourself (in Game 1) as a team to explore the unknown treasures together?", "In this game, how natural it is to think of the other person (in Game 2) and yourself as a team to explore the unknown treasures together?", and two similar statements where the word "natural" was replaced by "intuitive."

To determine group-level perspective taking across information sources, we averaged the "natural" and "intuitive" measures for the social round into one group-perspective-taking index, and averaged the measures for the mere-information round into a separate group-perspective-taking index (Cronbach's $\alpha = .83$ and $\alpha = .84$, respectively). Supporting our assumed mechanism, a paired samples *t*-test revealed that group-perspective-taking was higher for the social-information round ($M = 4.60$, $SD = 1.42$) than the mere-information round ($M = 3.54$, $SD = 1.57$), $t(80) = 5.52$, $p < .001$, Cohen's $d = 0.87$. These results suggest that participants are, in fact, more inclined to adopt a group-level perspective with social than mere-information sources.

Method

Participants. We opened 400 HITs to U.S.-based participants on MTurk and provided a $0.20 fixed payment plus a variable performance-based bonus. MTurk returned 403 participants ($M_{age} = 36.91$, $SD = 11.76$, 46% women; three people reported either non-numerical answers or

impossibly high numbers for their age and were excluded from calculations of mean age and standard deviation of age).

Procedure. The experimental setup was similar to Experiment 2, except that in this experiment we compared the mere-information condition to three social conditions in a between-participants design. Participants started with a zero balance and could earn money by choosing sites on three rounds. We used the same social-information condition as in Experiment 2, as well as the same mere-information condition, except that we replaced words referring to "choosing" with ones referring to "revealing," to be consistent with the choice-without-experience condition described below. The two new social conditions were "choice-without-experience" and "experience-without-choice."

In the choice-without-experience condition, the information provider could choose a site, but could not earn money from that site. Participants in this condition read, "M. F. has been randomly assigned to be the 'Revealer.' This means in each round, M. F. will choose 1 site to reveal to you. Then, you will make your selection. M. F. does not know what is contained in the sites, will not make any selections for themselves, and will not receive any bonus pay regardless of what you choose."

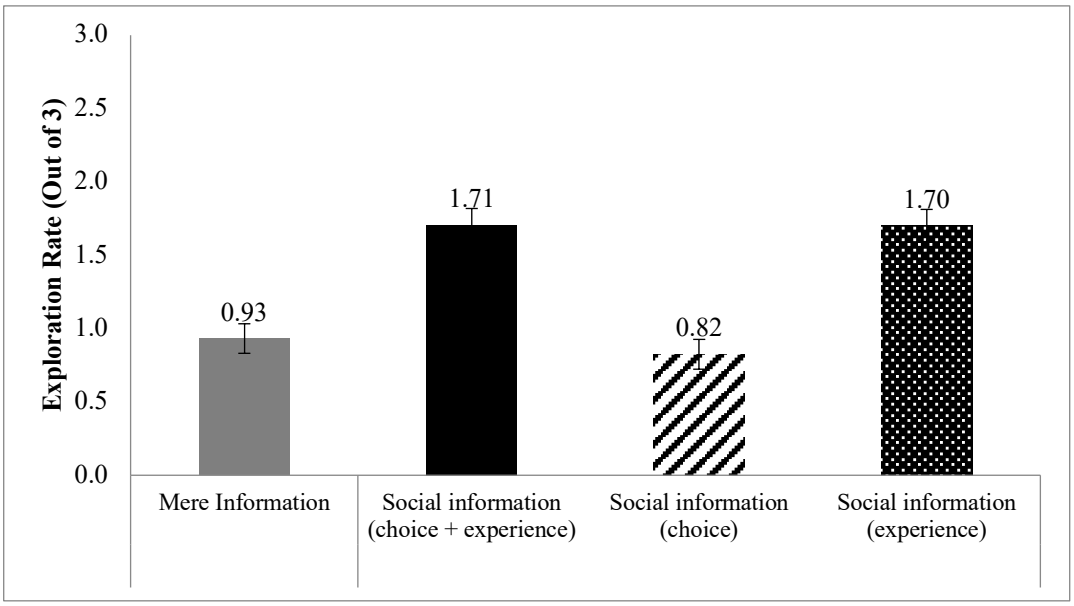
In the experience-without-choice condition, the information provider could not choose a site for themselves, but they could earn money from their assigned site. Participants in this condition read, "M. F. has been randomly assigned to have no choice of what they will receive. This means in each round, the computer program will automatically assign M. F. to get what is in a randomly chosen site. M. F. does not know what is contained in the sites."

Results and Discussion

We calculated participants' exploration score as the number of unknown sites they visited across the three games (between zero and three). A one-way between-participants ANOVA with 4 levels (mere information vs. social information vs. choice-without-experience vs. experience-without-choice) yielded the predicted main effect of information source, $F(3, 399) = 20.19$, $p < .001$, $\eta_p^2 = 0.13$ (see Figure 1.5). Replicating the basic effect, we found higher exploration rates in the social-information condition ($M = 1.71$, $SD = 1.14$) than the mere-information condition ($M = 0.93$, $SD = 1.02$), $t(399) = 5.19$, $p < .001$; Cohen's $d = 0.71$. Supporting the first hypothesis, pairwise contrasts reveal that exploration rates in the choice-without-experience condition ($M = 0.82$, $SD = 1.00$) were lower than the social-information condition, $t(399) = -5.82$, $p < .001$; Cohen's $d = 0.82$, and similar to the mere-information condition, $t(399) = -0.71$, $p = .478$; Cohen's $d = 0.11$. Supporting the second hypothesis, the exploration rates in the experience-without-choice condition ($M = 1.70$, $SD = 1.10$) were higher than in the mere-information condition, $t(399) = 5.16$, $p < .001$; Cohen's $d = 0.73$, and similar to the social-information condition, $t(399) = -0.02$, $p = .985$; Cohen's $d < .01$.

Figure 1.5

Exploration score as a function of information source



Note. Results of Experiment 3. Exploration rates were high when receiving information from a person who chose and experienced the outcome or a person who only experienced the outcome. Exploration rates were low when receiving information from someone who only chose an outcome or from a nonsocial source (mere information). Error bars represent +/-1 standard error.

The results of Experiment 3 provide further evidence for our proposed mechanism. First, we found more we-perspective-taking when getting information from another person versus a computer. Second, we found that the information provider's experience of a given outcome, rather than their choice of that outcome, plays a crucial role in others' exploration behavior. In Supplemental Experiment S3, we replicated the finding that another person's experience increases exploration.

Possibly, learning about a site from a social other who is only able to reveal, but not obtain the rewards, may have created social pressure not to explore, as exploring could convey a lack of appreciation for the effort and time devoted to selecting the revealed site. This would be an alternative explanation for why exploration was lower in the choice-without-experience

condition. Though worth mentioning, we believe it is less likely because it would assume participants were meaningfully motivated to convey appreciation to an anonymous player who was simply assigned the role of the "revealer" by the experimenter.

Taken together, these results support the group-level perspective-taking mechanism. At a group level, to re-experience a fellow group member's obtained rewards is redundant, which leads to exploration when outcomes are experienced by others. But if the same choice has been made by another group member without them experiencing the outcome, then repeating that same choice is more likely because the associated experience has not been exploited yet. This outcome rules out the alternative account whereby people explore more in social conditions due to a desire to express their agency or uniqueness. If this desire was a prime motivator, exploration rates should have increased when another person chose an experience, regardless of whether they experienced it or not.

Experiment 4: Social Exploration in Everyday Choice

The treasure-hunt paradigm used throughout our previous studies has the benefit of being simple and incentive-compatible; however, might the idiosyncrasies of this paradigm have lent themselves too well to the social-exploration effect? Will the effect generalize to the kinds of exploration decisions people make in their everyday lives? Experiment 4 was designed to test the social-exploration effect using a different paradigm and a more realistic context. Specifically, we tested participants' choice of movie clips (mimicking similar choices that people often make on YouTube, Netflix, etc.). Finding the effect in this context would provide evidence that it generalizes to exploration decisions more broadly.

Using the movie-choice context, we compared the mere-information condition to two social conditions, where the other person was closer to or more distant from the self. People tend

to blur the boundaries between themselves and interpersonally close (vs. distant) others (Aron et al., 1991), making it possible that people might then explore more with closer, rather than more distant others. However, because people tend to adopt a we-perspective so intuitively, including with people with whom they have minimal or no psychological connection (see Sloman & Rabb, 2016), the perceived closeness of a close (vs. distant) other may not influence exploration rates. Thus, this part of the investigation was exploratory.

Method

Participants. We opened 600 HITs to U.S.-based participants on MTurk and provided a $0.30 fixed payment. MTurk returned 633 participants. Prior to condition assignment, 28 participants failed an attention check which prevented participants who failed from continuing in the experiment. After excluding these participants, the final sample included 605 participants (M_{age} = 37.69, SD = 12.04; 50% women).

Procedure. This experiment used a 3 (mere-information vs. close-other information vs. distant-other information) between-participants design. After completing an attention check, all participants were asked to imagine they were on a video-sharing website and were choosing between 4 short videos (A, B, C and D), each lasting 5 minutes. Participants were not given information about any of the videos, except for rating information for one of the videos, which was revealed to the participant as being 7 out of 10. In the mere-information condition, this rating came from an algorithm associated with the website. In the close-other and the distant-other conditions, this rating came from an ostensible user of the website ("Alex") who had watched the video. In the close-other condition, Alex's geographical location was shown to be the same town in the U.S. as the participant. Note that while an unknown person in the same town might normally not be considered interpersonally close, in global online environments such

as YouTube, encountering another person from one's town probably does create a feeling of closeness. In the distant-other condition, Alex's geographical location was shown to be Auckland, New Zealand, which is geographically very distant from the U.S.

All participants then answered a comprehension check question, confirming they understood the rating of the video. They then answered our main dependent variable: they chose which of the 4 video clips they would like to watch. After their selection, all participants answered an open-ended question asking them to explain what factors they considered when making their choice. Participants in the close-other and distant-other condition then answered a manipulation check, rating how close they felt to Alex (1 = *not close at all*, 7 = *very close*). All participants then rated whether 7/10 was a good rating for a video, and answered an attention check asking which video's rating had been shown to them. Lastly, participants answered a second comprehension check asking them to report the source of the video rating information. Whether participants would actually watch a video or not following their choice was left ambiguous. They did not, in fact.

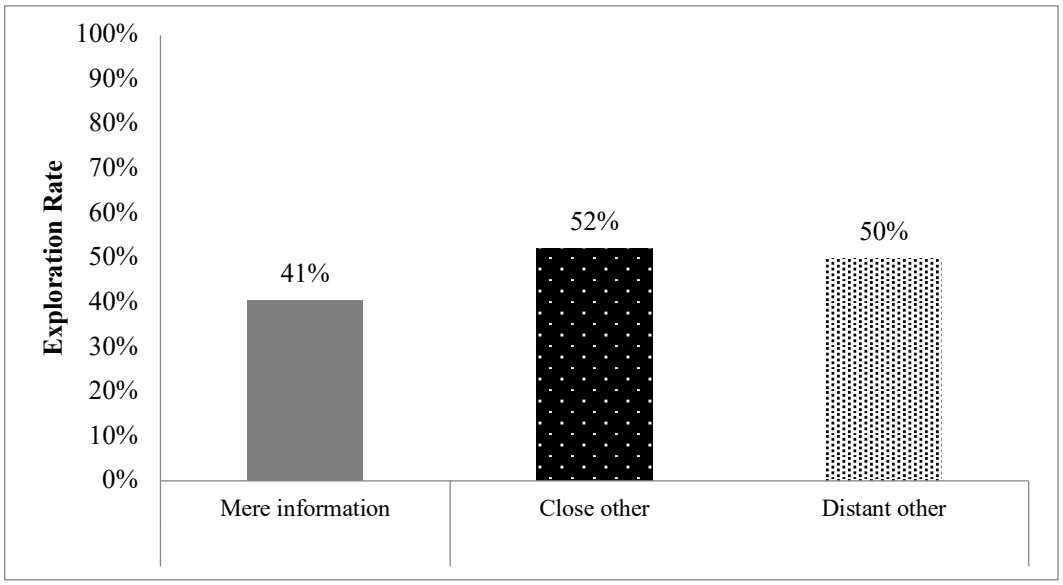
Results and Discussion

Exploration rate—whether participants chose to watch an unrated video clip—varied by condition, $\chi^2(2, N = 605) = 6.25$, $p = .044$, Cramer's $V = .10$, with the mere-information condition showing lower exploration rates (81/200, 41%) than the close-other condition (105/201, 52%) and the distant-other condition (102/204, 50%) (see Figure 1.6). Replicating our basic effect, we find that exploration rates were higher in the social-information conditions (close-other and distant-other conditions combined) (207/405, 51%) than in the mere-information condition, $\chi^2(1, N = 605) = 6.04$, $p = .014$. The odds ratio was 1.54, suggesting a small effect size.

Confirming our interpersonal closeness manipulation, participants in the close-other condition felt closer to Alex (M = 3.39, SD = 1.80) than those in the distant-other condition (M = 2.61, SD = 1.60), $t(403) = 4.65$, $p < .001$. Yet, feeling closer (vs. more distant) did not increase exploration, $\chi^2(1, N = 405) = 0.20$, $p = .652$. The odds ratio was 0.91, suggesting a trivial effect size.

Figure 1.6

Exploration rate as a function of information source



Note. Results of Experiment 4. Participants in the close-other and distant-other conditions explored more than those in the mere-information condition, but equally as each other.

Responses to the open-ended response mainly fell into 6 categories (see Table 5 in the SOM), the most common of which was, "relied on rating information" (131/786, 17%). No participant mentioned reasons related to adopting a we-perspective.

Taken together, the results of this experiment provide evidence that the social-exploration effect generalizes to the types of exploration contexts that people encounter in the real world. Further, we tested whether interpersonal closeness influences people's tendency to explore in

social contexts and did not find evidence that it did. Instead, it seems that minimal psychological connection is enough to activate the desire to explore, as long as the sociality of information sources is made salient.

General Discussion

Exploration decisions are ubiquitous (Melhorn et al., 2015; Wilson et al., 2014). People regularly face decisions between known, safer options and unknown, riskier options. In making these decisions, people may or may not be aware of when existing knowledge comes from other people's experiences. For instance, a person looking to buy a new smartphone can learn about options from other people describing the features of the phone, or they can learn the same information from less socially-salient sources, like product descriptions on the manufacturer's website. Our research contributes to the exploration literature by suggesting that source information plays a significant role in such decisions.

In a series of studies using minimalist and incentive-compatible tasks, we found evidence for the social-exploration effect: people explored unknown options more when information about known options came from another player than when the information source was not mentioned or was a computer program. This effect persisted across a wide range of outcome values (Experiment 1a), when the task was framed as a financial task that emphasized profit-maximization instead of the enjoyment of a game (Experiment 1b) and when the participants came from a collectivistic culture (Experiment 1c). The same effect was also obtained in a paradigm that mimicked commonly encountered decisions in everyday life, where participants were more likely to explore new movie clips that others had not seen before (Experiment 4).

We have theorized that this social-exploration effect occurs because, when people are aware that information about options comes from other people, they intuitively switch from an

individual-level perspective to a group-level perspective. That is, they switch from a me-and-you perspective to a we-perspective. When taking a we-perspective, people tend to explore more because they wish to diversify their experience as a group. Indeed, we found a greater tendency towards we-perspective in social conditions (Experiment 3 pretest). Further, we found the social-exploration effect was reduced when the options were in the domain of losses (vs. gains), which suppressed we-perspective (Experiment 2). We also found that the effect was reduced when the social information sources did not experience the outcomes associated with their information, because not yet having a group experience precluded participants from diversifying their group's experience (Experiment 3 and Experiment S3). The effect remained when the social information sources experienced the outcome associated with their information without having made a choice (i.e., when they were randomly assigned an outcome; Experiment 3).

Exploration as a Social Process

People explore their environment for a variety of reasons and frequently do so despite risks involved. For one, exploration reveals new information, which reduces epistemic uncertainty about the environment and its offerings (Speekenbrink & Konstantinidis, 2015). This knowledge can be beneficial in future interactions with the environment, especially over time. Indeed, people explore more when options are more varied and uncertain (Lejarraga et al., 2012) and when they expect to interact with the environment for a longer period of time (Carstensen, Isaacowitz, & Charles, 1999; Wilson et al., 2014). Beyond providing functional benefits, exploration also satisfies psychological needs. For instance, people explore because novel experiences provide stimulation (Hirschman, 1980; Raju, 1980; Zuckerman, 1979). People also explore purely to satisfy their curiosity—they seek information for the sake of alleviating uncertainty, even when it provides no tangible benefits (e.g., celebrity gossip; Golman &

Loewenstein, 2019; Loewenstein, 1994) and even when doing so results in negative experiences (e.g., mild electric shocks; Hsee & Ruan, 2016).

Previous work has largely understated how social the drive to explore may be at its core. Yet, exploration is rooted in social context. In ancient times, people explored together to accumulate collective knowledge about the environment (e.g., edible foods, safe shelter spaces); in modern times, scientists similarly explore ideas together to build a shared, collective body of knowledge (Higgins, 1996; Levine & Higgins, 2001). Our findings suggest that the tendency to consider the self and others as part of a group when exploring the environment is intuitive and easily activated. In our paradigm, this tendency led to more exploration in social contexts, despite the fact that participants could not influence other people's choices, communicate or manage others' impression of them, and despite conflicting motivations not to explore (e.g., the desire to maximize one's own individual outcomes). The ease with which participants in the current research adopted a we-perspective with others, which subsequently motivated their tendency to explore, speaks to the possibility that exploration is, at its core, a social process that is activated when working with others.

The "Social" in Social Cognition

The current work has implications for the study of social cognition. Traditionally, social cognition research assumed that the same principles that govern how people process information about objects also govern how people process social information. Over time, however, researchers have begun to uncover the ways in which people respond differently to information associated with social and nonsocial targets (Schlösser, Mensching, Dunning, & Fetchenhauer, 2015). For example, people automatically imitate the movements of a human hand faster than the movements of a functionally equivalent robot "hand" (Press, Bird, Flach, & Heyes, 2005), and

they exhibit better cognitive reasoning when testing social rules than when testing nonsocial rules. So, in the Wason Selection Task (Wason, 1968), people find it more intuitive to check for drinking violations—whether a person drinking alcohol is under 21—than check for violations of nonsocial propositions—whether a card showing the color brown on one side violates the proposition, "if a card shows an even number on one face, then its opposite face is red" (Cosmides & Tooby, 1992). In these cases, social cognition is governed by essentially the same cognitive principles that manage how people process information about objects, albeit in a deeper, better and sometimes faster way.

Our research identifies another difference between the processing of social and nonsocial information: the emergence of a we-perspective tendency in social, but not in nonsocial contexts. We find that, absent any direct relationship between decision makers and social information sources, decision makers default to incorporating their own and others' experiences into a unified, collective whole. That is, they intuitively consolidate their experiences with other people's experiences, rather than consider them as separate. This new angle highlights the uniqueness of the "social" aspect of social cognition and suggests fruitful avenues for future research.

Implications

Our findings inform the decision-under-uncertainty literature. This literature identifies systematic differences between description-based and experience-based decisions (Armstrong & Spaniol, 2017; Hertwig, Barron, Weber, & Erev, 2004; Martin, Gonzalez, Juvina, & Lebiere, 2014). Specifically, this work argues that people tend to make different decisions based on given descriptions of the outcome space (e.g., the potential outcomes and their corresponding probabilities; Kahneman & Tversky, 1979) or based on first-hand experiences with distributions

(e.g., sampling the distribution, experiencing the outcomes). Here, we highlight a third, hybrid way that people may become familiar with choice options: experiencing them through group-level perspective-taking. That is, we find that people may make decisions by drawing on descriptive information from social others that is combined with the perceived experiences of those social others.

Our research points to a spontaneous we-perspective and to a tendency to perceive others' experiences as linked to one's own. As such, it might appear inconsistent with research on perspective-taking failures, egocentrism and self-other empathy gaps (Epley, Keysar, Van Boven, & Gilovich, 2004; Keysar & Henly, 2002; Van Boven, Loewenstein, Dunning, & Nordgren, 2013). Are people intuitive perspective-takers, or are they ineffective at taking the perspective of others? While the answer is probably "in-between" (i.e., neither perfect nor horrible), the degree to which perspective taking occurs might depend on whether people have their own perspective on the issue before encountering others'. When they do, they tend to anchor on their own perspective more than they should. They might fail at perspective taking (Epley et al., 2004; Van Boven et al., 2013) or anchor on their own prior opinions and insufficiently adjust them when presented with pieces of advice (Bonaccio & Dalal, 2006; Yaniv & Choshen-Hillel, 2012). Yet in social exploration contexts, individuals lack personal experience with options in the choice set. In such contexts, egocentric tendencies cannot come to bear and a we-perspective (which requires perspective-taking), drives exploration behavior.

Psychologically, taking a we-perspective is a process similar to vicarious experience (only a we-perspective does not involve source confusion). And indeed, both phenomena may lead to both conformity and divergence. When people have vicarious experiences, they treat others' actions as their own and, at times, they feel they have made progress and can disengage

(e.g., vicarious licensing; Kouchaki, 2011); other times they seek consistency (e.g., vicarious sunk costs; Gunia, Sivanathan, & Galinsky, 2009; Olivola, 2018). When people want to explore, a we-perspective encourages exploration because another person's outcomes are considered group outcomes and one's goal becomes to diversify outcomes at the group level. However, in contexts where people want to exploit options, a we-perspective may well encourage exploitation for the same reason: because another person's outcomes are considered group outcomes, and the goal becomes to concentrate outcomes at the group level.

Alternative Explanations

We considered several alternative explanations for the social-exploration effect. One such explanation is that people explore more in social contexts than in mere-information contexts because they seek to express their individuality and uniqueness to other people (Ariely & Levav, 2000). This account suggests that people deliberately explore in social contexts as a form of impression management—to appear personally distinct and to avoid appearing to imitate others. If this were the case, the social-exploration effect should disappear in contexts that mitigate the value of impression management, such as when information recipients are unable to interact with information sources, when the options are non-taste-based (and therefore do not signal individual identities; Berger & Heath, 2007), or when outcomes are private. However, we find that people explore more in social contexts under these circumstances, which makes uniqueness seeking a less likely explanation.

A related possibility is that people care about differentiating themselves from others for private reasons, independent of impression management concerns. This account predicts that the information source's choice, but not necessarily their experience, should mitigate the social-exploration effect because repeating someone else's choices would undermine one's perception

of oneself as unique. Yet, that is not what we found. Additionally, this account would predict similar exploration rates in the gain and loss domains because the desire to distinguish oneself from others should apply similarly to both contexts. Contrary to this account, but consistent with the we-perspective account, we found that social exploration depends on the information source's experiences of outcomes and not on their choices (Experiment 3). It also attenuated when options were in the loss domain (Experiment 2). Together, these results suggest that personal motivations to express one's uniqueness or make choices that have not been made before (e.g., to be the first to sample a piece of information, to make an independent choice, to avoid feeling like a "copy-cat," etc.), did not drive the effect.

Possibly, the social-exploration effect results from people's desire to outperform others. Yet, this alternative, based on competition, is also difficult to reconcile with the attenuation of the effect in the loss domain because the competitive drive and the desire to obtain more rewards ought to apply similarly across gain and loss domains. We also tested competition more directly in a supplemental experiment (Experiment S1 in SOM) that manipulated whether outcomes were shared or not with the information source. We found no moderation of the effect. That is, when participants learned they would share their profits with the previous player, as they are both "a team," they were still more likely to explore compared to when they split their profits with a nonsocial source.

Other possible explanations include a desire to maximize information for the group and to avoid taking advantage of other group members' efforts. The former account supposes that people explore in social contexts because gathering additional information can directly benefit others, and the latter suggests that they do so because it is aversive to "free-ride" on resources that were earned by others. According to the maximizing account, participants would explore

more when information comes from social sources, regardless of whether they experienced the associated outcomes of their choice or not. This is because exploring provides more information for others independent of the source's experiences. The free riding account makes the same prediction because exploring avoids capitalizing on others' efforts independent of the source's experiences. Contrary to both accounts, we found in Experiment 3 that participants were less likely to explore when social sources do not experience outcomes. We also tested information maximization motives more directly in a supplemental experiment (Experiment S2 in SOM) and found no moderation of the effect.

Open Questions and Future Directions

Several open questions remain. First, we documented the social-exploration effect in contexts in which exploration was, to some extent, desirable. It is not yet clear how social reminders influence exploration in contexts in which individuals are more averse to exploration. Possibly, the social-exploration effect would be weaker. For example, higher stakes could dampen the social-exploration effect by lowering exploration both in social and mere information contexts and therefore, decreasing the difference between these conditions. Future research could test situations that suppress exploration (e.g., when unknown options involve high risk or high costs) as a boundary condition. Similarly, when exploitation is especially preferable (e.g., when known options involve high sentimental value or personal meaning), the social-exploration effect may be weaker.

Second, our experiments focused on exploration contexts in which individuals learned about a single other person's choices and experiences. Though this design captures the essential dichotomy between social and nonsocial exploration, future research could explore whether the individuality of the social entity—whether it is a single person or a group—influences the social-

exploration effect. It is possible that the requisite we-perspective tendency is activated by merely knowing that one is exploring with a social entity, regardless of whether they are an individual or a group. In this case, we would expect social exploration to be equally powerful in both contexts. However, past work on the identifiable victim effect (Jenni & Loewenstein, 1997) demonstrates that individual targets and group targets can elicit different reactions and treatment, which may translate to moderation of the social-exploration effect.

Lastly, future research could extend our findings by investigating other measures of we-perspective-taking in order to provide further evidence for the proposed mechanism. For instance, future experiments could examine whether individuals exploring in social contexts use more inclusive pronouns (e.g., "we" and "us") when describing their experiences in the game. We would indeed predict that the use of inclusive language would be higher for people exploring in social contexts than those exploring in mere information contexts.

Conclusion

We follow a rich tradition of behavioral decision researchers in employing a minimalistic paradigm in our empirical work, using monetary and experiential rewards. Despite its simplistic nature, this paradigm mirrors the characteristics of many real-life decisions. From decisions about financial investments, to entertainment, to education, people use information that comes from the experiences of others when choosing how and whether to explore their environment. Our paradigm was designed to capture the psychology underlying such decisions.

We found that increasing awareness that existing information had been obtained through other people's experiences encouraged exploration. For those seeking to encourage exploration in others, our findings offer a simple intervention: make salient the fact that existing information is available due to previous explorers.

Table 1.1

Overview of studies and exploration rates across studies

Experiment	*n*	Main Finding	Average exploration rate (across reward amounts and independent decisions) and effect sizes			
			Mere Info	Social Info	Statistical Test	Effect Size
1a	427	Participants were more likely to explore when the source of information about a known option was another player (vs. an unmentioned source).	55%	72%	$\chi^2(1, N = 427) = 12.83, p < .001$	Odds Ratio = 2.08
1b	251	Participants were more likely to explore when the information source was another player (vs. computer), regardless of framing the choice as a game or a profit-maximization task.	44%	71%	$\chi^2(1, N = 251) = 18.53, p < .001$	Odds Ratio = 3.10
1c	264	Participants in a collectivistic society were more likely to explore when the information source was another player (vs. computer).	13%	36%	$\chi^2(1, N = 264) = 17.80, p < .001$	Odds Ratio = 3.64
2 (pretest)	100	Framing of options as losses (vs. gains) reduced group-level perspective-taking.	N/A	N/A	$t(98) = 2.99, p = .003$	$d = 0.60$
2[a,b]	202	Only for gains (vs. losses), participants explored more when the information source was another player (vs. computer).	0.98[a]	1.98[a]	$F(1,198) = 22.43, p < .001$	$\eta_p^2 = 0.10$
3 (pretest)	81	Participants reported more group-level perspective-taking when the information source was another player (vs. computer).	N/A	N/A	$t(80) = 5.52, p < .001$	$d = 0.61$
3[b]	403	Participants explored more when the information source was another player who experienced the known option and either chose it or not, than when it was a computer or when it was another player who chose but did not experience the known option.	0.93	1.71	$t(399) = 5.19, p < .001$	$d = 0.71$
4	605	Participants were more likely to explore movie clips when the source of information was another player (vs. computer).	41%	51%	$\chi^2(1, N = 605) = 6.04, p = .014$	Odds Ratio = 1.54

[a] Results of Experiment 2 presented in this Table only include the gain conditions.

[b] Results reflect number of exploration decisions (up to 3).

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Chapter 2

Ending on a Familiar Note: Perceived Endings Motivate Repeat Consumption

Abstract

People fill their free time by choosing between hedonic activities that are new and exciting (e.g., exploring a buzzed-about restaurant) versus old and familiar (e.g., revisiting the same old spot). The dominant psychological assumption is that, holding constant factors like cost, availability, and convenience between acquiring such options, people will prefer novelty ("variety is the spice of life"). Eight preregistered experiments (total N = 5,889) reveal that people's attraction to novelty depends, at least in part, on their temporal context—namely, on perceived endings. As participants faced a shrinking window of opportunity to enjoy a general category of experience (even merely temporarily; e.g., eating one's last dessert before starting a diet), their hedonic preferences shifted away from new and exciting options and toward old favorites. This relative shift emerged across many domains (e.g., food, travel, music), situations (e.g., impending New Year's resolutions, COVID-19 shut-downs), and consequential behaviors (e.g., choices with financial stakes). Using both moderation and mediation approaches, we found that perceived endings increase familiarity preferences because they increase people's desire to ensure a personally meaningful experience on which to end, which returns to old favorites generally provide more than exploring novelty does. Endings increased participants' preferences for old favorites even when it meant sacrificing other desirable attributes (e.g., exciting stimulation). Together, these findings advance and bridge research on hedonic preferences, time and timing, and the motivational effects of change. Variety may be the "spice of life," but familiarity may be the spice of life's endings.

Imagine you have some free time this weekend that you can fill to your heart's desire. What kinds of experiences would you fill it with? On the one hand, you might consider doing something new and exciting, like watching a buzzed-about blockbuster that has been on your radar or springing for a lavish dessert that you have long wanted to try. On the other hand, you could simply return to an old favorite, like enjoying your go-to re-watch or trusty chocolate cake.

People face countless decisions regarding whether to fill their time exploring novel experiences or revisiting familiar ones, which end up wielding substantial impact on everyday well-being (Lyubomirsky, Sheldon, & Schkade, 2005; Sheldon, Boehm, & Lyubomirsky, 2013). In turn, a large psychological literature highlights the pull of novelty: People generally prefer doing something new and exciting over repeating already-consumed activities ("variety is the spice of life": Kahn & Ratner, 2005; Lyubomirsky et al., 2005; McAlister & Pessemier, 1982; Ratner, Kahn, & Kahneman, 1999; Read & Loewenstein, 1995; Sheldon et al., 2013; Simonson, 1990). Holding constant factors like cost, availability, and convenience between acquiring such options, most of us would likely choose to enjoy yet-unseen movies and long-envisaged desserts.

Now imagine the same dilemma with one small difference: Your free time this weekend happens to mark your last chance that you will be able to watch any movies for a while (perhaps you are approaching an especially busy time at work) or likewise consume any desserts (perhaps you are about to start a diet). Does this change your choice? If so, would you grow even more inclined to pursue a new and exciting option, or might you try to end things on a familiar note?

The current research explores how perceived endings, even the mere-impermanent ones that arise in everyday life (like in the above examples), influence the kinds of experiences people pursue. As we will review, perceived endings likely motivate people to try to maximize the present moment—to "end on a high note." The current research asks: What kinds of experiences

do people seek out in order to do that? We find robust evidence that endings shift people away from the tendency to chase enjoyment from new and exciting experiences and toward simply returning to old favorites—thereby revealing important nuances to current understandings of hedonic preferences, how those preferences change, and how to maximize well-being writ large. Even when people generally prefer novelty and variety in the things they consume, the old and familiar may also add its own spice to life as opportunities to enjoy either option draw to an end.

Novelty versus Familiarity

In the current research, we examine how people navigate choices between novel hedonic activities (i.e., enjoyable activities that they have not yet experienced) versus familiar hedonic activities (i.e., enjoyable activities that they have already experienced)[1]—thereby capturing the typical tradeoff that people commonly face when choosing how to fill their time in the present.

Novel and familiar activities sometimes differ in substantive ways that will differentially constrain people's behavior. Even if one has a clear preference to try out skydiving this weekend, for example, doing so is inherently less feasible than relaxing (yet again) at home. In many cases, however, decisions between novel and familiar options are otherwise similar in cost, availability, convenience, and so forth. Such decisions span across everyday offerings (e.g., choosing which Netflix offerings to watch or which ice cream flavors to enjoy—new interests or old favorites) as

[1] In theory, these dimensions are separable; a first-time option could feel familiar (e.g., by reading a lot about it beforehand) while a repeat option could feel unfamiliar (e.g., by having forgotten about it since last experiencing it). In such cases, our framework predicts that the construal of familiarity is operative; endings will prompt preferences for a more seemingly-familiar option (see General Discussion). However, here and throughout this article, we interchangeably refer to "novel" activities as equivalent to "first-time" activities and "familiar" activities as equivalent to "repeat" activities, as we assume that these pairings reflect the vast majority of cases.

well as more consequential dilemmas (e.g., having to allocate one's limited resources across different kinds of social events or passion projects).

To date, the dominant assumption in the literature is that people prefer novelty. Novelty provides many hedonic benefits, from absorbing attention and thus promoting immersion and savoring (Sansone, Weir, Harpster, & Morgan, 1992; Quoidbach & Dunn, 2013), to satisfying curiosity and expanding one's "experiential CV" (Keinan & Kivetz, 2011), to fostering creativity (Ritter et al., 2012) and leaving a rosy trace in memory (Ratner et al., 1999). People also derive social utility such that choosing novelty signals to others that they must have desirable traits (e.g., exciting, fun: Ratner & Kahn, 2002). Novelty also tends to be more intensely enjoyable than familiarity given that people have not yet experienced satiation or hedonic adaptation to novel stimuli (Frederick & Loewenstein, 1999; Raju, 1980); accordingly, people are slower to adapt to a stimulus as its novel features are made more salient (Langer & Moldoveanu, 2000; Redden, 2008; Quoidbach, Mikolajczak, & Gross, 2015). In light of these benefits, it is perhaps unsurprising that positive psychologists have explicitly prescribed people to pursue novelty and variety to maximize their hedonic experiences (Lyubomirsky et al., 2005; Sheldon et al., 2013). By the same logic, researchers have also suggested that people are inherently averse to repeat consumption, even absent an enjoyable novel alternative; one reason that people pursue novelty is simply to avoid repetition, due to the belief that many activities have little left to offer after doing them once in full (and thus seem like a waste of time to re-experience: O'Brien, 2019).

Despite diverse support for these claims, they may also paint an overly uniform portrait. People sometimes actively reject novelty and instead pursue repeats to maximize their hedonic experiences (e.g., as opposed to doing so only because of differential costs or due to passively following a routine); we choose to re-watch the same movies, reorder the same dishes, and so on.

People prefer old favorites and "familiarity's warm glow" (Titchener, 1915)—sometimes. It is therefore worth taking a closer look at potential psychological explanations for this discrepancy.

Previous research documents that certain kinds of stimuli provide more subjective value upon repetition than others, such as those that are complex rather than simple (Berlyne, 1970), experiential rather than material (Nicolao, Irwin, & Goodman, 2009), and strongly rather than weakly emotional (Chugani, Irwin, & Redden, 2015; Yang & Galak, 2015). These findings suggest that people may be more likely to engage in repeat consumption depending on features of the stimulus. People may also avoid repeat consumption for similar stimulus-level reasons (e.g., avoiding revisiting a cherished spot in town to protect one's fond memories: Zauberman, Ratner, & Kim, 2009). Individual-level factors might also matter, such as a person's affective state (e.g., positive mood: Kahn & Isen, 1993; feeling nostalgic: Wildschut, Sedikides, Arndt, & Routledge, 2006) and personality profile (e.g., trait sensation seeking: Zuckerman, 1979). People may also differentially prefer novel versus familiar experiences in response to discrete life events (e.g., the effect of the "fresh start" of New Year's Day on pursuing new goals: Dai, Milkman, & Riis, 2014; the effect of life stressors on seeking comfort foods: Wood, 2010). Finally, features of the choice context itself may further distinguish such preferences, such as when people make choices for the near versus far future (McAlister & Pessemier, 1982; Read & Loewenstein, 1995; Simonson, 1990) and in isolation versus with others (Ariely & Levav, 2000; Choi, Kim, Choi, & Yi, 2006; Ratner & Kahn, 2002; Winet, Tu, Chosen-Hillel, & Fishbach, 2022). These kinds of choice contexts are likely also influenced by mere framing effects (e.g., people are more likely to engage in repeat consumption when choices are framed as signaling loyalty rather than boredom: Fishbach, Ratner, & Zhang, 2011; for a review of other such frames, see Galak & Redden, 2018).

The current research investigates a more general (and yet-untested) factor that might importantly shape people's preferences beyond these known culprits. We explore how people navigate novelty and familiarity as a function of limitations on remaining opportunities to enjoy either—that is, how perceived endings might influence how people fill their time in the present.

The Psychology of Endings

Endings come in many shapes and sizes. Things end due to natural forces (e.g., the end of summer), time-bound plans (e.g., the end of a vacation), and unexpected events (e.g., the last week of restaurant operations before a pandemic shut-down). Things can end for good (e.g., one's last meal before death), and for the time being (e.g., one's last meal before starting a diet).

The current research focuses on such situations in which people perceive a shrinking window of opportunity to enjoy an activity category, even if these lost opportunities are merely temporary. That is, we focus on how, for a given activity category (e.g., watching movies), when opportunities to enjoy anything in that category will become lost (e.g., facing one's last movie night before a busy patch at work), this affects which thing people then pick for their last chance to enjoy the category at all (e.g., which movie people watch—a new release or an old favorite). We do not focus on the end components within a stimulus; these components could entail other novelty/familiarity dynamics, but such dynamics do not bear on our hypothesis (e.g., perhaps the last scene of a movie prompts viewers to recall the many preceding "familiar" scenes they have viewed to that point versus the dwindling few "novel" scenes left; or, perhaps a movie feels more familiar by the time that viewers reach the last scene—but these are unrelated to our hypothesis).

Previous research suggests that perceived endings will motivate general desires to maximize; people likely seek to "end on a high note." Evidence for this idea is vast. On average, for example, people prefer experiences whose sequences improve rather than decline over time

(despite being otherwise equivalent; Loewenstein & Prelec, 1993), that have happy rather than sad endings (Ross & Simonson, 1991), and that end with redemption ("all's well that ends well": Diener, Wirtz, & Oishi, 2001; Newman, Lockhart, & Keil, 2010). People reserve their favorite experiences for the end of a sequence ("saving the best for last": Ratner et al., 1999), and judge the same experience more favorably if it happens to occur at the end ("saving the last for best": de Bruin, 2005; Li & Epley, 2009). People want the bad news first and good news second (Legg & Sweeny, 2014). People recall the end moments of hedonic experiences better than most other moments (Fredrickson & Kahneman, 1993; Redelmeier & Kahneman, 1996), making end moments especially likely to steer future behavior (Garbinsky, Morewedge, & Shiv, 2014). Indeed, imminent endings motivate people to invest more time and effort into savoring what precious few moments they have left to have a given experience (e.g., college students engage in more college-related activities when graduation feels like it is looming; Kurtz, 2008). Likewise, in performance settings, people work harder in their final steps of goal pursuit (Hull, 1932) and pay closer attention during their final competitive attempts (Shah, Mullainathan, & Shafir, 2012).

These diverse examples illustrate that, generally speaking, ending contexts will likely impel people to choose whichever hedonic activities they think will help them "make the most" of the moment; knowing that one is about to have one's last time getting to enjoy an activity for some time may feel like an event that people will thus want to honor and match accordingly (Shu & Sharif, 2018). Returning to our research question, however: What kind of experience—a novel option or familiar option in that same activity category—do people pursue to make this happen?

The Safer Bet: A Hypothesized Shift Toward Familiarity

It is tempting to assume people might seek to make the most of these last consumption opportunities by choosing things that are new and exciting. After all, the stereotypical notion of

the "bucket list" describes people becoming motivated to pursue activities that they have always wanted to try, but have not yet experienced, as they approach major life endings. Aging out of a life stage predicts a higher likelihood of running one's first marathon and expressing interest in having an extramarital affair (Alter & Hershfield, 2014); imminently moving away from home predicts a higher likelihood of visiting yet-unvisited local landmarks (Shu & Gneezy, 2010); entering retirement predicts finally getting around to picking up new hobbies (Freund, 2020); and indeed, approaching death predicts desires for new experiences like exotic travel and doing daring activities, with clinicians even being encouraged to discuss these kinds of "bucket list" items with their patients to provide tailored end-of-life treatment (Periyakoil, Neri, & Kraemer, 2018). More broadly, studies on time perspective confirm that a tendency to prioritize one's immediate present experiences (versus one's future experiences) is positively correlated with the pursuit of hedonically stimulating behaviors (Keough, Zimbardo, & Boyd, 1999; Rothspan & Read, 1996; Zimbardo & Boyd, 1999; Zimbardo, Keough, & Boyd, 1997). More impermanent-ending contexts (e.g., one's choice of "the last dessert" before officially starting a diet) are clearly less dramatic than the contexts of these major life endings, but they might still share a similar psychological foundation; all else equal, "ending on a high note" should, in principle, be more easily accomplished by consuming novelty rather than familiarity, not least thanks to the inherently stimulating and yet-to-be adapted-to nature of novel experiences (e.g., Raju, 1980).

Alas, in practice, the reality of this choice is that all else is not equal, at least in one fundamental way: Yet-to-be-experienced novelty is riskier than tried-and-true familiarity. This basic fact forms the basis of our hypothesis: Ending contexts may instead, on average, shift people from exploring the new and exciting and toward simply returning to old favorites.

Indeed, a broader way for people to ensure they "end on a high note" could be to stick with what they know works for them—old favorites typically make for the safer bet as compared to novel equivalents, which could turn out to be better or worse (for yet-unforeseen reasons). In other words, given that perceived endings increase the psychological stakes of a choice, then ending contexts may make people averse to taking risks that could spoil the moment—and in turn push them to prefer the old and familiar more than they otherwise might.

Old favorites, by definition, include familiar features that people can typically rely on to be valuable again (otherwise they would not be old favorites). Novelty cannot promise such concrete and reliable value (e.g., one can be surer of what an old-favorite museum will be like, given their past experience(s) there, while a new museum could be a dud right from the get-go). A wide variety of research suggests that high-stakes contexts reduce people's risk tolerance ("Better the devil you know than the devil you don't"), ranging from research on attachment theory (e.g., infants with more uncertain child-caregiver relationships are less willing to leave the safety of their familiar caregiver's side to explore strange new environments: Ainsworth, Blehar, Waters, & Wall, 2015), to research on loss aversion and general threat management (e.g., when consumers feel unsafe—such as when making choices in crowded versus uncrowded contexts—they are more receptive to prevention-focused ads and more likely to choose safety-oriented products: Maeng, Tanner, & Soman, 2013), to research on everyday decision making (e.g., people prefer more familiar, yet harder, puzzle tasks when under pressure: Litt, Reich, Maymin, & Shiv, 2011; organizations return to more familiar, yet less creative, strategies in the face of competition: March, 1991; people confronted with scarce resources put more focus on problems at hand over less salient ones: Shah et al., 2012). Indeed, computational models of exploration-exploitation tradeoffs show that shrinking time horizons tend to reduce people's willingness to

incur the risks of exploring new environments in favor of exploiting the benefits of known ones (Wilson, Geana, White, Ludvig, & Cohen, 2014). As applied to our research, perceived endings might increase the appeal of familiar options because familiar options promise stimulus features that one can count on liking—at least well enough to not spoil what precious few moments remain before the ending—while taking a chance on novelty may indeed spoil it.

Safer How? Desires to End Meaningfully

Going further, a key question is: What specific features do old favorites uniquely promise that might satisfy people's desires to end well?

One straightforward answer is that people can be more certain about the hedonic quality of familiar stimuli (versus novel stimuli); people know for sure that they really did enjoy having that experience at least at some point in the past. Accordingly, perhaps endings motivate people to ensure hedonic quality (e.g., pure sensory pleasure) as best they can, which leads them to take the safer bet: the familiar option. In celebrating their last restaurant visit before starting a diet, people might prefer a restaurant where they know the food will be sufficiently tasty; this choice guarantees some certainty for indulging in one's "last hurrah"—for this reason, people may shift their preference toward a go-to spot with familiar, tried-and-true dishes, even if this means losing out on the more exciting but yet-untested dishes at a hot new spot they have been longing to try.

On the other hand, drawing on research from elsewhere in psychology, we hypothesize that people might also seek to "end on a high note" so as to ensure a different feature than pure hedonic enjoyment: to ensure personal meaning. This quality is something that people can also be characteristically certain of finding among the old and familiar (versus the new and exciting)[2].

[2] Of course, some familiar experiences decidedly lack positive qualities like meaning (e.g., presumably, few people would actively seek out their monotonous bumper-to-bumper commute, in any context; van Tilburg & Igou, 2012). Note that we focus on "old favorite" hedonic

That is: We posit that, when people try to end well, they mainly look for a personally meaningful experience on which to end—and this quality too is more certain to be found in old favorites (vs. novelty). In celebrating their last restaurant visit before starting a diet, people might prefer to have an experience that helps them poignantly honor the moment as best they can—and rather than primarily try to ensure that the food itself is tasty (i.e., to better ensure hedonic value), they may prefer to ensure that the experience has sentimental qualities (i.e., to better ensure personal meaning)—which, more often than not, will call for a familiar experience.

Defining "meaning." Scholars have long noted the difficulty of defining meaning (Leontiev, 2013). However, there is general consensus around the importance of personal value. Heintzelman and King (2013) define meaning as comprising "purpose," "mattering," and "coherence." Steger (2012) defines meaning as a "web of connections, understandings, and interpretations" that promotes "the sense that our lives matter, that they make sense, and that they are more than the sum of our seconds, days, and years" (p. 65). McAdams (1993) defines meaningful experiences as those that feed into one's life narrative and signal identity closeness; experiences that answer "Who am I?" are meaningful. As such, we broadly refer to people shifting toward preferring "meaningful" experiences as shifting toward preferring experiences with higher personal value (e.g., those that are more sentimentally linked with "who they are").

Endings prompt desires for personally meaningful—and thus familiar—experiences. With this definition in mind, other research suggests that ending contexts may

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activities, such that people indeed value them to begin with (in order to provide a fair comparison with exciting novel counterparts). In any case, an emerging line of research suggests that people often come to view even seemingly-dull routines (and the like) as surprisingly meaningful (Martela & Steger, 2016; Heintzelman & King 2019)—especially in response to losing them (Powell, Barasch, & Alter, 2022)—so they may fit into our framework just the same. We return to this potential (lack of) moderation in the General Discussion.

indeed shift preferences toward choosing a meaningful experience on which to end, which familiar experiences (relative to their novel equivalents) may be more disposed to offering, on average.

First, as reviewed, one's final consumption opportunity for a while likely feels like an event that people may prefer to match appropriately (akin to "occasion matching": Shu & Sharif, 2018)—and people might view something that is meaningful as an especially appropriate match for that moment (versus something that offers, for example, purely exciting stimulation). Broadly speaking, personal meaning is regarded as among the most elevated attributes with which to imbue an experience (King, Hicks, Krull, & Del Gaiso, 2006; King & Napa, 1998); seeking out meaningful experiences therefore may represent an especially befitting way to honor a special occasion. More specific to ending occasions, other research finds that endings (e.g., graduating from college) often elicit mixed emotions that are both positive and negative (e.g., feelings of poignancy: Ersner-Hershfield, Mikels, Sullivan, & Carstensen, 2008; Larsen, McGraw, & Cacioppo, 2001), in part because people are reminded of good times, but then lament that they are lost to the past[3] (Larsen, Hershfield, Cazares, Hogan, & Carstensen, 2021). In turn, returning to old favorites may better reflect this type of bittersweet experience than the uniform excitement of novelty would, as old favorites may be intertwined with one's closely-held and personally-meaningful memories (whose value need not come from pure hedonic pleasure, necessarily)—

[3] This kind of cognitive explanation suggests yet another mechanism that might push people toward familiarity in the context of endings—such that perceived endings remind people of their best past experience, which they then compare to an average (for instance) novel experience—but our studies, by design, will largely rule out this possibility alone as a driver (e.g., by having all participants compare equally desirable stimuli, and even the same identified stimulus, across temporal contexts). In any case, such an explanation suggests yet another reason why our hypothesized effect might emerge beyond the laboratory in everyday life.

and thus these revisits to the past may seem like especially good matches for ending contexts. Other occasions, like new beginnings, may instead be better suited to uniformly exciting novelty.

Second, socioemotional selectivity theory (Carstensen, Isaacowitz, & Charles, 1999) posits that shrinking time horizons motivate people toward emotionally rewarding goals (as compared to, for example, learning new things), which is typically tested in the context of end-of-life effects on who older adults spend time with. For example, older adults (versus younger adults) tend to express stronger preferences to spend more time with family and close friends and less time with strangers and new acquaintances (e.g., Carstensen, 2006; Ersner-Hershfield et al., 2008; Fredrickson & Carstensen, 1990). To our knowledge, a major reason for this shift is that novel tastes often take time to be acquired—and time is precisely what such individuals lack. This is especially true in the social domain, as usually tested in this literature. Trading one-liners with a stranger cannot compete with intimately joking with a friend—and so lacking the time it takes to turn strangers into friends makes otherwise fun novel interactions less attractive (and in turn makes already-rich interactions more attractive). As applied to our research, and extending far beyond end-of-life effects on social preferences, familiar experiences in general may become more attractive in ending contexts because people can be more certain about them striking a sufficiently meaningful chord in those last moments—without requiring further investment (a luxury that they lack) to make it meaningful. In the General Discussion, we return to how our research helps enrich and expand this typical understanding of socioemotional selectivity theory.

Third, people may also become more likely to prefer to end with familiar experiences because endings with meaning feel more psychologically fluent than endings without meaning. That is, familiar experiences may fit a natural relationship between endings and a desire for closure. One cannot help but notice the pervasive sentimental theme of ending on both familiar

and meaningful notes in hedonic cultural products like storytelling (e.g., protagonists who return home at the end of the "hero's journey," a narrative that dates back over 3,000 years; Campbell, 1949) and music (e.g., end-of-song codas that repeat earlier refrains: Perle, 1990)—the existence of which might be evidence of revealing this sort of ingrained preference. Indeed, people tend to prefer experiences that end with emotional closure (Schwörer, Krott, & Oettingen, 2020), and returning to old favorites (versus ending on a fun, but new, note) might be an especially good way to obtain this kind of closure. To the extent that familiar endings "just feel right," and such psychologically fluent experiences are pleasurably reinforced and adopted (Reber, Schwarz, & Winkielman, 2004), people might show a corresponding shift in their hedonic choices as well.

To sum, we have proposed that perceived endings (even impermanent ones) may shift hedonic preferences toward ending things on a meaningful note—and returning to the familiar (e.g., enjoying one's old-favorite dessert one last time before starting a diet) may, in most cases, afford just that. Familiar (versus novel) options should indeed represent the more meaningful option, on average, as people are more likely to have established a special connection that renders that option more meaningful than pure novelty (Carmon, Wertenbroch, & Zeelenberg, 2003; Heintzelman & King, 2019; Zhang, Kim, Brooks, Gino, & Norton, 2014). In one study, for example, nearly half of participants described the importance of familiar foods in terms of their nostalgic value (Wood, 2009). Associations between revisiting the ordinary and consequential outcomes like happiness grow stronger as people age (Bhattacharjee & Mogilner, 2014), further hinting that people may shift more toward familiarity in ending contexts.

To be sure, old favorites often make for a safer bet on both dimensions posed here—one's cherished restaurant (personal meaning) may also serve one's favorite foods (hedonic quality)—but our key points are that (i) both possibilities make the same directional prediction:

Ending contexts might directionally shift people's preferences away from novelty and toward familiarity; and that (ii) both possibilities share the same umbrella mechanism: This shift may reflect people being compelled away from the risks associated with novelty and toward the safer bets associated with familiarity with regard to having a positive experience; and lastly, that (iii) teasing apart these dimensions of "positive experience" as drivers of this preference shift can provide more nuanced insight into what kinds of experiences people try to optimize in the face of perceived endings. Such nuance bears not only on when and why people will exhibit such a shift, but it also reveals the specific kinds of utility that people derive from "familiarity's warm glow."

The Current Research

We sought answers to these questions by conducting 8 preregistered experiments (total N = 5,889), spanning a diverse range of contexts, measures, and participant populations. Across all experiments, we investigated whether people's preferences for familiar versus novel hedonic experiences change as a function of perceived endings—and if so, how and why they change.

We hypothesized that perceived endings would increase people's preferences for the familiar (over the novel), and that this would be driven by increased desires to end on the safer bet for having a positive experience—specifically, to have a more personally meaningful time. We designed our experiments such that they hold constant other differences between novel and familiar options that might otherwise influence people's preferences (e.g., cost, availability, convenience, and so forth), except in studies where we intentionally manipulated these features for purposes of hypothesis testing (e.g., to disentangle ensured hedonic quality from ensured personal meaning). To further serve this goal, our experiments largely focus on impermanent endings (e.g., people facing their "last chance for a while" to engage in a hedonic activity) as opposed to on literally-permanent endings (e.g., people facing the end of life). Such endings

entail explicitly temporary restrictions on future opportunities to enjoy a given experience, and are presumably more common than literally-permanent endings, yet are also far less studied in the literature (to our knowledge). In the General Discussion, we return to how these kinds of endings might compare and contrast with one another, and how future research might uniquely benefit from a further study of more impermanent endings (as most of our experiments help highlight).

Experiment 1 assessed the basic effect—whether perceived endings increase people's preferences for familiar (versus novel) activities—via hypothetical behavior across many domains. Experiments 2–4 extended this effect to real behavior. Experiments 5–8 again replicated this effect while also assessing mechanisms: Perceived endings may increase people's preferences for old favorites because they are more likely to offer a personally meaningful experience on which to end (beyond ensuring hedonic quality per se).

For each experiment, we predetermined sample sizes of at least 250 participants per experimental cell, depending on available resources. We report all manipulations, measures, and exclusions (if any). All data files, full original experiment materials, and preregistration documents have been made public for review on the Open Science Framework: https://osf.io/pf63y/.

We predetermined this sample size based on a rule-of-thumb of 100 participants per cell, and then for good measure, because our primary dependent variable is choice, we multiplied this number by 2.5x (following Simonsohn [2015], who advises collecting 2.5x one's original sample size to safeguard the power of replication attempts). For each experiment, we report a sensitivity analysis of the minimum effect size each of our samples had power to detect (all via G*Power [Faul, Erdfelder, Lang, & Buchner, 2007], assessing each critical test at $\alpha = 0.05$, 80% power).

Experiment 1:

Perceived Endings and Hedonic Preferences

[preregistration: https://aspredicted.org/5xy86.pdf]

In Experiment 1, we assessed initial evidence for how perceived endings influence the kind of activity that people choose to do—a new and exciting option versus an old-favorite equivalent—for this "last time" for a while to enjoy either. For robustness and generalizability, participants reported their preferences across 10 unique hedonic domains and we conducted this same experiment among three unique participant populations. In all cases, we hypothesized that perceived endings (versus control contexts) may increase people's preferences for old favorites.

Method

Participants. We launched the same experiment among three populations, comprising laboratory participants, student participants, and national online participants (total N = 1,124).

First, we requested 500 participants through Amazon's Mechanical Turk, yielding 501 (M_{age} = 35.81, SD_{age} = 10.44; 40.12% female; 25.95% non-White) who participated for $1.00 (sensitivity analysis: minimum effect size that can be detected by this sample size is w = 0.13).

Second, we requested 500 participants from our university subject pool, yielding 460 (M_{age} = 31.66, SD_{age} = 13.90; 48.70% female; 59.35% non-White) who participated for $1.20. Our pool is a mix of students, staff, and locals in a large Midwestern city in the United States (sensitivity analysis: minimum effect size that can be detected by this sample size is w = 0.13).

Third, we recruited 163 M.B.A. students from the same university (M_{age} = 31.05, SD_{age} = 4.37; 26.99% female; race/ethnicity not reported) who participated as part of a course. The experiment was made available to the total course enrollment of 165 students (sensitivity analysis: minimum effect size that can be detected by this sample size is w = 0.22).

Procedure. The study design was identical between our Amazon Turk participants and subject-pool participants: Participants were randomly assigned to a 2 (Context: Control versus Ending, between-subjects) × 10 (Domain: 10 unique scenarios, within-subjects) design. For M.B.A. participants, we turned the design fully within-subjects in order to make maximal use of the more limited sample size. Doing so also further generalizes the potential effects of ending contexts regardless of whether they are assigned between-subjects or within-subjects.

Participants evaluated 10 different scenarios, each involving a choice between hedonic activities in a different domain, and reported how they would behave in each. The scenarios included: seeing live bands, going to the beach, reading books, visiting cities, eating desserts, seeing movies, visiting museums, eating out at restaurants, socializing, and playing sports.

For each, participants faced a choice between pursuing a new activity that they would very much enjoy experiencing versus pursuing an already-consumed activity that they very much enjoyed experiencing, holding all else equal. For example, participants read the following for the "restaurants" scenario (see OSF for all 10 scenarios, which used similar phrasings as below):

> Imagine you have the choice between 2 restaurants: One is a restaurant that you have gone to before and absolutely loved eating at, and the other is a restaurant that you haven't gone to before and would absolutely love to eat at. Both cost the same money, time, etc. to go to. Imagine you now happen to have equal access to both (e.g., you're equally available and able to go to either). The choice is entirely yours for the choosing.
>
> Ok: Which restaurant would you choose to go to for your next restaurant opportunity?

Then, Control participants (MTurk: $n = 256$; subject pool: $n = 229$; M.B.A.: $n = 163$) made their choice between two options, comprising our key dependent variable (e.g., for "restaurants"): "In this case, I'd choose the restaurant that I haven't gone to before and would absolutely love to eat

at" (novel option) versus "In this case, I'd choose the restaurant that I have gone to before and absolutely loved eating at" (familiar option), with participants making 10 such choices in total.

We compared the choices of control participants to choices of Ending participants (MTurk: $n = 245$; subject pool: $n = 231$; M.B.A.: $n = 163$—the same sample), who followed the exact same procedures except they read the following additional text (e.g., for "restaurants"):

> There's one rule: It turns out, your next restaurant opportunity is the last time you'll be able to go to a restaurant for a while. Imagine that whatever you choose for this would be the last time you get to go to a restaurant for quite some time.

For each scenario, the presentation order of new and old activities was randomized between-subjects as written in the text and was randomized within-subjects as choice options. All participants were instructed to treat the scenarios independently (i.e., to assume their choices in one have no bearing on their choices in any other). Each scenario was presented on a separate page, with participants reading and reporting their responses one at a time in randomized order.

Other variables. At the end of the experiment and after making all 10 choices, participants reported their demographic information, rated their experiment confusion (1 = *not confusing*; 7 = *very confusing*), and rated whether mental images indeed came to mind for them (1 = *no, not really*; 7 = *yes, very much*). In this and other experiments, we assessed confusion and mental imagery to account for whether our Context manipulation had differential effects on what came to mind, since Ending participants had more information to read and process than Control participants (which could manifest as differences either way: as more or less confusing, and as more or less cognitively engaging). We assumed that an ideal test of our hypothesis would yield no such differences on these measures, as we intended in designing the stimuli. Finally, participants completed a manipulation check regarding their awareness of endings in each

scenario: “To what extent were you imagining each would be the last such opportunity to experience each thing for quite a while?” (1 = *definitely wasn’t imagining this*; 7 = *definitely was imagining this*). (Given their fully within-subjects design, M.B.A. participants only reported demographic information, and did not complete these condition-specific checks as they had experienced both conditions).

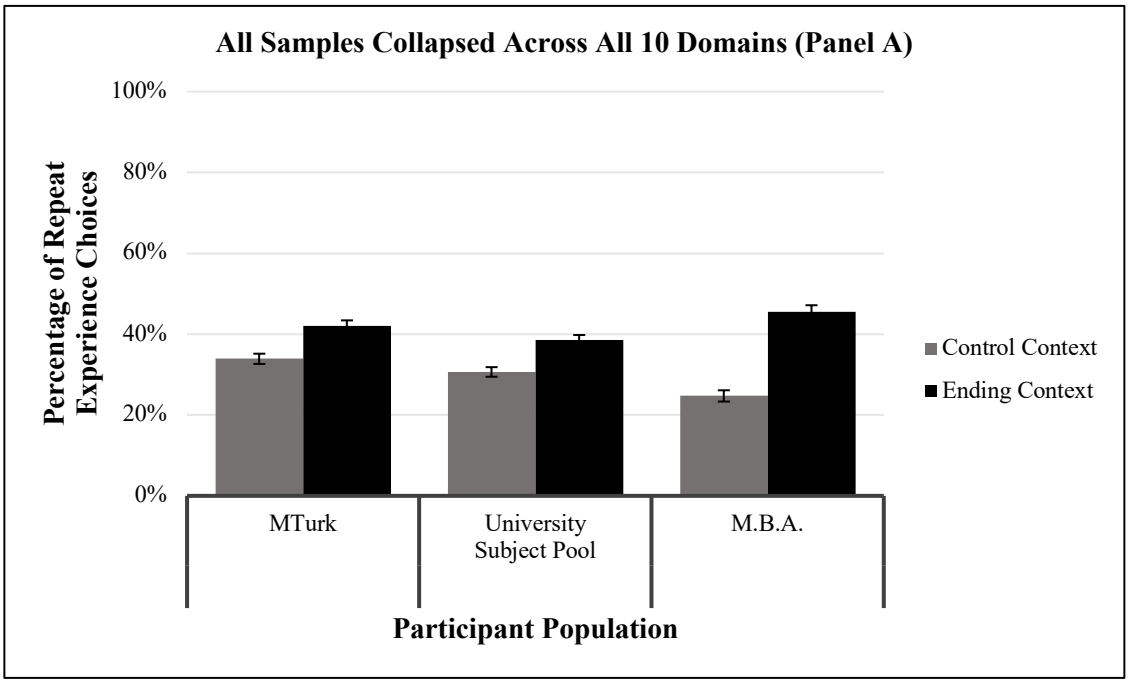
Results and Discussion

Separately for each participant population, we conducted repeated-measures logistic regression analyses, via the SPSS GEE command, entering Context, Domain, and their interaction term as predictors of activity choice (novel option versus familiar option).

Main results (activity choice). We observed robust support for the hypothesis in each participant population (see Figure 2.1, Panel A for overall means such that they collapse across domains, thus showing the average share of repeat choices per participant, out of 10 choices):

Figure 2.1

Experiment 1: Percentage of repeat choices by population and domain



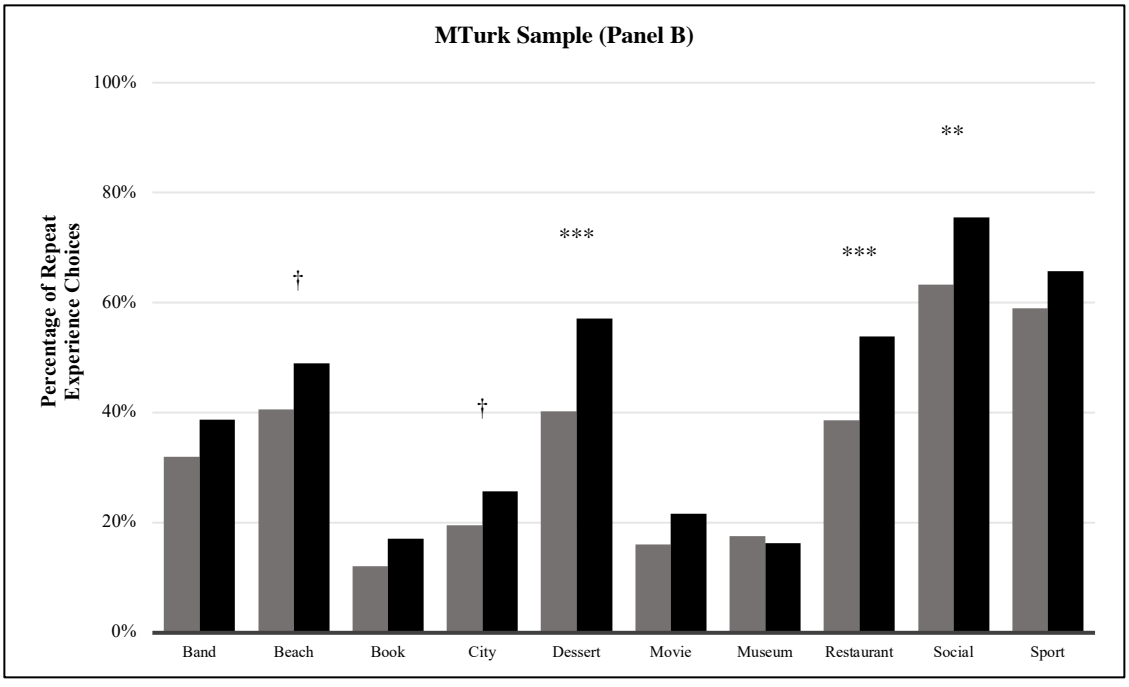
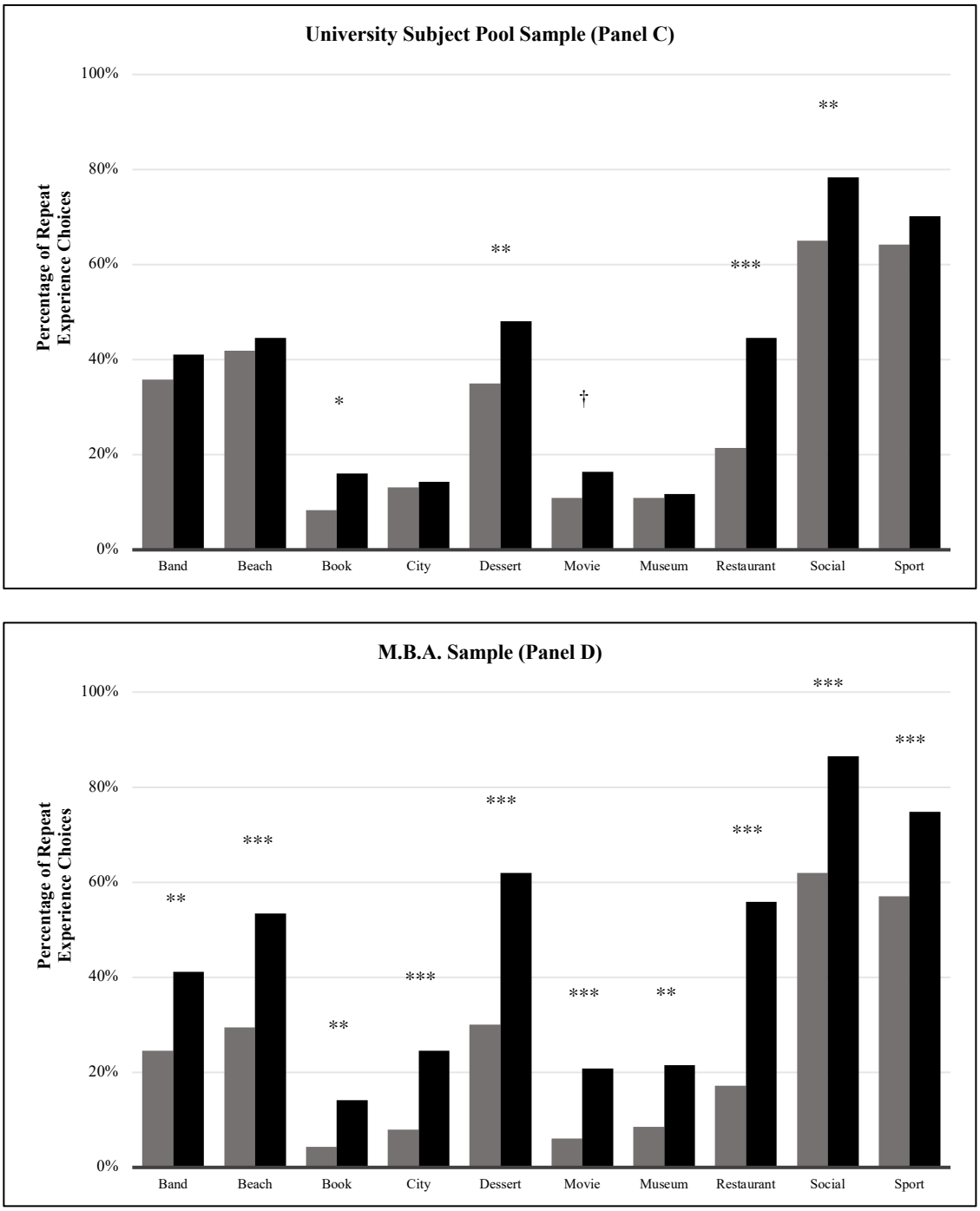


Figure 2.1 (*Continued*)

Experiment 1: Percentage of repeat choices by population and domain



Note. Panel A plots the mean percentage of repeat choices across all 10 domains (error bars show ±1 SE). For Panels B, C, and D, significance is flagged at: † = $p < .10$, * = $p < .05$, ** = $p < .01$, *** = $p < .001$.

First, among MTurkers (see Figure 2.1, Panel B): There was the critical main effect of Context, such that Ending participants were more likely to choose familiar activities (percentage of familiarity choices out of 10 choices: $M = 42.08\%$, $SD = 20.98\%$) than Control participants were ($M = 33.91\%$, $SD = 20.28\%$), Wald = 16.24, df = 1, $p < .001$, $w = 0.18$ (incidental main effect of Domain, Wald = 403.35, df = 9, $p < .001$, $w = 0.90$; interaction, Wald = 10.72, df = 9, $p = .296$, $w = 0.15$).

Second, this critical main effect of Context replicated among subject-pool participants (see Figure 2.1, Panel C): Ending participants were again more likely to choose familiar activities ($M = 38.53\%$, $SD = 19.21\%$) than Control participants were ($M = 30.66\%$, $SD = 17.99\%$), Wald = 17.89, df = 1, $p < .001$, $w = 0.20$ (incidental main effect of Domain, Wald = 663.35, df = 9, $p < .001$, $w = 1.20$; interaction, Wald = 24.84, df = 9, $p = .003$, $w = 0.23$).

Third, among M.B.A. students, too—in a fully within-subjects design—this critical main effect of Context also replicated (see Figure 2.1, Panel D): Ending participants were again more likely to choose familiar activities ($M = 45.46\%$, $SD = 21.75\%$) than Control participants were ($M = 24.72\%$, $SD = 17.82\%$), Wald = 85.00, df = 1, $p < .001$, $w = 0.72$ (incidental main effect of Domain: Wald = 305.72, df = 9, $p < .001$, $w = 1.37$; interaction, Wald = 24.83, df = 9, $p = .003$, $w = 0.39$).

Other results. The results of the manipulation check confirmed that Ending participants had endings on their minds to a greater degree than Control participants, both among MTurker participants ($M_{\text{Ending}} = 6.18$, $SD = 1.22$ versus $M_{\text{Control}} = 3.64$, $SD = 2.24$; $t(499) = 15.67$, $p < .001$, $d = 1.40$) and among subject-pool participants ($M_{\text{Ending}} = 5.00$, $SD = 1.85$ versus $M_{\text{Control}} = 2.72$, $SD = 1.97$; $t(458) = 12.84$, $p < .001$, $d = 1.20$).

In addition, there were *no* incidental differences between conditions on experiment confusion among MTurk participants (M_{Ending} = 1.73, SD = 1.58 versus $M_{Control}$ = 1.51, SD = 1.28; $t(499) = 1.70$, $p = .089$, $d = 0.15$), nor among the subject pool (M_{Ending} = 1.35, SD = 0.96 versus $M_{Control}$ = 1.39, SD = 0.99; $t(458) = 0.42$, $p = .677$, $d = -0.04$). We also found no such differences on mental imagery, neither among MTurk participants (M_{Ending} = 5.59, SD = 1.55 versus $M_{Control}$ = 5.44, SD = 1.59; $t(499) = 1.07$, $p = .284$, $d = 0.10$) nor among the subject pool (M_{Ending} = 5.07, SD = 1.72 versus $M_{Control}$ = 4.99, SD = 1.77; $t(458) = 0.51$, $p = .613$, $d = 0.05$). As stated, M.B.A. students did not complete either of these two condition checks, given their fully within-subjects design.

Finally, all patterns remain when re-analyzing the results while entering experiment confusion, mental imagery, and demographic information as covariates (see Supplemental Materials).

Experiment 1 reveals initial but robust evidence that ending contexts indeed shifted people's hedonic preferences, specifically by increasing preferences for the familiar.

Next, Experiments 2–4 sought to extend these findings beyond hypothetical scenarios by testing for the basic effect within specified contexts involving real behaviors with higher stakes.

Experiment 2:

New Year's Resolutions (real choice)

[preregistration: https://aspredicted.org/eg5hh.pdf]

In Experiment 2, we capitalized on the timing of a naturally occurring event that involves a temporary halt of hedonic activities: the New Year's holiday—in particular, the act of keeping a New Year's resolution. In December 2019, we recruited participants who expected to give something up for the New Year of 2020. We hypothesized that participants would shift their

preferences for how to spend this window of time in December—an ending context entailing choosing what to consume in the category of their chosen pleasure before giving it up for a while—toward familiar category options (versus novel category options). Then, via a longitudinal design, we tracked these same participants over time to assess whether their preferences shifted back to novelty once they were no longer caught within this ending context.

Method

Participants. We collected data in 3 waves across 3 months, all via Cloud Research. These specific recruitment dates (and only these dates) can be seen in our preregistration.

To begin, we sought to recruit participants who were planning to give something up as a New Year's resolution. Thus, upon launching Wave 1 (December 28th, 2019), we requested 500 participants. On the opening page, we screened participants for whether they intended to make a New Year's resolution to start on January 1st, 2020 (forced choice: *no*; *yes*). As preregistered, only those participants who indicated "yes" were retained and proceeded to the experiment measures; those who indicated "no" were piped to unrelated research for the remainder of data collection.

This process yielded a starting sample of 386 participants (M_{age} = 37.95, SD_{age} = 12.30; 50.78% female; 23.58% non-White) who began Experiment 2, all of whom were about to make a New Year's resolution (i.e., all of whom began the experiment already in an ending context).

Wave 1 (December 28th, 2019). These 386 participants then completed Wave 1 for $0.25.

Wave 2 (January 28th, 2020). Next, we contacted these 386 participants from Wave 1 to complete Wave 2. This process yielded 307 participants who completed Wave 2 for $0.25.

Wave 3 (March 28th, 2020). Last, we contacted the 386 participants from Wave 1 to complete Wave 3, all via the same recruitment procedures. This process yielded 278 participants, 257 of whom had completed both of the previous waves, who completed Wave 3 for $0.25.

Thus, all told: We tracked the same 257 participants (M_{age} = 39.92, SD_{age} = 12.56; 50.58% female; 20.23% non-White) who completed all 3 experimental waves, beginning in December 2019 and ending in March 2020, representing a retention rate of 66.58% (257 of 386) (sensitivity analysis: minimum effect size that can be detected by this sample size is w = 0.17).

Procedure. This experiment tracked the same participants over time, fully within-subjects. All participants took one survey at each wave's calendar date: Wave 1 (Ending wave, with endings salient), Wave 2 (Control wave, with endings less salient), and Wave 3 (another Control wave, with endings less salient). Each survey automatically recorded each participants' unique user ID, unbeknownst to the participants while participating in the experiment, thus allowing us to link their responses.

Wave 1: Saturday, December 28th, 2019 (Ending wave). To begin, participants best categorized a New Year's resolution that they would be implementing 4 days later (on January 1st, 2020). They chose from 1 of 10 options (shown in randomized order): going out to eat less often, shopping less often, watching less television, spending less time online, spending less time on the phone, spending less time gaming, cutting back on unhealthy drinks, cutting back on sweets, cutting back on a private bad habit, or cutting down on fun. The most popular resolution was "Cutting back on sweets," selected by 25.29% of participants (65 of 257).

Next came our key dependent variable. Participants were informed that they may be randomly invited to participate in an additional short follow-up study we would be launching at some point over the next 3 days (December 29th, 30th, or 31st), to start and complete during that

window. They were asked to choose which of two kinds of tasks they would want to complete in this other study, and were told that we would assign them whichever they chose. Shown in randomized order, they chose from "Tasks involving [description of novel option from resolution category]" versus "Tasks involving [description of familiar option from resolution category]."

For example, if participants indicated that they would be giving up "going out to eat less often," then they chose between "tasks involving places I've never eaten at before, but I'd love to eat at" (novel option) versus "tasks involving places I've eaten at before, and I love eating at" (familiar option) for this other study. Thus, all participants were in an ending context in the moment of their choice at Wave 1, which therefore allowed us to assess our hypothesis that participants may be more likely to choose familiarity in this wave as compared to other waves.

After making their choice, participants were informed that we would contact them during this window if they were indeed chosen and that a lack of contact meant they were not chosen (in reality, no participants were contacted). Finally, they completed Wave 1 by reporting their demographic information and any technical problems or confusion with the experiment (forced choice: *no*; *yes [explain]*), as well as two other items that we were simply curious to measure without a hypothesis (see preregistration): the timeline of their resolution (forced choice: giving up for *a few days*; *a few weeks*; *a few months*; *the whole year*) and their confidence in sticking with this timeline (forced choice: *definitely won't succeed*; *may succeed*; *definitely will succeed*).

Wave 2: Tuesday, January 28th, 2020 (Control wave). About one month later, participants were invited to participate in a new experiment, which was advertised without any explicit connection to Wave 1. We chose this date to match the calendar date number of Wave 1 and to capture a context in which such endings were presumably less salient than they were at Wave 1.

Participants began by completing the same key dependent variable from Wave 1: They were informed that we would be launching an additional short follow-up study at some point over the next 3 days (January 29th, 30th, or 31st), as in Wave 1, and they again chose between a novel and familiar option. Again, no participant was actually contacted for this follow-up study.

Participants finished Wave 2 by being reminded of their New Year's resolution from Wave 1, and were asked whether they were still upholding it (forced choice: *no*; *yes*). If not, they were also asked when they stopped (forced choice: *during the first week of January*; *the second week*; *the third week*; *the fourth week*), as well as why they stopped (forced choice: *because I freely wanted to*; *because it proved to be too hard*; *other [explain]*). As in our end-of-experiment questions from Wave 1, we included these items simply for curiosity. Finally, participants also reported any technical problems or confusions with the experiment (forced choice: *no*; *yes [explain]*).

Wave 3: Saturday, March 28th, 2020 (Control wave). Another two months later, participants were invited to participate in a third distinct experiment. They completed the same procedures as Wave 2, again indicating their choice between novel and familiar tasks for a potential follow-up study in the next 3 days (March 29th, 30th, or 31st). The only difference was that Wave 3 included additional choice options for our follow-up question about when participants stopped pursuing their resolution (if they stopped), one for each week throughout February and March.

Wave 3 serves a number of purposes. If participants make different choices at Wave 1 versus Wave 2 as proposed, perhaps this reflects boredom (i.e., choosing the other option for the sake of doing something different) or strategy (i.e., choosing the other option to increase one's chances of being invited to do the follow-up study, since their previous choice resulted in them

not being invited), as opposed to reflecting differential salience of endings. If so, then we should observe a similar shift in choice between Wave 2 and Wave 3. Likewise, perhaps there is something unique about Saturdays (Wave 1 day) versus Tuesdays (Wave 2 day) that explains the effect; if so, Wave 3 (also a Saturday) should resemble Wave 1. Also, Wave 3 is simply further away from Wave 1, adding additional assurance that the key difference captured by our longitudinal design is that the ending context of New Year's has faded in time.

Results and Discussion

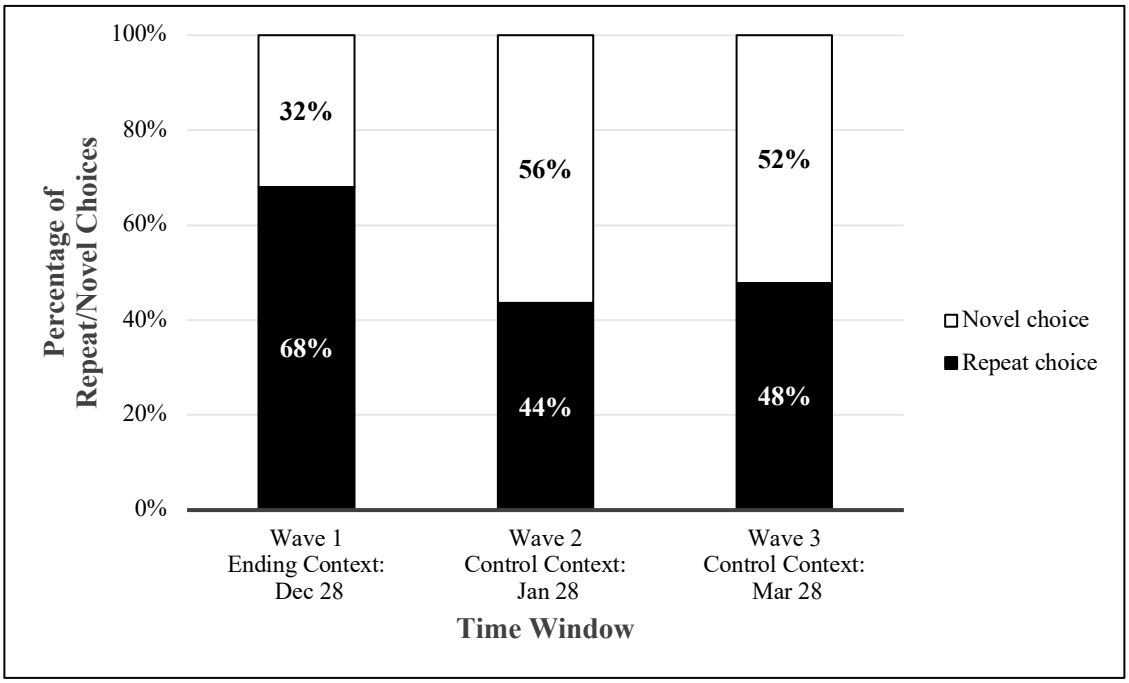
We conducted repeated-measures logistic regression analyses, via the SPSS GEE command, entering Wave as a predictor of activity choice (novel option versus familiar option).

Main results (activity choice). As hypothesized, participants' choices indeed depended on when they made them, Wald = 41.02, df = 2, $p < .001$, $w = 0.40$ (see Figure 2.2).

Participants indeed became more likely to choose old favorites rather than new interests at Wave 1 (an ending context right before implementing one's New Year's Resolution) as compared to Wave 2 (a control context one month later): 68.09% (175 of 257) of participants chose activities involving old favorites at Wave 1, but only 43.58% (112 of 257) of participants did so at Wave 2, Wald = 38.14, df = 1, $p < .001$, $w = 0.39$. Yet, two more months later at Wave 3, there was no such shift from Wave 2 to Wave 3 (47.86%, 123 of 257), Wald = 1.46, df = 1, $p = .227$, $w = 0.08$ (Wave 1 vs. Wave 3: Wald = 26.70, df = 1, $p < .001$, $w = 0.32$), suggesting Wave 1's ending context (and not some incidental change in preferences over time) uniquely shifted preferences toward familiarity.

Figure 2.2

Experiment 2: Percentage of participants choosing the novel vs. repeat option by time



Another way to gauge effect size is to explore individual profiles of preference changes. We found that a full 20.62% of the sample (53 of 257) fit our hypothesized pattern—that is, they chose familiarity in Wave 1, novelty in Wave 2, and novelty in Wave 3. Only 5.45% (14 of 257) shifted their responses conversely (novelty in Wave 1, familiarity in Waves 2–3), $\chi^2(1) = 22.70$, $p < .001$, $w = 0.58$. Table 2.1 shows all eight possible combinations; our hypothesized pattern was the second most common, which did not significantly differ from the first most common (all familiar choices at each time point), $\chi^2(1) = 0.70$, $p = .401$, $w = 0.08$.

Table 2.1

Experiment 2: Choice composition of novelty versus familiarity across each wave

Each specific choice composition across waves (from most common to least common)			*Percentage of participants who showed each specific choice composition across waves*
Wave 1	*Wave 2*	*Wave 3*	
Familiar	Familiar	Familiar	24.12% (62 of 257 participants)
Familiar	Novel	Novel	20.62% (53 of 257 participants)
Novel	Novel	Novel	17.51% (45 of 257 participants)
Familiar	Novel	Familiar	13.23% (34 of 257 participants)
Familiar	Familiar	Novel	10.12% (26 of 257 participants)
Novel	Familiar	Familiar	5.45% (14 of 257 participants)
Novel	Novel	Familiar	5.06% (13 of 257 participants)
Novel	Familiar	Novel	3.89% (10 of 257 participants)

Other results. At Wave 1, most participants expected to stick with their resolution for "the entire year" (62.26%, 160 of 257) and expected they "might succeed" (74.32%, 191 of 257). By Wave 2, most were still upholding their resolution (83.66%, 215 of 257), with the remainder lasting about 2 weeks (54.76%, 23 of 42) and citing "too difficult" (40.48%, 17 of 42) for why they stopped. By Wave 3, most were still upholding their resolution (64.20%, 165 of 257), with the remainder lasting about 6 weeks (45.66%, 42 of 92) and again citing "too difficult" for why they stopped (35.87%, 33 of 92). See Supplemental Materials for all of these exploratory results.

Finally, across waves, most participants reported having no technical problems with the survey (Wave 1: 98.83%, 254 of 257; Wave 2: 99.61%, 256 of 257; Wave 3: 100.00%, 257 of 257). All patterns remain when re-analyzing the results while entering these other variables, as well as demographic information, as covariates (see Supplemental Materials).

Experiment 2 replicates the basic effect in a naturalistic context with real behavior: Preferences for familiar options uniquely increased upon giving up that hedonic category.

Experiment 3:

Lab Challenge (real choice)

[preregistration: https://aspredicted.org/yd2ac.pdf]

Experiment 3 tested for this effect in the laboratory. We challenged participants to abstain from their most tempting hedonic activity for one week. Before they began this challenge, we invited them for a "last hurrah" in which they could choose to complete a task related to novel or familiar activities within their to-be-sacrificed category. We hypothesized that these participants would be more likely to choose familiarity (versus novelty) as compared to control participants who were assigned to skip the challenge and so made their choice outside of this ending context.

Method

Participants. This experiment was conducted via our university subject pool. We utilized a screening procedure near the start of the experiment (see Procedure section) such that we preregistered to recruit 300 participants who successfully passed, who then proceeded to complete the experiment. In total, we ended up recruiting 344 participants before meeting this threshold.

Thus, the current experiment involved 301 successfully-screened participants (representing an attrition rate of just 12.50%; M_{age} = 27.70, SD_{age} = 10.62; 66.78% female; 57.67% non-White; one participant did not report their age or ethnicity) who participated for $1.00 (sensitivity analysis: minimum effect size that can be detected by this sample size is Exp(B) = 1.80).

Procedure. Participants were randomly assigned to a 2 (Context: Control vs. Ending, between-subjects) design.

First, all participants were informed that the experiment involved pleasurable activities they do in daily life—"things that may technically take time away from doing work/staying focused on other goals, but we derive pleasure from them"—and indicated which of 10 activities they most enjoy (shown in randomized order): reading for fun, listening to podcasts/music for fun, eating junk food/sweets, drinking alcohol, smoking cigarettes/tobacco products, playing sports, using social media, ordering from/going out to restaurants, online shopping, or playing video games. The most popular option was "podcasts/music," selected by 23.26% of participants (70 of 301).

Next (screening procedure): All participants were informed that they would be entered into a lottery that would randomly select some of them to complete a self-control study, in which they would be required to give up their chosen hedonic category for one week (starting that day, immediately upon exiting the laboratory). They were asked whether they were willing to proceed (forced choice: *no*; *yes*), knowing they might be selected for this self-control study. As preregistered, only those participants who indicated "yes" were retained and proceeded to the experimental measures; those who indicated "no" were piped to unrelated research.[4]

Then, all successfully-screened participants clicked to the next page where they saw a waiting screen ("Random draw being calculated."), displayed for six seconds. Then, the experiment automatically continued to a new page where they were randomly assigned to

[4] Given this order of operations, the only question for which we could assess selective attrition was activity choice. Among those who opted out, their most popular option was also "podcasts/music," selected by 25.58% of them (11 of 43). In addition, we utilized a similar design in Experiment 7 except that we thought to include more participant-level variables before the screening item; as will be reported within that experiment, we found few selection differences.

condition. Control participants (n = 151) learned that they were not selected for this self-control study. They read:

> You have not been randomly selected. You are being asked to continue [chosen hedonic category] as usual. For this upcoming week, once you leave the virtual lab, please assume you will engage in [chosen hedonic category] as you normally would.

In contrast, Ending participants (n = 150) learned that they indeed were selected for this self-control study. They read:

> You have been randomly selected. Starting today, you are being asked to give up [chosen hedonic category] for 1 full week. For this upcoming week, once you leave the virtual lab, please assume you will not engage in [chosen hedonic category] at all until 1 week from today.

Before leaving the lab, all participants then proceeded to finish the current experiment in the same way, where we assessed our key dependent variable. They were reminded that the current experiment involved pleasurable activities that they regularly do in everyday life—and thus, for the current experiment, they would complete "a fun activity" involving the preferred hedonic category they had chosen earlier. Marking our key dependent variable, we gave participants the choice of what kind of fun activity to complete then and there. They chose one of two options (shown in randomized order): "An activity about [description of novel option]" versus "An activity about [description of familiar option]," with both falling within their preferred hedonic category.

For example, if a participant indicated that their preferred hedonic category was "eating junk foods/sweets," they later chose between completing "an activity about junk foods/sweets that you've never eaten before, but you've been wanting to enjoy" (novel option) versus "an

activity about junk foods/sweets that you've eaten before and enjoyed" (familiar option). Thus, we assessed our hypothesis that participants may be more likely to prefer familiarity when randomly assigned to believe they would be giving up this hedonic category over the next week.

Participants then completed their chosen task, which entailed writing about a favorite past experience involving their choice. As preregistered, this task is irrelevant to our hypothesis and will not be discussed further (i.e., our key dependent variable had already been measured at the moment of choice). We retain all responses (as written) in our data file for interested researchers.

Other variables. After making their choice and before writing, all participants were reminded of their chosen most-enjoyed category (e.g., "eating junk food/sweets") and rated how much they enjoy it in everyday life (1 = *not at all*; 7 = *very much*); how many times per week they engage in it (open-ended); how much of a challenge it ["would be" for Control; "will be" for Ending] to give up that category for the next week (1 = *not at all*; 7 = *very much*); and their confidence they ["would succeed" for Control; "will succeed" for Ending] in completing the challenge (forced choice: *definitely would/will succeed*; *might succeed*; *definitely wouldn't/won't succeed*). We included these measures simply for curiosity (see preregistration), and they were all presented after our dependent variable; we report these results in the Supplemental Materials.

Finally, all participants reported their demographic information and any technical problems or confusions with the experiment (forced choice: *no*; *yes [explain]*). They also completed an attention check regarding their condition (forced choice: *selected for the lottery*; *not selected for the lottery*), as well as a manipulation check regarding their awareness of endings: "When you were choosing which activity to do, to what extent were you imagining that this would be your last opportunity to experience that thing for 1 week?" (1 = *definitely wasn't imagining this*; 7 = *definitely was imagining this*). To end, all participants were informed that

there was no such self-control study, but we encouraged them to spend the week as instructed if they were interested.

Results and Discussion

We conducted logistic regression analyses, entering Context as a predictor of activity choice (novel option versus familiar option).

Main results (activity choice). We again observed the hypothesized shift: More Ending participants (66.00%, 99 of 150) than Control participants (54.97%; 83 of 151) chose familiarity over novelty, $B = 0.46$, $SE = 0.24$, $p = .051$, $\text{Exp}(B) = 1.59$.[5]

Other results. The results of the manipulation check confirmed that Ending participants had endings on their minds ($M = 3.85$, $SD = 2.40$) to a greater degree than Control participants ($M = 2.50$, $SD = 1.91$), $t(299) = 5.37$, $p < .001$, $d = 0.62$. Most participants (99.00%, 298 of 301) passed the attention check (basic effect changes from $p = .051$ to $p = .047$ if we exclude failures).

All patterns remain when re-analyzing the results while entering these other variables, as well as demographic information, as covariates (basic effect changes from $p = .051$ to $p = .022$; see Supplemental Materials).

Experiment 3 replicates the basic effect on real behavior in a controlled laboratory setting. This experiment further highlights the causal effect of ending contexts by avoiding

[5] As it turned out, this basic effect was the statistically weakest across all of our studies. One reason may be that our sample size proved to be relatively less well-powered (as reported, post-hoc sensitivity analyses revealed that the minimum critical effect size that can be detected by this sample size was $\text{Exp}(B) = 1.80$, while our observed effect size was $\text{Exp}(B) = 1.59$). A more substantive reason may be that the dependent variable involving completing a task "about" the novel or familiar option, rather than literally consuming the option itself. We did this for feasibility reasons, but in hindsight, it might also explain this smaller effect relative to our other studies; a task about the option may elicit generally less appeal in ways that disrupt the detection our hypothesized difference (e.g., perhaps some participants "checked out" altogether and chose more randomly). This methodological limitation mostly applies to Experiment 3; other experiments almost all test more direct consumption.

selection bias: It cannot be that because Ending participants were all willing to complete a self-control study, they were simply more prudent types of people than Control participants (which itself might explain why they were more attracted to familiarity), since all participants were screened for this willingness before their fate was decided (i.e., before their ending-related condition had been randomly assigned and made known). As in Experiment 2, we also tailored this experiment to each participant's actual preferences, thus further bolstering real-world relevance.

Experiment 4:

Gift Cards (real choice)

[preregistration: https://aspredicted.org/q6x89.pdf]

In Experiment 4, we sought to extend the basic effect further by assessing another kind of ending context and another real behavior—here via choosing between experiences with concrete monetary value. Participants reflected on the many (versus few) opportunities they would have to enjoy eating in restaurants in the near future. Next, they chose between real gift cards to spend at a desired familiar restaurant or a desired novel restaurant during this window, holding constant other features (e.g., cost). We hypothesized that participants would become more likely to choose gift cards for old-favorite restaurants when endings were made salient.

Method

Participants. We requested 500 participants through Cloud Research, yielding 501 (M_{age} = 36.93, SD_{age} = 11.29; 47.70% female; 24.35% non-White) who participated for $0.40. (sensitivity analysis: minimum effect size that can be detected by this sample size is Exp(B) = 1.58).

Procedure. Participants were randomly assigned to a 2 (Context: Control versus Ending, between-subjects) design.

To begin, participants were informed that they would be entered into a raffle for a $30.00 gift card to a restaurant of their choice, and that the gift card expired one month from today (i.e., their participation date—which was also when they would receive the gift card, if they won). We ran the experiment on January 14, 2020 (i.e., "pre-COVID," at least in popular American awareness).

Before making their choice, however, participants completed a reflection task. Based on random assignment, Control participants (n = 241) read:

> Consider how you'll have plenty of opportunities to go out to restaurants over the next month. Take some time to think about how common they will be, and about what specific things will encourage you to go out to restaurants many times throughout the next month.

Ending participants (n = 260) read:

> Consider how limited your opportunities will be to go out to restaurants over the next month. Take some time to think about how rare they will be, and about what specific things will prevent you from going out to restaurants much throughout this next month.

In both conditions, participants were instructed to write out their thoughts in response to the prompt via an open-ended essay box (as preregistered, we had no particular hypotheses relating to what participants wrote, and thus, this task will not be discussed further; we retain the essays in our data file for interested researchers).

All participants then chose a gift card for one of two options (shown in randomized order): "A restaurant I haven't gone to before" (novel option) versus "A restaurant I have gone to before" (familiar option). They also read: "After you choose, we'll ask you a series of questions,

including listing specific restaurants in your life that fit these criteria, which we'll use to get a gift card"; and, "One rule to know in advance is that both restaurants need to generally cost the same amount of money, time, etc., to go to." After choosing, participants listed a restaurant they had in mind for each category, as well as their e-mail address for linking their gift card if won.

Other variables. After choosing, all participants reported their demographic information and rated their confusion with the experiment (1 = *not confusing*; 7 = *very confusing*). They also completed an attention check regarding their condition (forced choice: *wrote about few future opportunities*; *wrote about many future opportunities*; *wrote about any opportunities*; *no such prompt*), as well as a manipulation check regarding their awareness of endings for their choice: "To what extent do you expect that your next restaurant opportunity will be your last restaurant opportunity for quite a while?" (1 = *definitely do not expect this*; 7 = *definitely expect this*). Upon completion of the experiment, we conducted the lottery as described and sent the winner their gift card.

Results and Discussion

We conducted logistic regression analyses, entering Context as a predictor of gift card choice (gift card to novel restaurant versus gift card to familiar restaurant).

Main results (gift card choice). We again observed the hypothesized shift: Significantly more Ending participants (67.31%, 175 of 260) than Control participants (48.96%, 118 of 241) chose gift cards to old-favorite restaurants, $B = 0.76$, $SE = 0.19$, $p < .001$, $\text{Exp}(B) = 2.15$.

Other results. The results of the manipulation check confirmed that Ending participants had endings on their minds ($M = 3.99$, $SD = 1.83$) to a greater degree than Control participants ($M = 2.46$, $SD = 1.79$), $t(499) = 9.44$, $p < .001$, $d = 0.84$, and there were no differences between conditions on experiment confusion ($M_{\text{Ending}} = 1.46$, $SD = 0.91$ versus $M_{\text{Control}} = 1.49$, $SD = 1.10$;

$t(499) = 0.40$, $p = .688$, $d = 0.04$). Most participants passed the attention check (95.41%, 478 of 501).

Finally, all patterns remain when re-analyzing the results while entering these other variables, as well as demographic information, as covariates, and also when we exclude all attention check failures (see Supplemental Materials).

Experiment 4 further extends our observed shift toward the familiar, here via the effects of another kind of ending context on real choices for upcoming dining opportunities.

Next, Experiments 5–8 sought to further replicate this basic effect while also shining light on why it occurs, assessed across a variety of mediation-based and moderation-based approaches.

Experiment 5:

Endings Increase Desires for The Safer Bet (And Thus Increase Familiarity Seeking)

[preregistration: https://aspredicted.org/7u5cq.pdf]

As reviewed in the Introduction, familiar options typically represent safer bets than their novel equivalents—returning to old and familiar experiences tends to ensure a certain level of known hedonic quality, and a certain level of known personal meaning, with which exploring new and exciting experiences typically cannot compete—and in turn, given that one's choices within ending contexts represent higher-stakes choices, a safer bet mechanism might generally explain the robust shift toward familiarity that we have consistently observed thus far.

Before attempting to disentangle people's specific safer-bet motivations (i.e., ensuring hedonic quality versus ensuring personal meaning) in driving this effect, Experiment 5 sought to assess this broader safer-bet mechanism more generally. This experiment essentially served as a replication of Experiment 1, except that we also asked participants to report on why they chose

the familiar or novel option they chose (for each domain). We hypothesized that ending contexts would again shift participants' preferences toward familiarity, and that this shift would be explained by a general effect of endings increasing participants' desires to take the safer bet.

Method

Participants. We requested 500 "Cloud Approved" participants through Cloud Research, yielding 501 (M_{age} = 40.41, SD_{age} = 12.24; 43.51% female; 27.94% non-White) who participated for $1.00 (sensitivity analysis: minimum effect size that can be detected by this sample size is w = 0.13).

Procedure. Participants were randomly assigned to a 2 (Context: Control versus Ending, between-subjects) × 10 (Domain: 10 unique scenarios, within-subjects) design.

The study design was essentially identical to Experiment 1—participants indicated their preferences for a novel activity versus a familiar activity for each of 10 hedonic domains, having been randomly assigned to a Control condition (n = 248) or an Ending condition (n = 253). Again, we held cost and other such differences between the activities constant. There were two differences to note between the design of this experiment relative to the design of Experiment 1:

First, of minor importance and simply for conceptual clarity, we added text to the Control condition (for each domain) that specified its intended non-ending nature. For example, in the "restaurants" scenario, Control participants read:

> It turns out, your next restaurant opportunity is by no means the last time you'll be able to go to a restaurant for a while. Imagine that after whatever you choose for this would be just one of many times you get to go to a restaurant for quite some time.

As before, Ending participants instead read:

> It turns out, your next restaurant opportunity is the last time you'll be able to go to a restaurant for a while. Imagine that whatever you choose for this would be the last time you get to go to a restaurant for quite some time.

Second, and of substantive importance, we assessed why participants made their choice. Participants completed a single item for each domain; for each domain, it was presented in randomized order with the dependent variable (choice of familiarity versus novelty). It was:

> Given this particular occasion, which is more important for your choice?: Taking a risk to max out your experience (where you shoot for the best possible time, even if there's a bigger chance you'll only have a so-so time), or wanting a safe bet for your experience (where you can guarantee you'll have a good enough time, even if it means missing out on other desirable options)

This item was rated from 1 (*better to try to max out*) to 10 (*better to choose the safe bet*). For each domain, for each participant, we randomized the presentation order of scale anchors (sometimes "max out" was described first and placed on the left-most scale anchor, and other times "safe bet" was described first and placed on the left-most scale anchor instead).

Other variables. At the end of the experiment and after making all 10 choices, all participants rated the same other variables from Experiment 1. They also completed an additional attention check regarding what the experiment was about (forced choice: *Enjoyable experiences*; *Painful experiences*; *Neutral experiences*), as well as a no-penalty honesty check regarding whether we should trust their responses as genuine (forced choice: *no*; *yes*).

Also at the end of the experiment, and of more substantive relevance, we were curious to assess some initial evidence for the specific reasons underlying a potential shift to less-risky familiarity (which we will then directly assess in all remaining experiments). All participants

completed a single item asking them to report which of two options better described the kind of experiences they had been intending to choose, presented in randomized order: an option that would describe their prior choices as maximizing hedonic quality ("An experience that's high on Pure Pleasure [e.g., going to a restaurant that has extraordinary/amazing-tasting food], but it happens to be low on Sentimental Connection [e.g., this same restaurant doesn't really mean much to you on a personal level])" versus another option that would describe their prior choices as maximizing personal meaning ("An experience that's high on Sentimental Connection [e.g., going to a restaurant that has a uniquely special place in your "life story"/"who you are"], but it happens to be low on Pure Pleasure [e.g., at this same restaurant, the taste of the food is bland/unremarkable])." We hypothesized that more Ending participants than Control participants would indicate that their prior choices had reflected them trying to maximize personal meaning.

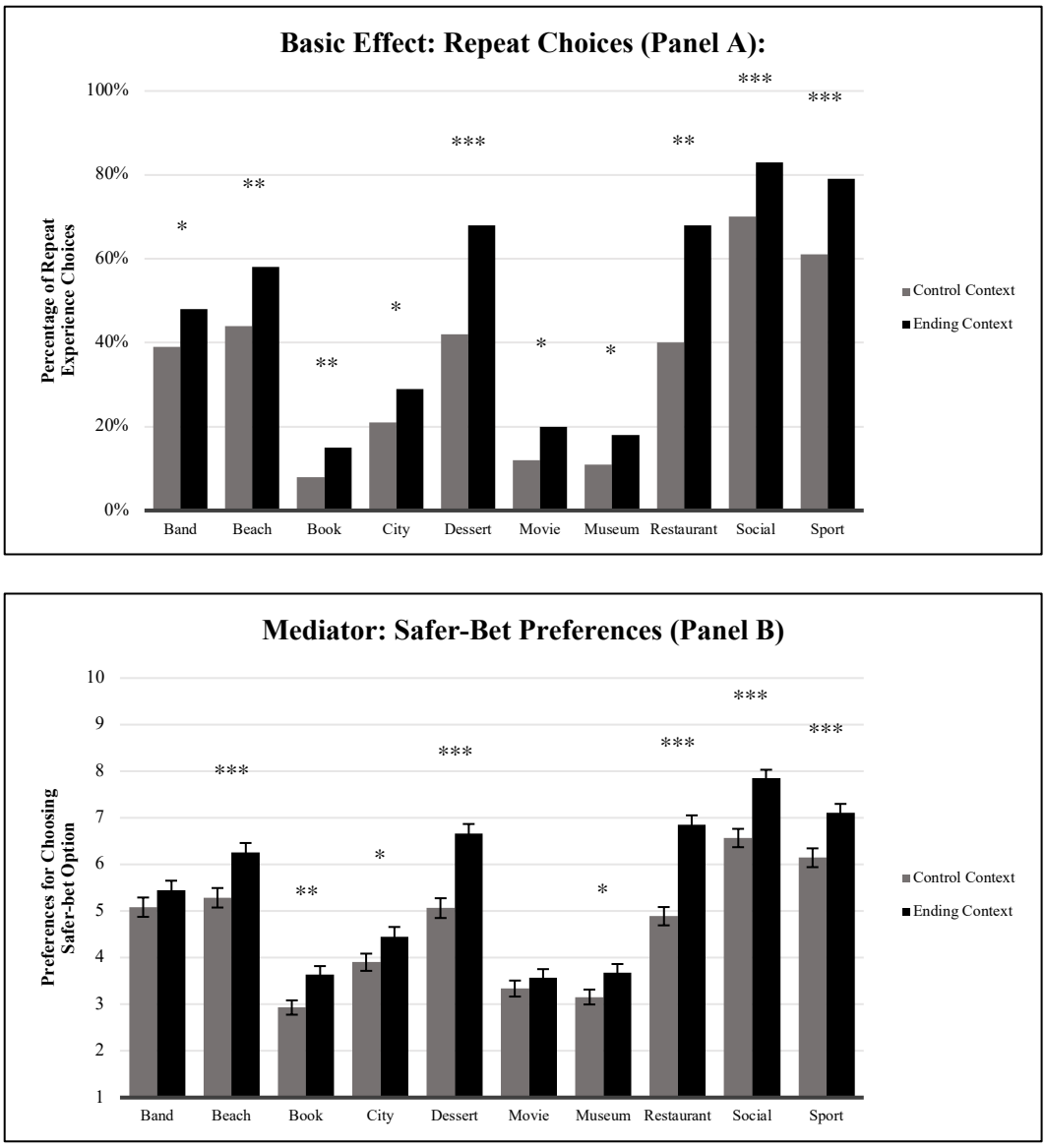
Results and Discussion

Main results (activity choice). We conducted repeated-measures logistic regression analyses, via the SPSS GEE command, entering Context, Domain, and their interaction term as predictors of activity choice (novel option versus familiar option).

The basic effect again replicated (see Figure 2.3, Panel A): The critical main effect of Context showed that Ending participants were significantly more likely to choose familiar activities (percentage of familiarity choices out of 10: $M = 48.61\%$, $SD = 19.68\%$) as compared to Control participants ($M = 34.76\%$, $SD = 21.27\%$), Wald = 48.17, df = 1, $p < .001$, $w = 0.31$ (incidental main effect of Domain: Wald = 593.48, df = 9, $p < .001$, $w = 1.09$; interaction, Wald = 15.80, df = 9, $p = .071$, $w = 0.18$).

Figure 2.3

Experiment 5: Percentage of repeat choices by hedonic domain (Panel A), along with participants' self-rated preferences toward the safer bet for each domain as well (Panel B)



Note. Significance is flagged at: † = $p < .10$, * = $p < .05$, ** = $p < .01$, *** = $p < .001$.

Main results (reason for choice). For the mechanism items, we recoded responses where appropriate, such that higher ratings (via a single item ranging from 1 to 10, rated for each of the 10 domains) indicate a stronger preference for safer bets (vs. taking a risk), and then

conducted the same analysis as above except via linear regression (rather than logistic regression).

In turn, we indeed found evidence for why participants made these choices (see Figure 2.3, Panel B): As hypothesized, Ending participants were significantly more motivated to reduce risk and choose the safer bet (overall, across domains: $M = 5.55$, $SD = 1.41$) as compared to Control participants (overall, across domains: $M = 4.64$, $SD = 1.64$); main effect of Context, Wald = 44.85, df = 1, $p < .001$, $w = 0.30$ (incidental main effect of Domain: Wald = 755.99, df = 9, $p < .001$, $w = 1.23$; interaction, Wald = 35.44, df = 9, $p < .001$, $w = 0.27$).

Mediation analyses. To more directly assess this driving influence, we conducted a mediation analysis (SPSS PROCESS Model 4, 5,000 bootstrapped iterations: Hayes, 2017) using Context (Control versus Ending) as the independent variable, activity choice (novel versus familiar option, across all domains) as the dependent variable, and safe-bet ratings (across all domains) as the mediator. This analysis indeed revealed a significant indirect effect via safe-bet ratings, Indirect Effect = 0.10, $SE = 0.02$; 95% CI_{boot} [0.07, 0.13], indicating mediation.

End-of-experiment item: Ensuring personal meaning. Providing initial support for teasing apart specific reasons for what makes familiar options less risky (and thus more appealing in the context of endings), we further found using logistic regression that more Ending participants (36.76%, 93 of 253) than Control participants (23.39%, 58 of 248) reported that their primary motivation for their prior choices had been to maximize personal meaning (over stimulus quality), $B = 0.64$, $SE = 0.20$, $p = .001$, Exp(B) = 1.90. As noted, we will directly unpack this idea in all remaining studies.

Other results. The results of the manipulation check confirmed that Ending participants had endings on their minds ($M = 9.17$, $SD = 1.43$) to a greater degree than Control participants

(M = 3.01, SD = 2.89), $t(499) = 30.29$, $p < .001$, $d = 2.70$, though there were differences between conditions on experiment confusion (M_{Ending} = 1.59, SD = 1.43 versus $M_{Control}$ = 1.85, SD = 1.60; $t(499) = 1.96$, $p = .051$, $d = 0.17$); no such differences emerged for mental imagery (M_{Ending} = 8.17, SD = 1.86 versus $M_{Control}$ = 7.92, SD = 2.12; $t(499) = –1.40$, $p = .161$, $d = 0.13$). Most participants passed the attention check (99.00%, 496 of 501) and the honesty check (99.40%, 498 of 501).

Finally, all patterns remain when re-analyzing the results while entering experiment confusion, mental imagery, and demographic information as covariates, and when excluding attention check and honesty check failures (see Supplemental Materials).

Experiment 5 again replicates the basic effect while also shining initial light on why it occurs, at least at a general level: Ending participants shifted away from novelty and toward familiarity because they became more interested in safer bets, and thus in familiar options.

Next, in Experiment 6, we assess more specific process evidence. In doing so, we also address a potential confound between hedonic quality and meaning within the mediator item in this experiment, such that participants may have calculated a generally higher expected value for the familiar option versus the novel option—as we pitted a familiar option with some guaranteed quality against a novel option with an uncertain peak. This set-up was designed to mimic real-world differences between familiarity and novelty, but nonetheless warrants a methodologically cleaner test. In Experiment 6, we manipulate and equally guarantee hedonic quality and meaning.

Experiment 6:

Ensuring Meaning Dominates Ensuring Hedonic Quality—Specifically for Endings

[preregistration: https://aspredicted.org/nt9dx.pdf]

In what specific ways are familiar options "safer bets" (compared to novel options) that become more attractive in the context of endings? Next, expanding on the initial insights from our end-of-experiment item in Experiment 5, Experiment 6 directly teased apart the two underlying possibilities we reviewed earlier: ensuring the hedonic quality of the stimulus itself versus ensuring personal meaning. Using a moderation-based approach, we tested our hypothesis that ensuring personal meaning is the stronger driver of the effect, between these two possibilities. To take the restaurant domain as an example, endings may prompt people to choose a restaurant with sufficiently good-tasting food above all, independent of one's own sentimental connection to it (i.e., perhaps endings lead people to care mostly about ensuring hedonic quality, even if that means sacrificing personal meaning); or, endings may prompt the opposite: people may instead choose a restaurant they feel personally connected to above all, independent of the food's taste (i.e., perhaps endings lead people to care mostly about ensuring personal meaning, even if that means sacrificing hedonic quality). We hypothesized that the latter takes priority specifically in ending contexts (versus any context)—as revealed when people are forced to choose.

Method

Participants. We requested 2,000 "Cloud Approved" participants through Cloud Research, yielding 2,004 (M_{age} = 40.39, SD_{age} = 12.58; 48.25% female; 25.65% non-White) who participated for $0.30 (sensitivity analysis: minimum effect size that can be detected by this sample size is $\eta_p^2 = 0.06$).

Procedure. Participants were randomly assigned to a 2 (Context: Control versus Ending, between-subjects) × 2 (Hedonic Quality: High versus Low, between-subjects) × 2 (Personal Meaning: High versus Low, between-subjects) design (thus yielding 8 unique cells; *n*s ≥ 228).

All participants evaluated the same single domain, new to this experiment, involving taking a leisurely walk. To begin, they read:

> Imagine you're heading out to take a walk around town. There are various kinds of routes in town to take. However, due to temporary construction lasting only for today, here's the specific route that you have to take…

Participants were then randomly assigned to condition, whereby we varied the hedonic properties of the route as well as its temporal context.

Regarding hedonic properties, all participants learned two of them: where the route fell on Hedonic Quality (High or Low), and also where the route fell on Personal Meaning (High or Low), presented in randomized order. For Hedonic Quality, they read that this particular route "happens to rank high [low] on exciting stimulation for you (for example: this route has many [few] interesting sights, smells, and sounds for you to explore/enjoy." For Personal Meaning, they read that this same route also "happens to rank high [low] on sentimental connection for you (for example: this route is related [unrelated] to your 'life story'/'who you are' in this town)."

With their specific route configuration in place, all participants were further assigned to one of two Context conditions that varied the walk's temporal backdrop. Control participants faced no end-related restrictions; they read:

> The season is in full swing. This means that, after today's walk, you'll still be able to take many more walks (anywhere) this season; today by no means marks your last time this season taking a walk like this.

By contrast, Ending participants read:

> The season is coming to an end. This means that, after today's walk, you will not be able to take any more walks (anywhere) this season; today marks your last time taking a walk like this.

All other procedures and phrasings were identical across conditions, with all participants completing the same key dependent variable: They rated a single item regarding their reactions to taking their specifically configured route, from –5 = *Terrible fit for this particular occasion; I'm especially bummed*, to +5 = *Perfect fit for this particular occasion; I'm especially thrilled* (with the scale midpoint 0 = *Doesn't matter; no strong feelings either way*). We hypothesized to find a critical 3-way interaction, such that imbuing the route with high personal meaning might boost evaluations regardless of the route's hedonic quality (and vice versa)—but the boost from meaning would be *even more pronounced* for Ending participants than the boost from hedonic quality for Ending participants, thus highlighting a special link between meaning and endings.

Other variables. At the end of the experiment, all participants completed the same other variables from Experiment 1, plus three attention checks: one for what the experiment was about (forced choice: *taking leisurely strolls through town*; *reading books by William James*; *planning Halloween parties*); one for their assigned route condition (forced choice from 1 of 4 options, corresponding to each combination of high/low hedonic quality and high/low meaning); and one for their assigned context condition (forced choice from 1 of 2 options: the Control prompt or the Ending prompt). They also completed the same honesty check from the previous experiment.

Also at the end of the experiment, and of more substantive relevance, we asked all participants a confirmation question whereby they categorized each of two walking routes, generally speaking: one that is "high in sentimental connection + low in exciting stimulation"

and another that is "low in sentimental connection + high in exciting stimulation." Namely, we asked participants to designate one of these routes as being more likely an "old route (done it before)" and the other as being more likely a "new route (haven't yet done it)." We hypothesized that most participants would categorize these routes as we assume they exist in daily life (i.e., that participants would categorize the sentimental route as the old one and the stimulating route as the new one). Such a finding would help confirm that, back in our main results, if Ending participants indeed show a unique boost toward the high-meaning option (over the high-quality option), then this effectively translates into showing increased preferences for familiarity (over novelty), as in prior studies.

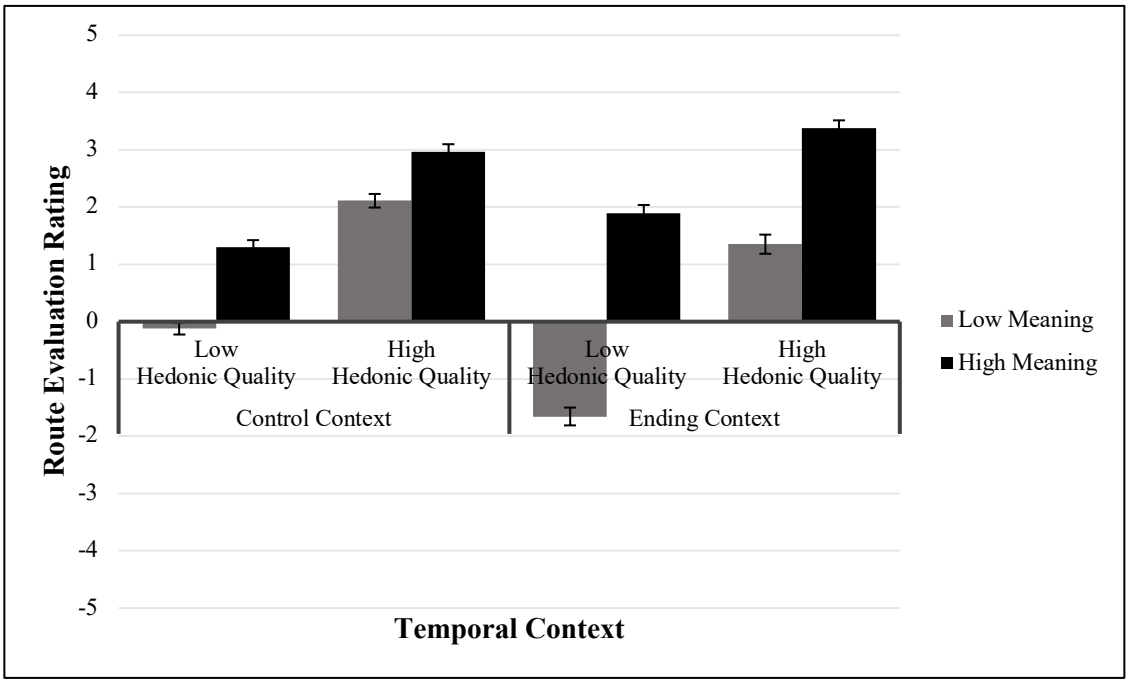
Results and Discussion

We conducted Univariate GLM analyses entering Context (Control versus Ending), Hedonic Quality (High versus Low), Personal Meaning (High versus Low), and their interaction terms as independent variables, and route evaluation (single item ranging from -5 to +5) as the dependent variable.

Main results (route evaluation). See Figure 2.4 for all results. Of key interest, we indeed observed a significant 3-way interaction as hypothesized, $F(1, 1996) = 6.16$, $p = .013$, $\eta_p^2 = 0.003$ (All remaining output aside from this key interaction: Main effect of Context, $F(1, 1996) = 11.28$, $p = .001$, $\eta_p^2 = 0.01$; Main effect of Hedonic Quality, $F(1, 1996) = 476.78$, $p < .001$, $\eta_p^2 = 0.19$; Main effect of Meaning, $F(1, 1996) = 417.29$, $p < .001$, $\eta_p^2 = 0.17$; Context*Hedonic Quality, $F(1, 1996) = 2.42$, $p = .120$, $\eta_p^2 = 0.001$; Context*Meaning, $F(1, 1996) = 73.40$, $p < .001$, $\eta_p^2 = 0.04$; Hedonic Quality*Meaning, $F(1, 1996) = 29.29$, $p < .001$, $\eta_p^2 = 0.01$).

Figure 2.4

Experiment 6: Mean route evaluation ratings across conditions



Note. Error bars show ±1 SE.

First, generally speaking (and unsurprisingly): Pairwise comparisons show that imbuing the route with hedonic quality indeed boosted positive evaluations of it, regardless of its personal meaning and its temporal context. Control participants, for example, evaluated the low-meaning route more positively when that route had higher (M = 2.11, SD = 1.89) versus lower (M = –0.12, SD = 1.67) hedonic quality, $t(508) = -14.15$, $p < .001$, $d = 1.25$), and they likewise evaluated the high-meaning route more positively when that route had higher (M = 2.97, SD = 1.94) versus lower (M = 1.30, SD = 1.96) hedonic quality, $t(485) = -9.42$, $p < .001$, $d = 0.86$). However, such results are unsurprising on their own because, all else equal, hedonic quality is also presumably a desirable property for any experience to have.

Second, generally speaking (and also unsurprisingly): Pairwise comparisons show that imbuing the route with meaning indeed boosted positive evaluations of it, regardless of its hedonic quality and its temporal context. Control participants, for example, evaluated the low-hedonic-quality route more positively when that route was more (M = 1.30, SD = 1.96) versus less (M = –0.12, SD = 1.67) personally meaningful, $t(514) = -8.87$, $p < .001$, $d = 0.78$), and they likewise evaluated the high-hedonic-quality route more positively when that route was more (M = 2.97, SD = 1.94) versus less (M = 2.11, SD = 1.89) personally meaningful, $t(479) = -4.92$, $p < .001$, $d = 0.45$). Much like hedonic quality, such results are unsurprising on their own because, all else equal, personal meaning is presumably a desirable property for any experience to have.

Third, and most informatively: The 3-way interaction indicates that this positive effect of personal meaning was uniquely stronger than the effect of hedonic quality for one group of participants: our Ending participants. Ending participants, as compared to Control participants, evaluated the route as especially positive when it was more (versus less) personally meaningful (Ending Context: $M_{\text{High Meaning}} = 2.65$, $SD_{\text{High Meaning}} = 2.34$ vs. $M_{\text{Low Meaning}} = -0.06$, $SD_{\text{Low Meaning}} = 2.97$; Control Context: $M_{\text{High Meaning}} = 2.08$, $SD_{\text{High Meaning}} = 2.12$ vs. $M_{\text{Low Meaning}} = 0.99$, $SD_{\text{Low Meaning}} = 2.10$; $F(1, 1996) = 73.40$, $p < .001$, $\eta_p^2 = 0.04$), relative to how positively they evaluated the route when it was of higher versus lower hedonic quality (Ending Context: $M_{\text{High Quality}} = 2.36$, $SD_{\text{High Quality}} = 2.65$ vs. $M_{\text{Low Quality}} = 0.17$, $SD_{\text{Low Quality}} = 2.93$; Control Context: $M_{\text{High Quality}} = 2.52$, $SD_{\text{High Quality}} = 1.96$ vs. $M_{\text{Low Quality}} = 0.59$, $SD_{\text{Low Quality}} = 1.95$; $F(1, 1996) = 2.42$, $p = .120$, $\eta_p^2 = 0.001$), with the three-way interaction being significant, as reported above.

Put another way: Regardless of the hedonic quality of a stimulus, its personal meaning matters more in the context of endings (the extent of meaning here shifted route evaluations by 2.59 points on average)—even more than it usually matters without endings looming (the extent

of meaning here shifted route evaluations by 1.10 points on average). These differences are larger in the context of endings than the differences for hedonic quality, regardless of the route's personal meaning (the level of hedonic quality here shifted route evaluations by 2.19 points on average)—showing it matters more than when endings are not looming (the level of hedonic quality here shifted route evaluations by 1.92 points on average). While both personal meaning and hedonic value mattered for route evaluations in general, when things were ending, how personally meaningful the route was mattered more than the hedonic quality of the route.

Other results. There were incidental differences between conditions on experiment confusion (M_{Ending} = 1.53, SD = 1.10 versus M_{Control} = 1.69, SD = 1.28; $t(2{,}002) = 3.12$, $p = .002$, $d = 0.14$) (all effects hold when controlling this variable), and no differences on mental imagery (M_{Ending} = 5.04, SD = 1.64 versus M_{Control} = 5.03, SD = 1.64; $t(2{,}002) = –0.20$, $p = .842$, $d = 0.01$). Most participants passed the attention check for what the experiment was about (98.75%, 1,979 of 2,004), the attention check for their route condition (89.87%, 1,801 of 2,004), the attention check for their context condition (95.61%, 1,916 of 2,004), and the honesty check (99.10%, 1,986 of 2,004).

In addition, as revealed by our end-of-experiment confirmation item, nearly all participants (90.27%, 1,809 of 2,004) indeed categorized a route that was highly meaningful (but low on hedonic quality) as more likely to be the old route and a route that was high on hedonic quality (but low on meaning) as more likely to be the new route. These findings simply confirm that our observed boosts to evaluations of personally-meaningful routes (in our main results) are likely to reflect increased preferences for familiar experiences over novel ones, as in our prior studies.

Finally, all patterns remain when re-analyzing the results while entering experiment confusion, mental imagery, and demographic information as covariates, and when excluding attention check and honesty check failures, and also our route-categorization item (see Supplemental Materials).

Experiment 6 highlights a special link between meaning and endings as hypothesized—by forcing participants to choose between meaning and hedonic quality, we directly establish that the desire to ensure a personally-meaningful experience is an especially strong driver of the basic effect, compared to the desire to ensure hedonic quality (another clearly important feature of any hedonic experience). Participants still preferred to end meaningfully even when they knew that it meant sacrificing other thrills; dull endings remained desirable so long as they felt meaningful.

Experiment 7:

Meaning as Mediator (real choice)

[preregistration: https://aspredicted.org/vx5t9.pdf]

To sum our mechanism evidence so far: Experiment 5 showed that the basic effect (i.e., a directional shift toward familiarity in the context of endings) is driven by endings increasing people's desires to take the safe bet for having a positive experience, which tend to be better satisfied by familiar experiences than novel ones. Experiment 6 then unpacked the underlying components of this ensured positive experience, showing via a moderation-based approach that people are specifically driven to ensure a meaningful ending (versus ensuring hedonic quality).

Putting these findings together, Experiment 7 returned to a mediation-based approach but we now assessed this personal meaning dimension more precisely (i.e., beyond a "safer bet" writ large). We hypothesized that endings would again shift preferences toward the familiar—and that this occurs because endings specifically shift preferences toward a safer bet on meaning.

Method

Participants. Similar to Experiment 3, we utilized a screening procedure near the start of the experiment (see Procedure section) such that we preregistered to recruit 575 participants who successfully passed, who then proceeded to complete the experiment. In total, we ended up recruiting 618 participants before meeting this threshold.

Thus, the current experiment involved 580 successfully-screened participants (representing an attrition rate of just 6.30%[6]; M_{age} = 40.66, SD_{age} = 13.15; 48.79% female; 27.83% non-White) who participated for $1.50 (sensitivity analysis: minimum effect size that can be detected by this sample size is Exp(B) = 1.53).

Procedure. Participants were randomly assigned to a 2 (Context: Control versus Ending, between-subjects) design.

In addition to this experiment's main goals of replicating the basic effect and assessing meaning-specific mediation, we also extended the paradigm to another new domain involving real behavior: listening to one's choice of music. Procedures were generally similar to the Lab Challenge from Experiment 3—participants were assigned to either give up (or not give up) listening to music for pleasure for one week and we measured what kind of song they chose for their "last hurrah" (or not, i.e., without any restrictions)—a new interest versus an old favorite—except we also assessed the reason for their choice.

[6] As seen in our preregistration, we had requested 575 participants in order to account for any potential attrition based on this screening item, with the goal of yielding ~500 successfully-screened participants. Unbeknownst to us when designing the experiment, however, the posting of the experiment would exclude any participants who would be screened out from counting toward our requested quota (i.e., by us requesting 575 participants, the platform then collected approximately 575 successfully-screened participants, which we learned upon checking the data). When re-running all analyses only including the first 575 chronologically-participating—as had been our preregistered intended mark—our attrition rate was just 6.43%, and all reported results remain unchanged (see Supplemental Materials).

The experiment was not made available to anyone participating on a phone or tablet. Right at the start of the experiment, all participants reported their demographic information, how much they enjoy listening to music (1 = *not at all*; 7 = *very much*), what they usually do when listening to music (forced choice: *explore yet-unheard music*; *relisten to already-heard music*; *a mix of these*), and how frequently they listen to music (forced-choice from *0 days week, on average* to *7 days a week, on average*).

They then completed our screening procedure: As in Experiment 3, all participants were informed that they would be entered into a lottery that would randomly select some of them to complete a self-control study, in which they would be required to give up listening to music for one week (starting that day, immediately upon finishing the experiment). They were asked whether they were willing to proceed (forced choice: *no*; *yes*), knowing they might be selected for this self-control study. As preregistered, only those participants who indicated "yes" were retained and proceeded to the experimental measures; those who indicated "no" were thanked and dismissed[7].

All "yes" participants (i.e., our entire reported sample from this point on, N = 580) then proceeded to complete the experiment. First, all participants were asked to identify a novel song (a "new and exciting" song that they have "never heard before at all; it's 'still on [their] list'")

[7] Having learned from Experiment 3—where we did not put participant-level questions before the screening item (and thus could not assess selective attrition)—note here that we intentionally included the demographic/music questions before the screener, allowing us to assess these potential effects. As we reported earlier, we ended up recruiting a total of 618 participants before meeting this threshold (representing an attrition rate of just 6.14%). Participants who opted out tended to be older (p = .039), and to more frequently listen to music (p = .008) than participants who opted in (we found no other differences beyond these); when re-running all analyses when controlling for these variables, all reported results remain unchanged (see Supplemental Materials).

and a familiar song (an "old and familiar" song that they have "heard before many times"), in randomized order. Via guided prompts, participants were instructed to identify each song via browsing YouTube and then uploading each of these YouTube Links within the experiment survey.[8]

Next, participants were then randomly assigned to condition. We conducted the self-control lottery exactly as described, with some participants learning they were not selected for this self-control study (Control condition, $n = 288$; thus, these participants faced no looming endings) and others learning they were selected for this self-control study (Ending condition, $n = 292$); thus, these participants indeed faced looming endings, having to give up listening to music for the next week, to begin right after completing the experiment.

Then, serving as our key dependent variable, all participants were asked to choose which of their two songs they wanted to listen to, right then and there as part of the experiment: their self-chosen "new and exciting" novel song or their self-chosen "old and familiar" repeat song (with these two choice options piped and displayed in randomized order). We hypothesized that more Ending participants would choose to re-listen to the familiar song relative to Control participants.

In addition, presented in randomized order with this dependent variable[9], all participants indicated why they were making this choice, which served as our proposed mediator. They were asked: "What are you mainly trying to prioritize regarding your choice?", rated from 1 (*I prefer*

[8] Simply for thoroughness (as preregistered), we also asked participants to rate each song on basic dimensions like genre and length. There were no systematic differences across songs or conditions (see Supplemental Materials).

[9] Due to a programming error that we only discovered upon downloading the data, this presentation order had not been randomized for Ending participants, who all rated the dependent variable first, followed by the mediator item.

something that's maximally fun/stimulating, even if I don't yet have deeper sentimental feelings for it) to 7 (*I prefer something that I have deeper sentimental feelings for, even if it's not maximally fun/stimulating*).[10] As in our previous experiment, we randomized the presentation order of scale anchors (sometimes "fun" was described first and placed on the left-most scale anchor; other times "sentimental" was described first and placed on the left-most scale anchor instead). We hypothesized that Ending participants would uniquely shift their preferences toward personal meaning, explaining their potential increase in choosing the familiar (rather than the novel) song.

Other variables. Whatever participants chose, we then played this video with its audio in full (piped in from participants' uploads), embedded in our survey to ensure they listened. After, all participants rated their enjoyment of the experience (1 = *very negative*; 11 = *very positive*); completed a manipulation check regarding the extent to which they were assuming they were listening to music for the last time for a week (1 = *definitely wasn't imagining this*; 7 = *definitely was imagining this*); completed an attention check regarding their Context condition (forced choice: *selected for the lottery*; *not selected for the lottery*); reported any technical problems or confusions (forced choice: *no*; *yes [explain]*); and completed the same honesty check from previous experiments. To end, all participants were informed that there was no such self-control study, but we encouraged them to spend the week as instructed if they were interested.

[10] We opted to use a single item that forced a tension between quality and meaning, rather than measure each of these dimensions separately, to help isolate which feature matters more for explaining the basic effect. Separate items would invite participants to rate both equally highly (which they unsurprisingly should do if they are able to, as both are desirable properties for an experience to have), thereby obscuring the statistical detection of which matters more.

Results and Discussion

Main results (song choice). We conducted logistic regression analyses, entering Context as a predictor of song choice (novel song versus familiar song).

As hypothesized, participants' song choice depended on temporal context: Significantly more Ending participants (59.59%, 174 of 292) than Control participants (39.93%, 115 of 288) chose to listen to their own self-selected "old and familiar" song on their list over their own self-selected "new and exciting" song on their list, $B = 0.80$, $SE = 0.17$, $p < .001$, $\text{Exp}(B) = 2.22$.

Main results (reason for song choice). For the mechanism item, we recoded responses where applicable, such that higher ratings indicate a stronger motivation to choose a personally-meaningful song (vs. a maximally fun/stimulating song), and conducted an independent-samples t-test with condition as the independent variable and this rating (1-7) as the dependent variable.

We again found evidence for why participants made these choices: As hypothesized, Ending participants were significantly more motivated to listen to a personally-meaningful song ($M = 4.22$, $SD = 2.20$) as compared to Control participants ($M = 3.52$, $SD = 1.86$), $t(578) = –4.14$, $p < .001$, $d = 0.34$.

Mediation analyses. In turn, mediation analyses (SPSS PROCESS Model 4, 5,000 bootstrapped iterations) using Context (Control versus Ending) as the independent variable, song choice (novel versus familiar song) as the dependent variable, and meaning ratings as the mediator revealed a significant indirect effect via meaning, Indirect Effect = 0.62, $SE = 0.16$; 95% CI_{boot} [0.33, 0.95], indicating mediation.

Other results. The results of the manipulation check confirmed that Ending participants had endings on their minds ($M = 5.40$, $SD = 1.89$) to a greater degree than Control participants ($M = 2.62$, $SD = 2.10$), $t(578) = –16.77$, $p < .001$, $d = 1.39$. Most participants passed the

attention check (98.79%, 573 of 580) and the honesty check (99.14%, 575 of 580), and reported having no technical problems (98.10%, 569 of 580). Regarding our general start-of-experiment items, participants largely reported enjoying listening to music in everyday life (overall, M = 6.26, SD = 0.94), listening to a mix of new and old music (overall, chosen by 48.62%, 282 of 580; with the remaining 47.76% mostly re-listening to old music, and 3.62% mostly listening to new music), and listening to music frequently (overall, M = 6.45, SD = 1.80, i.e., about 5 days a week). Regarding their actual enjoyment of their chosen song within the experiment, participants (unsurprisingly) highly enjoyed whatever they freely chose (overall, M = 9.55, SD = 1.84).[11]

Finally, all patterns remain when re-analyzing the results while entering all demographic information as covariates, as well as actual song enjoyment and general music preferences, and also when excluding participants who failed any of the checks (see Supplemental Materials).

Experiment 7 provides direct support for our proposed process of personal meaning in driving the basic effect. Having to give up listening to music for the coming week prompted participants to care more about ending on a meaningful note (even if that note was not maximally hedonically stimulating)—which explains why they selected a familiar song on which to end.

[11] While actual enjoyment is an important matter to understand well, we did not preregister to find any particular pattern regarding this item, as it is independent from the basic effect on choice and preferences documented in the current paper. Moreover, the question of whether choices indeed translate into actual experience is a large one that this particular experimental design may not be well-suited to answer. Unrelated factors like ceiling effects (given such high average enjoyment) and experimenter demand or social desirability effects (there may be a tendency to report artificially-high enjoyment to appear to justify one's earlier choice) suggest that more carefully-designed studies may be necessary to gauge actual enjoyment. We discuss this issue and provide preliminary data on this topic in the General Discussion. See Supplemental Materials for all specific enjoyment ratings from this experiment.

Experiment 8:

Naturalistic Application As COVID-19 Closed Hedonic Windows

[preregistration: https://aspredicted.org/db992.pdf]

Finally, we took advantage of a naturalistic shock—COVID-19 activity closures—to explore these dynamics in a new real-world context. As the ongoing COVID-19 pandemic first took hold of American attention in Spring 2020, the restaurant industry was at the forefront of activities announcing temporary closures (i.e., to in-person dining: Severson & Moskin, 2020). This created an unusual but consequential ending window whereby people faced dwindling opportunities to enjoy dining out at restaurants for the coming future (news outlets provided ongoing coverage as states announced stay-at-home orders on a rolling basis over the course of three weeks, starting as early as March 19th, 2020; Mervosh, Lu, & Swales, 2020). Experiment 8 capitalized on this window. We assessed participants' preferences for pursuing desired familiar restaurants versus desired novel restaurants as their restaurant opportunities were coming to a halt. We hypothesized that participants would be especially likely to prefer an old-favorite restaurant for their "last time out" before closures took effect—and that the motivational pull of endings in prompting a return to personally-meaningful experiences would drive this shift.

Method

Participants. We requested 600 participants through Cloud Research, yielding 607 (M_{age} = 36.28, SD_{age} = 11.67; 37.73% female; 27.68% non-White) who participated for $0.70. (sensitivity analysis: minimum effect size that can be detected by this sample size is Exp(B) = 1.51).

Procedure. We administered two separate experimental tasks, all with the same participants, taken one at a time in randomized order. We launched the single survey link, which contained all measures, on March 25, 2020.

In both tasks, participants were asked to choose which of two restaurants they would prefer: "A restaurant that I haven't gone to before and would absolutely love to eat at" (novel option) versus "A restaurant that I have gone to before and absolutely loved eating at" (familiar option). The central idea behind each task was to assess whether restaurant shutdowns were leading people to shift toward familiar (vs. novel) restaurant choices for their final opportunities to enjoy dining in restaurants. We attempted to bottle this idea in two different ways:

Task 1. For Task 1, participants imagined going to dine in person at a sit-down restaurant, and were randomly assigned to a 2 (Context: Control versus Ending, between-subjects) design. In the key Ending condition, participants (n = 303) were instructed to make their choice as if it were "1 week ago," marking their last time going out to dine indoors before the shutdown. For a non-ending comparison, Control participants (n = 304) were instead instructed to make their choice as if were "2 months ago," before any knowledge of impending restaurant shutdowns. We hypothesized that more Ending participants than Control participants would report preferring to dine in a familiar restaurant (versus a novel restaurant).

Task 2. If we indeed found this effect, however, one alternative explanation is that Ending participants chose the familiar option because they were signaling support for known or close others during hard times and not because restaurants opportunities were ending, per se.

We designed Task 2 to rule out this possibility. Participants were again randomly assigned to a 2 (Context: Control versus Ending, between-subjects) design. In the key Ending condition, participants (n = 304) were asked to indicate which of two descriptions (using the

"familiar" and "novel" options as above) better described where they dined in person most recently for their final indoor dining opportunity before the impending shutdown (hence, as opportunities to dine indoors were ending, this choice represents an ending context during this temporal window). For our non-ending comparison in this task, Control participants (n = 303) were asked to indicate which of these same descriptions better described from where they still intended to order delivery for their next food delivery opportunity (hence, as delivery services were still available after indoor dining shut down, this choice does not represent an ending context, even though it occurs in the same temporal window). In this task, if participants simply want to signal support for known or close others during hard times, then they should be just as likely to choose a familiar option for both indoor dining and delivery. However, we hypothesized that participants would be more likely to choose a familiar (vs. novel) restaurant specifically in an ending context (i.e., what they did for their final dine-in opportunity) rather than in a non-ending context during the same hard times (i.e., what to do for their next delivery opportunity).

After making their choices, we also asked all participants (separately for each task) why they chose a particular option, as rated on various dimensions. Of key interest, they rated the dimension of personal meaning via the following item—"Because this kind of place, during this window, is especially important for my meaning/connection (e.g., I feel close with this place and its people)"—from 1 (*not an especially big part of my thinking here*) to 7 (*an especially big part of my thinking here*).[12] Also separately for each task, all participants completed a manipulation

[12] All participants also rated two other dimensions (all presented in randomized order and rated via the same scale): Hedonic Pleasure: "Because this kind of place, during this window, is especially important for my hedonic pleasure (e.g., the pure taste of the food itself, and only the taste"); and Mere Certainty: "Because this kind of place, during this window, is especially important for my mere certainty (e.g., it's just a place I happen to know well about, even if I know it's not good or to my taste, and even if I know I have no personal connection there)." As can be seen in our preregistration, we did not preregister hypotheses for these two items; instead,

check regarding the extent to which they assumed they were making their "last" such choices when making each choice, as in previous studies (1 = *not at all*; 7 = *very much*).

Other variables. At the end of the experiment and after both tasks, participants reported their demographic information and any technical problems or confusions with the studies (forced choice: *no*; *yes [explain]*). We also asked participants to report their current quarantine status (forced choice: *currently self-quarantined*; *plan to self-quarantine in the near future*; *no plans to self-quarantine*), their current location (city and state), and the state of the "restaurants, stores, and other public offerings" near them (forced choice: *most things are now closed*; *most things are still open*). Finally, we were also curious to assess participants' general thought content during this unique window. They independently rated two items in randomized order: the extent to which pandemic shutdowns were making them think about exploring novel experiences, and also simply returning to old-favorite experiences, with each rated from 1 (*not at all*) to 7 (*a lot*).

Results and Discussion

Main results (restaurant choice). Separately for each task, we conducted logistic regression analyses entering Context (Control versus Ending) as a predictor of restaurant choice (novel restaurant versus familiar restaurant).

For Task 1: As hypothesized, significantly more Ending participants (75.91%, 230 of 303) than Control participants (60.20%, 183 of 304) preferred to dine in at a familiar restaurant rather than a novel restaurant, $B = 0.73$, $SE = 0.18$, $p < .001$, $\text{Exp}(B) = 2.08$.

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we specifically preregistered the hypothesized effect for meaning, as reported. Because the pandemic shutdown is such a unique context, we were curious to assess other potential motivations people might have wanted to satisfy, measured independently (e.g., note that participants were free to rate all three dimensions equally highly, by design; but doing so also obscures the statistical detection of which matters "most"). See Supplemental Materials for details and results. Later in the main text, we report key insights when comparing all dimensions (see next footnote, in Experiment 8's Results section).

Likewise, for Task 2: Significantly more Ending participants (80.59%, 245 of 304) than Control participants (62.71%, 190 of 303) chose a familiar restaurant experience rather than a novel one, $B = 0.90$, $SE = 0.19$, $p < .001$, $\text{Exp}(B) = 2.47$.

Main results (reason for choice). Separately for each task, we conducted independent-samples t-tests with Context (Control versus Ending) as the independent variable and ratings of pursuing personal meaning (single item ranging from 1–7) as the dependent variable.

For Task 1: As hypothesized, Ending participants were significantly more likely to make their restaurant choice based on personal meaning ($M = 4.62$, $SD = 1.94$) as compared to Control participants ($M = 4.05$, $SD = 1.87$), $t(605) = 3.67$, $p < .001$, $d = 0.30$.

Likewise, for Task 2: Ending participants were again significantly more likely to make their restaurant choice based on personal meaning ($M = 4.73$, $SD = 1.83$) as compared Control participants ($M = 4.24$, $SD = 1.89$), $t(605) = 3.24$, $p = .001$, $d = 0.26$.

Mediation analyses. To more directly assess this driving influence: Separately for each task, we conducted a mediation analysis (SPSS PROCESS Model 4, 5,000 bootstrapped iterations) using Context (Control versus Ending) as the independent variable, restaurant choice (novel versus familiar restaurant) as the dependent variable, and meaning ratings as the mediator. This analysis indeed revealed a significant indirect effect via meaning ratings, for both Task 1, Indirect Effect = 0.19, $SE = 0.06$; 95% CI_{boot} [0.08, 0.32], as well as for Task 2, Indirect Effect = 0.11, $SE = 0.04$; 95% CI_{boot} [0.04, 0.21], indicating mediation.[13]

[13] Of all three rated dimensions (see Supplemental Materials): Hedonic Pleasure was highest, but showed no difference across conditions and thus did not mediate the effect. Mere Certainty resembled the patterns of Personal Meaning, and the two were highly correlated at $r = .34$ (Task 1) and $r = .31$ (Task 2), ps $< .001$. When re-running our main mediation analyses while controlling for Hedonic Pleasure and Mere Certainty, all results are unchanged: The indirect effect of Context on restaurant choice, via Personal Meaning, remained significant (Task 1:

End-of-experiment items: General thought content as COVID-19 closures unfolded. Along with these main results, we also happened to observe parallel effects on our end-of-experiment items regarding general thought content: Participants reported that their thoughts were dominated by desires to enjoy familiar experiences (M = 4.71, SD = 1.72) relative to desires to enjoy new experiences (M = 3.25, SD = 1.90) as things around them were shutting down, $t(606) = 14.30$, $p < .001$, $d = 0.81$ (see Supplemental Materials).

Other variables. For Task 1, the results of the manipulation check confirmed that Ending participants had endings on their minds (M = 5.21, SD = 1.83) to a greater degree than Control participants (M = 3.04, SD = 2.09), $t(605) = 13.61$, $p < .001$, $d = 1.11$; the same difference was found for Task 2 (M_{Ending} = 4.82, SD = 1.91 versus $M_{Control}$ = 3.68, SD = 1.93; $t(605) = 7.26$, $p < .001$, $d = 0.59$. Most participants reported having no technical problems (99.34%, 603 of 607). Most participants reported they were currently self-quarantining (85.83%, 521 of 607), while some reported planning to self-quarantine in the near future (5.44%, 33 of 607) and others reported not having any plans to self-quarantine (8.73%, 53 of 607). As can be seen in our data file, we recruited participants from across the country (there are over 500 unique city/state responses; we encourage researchers to explore these data for their own research).

Finally, all patterns remain when re-analyzing the results while entering all checks and demographic information as covariates (see Supplemental Materials).

Experiment 8 again replicated the basic effect, as well as the mediating role of meaning in producing it, this time across varied experimental tasks related to more naturalistic ending contexts.

Indirect Effect = 0.12, SE = 0.05; 95% CI_{boot} [0.03, 0.22]; Task 2: Indirect Effect = 0.06, SE = 0.03; 95% CI_{boot} [0.01, 0.14]).

General Discussion

How do people fill their free time? Countless answers to this question boil down to just one of two kinds. On the one hand, we can return to the past and repeat experiences we have had before; we could re-watch a classic movie, reorder a favorite dish, and revisit a beloved travel destination. On the other hand, we can venture into the future and try something new; we could check out the newest blockbuster, spring for the soup du jour, and take the road less traveled.

The current research reveals that people adjust their answer to this question depending on their temporal context. In ending contexts—that is, when people perceive a shrinking window to enjoy some type of activity (even merely temporarily)—their hedonic preferences directionally shift away from novelty and closer to repeat consumption (e.g., holding constant factors like cost, availability, and convenience between such options). We observed this effect across many populations, hedonic domains, instantiations of ending contexts, and actual choices and behaviors. Furthermore, we found that this effect appears to be driven by a desire to avoid risk and end things on a safely-known note—specifically, on a personally meaningful note—which familiar experiences typically promise more so than new ones. Moreover, this shift occurs even when valuing meaning means sacrificing other desirable attributes (e.g., exciting stimulation).

Insights and Implications

On the one hand, some of our results replicate and extend the widely-documented appeal of novelty (e.g., Lyubomirsky et al., 2005; Sheldon et al., 2013): Although 3 of our 7 applicable experiments (i.e., all but Experiment 6) showed that ending contexts led to an absolute shift in preferences (the majority of participants shifted from preferring novelty to preferring familiarity: Experiments 2, 4, and 7), the other 4 experiments showed a relative shift (Ending participants shifted their preferences toward familiarity more than Control participants, but in both

conditions, the majority still consistently preferred novelty [Experiment 1 and 5] or familiarity as they did to begin with [Experiment 3 and 8]). Either way, the fact that we found that ending contexts (even impermanent ones) consistently shifted people closer to the familiar—in spite of novelty's well-established gleam—is perhaps a testament to their own unique pull that is yet-underemphasized in the literature.

Indeed, a large psychological literature has examined the relative value of pursuing new ventures versus sticking with the status quo (e.g., research on exploration-exploitation tradeoffs: see Mehlhorn et al., 2015 for a review). An equally impressive literature has examined resource restrictions and how they affect psychological functioning (e.g., opportunity scarcity: see Shah et al., 2012 for a review). Our research makes numerous contributions at the intersection between these long-standing lines of research:

The meaning of "high note." Our findings unveil nuance to what people really mean by "ending on a high note." All else equal, this motivation often translates into an increased pull to end on a meaningful note. As such, we add to research on permanent endings and end-of-life effects, particularly as studied by socioemotional selectivity theory (SST; Carstensen et al., 1999), in several substantive ways (as we detail below); taken together, our findings help bolster key theoretical claims and suggest that end-effects are perhaps even more malleable and broadly applicable (e.g., across domains, types of endings, and so forth) than has been shown previously.

First, SST research typically examines preference shifts in the context of permanent (or seemingly-permanent) endings (e.g., dying, moving across the country); while such experiences are unambiguous ending contexts, there is still ambiguity about the boundaries of what people perceive as "endings." For SST's predictions to apply, do endings require finality (whereby opportunities are lost forever), or are explicitly non-final endings (whereby lost opportunities

will return indefinitely) treated the same way? Our findings reveal that end-effects do not require a literal loss of opportunities, and can simply entail perceived restrictions (e.g., Experiment 4). As such, we hope to plant a seed for future research to explore further nuances of impermanent endings—which are presumably common in everyday life, yet are less studied in the literature.

Second, we advance SST by providing empirical evidence for one of its key theoretical predictions: that endings prompt a pursuit of meaning. The theoretical underpinnings of SST predict meaningful pursuits of all kinds, yet, to our knowledge, the typical empirical evidence to date comprises studies that largely assess behavioral outcomes of a presumed desire for meaning (rather than direct ratings of meaning), within the social domain alone (e.g., older adults' partner choices). Our findings reveal direct empirical support for meaning, far beyond the social domain.

Third, to our knowledge, one key facet of SST is that shrinking time horizons make people prefer socializing with close others because they lack the time needed to turn strangers into friends (and thus fail to provide experiences that can be meaningfully enjoyed right now, during the only window such individuals have left; e.g., Fredrickson & Carstensen, 1990). Our findings reveal that such a "learning curve" is not required for SST-type effects to still emerge. Indeed, note that this assumption pits a familiar option against a novel option that is objectively worse (e.g., a warm interaction with a friend versus an awkward interaction with a stranger)—which confounds a pursuit of meaningful experiences with a pursuit of any superior experience. We rule out this potentially-confounding explanation by (i) directly measuring meaning (as noted above); (ii) examining domains for which first-time exposures should be immediately enjoyable without any "warm up" (indeed: long-desired new restaurants, for example, are not the same as awkward new strangers); and (iii) holding constant other dimensions of value between novel and familiar options (including cases in which the familiar choice came with sacrificed desirability).

The fact that SST-type effects still appear to emerge in such conditions suggests they are influenced by additional mechanisms and are perhaps even more broadly applicable than currently depicted in the literature.

Lastly, our findings shed light on the extent to which the pursuit of familiar experiences in ending contexts includes the pursuit of repeat experiences. In a typical SST study, participants might be found to prefer the familiar choice of socializing with close others—but note that each of these interactions is still its own new experience, in a way that is not identical to re-watching the same exact movie or re-listening to the same exact song (for example). Our findings reveal that SST-type predictions may hold for truly-repeated experiences beyond the "familiar" aspect of those experiences per se (O'Brien, 2021), further expanding the potential parameters of SST.

Together, these insights hint that the findings of Carstensen and others perhaps under-emphasize the influence of endings on people's everyday life experiences. As reviewed in the Introduction, many aspects of life in general seem to include elements of "coming full circle," such that endings prompt a return to familiar themes. This pattern perhaps suggests a broader psychological link between endings, familiarity, and meaning that manifests in other interesting and yet-undiscovered ways, and that operates through a variety of processes that could include (but are not limited to) socioemotional selectivity effects, occasion matching, processing fluency, risk aversion and general threat management—and/or other related, still-unknown dynamics.

Considering this broader effect of endings allows one to valuably revisit past research on maximization motivations in ways that the typical scope of socioemotional selectivity would not have incorporated. For example, the finding that endings prompt more cheating behavior (e.g., Effron et al., 2015) would, on the surface, seem at odds with the finding that endings prompt less cheating behavior (Touré-Tillery & Fishbach, 2012). Our findings help explain this discrepancy:

If all participants were really motivated to end meaningfully (as our findings suggest), then the two experimental contexts must have included their own unique cues that changed what participants found meaningful (e.g., cueing different aspects of "who they are" to be more or less salient—such as the desire to be competent and thus maximize earnings versus the desire to be moral and thus maximize integrity). As another application, our findings suggest the connection between endings and novelty seeking (e.g., stereotypical notions of the "bucket list") may be exaggerated. Past studies tend to only assess the effects of endings on people's preferences for new activities. Shu and Gneezy (2010), for example, find that people put off visiting novel landmarks until they are expecting to move away, but they do not assess whether people also become similarly (or more) motivated to revisit familiar spots in town. Current understandings of such effects may be skewed toward novelty to the extent that studies only measure novel outcomes. Pursuing novelty may not be the only thing people do when facing endings, nor may it always be the most likely.

A closer look at hedonic adaptation, variety seeking, and related processes. Our findings also highlight that the pull of novelty might not be as uniformly strong as depicted in the literature and perhaps in popular culture (e.g., "bucket lists"). We find that endings increase people's preferences to return to old favorites by their own free choice—old and familiar experiences may not hopelessly gather as much dust as once assumed. This finding is noteworthy in light of positive psychology's traditionally grim view of constructs like hedonic adaptation as an inevitably dulling force on the value of people's experiences (e.g., "Hedonic adaptation can be resisted, but only with conscious, active efforts": Lyubomirsky, 2010, p. 219; "What we miss is one simple thing: Once we have owned the car for a few weeks, other things will be on our minds while driving and we would feel just as well driving a cheaper alternative": Schwarz &

Xu, 2011, p. 144; "This point cannot be overstated: Every desirable experience is transitory": Myers, 1992, p. 53). Our findings emphasize the need for a better understanding of when people overlook the old and familiar beyond the fact that they simply will (Galak & Redden, 2018).

One relevant question on this front is whether our participants were "choosing correctly" by shifting to familiarity in the context of endings. That is, are endings actually any better when people choose to end on a familiar note than if they instead are given a novel equivalent? We suspect the answer is often yes, assuming one trusts that people generally choose things that they will like (albeit imperfectly on the margins: Gilbert & Wilson, 2000). Note that we designed our measures to assess choices that, in principle, bear directly on how people plan and spend their time in the future, including non-hypothetical decisions that tap directly into hedonic preference.

In any case, we were also curious to test this claim with some initial data: In a post-test emulating the "song" design from Experiment 7 (see Supplemental Materials; preregistration: https://aspredicted.org/ee5fz.pdf), we recruited a new sample of participants (N = 503; M_{age} = 40.21, SD_{age} = 12.41; 56.06% female; 24.65% non-White; sensitivity analyses' minimum critical effect size, d = 0.25), all of whom were assigned to a single Ending condition. They made their choice for their last song (novel versus familiar song)—and then, of key interest, we randomly assigned some participants to either listen to the song they chose or listen to whichever song they did not choose (a feature that circumvents the measurement issues posed back in Footnote 10). Upon completion, they reported their evaluation of this last listening experience (1 = *very negative*; 11 = *very positive*). As it turned out, participants who chose to end on a familiar note were right to do so: They enjoyed their last listening experience very much (M = 10.03, SD = 1.43)—and in fact, if they were instead given their novel song to end on, they would have enjoyed this last listening experience less (M = 8.42, SD = 2.53), $t(309) = -6.78$, $p < .001$, d =

0.79. By the same token, the opposite pattern emerged the other way around: While participants who chose to end on a novel note also enjoyed their last listening experience very much (M = 9.30, SD = 2.04)—they would have enjoyed being given their familiar song to end on instead just as much as what they chose, if not more (M = 9.69, SD = 1.62), $t(190) = 1.41$, $p = .161$, $d = 0.21$.

Thus, all told: Although much more research is surely needed on this front, it is worth speculating on the practical implications of our research as it now stands. Again, assuming that people's reported preferences reflect their actual experiences, then our research suggests nuances to psychological models of boosting present enjoyment. When people grow bored of something, for example, many positive psychologists prescribe that they consume something novel or varied (e.g., Lyubomirsky et al., 2005; Sheldon et al., 2013). Our findings suggest that such models could fruitfully incorporate temporal context, to consider that people may become more likely to prefer (and perhaps even benefit from) consuming the same old things—specifically in ending contexts, including impermanent ones. This idea adds to recent findings that emphasize the unforeseen value of repeat experiences (O'Brien, 2019, 2021) by suggesting that endings may orient people toward this value.

People may especially enjoy returning to the old and familiar when opportunities feel limited and especially enjoy trying new things when opportunities feel vast.[14] For themselves, people could structure their hedonic activities in anticipation of this effect—rather than revisit one's old-favorite attraction on Day 1 of a trip to a familiar vacation spot, travelers could save this ritualistic return for the last day of the trip instead. For others, people could strategically

[14] Our experiments tested the effects of perceived ending contexts on enjoying that exact experiential category, but to the extent that desires for meaning underlie the effect, then similar shifts might also emerge for unlinked events as well; facing the last days of a trip might make an old-favorite movie more appealing even if one can still freely watch that movie after returning home. Many of the ideas that we propose in this section could test this possibility.

exploit this effect— marketers might advertise vintage offerings near the ends of seasonal cycles; a café that is set to temporarily close for renovations might make more money on tried-and-true best sellers during the weeks leading up to closing; and so on. Experiment 4's writing task (in which participants brought to mind different upcoming plans) hints at one broadly applicable manipulation of ending contexts. Rather than needing to await an actual closure, for example, a café could simply make a busy (versus less busy) future more salient to customers so as to spur their desire for familiar (versus novel) offerings. Perhaps such psychological framings could be useful for addressing larger societal problems as well—nudging people toward repeat consumption (e.g., by emphasizing endings and last chances) could subtly encourage sustainable consumption by curbing the waste that necessarily accumulates from perpetual novelty-seeking.

Other Future Directions

These broader insights also invite novel research on the basic effect itself. One question pertains to whether idiosyncratic features of our study designs partly contributed to the effect. For example, in Experiments 5-8 (which tested process), we typically gave participants a tradeoff between hedonic quality and meaning, and we found that endings pulled them toward meaning—but, perhaps people might not spontaneously bring to mind such dynamics. In a second separate post-test (see OSF; preregistration: https://aspredicted.org/in9c7.pdf), we recruited a new sample of participants (N = 250; M_{age} = 41.28, SD_{age} = 13.13; 50.40% female; 26.40% non-White; sensitivity analyses' minimum critical effect size, w = 0.18), all of whom were asked to imagine it was their "last night in town for a while"—and to spontaneously write, via an open-ended text box, the kinds of features they hoped to ensure this night would have (300-character minimum). After writing, we piped back their freely-written response and asked them to code it via 1 of 3 forced-choice options (shown in randomized order): whether they mostly described "ending on a

personally-meaningful/sentimental note"; "ending on a purely-pleasurable/hedonic note"; or "neither of these" options. Just 2.00% of participants chose "neither" (5 of 250); of the rest, more participants spontaneously prioritized ending meaningfully (spontaneously generated by 63.60%, 159 of 250) as compared to ending hedonically (spontaneously generated by 34.40%, 86 of 250), $\chi^2(1) = 21.75$, $p < .001$, $w = 0.30$. (All written responses, in full, are in our data file.)

More generally, because we assessed many varied contexts, we presume this basic effect should emerge regardless of whether ending contexts entail dwindling time, money, or energy; whether they are intentional or unplanned, recurring or rare; and so on. In any case, a systematic taxonomy of different instantiations of ending contexts would prove useful for future research. As noted, one's very last choice of what to consume before an ending is not exactly the same as one's last window of choices before an ending (during which endings are top of mind but one's choices are not yet literally "the final" choice); facing last opportunities are not exactly the same as facing limited opportunities (yet some of our experimental measures tilted more toward the latter, e.g., in Experiment 4); and shorter ending periods are not exactly the same as longer ending periods (including, e.g., permanent endings), and thus pose different threats. We suspect that differences in how such parameters bear on our results will be a matter of degree; the more that one's "end moment" feels like a personally-meaningful "event" (to which the above possibilities, and others, could serve as inputs), the more we would expect our theorized dynamics to play out. Another possibility is that different underlying mechanisms may drive the same effect, depending on the kind of event. In some domains (e.g., socializing, as is assessed in the research of Carstensen and others), endings might lead people to worry about their limited time frame for warming up to a stranger; in other domains (e.g., solo leisure activities, like those that we assess in our research), endings might lead people to worry about being blocked from

adequately satisfying a new, piqued interest—but note that in either of these cases, our theory predicts that endings should produce the same directional shift toward familiarity, simply for different psychological reasons.

More research should also scale the basic effect to field settings. Borrowing from major life endings, one suggestive (albeit surely multiply-determined) analysis of prisoners' last meals on death row finds that they tend to choose familiar foods, like their go-to cheeseburger, despite being offered a vast array of possibilities (including other foods that they may have always wanted to try: Jones, 2014). Other field settings might be able to capture such dynamics for more endings as well. The popular music streaming service Spotify offers its users custom-curated playlists, including "Release Radar" and "Discover Weekly" playlists filled with novel songs that users are likely to enjoy, as well as "On Repeat" and "Repeat Rewind" playlists filled with familiar songs based on users' own past song choices (Jacobson et al., 2016). Such offerings parallel our own Experiment 7 in ways that may be fruitfully tapped by future field research.

Future research should also further explore boundaries. First: Our framework suggests that conditions that dissociate perceptions of meaning from repeat options should attenuate the effect. Perhaps stereotypical notions of the "bucket list" rely on this idea—if people at long last pursue an activity that they have spent a lifetime thinking about, it may feel like an old favorite despite technically counting as a novel experience. This might explain why, at least in some cases, other research ostensibly suggests that endings increase novelty seeking over familiarity seeking, as reviewed in the Introduction—finally visiting a local landmark upon moving away (Shu & Gneezy, 2010), or finally getting around to running one's first marathon upon aging out of one's youth (Alter & Hershfield, 2014) might actually reflect the pull of familiarity—these

endings may motivate people toward meaningful activities with which they personally identify, and that these activities can also feel "familiar" despite people having never done them before.

Second: Future research should further unpack how our observed patterns are affected by different methods of measurement. As discussed, there may be differences in how "direct" these measures are (e.g., Experiment 3's measures were about consuming one's choice versus literally consuming the choice itself). Moreover, 3 of our 8 experiments assessed hypothetical laboratory judgments, which may carry unique self-report biases (e.g., experimenter demand, social desirability) that are not present in real, everyday decision making. Fortunately, our other experiments help speak to these concerns, at least in part (e.g., participants made real, incentive-compatible choices with real financial costs [e.g., $30.00 gift cards] and time costs [e.g., actually having to then complete one's chosen activity]). Some of these self-report biases, in fact, work against our hypothesis (e.g., people tend to seek variety more when in public versus in private [Ratner & Kahn, 2002], which, if anything, would predict all participants to shift toward novelty). In any case, self-report bias is a valid concern that our aforementioned proposals of field applications could further rule out. Assessing individual differences in sensitivities toward endings represents another way to gain traction, as certain individuals might show stronger or weaker effects in theoretically consistent ways (e.g., maximizers may prefer to "make the most" of last opportunities relative to satisficers; Schwartz et al., 2002; people from cultures that view events over time as unfolding linearly, as opposed to cyclically, may treat ending moments as more precious: Ji, Nisbett, & Su, 2001).

Third: What kinds of familiarity "count"? Highly dull routines (despite being old and familiar) presumably never make it to one's hedonic consideration set—in any context—and so may not be increasingly preferred in ending contexts. However, we suspect that even the ending

of more negative familiarity may prove to be surprisingly positive for how people regard it in the long run (e.g., Powell, Barasch, & Alter, 2022); perhaps the dullness of a familiar activity wields a less-detracting effect on people's desires to return to it at a distance (Trope & Liberman, 2010). Relatedly: What kinds of endings "count"? In general, if ending contexts do not feel higher-stakes to begin with, then safer-bet motivations (and all that follows from this in our theorizing) should be less activated. For example, the intensifying effect of endings are more pronounced when they have obvious personal relevance (Tully & Meyvis, 2016), which may explain why we happened to observe domain-level fluctuations in effect sizes and occasional (but inconsistent) interactions with Domain (e.g., Experiment 1); one's last chance to savor a dessert may feel more personal than one's last chance to schedule a work task. Moreover, other temporal markers might also produce different kinds of effects. We explored this idea in Supplemental Experiments S1–S4 (see Supplemental Materials). Although other markers (e.g., new beginnings: one's "first time in a while") sometimes shifted preferences toward familiarity, the markers that most strongly and consistently showed this effect across all supplemental studies were those entailing endings—perhaps hinting at a uniquely tight link between meaning, familiarity, and endings specifically.

In sum, the current research highlights an underemphasized "spice" of the familiar in everyday hedonic decision making—an insight that both qualifies and advances existing understandings of novelty's well-established gleam. As your free time this weekend draws to a close and you begin gearing up for a stressful patch of work ahead, you may find yourself wanting to end on a high note. Simply winding down with an old favorite may do just the trick.

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Chapter 3

Reintroducing Familiarity: How Callbacks Improve Media Experiences

Abstract

Consumers find both new and exciting experiences and old and familiar experiences favorable for separate reasons, yet how might they be combined to make the best of both worlds? In experiences that unfold over time (e.g., narratives and media), many individual moments can dynamically vary in their novelty and familiarity. The current research proposes that one particular type of arrangement of novel and familiar experiences may be more favorable than others: called "the callback." The callback improves narrative experiences by reintroducing familiar elements at the end of a narrative in a way that gives them new insight. Across two experiments (N = 1,000), we find that the callback makes narratives more favorable on affective measures (e.g., enjoyment, satisfaction), and improves marketing-specific outcomes (e.g., willingness to pay, word of mouth). Further, we find that the effect is driven by a sense of "narrative guidance"—the perception that the creator of the experience has constructed the experience in a way that guides the audience through the narrative and helps them interpret it in the most enjoyable and satisfying way. These findings provide an empirical test of a ubiquitous narrative technique and contributes to theory on novelty and familiarity seeking and narrative psychology.

Imagine you are watching TV on a Sunday night when a commercial comes on for Energizer batteries. A pink toy rabbit becomes so energized by its power source – an Energizer battery – that it zooms off of the screen. Next, a commercial for Nasotine Sinus Relief nasal spray comes on. It shows a man sniffling with springtime allergies, followed by a doctor describing the benefits of Nasotine. Suddenly, the Energizer Bunny barrels back across the screen, which completely interrupts the doctor's presentation and reveals that the current advertisement is actually a continuation of the earlier Energizer advertisement. In less than thirty seconds, this real Energizer ad (Anand, 2021) demonstrates a powerful narrative technique that blends novelty and familiarity in a particular way to improve media and marketing consumption experiences, called the "callback."

A callback is when the end of a narrative takes a familiar element from earlier in the narrative and reintroduces it near the end with the intention that it will be understood in a new and different way. That is, a callback is when some element from the beginning or middle of a story (e.g., an earlier story event, character, joke, etc.) returns at the end of the story with some novel insight into its meaning, thus prompting the audience to reshape their understanding of that element. In the Energizer ad, for example, audiences initially understand the toy rabbit as an object that must be contained to the first half of the commercial, but when it returns in the second half, audiences are prompted to update their understanding.

The technique is a mainstay in many forms of narrative media (e.g., movies, TV, novels, speeches, etc.), but the most canonical examples arguably come from stand-up comedy. Comedians frequently end comedy sets by reintroducing an earlier joke in a way that connects to the final joke, giving it new meaning. Visual narratives make use of this technique often and to

great effect. *Citizen Kane*, often at the top of critics' 'Greatest Movies of All Time' lists, famously ends by referencing its opening scene and revealing new insight into its meaning.

Though the callback is a common narrative device, its effect and psychological underpinnings are not well understood. To our knowledge, no empirical work has formally defined the concept of the callback, tested its effectiveness, explored it parameters, or examined its psychological mechanisms. The current research seeks to address this gap in the literature by presenting a theoretical framework for understanding the concept and by testing it empirically. This scientific basis may help creators of narrative media and marketing professionals better understand what the callback is and how to use it.

Across two studies, we demonstrate that the callback makes narrative experiences more enjoyable and satisfying, and yields more favorable responses on marketing-specific metrics (e.g., willingness to pay, word of mouth). Further, we demonstrate the psychological mechanism: the callback leads audiences to feel that the narrative's creator has taken an active role in escorting them through the experience, helping them interpret it in the best way. We dub this concept "narrative guidance."

This work contributes to theory in two areas. First, we seek to add nuance to the literature on novelty and familiarity seeking by highlighting how novelty and familiarity intertwine within experiences that unfold over time (e.g., narratives). Second, we seek to expand the literature on narrative psychology by highlighting the role of the audience-creator relationship in consuming narrative experiences.

Media Experiences and Narrative Psychology

Consumers are surrounded by narrative experiences.[15] In fact, stories are fundamental to how consumers navigate the world (Polletta et al., 2011), whether by consuming entertainment media through movies, television, and books, learning about the world through news and podcasts, or swapping stories with friends and neighbors. Stories are essential to what makes us human (Gottschall, 2012) and help us make sense of our lives (McAdams & McLean, 2013; Pennebaker, 1997).

Consumers are naturally inclined toward narratives, which makes storytelling a powerful tool for attitude, belief, and behavior change. At a basic level, consumers pay close attention to stories (Hillier et al., 2016) and remember them well (Heath & Heath, 2007). Interpersonally, narratives can help reduce violence (Bilali, 2022) and prejudice (Johnson et al., 2013) and help people behave more prosocially (Ma, 2020). For example, people who watch storylines on TV that involve organ donation are more likely to become organ donors themselves (Morgan et al., 2009). Narratives can also promote healthier behaviors (Murphy et al., 2013), for example, by causing people to take skin cancer more seriously (Dillard et al., 2018).

Narratives have been used in a variety of interventions because they are highly persuasive. Stories have been shown to persuade juries more effectively than logical arguments (Pennington & Hastie, 1991) and bridge moral and political divides (Kubin et al., 2021). This persuasiveness makes narrative an especially important tool for marketers as well, who can use it to strengthen consumer relationships with products and brands (Woodside et al., 2008).

[15] Throughout this article, we use the terms "narrative," "media experience," and "story," interchangeably. These phrases refer to the common notion of story—an account of related events or experiences that happen in a sequence—as consumed in media contexts (e.g., movies, TV, books).

"Narrative transportation" drives the power of stories (Gerrig, 1993; Green & Brock, 2000; Green, 2004). This concept describes narrative consumption as a process whereby audiences become emotionally and cognitively immersed in a narrative experience, akin to travel. As Gerrig describes it, story carries the audience away from their "world of origin" (reality) and into a new narrative world, and much like actual travel, the "traveler" (audience) often returns from the journey having been transformed by the experience. Indeed, stories converge an audience's mental processes (e.g., attention) on the narrative experience, causing the audience to empathize with story characters, vividly imagine story events, and psychologically detach from reality (Gerrig, 1993; Green, 2004; van Laer et al., 2004), thus leading to greater enjoyment of media experiences (Green et al., 2004)

Modern text analysis tools and the availability of large datasets of narrative text have allowed for new insights about the structure of stories, including by championing the concept of narrative circuitousness. Some work has examined the most common emotional arcs of stories, revealing that there are six basic shapes to narratives (e.g., rags to riches, Cinderella, Oedipus, etc.), while other work has explored foundational storytelling features like cognitive tension and narrative staging (Boyd et al., 2020). Yet other work explores the ways in which stories unfold (Toubia et al., 2021).

This work explores how stories move through semantic space, proposing both speed (how quickly stories cover ground) and volume (how much ground stories cover) as key metrics—but most relevant to the current research is the metric of circuitousness. Circuitousness is the idea that stories may take indirect (vs. direct) paths between topics. Rather than optimize efficiency by taking the shortest path, circuitous stories wander and repeatedly return to familiar moments. When done well, as we argue the callbacks does, circuitousness may deepen the

audience's relationship with narrative by allowing familiar moments to settle before revisiting them. By also giving new insight to familiar moments, the callback may work especially well as a potent form of circuitousness. In the next section, we discuss how the callback makes use of both novelty and familiarity to create enjoyable and satisfying experiences.

Novelty "Versus" Familiarity

Every consumer experience can be defined by two qualities: novelty and familiarity. Decades of research have found that both are enjoyable for different reasons—novelty tends to be exciting and stimulating, while familiarity tends to be reliable and meaningful. While research on novelty and familiarity seeking has traditionally conceptualized these qualities to be dichotomous, our investigation of the callback highlights how they intertwine within experiences, especially when they unfold over time or semantic space (e.g., media experiences).

Novelty can grab attention, captivate the imagination, and help stave off boredom from repeating old and familiar experiences. Consumers seek novel experiences to satisfy a need for excitement and stimulation in the moment (Berlyne, 1970; Raju, 1980) and retrospectively, by building out an "experiential CV" (Keinan & Kivetz, 2011). Consumers pursue novelty in private to satisfy their intrapersonal wants and needs, as well as in public to satisfy interpersonal wants and needs (e.g., to present oneself to others in a positive light; Ratner & Kahn, 2002). Novelty can be attractive for novelty's own sake, leading people to choose less-preferred experiences simply because they are more novel (Ratner, Kahn, & Kahneman, 1999). Hedonic adaptation causes experiences to become less stimulating as they become more familiar, with psychologists long advising strategies to inject novelty and variety into experiences to improve them (Lyubomirsky et al., 2005). For example, Sheldon and colleagues (2013) find that college students were happier when they performed a wider variety of prosocial acts (e.g., cooking for

others, letting cars merge in front of them on the highway), than students who repeated the same ones over and over (i.e., "variety is the spice of happiness," Sheldon et al., 2013). In sum, consumers value novelty and pursue it accordingly.

Familiarity is attractive for other reasons. Consumers gravitate towards opportunities that offer comfort, minimal risk, and the ability to have deeper and more meaningful experiences. Familiar experiences are processed more fluently, making them feel easier on the mind, more pleasant to understand, and generally less risky (Song & Schwarz, 2009; Zajonc, 1968; Zajonc, 1980). Consumers enjoy being able to quickly and smoothly interpret familiar stimuli, which has positive downstream consequences, like finding familiar things to be more aesthetically-pleasing (Reber, Schwarz, & Winkielman, 2004), truer (Hansen, Dechêne, & Wänke, 2008; Reber & Schwarz, 1999), and more meaningful (Heintzelman, Trent, & King, 2013). Consumers often reminisce about past events because they are meaningful, even though doing so can come with a tinge of sadness (Sedikides et al., 2004). In fact, as the opportunities to have an experience run low and become more precious, consumers turn to familiar experiences because they are more meaningful (Carstensen, 2006; Carstensen et al., 1999; Winet & O'Brien, 2022). Generally, familiarity promotes liking, as with social partners (Laham et al., 2009; Reis, et al., 2011), and brands and products (Janiszewski, 1993). Indeed, it is no accident that consumers regularly return to their favorite books, TV shows, and movies, to experience them again (Russell & Levy, 2012). While familiar experiences lack the sheer thrill and enjoyment that comes with novelty, they do often provide deeper connections and more meaningful experiences. Accordingly, there is certainly no shortage of familiarity-seeking behavior among consumers.

For all the distinct benefits of novelty and familiarity, relatively little research has explored the ways in which both qualities intertwine within experiences. Past work has added

nuance by demonstrating how novelty and familiarity exist more on a continuum than a dichotomy. Research on moderate incongruity effects shows how products benefit most from being somewhat novel and somewhat familiar, simultaneously (Myers-Levy & Tybout, 1989). Research on product adoption shows that when consumers consider new products, they weigh their (familiar) existing knowledge against the (novel) innovativeness of the product to decide whether to adopt (Moreau et al., 2001). Similarly, past work finds that atypical products and experiences are attractive because they carry the exciting or informative benefits of novelty while simultaneously being linked to familiar knowledge structures that help make sense of them. For example, atypical songs are more popular (Berger & Packard, 2018) and atypical scientific papers get cited more (Uzzi et al., 2013). The continuum model helpfully illustrates that novelty and familiarity exist on opposite sides of a spectrum, but how might this apply when considering the individual moments that make up a given experience?

Experiential consumption (e.g., narrative) is composed of a series of moments in sequence, where each moment carries its own balance of novelty and familiarity. Whereas material consumption (e.g., buying an appliance) may involve a more static trade-off between novelty and familiarity (e.g., how novel or familiar an appliance seems at the point of purchase), experiential involves a dynamic trade-off that changes from moment to moment. By taking a closer look at the individual moments in an experience, we can begin to parse the novelty and familiarity of individual elements.[16]

[16] Work on hedonic adaption takes into account some of this dynamism by addressing the general process by which novel experiences become familiar and lead to a slow decay of enjoyment, as well as by proposing strategies for injecting novelty/variety into experiences to resist this decay (e.g., new consumption methods: O'Brien & Smith, 2019; taking breaks: Nelson & Meyvis, 2008; Zhang et al., 2014; adding a variety of activities: Sheldon et al., 2013). However, this work centers more on the general detrimental effect of familiarity through habituation (e.g., being bored by hearing the same story again), whereas the current research

Past work on repetition-break structure illustrates how arranging novelty and familiarity in certain patterns within an experience can influence how it is evaluated. This work finds that consumers prefer when patterns of familiarity get established, then violated through a final instance of novelty (i.e., the "Rule of Threes"; Loewenstein & Heath, 2009; Loewenstein, Raghunathan, & Heath, 2011). To illustrate, the authors describe MasterCard's wildly successful "Priceless" campaign which followed exactly this formula (e.g., "Two tickets: $28. Two hot dogs, two popcorns, two sodas: $18. One autographed baseball: $45. Real conversation with 11-year-old son: priceless."). As this work illustrates, the pattern in which the novel and familiar elements are arranged is essential to understanding what makes the experience enjoyable and satisfying. The repetition-break effect suggests that different arrangements may be more or less favorable. For one, the exact inverse of the repetition-break arrangement may be similarly or even more favorable. That is, instead of interrupting a pattern of familiarity with an instance of novelty, creators could interrupt a pattern of novelty with an instance of familiarity. Such an arrangement would constitute a callback. Next, we will formally define the concept of the callback and discuss whether and how media experiences could be improved by reintroducing familiarity.

Defining the Callback

Given the ubiquity of the callback in media, it is not surprising that many comedians, writers, and laypeople have theories about what constitutes a callback and what makes it an effective narrative technique (e.g., Myers, 2016). However, to our knowledge, little academic work has been conducted on this topic and what exists has been exclusively theoretical or

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focuses on the inherently intertwined nature of novelty and familiarity at a within experience level and considers how best to structure this architecture.

qualitative (Tobin, 2009), or focused exclusively on funniness (Topolinski, 2014), or both (Aaker & Bagdonas, 2021; Chauvin, 2017; Hale, 2018). In the current research, we empirically test the general favorability of the technique (both inside and outside the domain of humor) and discuss the boundaries of our formal definition.

Formally, we define the callback as: A moment in a narrative experience in which the creator reintroduces a familiar moment from the beginning or middle again at the end with the intention of having the audience understand that familiar moment in a new way. We will unpack each aspect of this definition, including the within-experience reference point, the updated understanding of familiar stimuli, the ending placement, and the creator's intention.

Earlier and within-experience. Firstly, the callback is an earlier, within-experience phenomenon. This means the narrative's creator presents a moment that happens earlier in a given experience again within that same experience. This aspect creates a shared reference point with the audience and is a signal that the audience and the creator have a shared reality. The callback references something within the same experience that contains the callback itself, which creates an internal reference. A reference in media creates meaning by tapping into a shared understanding between the creator and the audience of what the referenced thing signifies. An external reference may mention any conceivable thing in the universe, but it will only be meaningful insofar as the audience shares the same understanding as the creator. For example, without further context, a reference to the sitting president could elicit very different understandings depending on whether a given audience member is liberal or conservative. An internal reference provides inherent context within the context of the story, which creates a shared reality (e.g., in a story about a liberal protagonist, a reference to a liberal president will be

understood as positively valenced, regardless of the audience's politics).[17] Because the motivation to share reality with others is a basic drive (Levine & Higgins, 2001) that is crucial to social connection (Rossignac-Milon et al., 2021), the callback may signal alignment between the audience and the creator that may bolster enjoyment and satisfaction (Aaker & Bogdonas, 2021).

A second potential benefit of the internal reference is that it may create a sense of coherence within the narrative. Consumers value coherence because it helps them make sense of the world (Topolinski, 2017). By creating novel connections between seemingly disparate moments, the callback may make experiences more enjoyable and satisfying simply because it makes the experience feel well-structured and coherent (Topolinski, 2017; Topolinski & Strack, 2009a; Topolinski & Strack, 2009b).

Sense remaking. Secondly, the callback involves prompting the audience to update their understanding of a familiar stimuli. This means the creator presents a moment that is initially meant to be understood one way, then reintroduces that moment again later such that it will be understood in a new and different way. The desire to make sense of the world is a fundamental human drive, much like hunger or sexual desire (Chater & Loewenstein, 2016), and narrative is a key source of meaning making (e.g., Pennebaker, 1997). Sense making is both enjoyable and satisfying, as illustrated by moments of insight (Topolinski & Reber, 2010), especially when it changes how we make sense of a greater number of our experiences (Chater & Loewenstein, 2016; Howe et al., 2022). Perhaps remaking sense of familiar experiences allows audiences to

[17] The boundary between external and internal references is somewhat permeable. If, for example, a movie sequel references its predecessor, this could be an external reference if one considers them to be different movies, or an internal reference if one considers them to be part of a larger whole. This distinction is subjective and may vary across stimuli, domains, and individuals. Similarly, an external reference to a cultural touchpoint for which there is a universally-shared understanding (e.g., pizza tastes good) may operate similarly to an internal reference.

"double dip" into the well of meaning by extracting multiple varieties of meaning from the same experience. Consistent with this notion, circuitous experiences promote greater learning by making it easier to integrate novel information into familiar knowledge structures (Harden et al., 1999). This combination of familiar knowledge with novel insights may make the callback particularly enjoyable and satisfying.

In media, this notion of remaking sense may help to explain the popularity of many genres and tropes. Mysteries, for example, are nearly always built clues an audience initially understands to be innocuous until the climax reveals that those clues had been central to the story all along. This storytelling device, known as the "twist ending," is perhaps the most potent form of callback. Effective twist endings cause the audience to remake sense to the greatest possible extent by revealing a drastically new way to understand as many earlier, familiar events as possible (Tobin, 2009). One of the most famous twist endings is in *The Sixth Sense*, in which the main character is revealed to have been dead all along. This revelation prompts the audience to reconsider their understanding of nearly all of the preceding events of the story, and is punctuated by literal flashbacks to multiple earlier, familiar scenes to help the audience engage with the sense remaking process. One could imagine a much less enjoyable and satisfying version of this story in which the main character is revealed to be dead up front—this would technically provide the audience with the same information but would give them an initially correct understanding, rather than the more satisfying incorrect understanding that gets updated to the correct understanding.[18]

[18] This is not to say that an effective callback must necessarily trick the audience into adopting false beliefs—while especially potent callbacks often do this, it is by no means a requirement. Many examples involve a simple unfolding of events that causes sense remaking. For example, a story that ends where it began (i.e., a full-circle ending) will promote sense remaking without

As this example illustrates, callbacks are inherently surprising because sense remaking is inherently surprising. While sense making is the normal processing by which people organize incoming information about the world, sense remaking involves changing one's existing beliefs. Sense remaking catches the audience off guard because their expectations refer to their initial understanding, and so any update to that understanding will be a violation of expecations. The magnitude of the need for sense remaking likely corresponds with the degree of surprise and may be especially predictive of callback effectiveness.[19]

Endings. Thirdly, the callback is best suited for the end of an experience. This occurs for two reasons. One is that consumers prefer when experiences end on a familiar note (Carstensen, 2006; Carstensen et al., 1999; Winet & O'Brien, 2022) in part because they help provide meaningful and well-rounded endings (Schwörer et al., 2020). Effective stories generally end with a denouement—a resolution that ties up all the loose strands of the plot—and because callbacks naturally pick up earlier, familiar moments, they may be most effective during endings. The second reason is that the audience must form and solidify their initial understanding in order for that understanding to be updated later. Naturally, this means the callback must occur later in the narrative, but also, it may be helpful to create more psychological distance between the initial

deceiving the audience into adopting an incorrect initial understanding of the circumstances, but rather just gives them a different initial understanding.

[19] While surprise is inherent to the callback, suspense and curiosity need not be. Mysteries illustrate how stories create knowledge gaps that pique curiosity and create suspense before using a callback to enjoyably and satisfyingly resolve them; however, twist endings like the one in *The Sixth Sense* illustrate that sometimes the most effective callbacks work precisely because they do not create knowledge gaps (that the audience is aware of). The need for any sense remaking at all may come as an enjoyable and satisfying surprise.

understanding and the callback.[20] This distance may allow the initial understanding to solidify with more conceptual closure (Harden et al., 1999), which may make the process of sense remaking that much more meaningful. It is likely no coincidence that many examples of callbacks in media like *Citizen Kane* and *The Sixth Sense* introduce a callback at the end which references the beginning. This bookended narrative structure creates the maximum amount of psychological distance between the point at which the initial understanding is created and the point at which the callback prompts the need to remake that understanding. Thus, the callback may be optimally located at the end of a narrative.[21]

Creator intention. Fourthly, the callback is defined by the intention of the creator to prompt sense remaking, rather than the audience's actual experience of sense remaking. The callback is a function of the narrative structure itself and its particular distribution of novelty and familiarity within the experience, not necessarily of the audience's response to it. Much like how a movie twist is still a movie twist regardless of whether the audience sees it coming (e.g., if they have seen the movie before), a callback is still a callback regardless of whether the audience sees it coming. Granted, we would expect the enjoyment and satisfaction of a callback to diminish when poorly executed (e.g., by being too easy to anticipate) and when sense remaking is undercut for whatever reason (e.g., by missing the initial understanding); however, as long as the

[20] Psychological distance can be created by different means, including time (Trope & Liberman, 2010). Another is by creating a distinct psychological boundary, as occurs in the opening example of this paper with the bogus transition between commercials.

[21] Granted, a callback that happens to occur earlier in the narrative (e.g., in the middle) might activate the same basic psychological processes as one that occurs at the end; however, in this research, we restrict our investigation to the most prototypical example of the callback, which we argue occurs at the end.

creator intended to include a callback as defined above, then the criteria for the callback are satisfied.

Let us clarify how some common, related storytelling devices do and do not necessarily involve a callback (see Table 3.1). Consider the following, which necessarily involve a callback: twist endings, full-circle endings, bookends, foreshadowing (see also: Chekhov's Gun), flashbacks and flash forwards (see also: in-medias res), meaningfully-echoed phrases, perspective-reversals, and running gags. Each device involves a creator reintroducing an earlier, familiar moment in a way that is meant to be understood in a new and different way (e.g., a gun on the wall may foreshadow violence, but the exact nature of that violence will only be understood later through a callback). Seemingly related storytelling devices may not involve a callback because they do not occur within-experiences, do not occur at the end, do not reflect creator intentions, or do not involve sense remaking. First, let's consider summaries, mere repetition, and motifs. Each reaffirms an initial understanding without any attempt to update it to a new and different understanding (e.g., a TED talk in which the speaker simply summarizes the main takeaways is not employing a callback). Continuity, epilogues, and general learning are involved in creating the initial understanding in the first place, but do not involve returning to update that understanding later in a new and different way (e.g., the continuity of a story moves from Point A to Point Z without ever revisiting Point A). Easter eggs, hidden meanings, and double entendres involve sense remaking but make all possible understandings available simultaneously without returning to something earlier in the experience (e.g., a double entendre offers multiple possible interpretations but they are all presented simultaneously).

Table 3.1

Examples of storytelling devices that do and do not characterize a callback

		Criteria that characterize a callback			
Storytelling device	*Capable of characterizing a callback*	*Within-experience reference to earlier moment*	*Occurs at the end*	*Involves sense-remaking process*	*Callback intended by creator*
Twist endings	Yes	X	X	X	X
Full-circle endings	Yes	X	X	X	X
Bookends	Yes	X	X	X	X
Foreshadowing	Yes	X	X	X	X
Flashbacks and flash forwards	Yes	X	X	X	X
Meaningfully-echoed phrases	Yes	X	X	X	X
Perspective-reversals	Yes	X	X	X	X
Running gags	Yes	X	X	X	X
Summaries	No	X	X	-	X
Mere repetition	No	X	X	-	X
Motifs	No	X	X	-	X
Continuity	No	X	X	-	X
Epilogues	No	X	X	-	X
General learning	No	X	X	-	X
Easter eggs	No	-	X	X	X
Hidden meanings	No	-	X	X	X
Double entendres	No	-	X	X	X

Note. The "X" symbol denotes that a given criterion is capable of being met for a given storytelling device.

Narrative Guidance

As described earlier, the power of narrative comes from making travelers out of audiences as they become immersed in narrative worlds. Much focus has been put on the features of narrative consumption that lead to greater narrative transportation. Specifically, past work has explored the content of narratives, the relationship between audiences and the content, and also the relationship between creators and the content (van Laer et al., 2014), but to our knowledge, no research has focused on the audience-creator relationship. We argue that this relationship manifests as a form of guidance through the narrative experience by the creator on behalf of the audience, which is both favorable and bolstered by the use of the callback.

The concept of narrative transportation implies that the less the audience thinks about the creator, the better, because it may detract from the audience's immersion into the narrative. While this may be true, consumers may nevertheless think about the creator implicitly or explicitly, as an inherent aspect of narrative consumption. Imagine a consumer who begins streaming a TV show. As they watch, they must continually decide whether to keep watching or try something else. They might use their present enjoyment (or enjoyment up until that point) as a benchmark. However, they might also try to infer whether the show will get more or less enjoyable moving forward. One way consumers may make this inference is by thinking about the creator.[22] Implicitly or explicitly, they might consider features like the production quality, acting ability, cinematography and so on, to infer how willing and able the creator will be to deliver a good experience from now on. For example, a fan of a showrunner's past work might wait longer to change the channel than someone who is not a fan—not because the content or the audience's relationship to the content is different, but because the audience's relationship to the creator is different.

The strength of the audience-creator relationship may strengthen narrative transportation. One reason a fan, for example, might watch longer is because they are more immersed in the content and less distracted by doubt. Whereas a fan might trust the creator more and thus allow themselves to be carried away by the narrative, a non-fan might constantly wonder in the back of their minds, "Do I trust this creator to deliver?" On some level, audiences are likely always somewhat aware that they are visiting a narrative world that has been created by someone (i.e.,

[22] Here and throughout, we use the singular form of the word "creator." Though narrative experiences can be created by multiple people and even teams of hundreds of people, we argue that the audience's relationship to the creator (or creators) is most fundamentally about knowing that some social agent created the experience, rather than necessarily being about any specific knowledge about the creator (or creators).

they never fully confuse the narrative with reality), and it is possible that their inferences about that creator may influence how enjoyable and satisfying their experience is. But what qualities in a creator would lead audiences to evaluate a narrative more favorably?

Consuming media is essentially a one-way conversation between a creator and an audience. There are conversational norms in normal two-way conversations that dictate what is appropriate (e.g., one must stay on topic; Grice, 1975), but unlike normal conversations, audiences have limited autonomy to control their own experience. For example, a consumer cannot nudge the creator to explore certain storylines or urge them to "get to the point." For this reason, consumption experiences involve an asymmetric power structure in which the consumer experience is in the hands of the creator. Without being able to use any input from the audience, the creator attempts to bring the audience on a tour of their carefully-constructed narrative world—essentially serving as a narrative tour guide.

We posit that an important aspect of the narrative experience is knowing one has an effective narrative tour guide. Being assured that one is "in good hands" may put the audience's mind at ease, allowing them to feel immersed in story, and being uncertain may be distracting and uncomfortable.

We propose that the qualities of an effective narrative tour guide are much like those of an actual travel tour guide. Research from the tourism industry finds that effective guides fill both a leadership and middleman role, by both being in charge of their group while also serving as their cultural broker, helping to interpret the experience for the group's benefit (Cohen, 1985). Three crucial factors emerge as the qualities of an effective tour guide: professional competence, interpersonal skills and organization, and empathy (Huang et al., 2010).

We propose that narrative guidance follows similar tenets as travel guidance. The narrative guide necessarily fills a leadership role by exerting total control over the narrative, and they fill a middleman role by helping the audience understand the experience correctly. Importantly, the narrative guide, like a travel guide, must demonstrate professional competence via having a firm grasp on the content of the experience, interpersonal skills and organization via an ability to structure the experience in a way that most benefits the audience, and empathy via an ability to understand and cater to the anticipated psychological needs of the audience as they experience the story.

Crucially, we argue that the callback is an effective tool for improving narratives specifically because it increases the sense of narrative guidance. When a creator uses the callback, it is a powerful demonstration of all three qualities of narrative guidance at once. Firstly, it shows the creator has a deep knowledge of the content because they are able to construct both an initial understanding of a moment, then construct one or more novel understandings of that same moment. This signals that the creator understands the content beyond a superficial level, and is able to capture more depth by understanding the experience from multiple angles (e.g., by creating a twist ending that reshapes the rest of the story). Secondly, a callback shows the creator has organized the architecture of the story in a particular way to deliver a version of the story that is presented in the most structurally enjoyable or satisfying way (e.g., by providing and withholding certain information to create a satisfying twist ending). Thirdly, a callback shows the creator is able to take the perspective of the audience and anticipate what they will be thinking and feeling throughout the story in order to deliver a satisfying experience (e.g., by understanding how best to deliver a twist to make it feel most

satisfying). By tapping into all three facets of narrative guidance, the callback may be an especially useful narrative technique for increasing enjoyment and satisfaction.

The Current Research

To explore the psychology of callbacks, we conducted two experiments. Across these experiments, we examine whether consumers do, in fact, find narrative experiences more favorable when they use the callback, whether the benefit of the callback applies outside of the domain of humor, and whether the effect extends to marketing-relevant dependent variables (e.g., willingness to pay, word of mouth). We also explore the driving effect of narrative guidance as a mediator.

Our main hypothesis is that the callback will increase favorability ratings of narrative experiences. Experiment 1 assessed the basic effect by measuring whether the presence (vs. absence) of a callback in a story would increase its affective and marketing-related ratings of favorability. Experiment 2 assessed the effect by measuring whether a deeper callback (i.e., that reaches further back in the narrative experience) in a set of jokes would increase its favorability relative to a shallower callback (i.e., that reaches less far back), and also assessed whether narrative guidance mediated the effect All data files, full original experiment materials, and preregistration documents have been made public for review on the Open Science Framework: https://osf.io/n3d2x/?view_only=7211864b186c47808d66fe0e2443cbc3.

Experiment 1: The Callback Improves Favorability

In Experiment 1 (preregistered), we tested whether incorporating a callback into a story would make it more favorable to readers. Participants read a story that included a key piece of information either at the end in the form of a callback or near the beginning as non-callback. Participants evaluated their story on a series of general affective dimensions (e.g., enjoyment,

satisfaction) and marketing-specific dimensions (e.g., willingness to pay, word of mouth). We predicted that readers would find a story with a callback to be more favorable across all measures than a story without a callback.

Method

Participants. We requested 500 participants through Prolific Academic, yielding 500 participants (M_{age} = 39.89, SD_{age} = 13.88; 47.80% female; 23.00% non-white) who participated for $0.60.

Procedure. Participants were randomly assigned to a 2 (callback, between-subjects: yes vs. no) design.

All participants began by learning that they would read and evaluate a story. All participants read a story called "Chess" (Juyal, 2016). It was told from the perspective of a young college student who approaches an old man in the park who is sitting alone in front of a chess board. They begin playing chess and throughout the interaction, the old man continually touches the pieces in a peculiar way (e.g., "…he lightly touched the pieces one by one, as if caressing them," and "And he seemed to have developed this strange habit of suddenly swooping his hand across the board and lightly touching the pieces, sometimes his and sometimes mine."). The old man proceeds to thoroughly beat the young college student, who becomes exasperated. Upon being defeated, the young college student quips that the old man spent too much time touching the pieces and the old man smiles before leaving the table.

Our key manipulation involved revealing an important piece of information, which is that the old man was blind and presumably touches the pieces in order to understand where they are on the board. Importantly, we manipulated whether this information was revealed early or late in the story. In the callback condition, participants learn this information at the very end, thus

causing participants to update their initial understanding of the old man's behavior. The last line reads, "He lightly touched around for his white walking stick. Somehow, the smile extended to his white, visionless eyes," thus revealing the signals of the old man's blindness to the protagonist and thus also to the participant. In the control condition, participants learn this information near the beginning when the protagonist first encounters the old man, thus providing participants with a correct initial understanding that would not need to be updated later. Before the protagonist sits down to play, participants read, "I didn't realize it at the time, but he was blind, with a white walking stick and white, visionless eyes," thus revealing the old man's blindness to the participant in prospect.

By rearranging the order in which the information is presented without changing the content of the story itself, story recipients either adopt a mistaken understanding of the situation before ultimately updating to the correct one later, or they adopt to the correct understanding right away. The information in the story is consistent across conditions, and thus only the degree to which the recipient's understanding changes over the course of the story is manipulated.

In the callback condition, participants (likely) experience a large change in their understanding of the story from the beginning and middle to the end. The peculiar touching behavior is presented at first as a strange behavior by what must be a strange elderly man. Only later is this behavior revealed to be the normal behavior of a blind person.[23] This version of the

[23] Whether the participant recognizes the correct interpretation initially (i.e., before it is explicitly revealed by the creator) is likely important for enjoyment, as there will be a greater degree of surprise. However, our argument is that the perceived intent of the creator is the defining feature of the callback. Whether or not the story recipient foresees the callback will make for a less or more effective callback because it removes this element of surprise, but we argue that the callback will still be understood to be a callback and will thus show some degree of positive favorability.

story contains a callback because recipients must restructure their understanding of the familiar moment.

In the non-callback condition, participants experience a more straightforward version of the story in which they adopt a particular interpretation of the situation and hold that interpretation through to the end. This version of the story does not contain a callback; recipients are not forced to restructure their understanding at any point.

To measure favorability of the story, we collected four types of measures. First, we recorded participants' star rating of the story, then a series of affective ratings, followed by a hypothetical willingness-to-pay (WTP) value and a word-of-mouth (WOM) likelihood measure.

Star rating. Participants were asked to leave a review of the story using a continuous 0–10 star-rating system. Participants saw 10 grey stars in a horizontal row which could be clicked anywhere along the row to record their rating. The rating was recorded at the level of precision of one-hundredth of a point. This rating appeared as a number next to the scale, and the stars to the left of the clicked point would appear in yellow (including a semi-filled star, where applicable).

General affective ratings. Participants rated the story on a series of eight affective measures, presented in randomized order. Participants were asked to report the extent to which they found the story likable, enjoyable, pleasurable, rewarding, satisfying, fulfilling, meaningful, and insightful. Responses were recorded on a 1–7 scale with 1 indicating "not at all" and 7 indicating "extremely." For example, for satisfaction, we asked: "To what extent did you find this story satisfying?" (1 = *not satisfying at all*, 7 = *extremely satisfying*).

Willingness to Pay (WTP) (hypothetical). Participants were asked to imagine a website on which readers could read new short stories by up-and-coming authors. On this site, visitors

would need to pay a small fee (between $0.00 and $1.00) in order to read any given story. Participants imagined that the site was trying to determine what would be a fair price to charge for the story they had just read. They then entered a number between 0 and 100 into a blank text box, indicating the number of cents they felt the story was worth.

Word of Mouth (WOM). Participants were asked, “How likely would you be to recommend this story to a friend or colleague?” (1 = *not likely at all*, 7 = *extremely likely*).

Near the end of the experiment, we collected a manipulation check, measuring the extent to which participants felt the story used a callback: “To what extent did the author use "callbacks"? Meaning: Things were mentioned in the beginning or middle of the story that got mentioned again at the end to make you understand them in a new way.” (1 = *not at all*, 7 = *very much*).

We also collected four items as covariates to capture the extent to which the story followed proper storytelling etiquette. Though our manipulation merely rearranges story information without changing the content of the story itself, it is possible that our manipulation confounds callback use with other incidental differences in adherence to unwritten storytelling rules and norms. To put it another way, we might find that one story is more favorable not because of callbacks, per se, but because one story may follow storytelling etiquette more closely and thus may feel more valid or appropriate. To account for this possibility, we asked participants to report the extent to which the story broke storytelling rules (reverse-coded), was an acceptable story in terms of storytelling etiquette, had a coherent narrative, and was a story that made sense (1 = *not at all*, 7 = *very much*). These measures were then included as covariates to isolate the key effect of callback use.

Lastly, participants reported their demographic information, any technical difficulties or confusions (forced choice: *no*; *yes [explain]*), completed a no-penalty honesty check concerning whether we should take their responses as genuine (forced choice: *no*; *yes*), and answered an attention check asking, "Which of the following did NOT happen in the story?" (forced choice: *A college student beat an old man at chess*; *An old man beat a college student at chess*; *An old man was blind*; *A college student was exasperated*).

Results and Discussion.

For each dependent variable, we conducted an independent-samples t-test, entering condition (callback vs. non-callback) as the independent variable. Consistent with our hypotheses, we found across all measures that the story was more favorable when it used a callback than when it did not. Moreover, this result holds when controlling for all story-etiquette measures and all checks (attention check, honesty check, and technical-issue check).

Main results (star rating). As hypothesized, the story received a higher star rating when it used a callback ($M = 7.25$, $SD = 2.08$) than when it did not use a callback ($M = 6.45$, $SD = 2.00$), $t(498) = 4.42$, $p < .001$, Cohen's $d = 0.40$. This result shows the benefit of including the callback was a 12% increase in star rating.

Main results (affective ratings). First, we assessed the appropriateness of combining all eight affective measures into a single composite measure. We found that the measures were highly reliable ($a = .98$), thus we combined them into a single measure. Second, we conducted an independent-samples t-test, with condition as the independent variable and the affective composite measure as the dependent variable. We find the hypothesized effect: the story was rated higher as more affectively favorable when it used a callback ($M = 5.14$, $SD = 1.42$) than

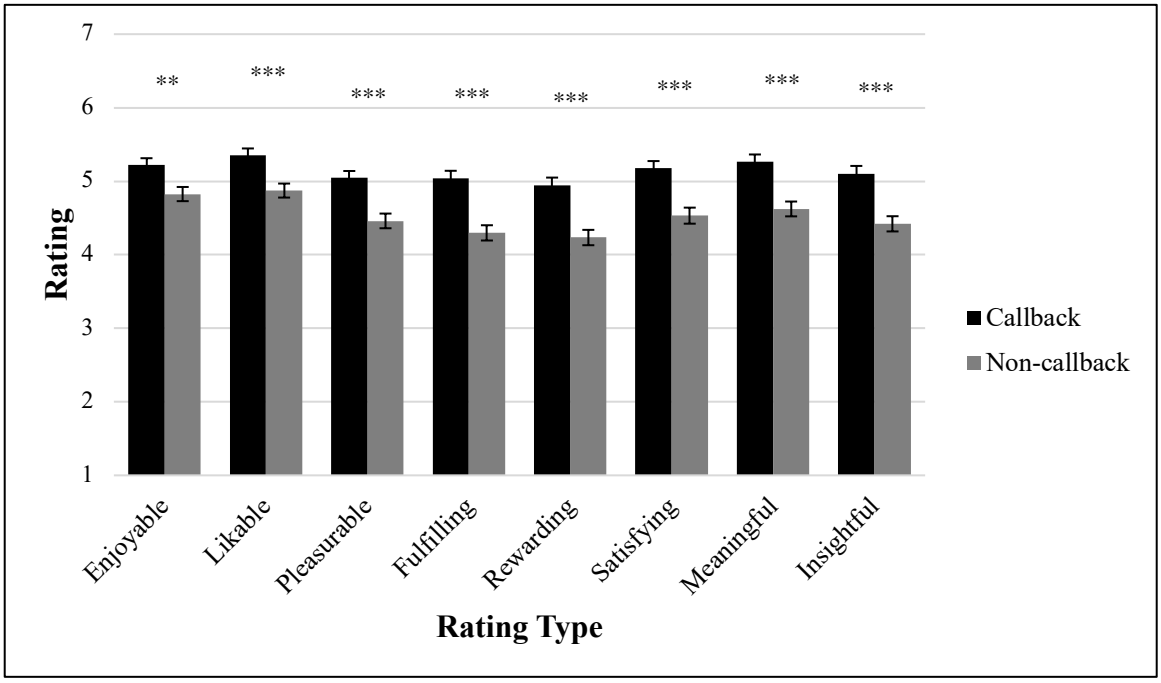
when it did not use a callback ($M = 4.53$, $SD = 1.47$), $t(498) = 4.71$, $p < .001$, Cohen's $d = 0.42$ (see Figure 3.1).

Main results (willingness to pay). As hypothesized, the story received higher hypothetical willingness-to-pay values when it used a callback ($M = 36.00$, $SD = 33.68$) than when it did not use a callback ($M = 24.04$, $SD = 25.89$), $t(498) = 4.45$, $p < .001$, Cohen's $d = 0.40$. This result shows the benefit of including the callback was a 50% increase in hypothetical willingness to pay.

Main results (word of mouth). As hypothesized, participants reported being more likely to share the story with a friend or colleague when it used a callback ($M = 4.35$, $SD = 2.00$) than when it did not use a callback ($M = 3.49$, $SD = 0.12$), $t(498) = 4.98$, $p < .001$, Cohen's $d = 0.45$.

Figure 3.1

Experiment 1: Mean affective favorability ratings across conditions



Note. Error bars show ±1 SE. Significance is flagged at: * = $p < .05$, ** = $p < .01$, *** = $p < .001$.

Other results. The manipulation check confirmed that participants perceived the story as making greater use of callbacks in the callback condition ($M = 4.73$, $SD = 1.69$) than in the non-callback condition ($M = 3.52$, $SD = 1.53$), $t(498) = 8.40$, $p < .001$, Cohen's $d = 0.75$. The conditions did not differ on technical issues ($M_{Callback} = 1.01$, $SD_{Callback} = 0.11$ versus $M_{Non\text{-callback}} = 1.00$, $SD_{Non\text{-}callback} = 0.06$, $t(498) = 1.02$, $p = .309$, and most participants passed the attention check (87.60%, 438 out of 500) and honesty check (99.60%, 498 out of 500).

The storytelling-etiquette measures showed that there were small differences in storytelling etiquette for three out of the four measures, such that the story with the callback (vs. no callback) was rated as not breaking storytelling rules to an equal degree, but was also rated as more acceptable in terms of storytelling etiquette, more coherent, and as making more sense (overall: $M_{Callback} = 5.81$, $SD_{Callback} = 1.00$ versus $M_{Non\text{-}callback} = 5.43$, $SD_{Non\text{-}callback} = 1.05$), $t(498) = 4.11$, $p < .001$, Cohen's $d = 0.37$.

Notably, we find that all patterns remain when re-analyzing the results while controlling for these storytelling-etiquette measures, and also while controlling for technical issues and demographic information, as well as when excluding attention check and honesty check failures (See Supplemental Materials).

The results of Experiment 1 suggest that callbacks improve stories. Even while keeping the literal content of the story the same, simply moving a key piece of information to a particular point in the story prompted participants to both form and then reform their understanding of the events. This updating of their understanding led participants to feel more positively toward the story, finding it more enjoyable and more satisfying, and more meaningful. Moreover, these positive feelings translated to the kinds of outcomes that are most relevant to marketers and media creators: participants gave higher overall star-ratings to stories with callbacks, reported

believing stories with callbacks would be worth paying more for, and reported being more likely to share the story with friends and colleagues through word of mouth.

Despite holding the story content constant, we found that the callback story was perceived as following storytelling etiquette more so than the non-callback story. We speculate that participants were responding, either consciously or unconsciously, to a missed opportunity in the non-callback condition to have withheld the key piece of information, which would have created a greater sense of narrative tension.

Experiment 1 showed the benefits of callbacks using a particular story. However, a potential concern with this design is that there the callback was really just a means to satisfy the audience's curiosity. Where a callback requires formulating and then reformulating one's understanding, curiosity involves opening a knowledge gap and then filling that gap. In this story, participants may have considered the old man's behavior to be an open question that demanded an explanation, rather than simply accepting it as a signal of his strangeness.

In Experiment 2, we test the effect of callbacks using stimuli that include a setup that does not pique the audience's curiosity to begin with, making the callback all the more unexpected. Further, we ran Experiment 2 using a different narrative format: jokes. We test whether callbacks make jokes better, and furthermore, we test why callbacks are effective. Specifically, we explore the possibility that callbacks make audiences feel and appreciate the guiding hand of the narrative's creator.

Experiment 2: The Callback Improves Favorability Through Narrative Guidance

In Experiment 2 (preregistered), we tested whether incorporating a callback into a set of jokes would make them more favorable to audiences. Participants read two jokes that either included a deeper callback (where the ending references the earlier joke) or a shallower callback

(where the ending references the later joke). Further, we explored mechanism by testing whether callbacks were more effective because they increased perceptions of narrative guidance. We collect measures of this construct and test whether it mediates the effect of condition on affective favorability.

Method

Participants. We requested 500 participants through Prolific Academic, yielding 500 participants (M_{age} = 38.22, SD_{age} = 12.69; 53.60% female; 24.40% non-white) who participated for $0.90.

Procedure. Participants were randomly assigned to a 2 (callback depth, between-subjects: deeper vs. shallower) design.

The procedure was largely similar to Experiment 1 except that the focal stimuli were jokes instead of stories, we collected a series of narrative guidance measures to test mechanism, we collected new etiquette measures, and we randomized the presentation order of the dependent variables (specifically, we randomized the order of the dependent variables and the order in which the dependent variables versus the narrative guidance items were presented).

Participants learned that they would read and evaluate a pair of jokes. All participants learned that for each joke, they would click a button to reveal the punchline, then they practiced using this button on a placeholder joke. All participants then read two jokes on separate pages, which, when read together comprise a well-established, canonical example of a callback (see Figure 3.2). The first joke was called “Brick Joke,” and in it, an eccentric man wants to build a house using 99 bricks but can only buy bricks in packages of 100. He builds the house then

throws the leftover brick in the air. The punchline is that the brick does not come back down.[24] The second joke was called "Airplane Joke," and in it, a woman boards a flight with a parrot hidden under her shirt. Once in flight, the pilot of the plane walks by, smoking a cigar. The pilot notices the parrot and admonishes the woman for smuggling the parrot on the plane and the woman admonishes him in return for smoking on the flight. She throws his cigar out of the window and he throws the parrot out of the window. The pilot returns to the cockpit and sees the parrot hit the windshield with something in its mouth.

In the deep callback condition, the item in the parrot's mouth is the brick from the earlier joke. This callback is deep because the audience's initial understanding of the fate of the brick is firmly established and thus requires a major update to incorporate the new information. There is an implicit assumption that jokes are self-contained, which solidifies the expectation that the contents of the "Brick Joke" contain everything participants will ever learn about that brick. The surprising return of the brick forces participants to update their initial understanding and update it—it turns out that the fate of the brick was not just that it did not fall back down, but also that it was caught by a parrot embroiled in a whole other set of circumstances.

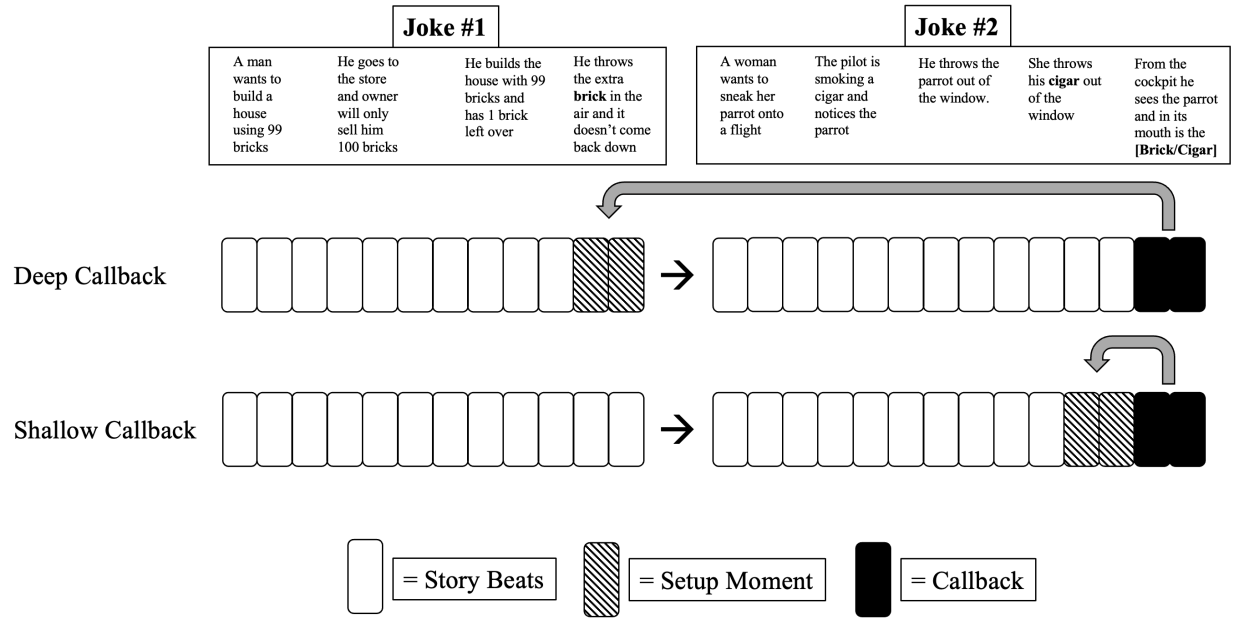
In the shallow callback condition, the item in the parrot's mouth is the cigar from the later joke. This callback is shallow because the audience's initial understanding of the fate of the cigar is only somewhat solidified and thus only requires a minor update to incorporate this new information. In the context of the joke, the cigar had only been thrown out of the window moments earlier. The fact that it was thrown out of a plane window suggests that it was unlikely

[24] The fact that the humor is unclear in this joke is useful for our purposes because later, participants will evaluate the favorability of the entire joke set and their evaluations will narrowly indicate the value of the callback (and not any other incidental, non-callback-related humor in the joke set).

to return to the story (thus helping to somewhat solidify that the fate of the cigar is settled). However, unlike the brick, which the joke set appears to have clearly moved on from, the Airplane Joke could reasonably continue discussing the fate of the cigar without that seeming as unexpected, thus making the cigar's fate less firmly settled.

Figure 3.2

Experiment 2: Diagram of experimental conditions



Note. This diagram demonstrates how the deep callback condition involves a reference that reaches from a later joke into an earlier joke and the shallow callback condition involves a reference that reaches from a later moment in a joke to an earlier moment in the same joke.

Narrative guidance (mechanism). To understand why the callback improves favorability, we collected six items testing the role of perceived narrative guidance, presented in randomized order. These measures included items relating to the creator's perceived active listening and empathy, active structuring of the experience, and perceived efforts make the entire experience worthwhile. Each item was rated on a 1–7 Likert scale (1 = not at all, 7 = completely). The specific items were: "To what extent did you feel like the person who wrote

these jokes understood what the audience would be thinking and feeling?"; "To what extent did you feel like the person who wrote these jokes was responsive to what the audience would be thinking and feeling?"; "To what extent did you feel like the person who wrote these jokes arranged the joke set in a particular way so the audience would be guided through the experience?"; "To what extent did you feel like the person who wrote these jokes arranged the joke set in a particular way so the audience could get the most out of their experience?"; "To what extent did you feel like the person who wrote these jokes respected the audience's time and attention?"; and "To what extent did you feel like the person who wrote these jokes made sure every bit of the experience was worthwhile?"

We also collected new joke-comprehensibility measures (as opposed to strictly joke-etiquette measures) to reflect the new experimental manipulation. Unlike in Experiment 1 where the callback was posited as a feature that storytellers could or could not employ without necessarily changing the fundamental nature of the story as being a proper story, in contrast, in Experiment 2 the callback is the joke. In the case of stories, the callback is a tool to improve stories, but in the case of jokes, the callback can be the exact feature that causes something to be a joke. To illustrate, imagine the two jokes from the manipulation in Experiment 2, but the joke set ends with the parrot having neither a brick nor a cigar in its mouth—here, the "joke set" ceases to contain a joke at all and instead simply becomes a string of strange and improbable events.

Our experimental design uses a manipulation of callback depth that provides a theoretically clean distinction between deeper and shallower callbacks without changing the whether the joke set contains a joke; however, because using similar etiquette measures as Experiment 1 could be misconstrued by participants as measures of joke quality, we instead

collect measures of joke comprehensibility. Responses were recorded on a 1–7 scale with 1 indicating "not at all" and 7 indicating "very much." The specific items here: "How confusing was this joke set?"; "How hard to follow was this joke set?"; "How unclear was this joke set?"; and "How unintelligible was this joke set?"

Lastly, after collecting the manipulation check, we collected a new attention check asking, "Which of the following did NOT happen in the joke set?" (forced choice: *A potted plant was thrown in the air*; *A brick was thrown in the air*; *A parrot was thrown out of a window*; *A cigar was thrown out of a window*). Next, we also included a measure of whether participants had heard either of the jokes before (forced choice: *no*; *yes*).

Results and Discussion.

As in Experiment 1, we conducted an independent-samples t-test for each dependent variable, entering condition (deep callback vs. shallow callback) as the independent variable. Consistent with our hypotheses, we found that the joke set was more favorable across all measures when it used a deeper (vs. shallower) callback. Moreover, this result holds when controlling for all joke-comprehensibility measures and all checks (attention check, honesty check, technical-issue check, and joke-familiarity check).

Main results (star rating). As hypothesized, the joke set received a higher star rating when it used a deeper callback ($M = 3.81$, $SD = 3.00$) than when it used a shallower callback ($M = 2.28$, $SD = 2.25$), $t(498) = 6.45$, $p < .001$, Cohen's $d = 0.58$. This result shows the benefit of including a deeper (vs. shallower) callback was a 67% increase in star rating.

Main results (affective ratings). First, we assessed the appropriateness of combining the eight affective measures into one composite measure. We found that these measures were again highly reliable ($a = .98$), so we combined them into a single measure. Second, we conducted an

independent-samples t-test with condition as the independent variable and the affective composite measure as the dependent variable. We find the hypothesized effect: the joke set was rated as more affectively favorable when it used a deeper callback ($M = 2.92$, $SD = 1.74$) than when it used a shallower callback ($M = 2.04$, $SD = 1.33$), $t(498) = 6.32$, $p < .001$, Cohen's $d = 0.57$.

Main results (willingness to pay). As hypothesized, the joke set received higher hypothetical willingness-to-pay values when it used a deeper callback ($M = 15.23$, $SD = 23.94$) than when it used a shallower callback ($M = 8.90$, $SD = 18.79$), $t(498) = 3.29$, $p = .001$, Cohen's $d = 0.29$. This result shows the benefit of including a deeper (vs. shallower) callback was a 71% increase in hypothetical willingness to pay.

Main results (word of mouth). As hypothesized, participants reported being more likely to share the joke set with a friend or colleague when it used a deeper callback ($M = 2.68$, $SD = 1.98$) than when it used no callback ($M = 1.66$, $SD = 1.27$), $t(498) = 6.85$, $p < .001$, Cohen's $d = 0.61$.

Mediation analysis (narrative guidance). First, we assessed the appropriateness of combining all narrative guidance items into a single composite measure. We found all six items to be highly reliable ($a = .95$) and thus we combined them into a single narrative guidance measure. Comparing across conditions, we find that when the joke set used a deeper callback, the creator was rated as engaging in more narrative guidance ($M = 4.14$, $SD = 1.70$) than when it used a shallower callback ($M = 2.89$, $SD = 1.48$), $t(498) = 8.74$, $p < .001$, Cohen's $d = 0.78$. This effect holds across all narrative guidance measures independently (See Figure 3.3).

Next, to more directly test the driving influence of narrative guidance, we conducted a mediation analysis (SPSS PROCESS Model 4, 5,000 bootstrapped iterations: Hayes, 2017) using

condition (callback depth: deeper vs. shallower) as the independent variable, the affective favorability composite rating as the dependent variable, and the narrative guidance composite measure as the mediator. This analysis yielded a significant indirect effect of condition via narrative guidance, Indirect Effect = 0.90, *SE* = 0.12, 95% CI_{Boot} [0.69, 1.14].

Figure 3.3

Experiment 2: Mean affective favorability ratings across conditions (Panel A), along with narrative guidance ratings across conditions (Panel B)

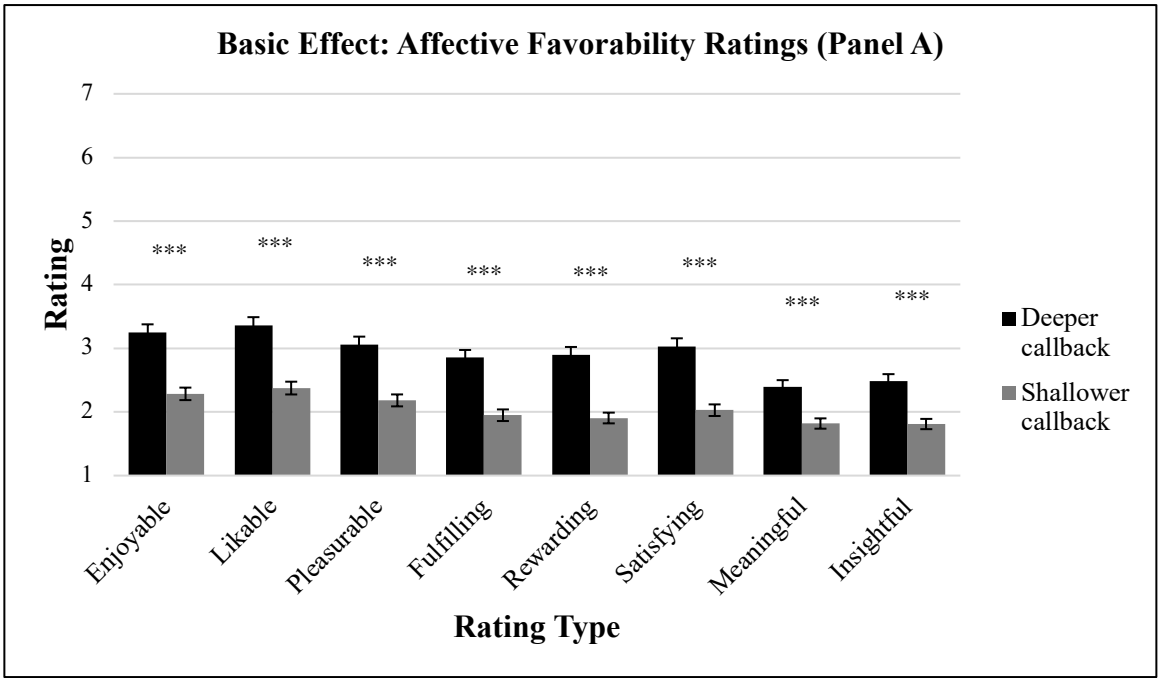
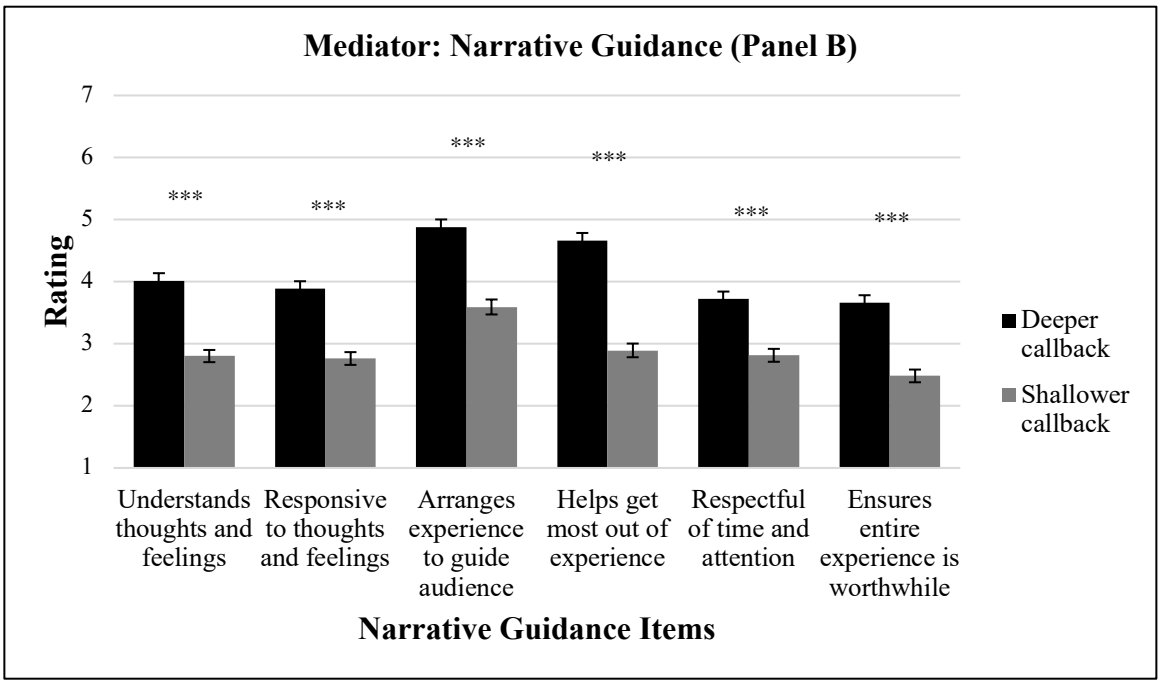


Figure 3.3 (*Continued*)

Experiment 2: Mean affective favorability ratings across conditions (Panel A), along with narrative guidance ratings across conditions (Panel B)



Note. Error bars show ±1 SE. Significance is flagged at: * = $p < .05$, ** = $p < .01$, *** = $p < .001$.

Other results. The manipulation check confirmed that deeper-callback participants felt the joke set made use of callbacks ($M = 5.62$, $SD = 1.48$) to a greater degree than shallower-callback participants ($M = 3.87$, $SD = 1.60$), $t(498) = 12.67$, $p < .001$, Cohen's $d = 1.13$. The conditions did not differ on technical issues ($M_{Deeper} = 1.00$, $SD_{Deeper} = 0.06$ versus $M_{Shallower} = 1.01$, $SD_{Shallower} = 0.09$), $t(498) = 0.56$, $p = .578$, most participants passed the attention check (95.60%, 478 out of 500) and honesty check (99.80%, 499 out of 500), and most participants had not heard either of the jokes before (97.60%, 488 out of 500).

The joke-comprehensibility measures showed that there were small differences across conditions for three out of the four measures, such that the joke set with the deeper (vs.

shallower) callback was rated as equally hard to follow but also less confusing, unclear, and unintelligible (overall: M_{Deeper} = 2.79, SD_{Deeper} = 1.60 versus $M_{Shallower}$= 3.16, $SD_{Shallower}$ = 1.68), $t(498) = -2.55$, $p = .011$, Cohen's $d = -0.23$.

Notably, we find that all patterns remain when re-analyzing the results while controlling for these joke comprehensibility measures, and also while controlling for technical issues and demographic information, as well as when excluding attention check and honesty check failures and people who had heard either of the jokes before (see Supplemental Materials).

Experiment 2 further extends our finding that callbacks improve the favorability of narrative experiences by showing the effect extends beyond storytelling to joke-telling, and that the benefit of callbacks is driven by perceptions of narrative guidance—the idea that the creator has crafted the experience in a way that shows audiences how best to consume it.

General Discussion

In the current research, we empirically demonstrate the benefits of having callbacks in media and narrative consumption experiences. Across two studies, we show how creators can reuse familiar moments in narrative experiences to improve their overall favorability. Specifically, we show in both humorous and non-humorous contexts that reintroducing familiar moments in a way that prompts audiences to understand them in a different way can increase enjoyment, satisfaction, and meaningfulness, can increase overall ratings on a star-rating scale, can increase reported likelihood of social sharing via word of mouth, and can increase hypothetical willingness to pay.

These findings provide empirical support for a narrative technique that has been long theorized about, but has yet to be demonstrated in a controlled laboratory setting. Here, we provide an empirical, scientific basis for the psychology of the callback to better understand how

this narrative technique can be applied by media creators and marketing professionals alike. Further, we provide evidence for the underlying psychological driver for the effect: making audiences feel like the creator has constructed the experience in a way that guides them through the narrative to interpret it in the most enjoyable and satisfying way.

These findings add nuance to current understandings of novelty and familiarity seeking by highlighting how consumers may prefer certain patterns of novelty and familiarity across a given experience. Existing models represent stimuli as either as dichotomously novel or familiar (e.g., Keinan & Kivetz, 2011), or as a balancing of both qualities simultaneously (Myers-Levy & Tybout, 1989), whereas the current findings highlight how experiential stimuli are composed of many individual moments that vary dynamically in novelty and familiarity over the course of the experience. The benefits of the callback demonstrate one particularly enjoyable and satisfying arrangement of novel and familiar moments.

Our findings also add nuance to current understandings of narrative psychology by highlighting how audiences' narrative consumption experiences are influenced by their relationship to the narrative's creator. Research in this area has generally focused on the content of narratives (e.g., Green, 2004), yet the current research finds that audiences' evaluations are also influenced by perceptions of the creator that are created over the course of the experience. Specifically, audiences can develop a perception of the creator as an effective narrative guide—a creator who constructs the experience in a way that escorts the audience through the experience to facilitating the most enjoyable and satisfying interpretations.

Implications

Scientific evidence for what constitutes a callback has been scant and lay theories about the callback have been plenty—the current research provides a framework from which media

creators and marketers can better understand what this narrative technique is and how to apply it. Many disparate narrative constructs can now be understood as members of the same conceptual family. For example, time loop stories (e.g., the movie *Groundhog Day*) and twist endings (e.g., *The Sixth Sense*) can now both be understood as operating on callback psychology. Just as twist endings reintroduce earlier, familiar moments and imbue with new meaning, Phil Connors relives Groundhog Day over and over, each time reliving earlier, familiar moments that get imbued with new meaning with every novel attempt Phil makes to understand and escape the time loop.

Our framework makes clear what factors activate the psychology of the callback and also informs media creators on how to strengthen them. For example, a novelist who is including a twist ending in their story may wonder how to make it a stronger callback. Our work suggests that, alongside the revelation of the twist, the novelist should consider including flashbacks to the familiar moments whose meaning is being remade. Doing so should strengthen the audience's understanding of how their initial understanding is being updated to the new understanding—and this addition should increase the sense that the novelist is an effective narrative guide, thus increasing the audience's sense of enjoyment and satisfaction.

As the opening example illustrates, the callback is a narrative technique that is available to marketers as well as media creators. The psychology of the callback applies to narrative experiences broadly, which can take many forms, including common marketing contexts like television and print advertising. Given that past understandings of the callback have largely come from theoretical and lay insights (and trial and error, on the part of storytelling professionals), the findings of the current research should provide a solid scientific basis from which to think through when and how to apply the callback in media experiences and advertising campaigns.

Limitations and Future Directions

In the current research, we defined the callback in terms of what we determined to be its prototypical form. Thus, we did not comprehensively test many conceivable boundaries of the various components of our definition.

First, we do not explore the boundaries of what counts as "within" an experience. These boundaries are likely to be subjective and idiosyncratic and thus make it difficult to precisely define the boundaries of the callback. For example, it is clear that two scenes from the same movie are within the same experience, and it is also clear that two scenes from two different movies are not within the same experience, but it is less clear whether a scene from a movie and a scene from its sequel could be considered within the same experience. Further complicating things, the same question applies for reboots, parodies, offshoots, and even other movies in the same genre. For example, if a sports movie subverts a common sports movie trope, is that a callback to other sports movies? Future research should explore the boundaries of what types of experiences can realistically be tied together via a callback.

Second, we do not test the callback in non-ending locations. The findings from experiment 2 demonstrate that deeper callbacks are preferred to shallower ones, which suggests that the optimal callback would happen at the end and reference something from the beginning. However, holding callback depth constant, might the callback be equally effective at the end as elsewhere? For example, might a callback at the end of a narrative that references an event in the middle be as effective as a callback in the middle that references an event at the beginning? Based on existing work which finds that consumers prefer familiar endings because they are meaningful (Winet & O'Brien, 2022), we expect that ending on a callback will be especially effective, but future research should test this possibility.

Third, we do not manipulate creator intent. The findings from Experiment 2 demonstrate that the extent to which audiences perceive the creator to have stepped into the role of a narrative guide explains the effect of the callback on favorability, but we did not test whether intentionality might moderate the effect. Presumably stepping into the role of narrative guide will invariably be interpreted as intentional, but it is also possible that a callback itself could be unintentional. We anticipate that framing a callback as unintentional would reduce the effect because it would reduce narrative guidance perceptions, but future work should test whether this is indeed the case.

One common way that the callback occurs in the wild that we did not test is the running gag. This is where a familiar joke is brought back repeatedly throughout a narrative experience (ideally not so often that they become expected and tedious). We test the effect of a single callback at the end of a narrative experience, but it is possible that multiple callbacks to the same familiar moment may stay consistently favorable or may even get increasingly favorable. This notion would be consistent with the idea that repeatedly returning to a familiar topic can benefit learning (see: spiral curriculum; Harden et al., 1999). The study of running gags and their non-humorous callback equivalents would be a valuable avenue for future research.

Lastly, another space in which the callback is common and may be applied is in everyday conversation. Unlike media consumption contexts, which are one-way communications guided by a creator and received by an audience, conversations are two-way communications in which a kind of narrative experience is both created by- and received by- everyone participating. Would callbacks be effective in these two-way communication contexts? And if so, would the favorability of the experience be driven by the same mechanism as in narrative contexts? We anticipate that people whose conversation partners use callbacks would indeed find the

conversations more favorable, and for the same reason: because they perceive their partner to be an effective guide through the experience.

Conclusion

We show that the callback is a powerful tool for improving media experiences. By reintroducing familiar moments from earlier in an experience, creators make narratives more enjoyable and satisfying because doing so makes the creator seem like a more effective narrative guide. And if the Energizer Bunny comes barging in, just know that it's only a callback.

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Appendix A: Story Stimuli (Chapter 3, Experiment 1)

[Callback condition]:

Chess

Logan Greer

As a newly crowned college chess champion, I was quite amused to see an old man alone on the park bench, a fully arranged chess board at the table in front of him. His gaze was fixed on the board as he lightly touched the pieces one by one, as if caressing them.

Why not have some fun, I thought, and asked him if he wanted to play a game. He merely nodded in approval, his gaze never leaving the board. Full of confidence, I played the knight as my first move, hoping to end the game quickly. The old man extended his hand to lightly touch the knight at its new position, then waited for the briefest of moments and made his move.

In the next half hour, I was thoroughly schooled in the art of chess. It appeared as if the old man had already anticipated all my moves and prepared a counter for them. And he seemed to have developed this strange habit of suddenly swooping his hand across the board and lightly touching the pieces, sometimes his and sometimes mine. The sore loser in me was already getting exasperated by this harmless act, but I gritted my teeth and carried on.

Finally, with an inevitable checkmate, my agony was over. Desperately hoping to hide my frustration, I decided to have the final say. Thus, out came out the words: "Great game sir, although I must add that sometimes you took too much time caressing those precious pieces of yours."

The old man just smiled at this and gingerly got up. He lightly touched around for his white walking stick. Somehow, the smile extended to his white, visionless eyes.

[Control condition]:

Chess

Logan Greer

As a newly crowned college chess champion, I was quite amused to see an old man alone on the park bench, a fully arranged chess board at the table in front of him. His gaze was fixed on the board as he lightly touched the pieces one by one, as if caressing them. I didn’t realize it at the time, but he was blind, with a white walking stick and white, visionless eyes.

Why not have some fun, I thought, and asked him if he wanted to play a game. He merely nodded in approval, his gaze never leaving the board. Full of confidence, I played the knight as my first move, hoping to end the game quickly. The old man extended his hand to lightly touch the knight at its new position, then waited for the briefest of moments and made his move.

In the next half hour, I was thoroughly schooled in the art of chess. It appeared as if the old man had already anticipated all my moves and prepared a counter for them. And he seemed to have

developed this strange habit of suddenly swooping his hand across the board and lightly touching the pieces, sometimes his and sometimes mine. The sore loser in me was already getting exasperated by this harmless act, but I gritted my teeth and carried on.

Finally, with an inevitable checkmate, my agony was over. Desperately hoping to hide my frustration, I decided to have the final say. Thus, out came out the words: "Great game sir, although I must add, sometimes you took too much time caressing those precious pieces of yours."

The old man just smiled at this and gingerly got up to leave.

Appendix B: Joke Stimuli (Chapter 3, Experiment 2)

Brick Joke

Once upon a time, there was a man who wanted to build a house. But, being a little eccentric, he wanted to build the house using only 99 bricks. So he went to the hardware store and said, "Hello, I'd like to buy 99 bricks." The owner of the store told him, "I'm sorry, we only sell bricks in quantities of 100." "Can't you cut me a deal or something?" the man asked. "Nope, sorry," replied the owner. So the guy bought 100 bricks. He took the bricks back to his lot, and he built a house using 99 bricks. Now, if you do the math, 100 minus 99 is 1, so he had one brick left. And he took that brick, and he just chucked it, way up in the air and... it didn't come back down!

Airplane Joke

One day a woman decided to go on a vacation with her parrot. She wanted to go to France but the only tickets she could get were No Parrot, No Smoking tickets. Not wanting to leave her parrot at home, she simply stores the parrot in her shirt and acts like she's pregnant. While on the plane, the pilot is walking down the aisle smoking a cigar. He stops by the woman and asks her how her flight is going. Before she can reply, the parrot says, "*Squawk* It's going great." Surprised, the pilot pulls the parrot out from under her shirt. "You can't have a parrot on this plane!" he says. She snatches the cigar from his mouth and says, "Well *you* can't have a cigar on this plane!" Enraged, the pilot grabs her parrot and throws the parrot out the window. Furious, the woman throws his cigar out the window. The pilot returns to the cockpit and is pissed off about losing his cigar. He's about to light up another one when he hears a knock on the windshield. He looks up and sees the parrot. And you'll never guess what was in its mouth...
[Deep callback condition]: "The brick!"
[Shallow callback condition]: "The cigar!"