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OVERVIEW

This dissertation is comprised of three distinct works of research that investigate inconsistencies in people's judgments and decisions with the goal of deriving insights that can inform our understanding of how people make economic decisions. The first two chapters focus on intertemporal choice, while the third chapter examines people's marketplace inferences.

The first and second chapters evaluate the common assumptions in the existing time discounting literature and propose new accounts for how consumers make intertemporal choices that can explain behaviors that deviate from the predictions of traditional time discounting models.

Specifically, the first chapter examines how consumers' subjective financial periods—the idiosyncratic time categories they use to manage their financial goals—affect their intertemporal choice over financial benefits. Consumers are more impatient when choosing between options that are in different subjective financial periods than when choosing between options within the same subjective period. This leads them to exhibit time-inconsistent preferences, which have commonly been attributed to present bias in prior research.

The second chapter documents how consumers' impatience to receive progress signals—actions or events that provide information about the progress of delayed rewards—separately from their impatience to receive the actual rewards, affects their intertemporal choice. Holding reward timings constant, people exhibit higher impatience for options that offer sooner progress signals, which offer sooner uncertainty reduction. This phenomenon is not well-accommodated within the traditional time discounting framework, which assumes that consumers' choices are based on the valuation of the rewards as a function of their timing.

The last chapter examines how people compare different people's liking of an option based on their consideration sets, specifically the size of the sets (i.e., the number of items included in the consideration set). People infer that a consumer with a smaller consideration set likes an option in the set more than another consumer with a larger consideration set that includes the same option. However, this inference is subject to change depending on how salient the superordinate category structure of the options is.

CHAPTER 1.

CROSS-PERIOD IMPATIENCE: SUBJECTIVE FINANCIAL PERIODS EXPLAIN TIME-INCONSISTENT CHOICES[1]

ABSTRACT

Inconsistency in consumer time preferences has been well-established and used to explain seemingly short-sighted behaviors (e.g., failures of self-control). However, prior research has conflated time-inconsistent preferences (discount rates that vary over time) with present bias (greater discounting when outcomes are delayed specifically from the present, as opposed to from a future time). This research shows that time-inconsistent preferences are reliably observed only when choices are substantially delayed (e.g., months into the future), which cannot be explained by present bias. This seeming puzzle is explained by a novel cross-period discounting framework, which predicts that consumers are more impatient when choosing between options occurring in different subjective financial periods. As a result, they display inconsistent time preferences and are less willing to wait for an equally delayed outcome specifically when a common delay to both options moves the larger-later option into a subsequent financial period. Six studies and multiple supplementary studies demonstrate that sensitivity to subjective financial periods accounts for time-inconsistent consumer preferences better than current models of time discounting based on present bias.

Keywords: intertemporal choice, time discounting, categorization, mental accounting, budgeting, impulsivity, present bias

[1] This chapter is published as Jang, Minkwang and Oleg Urminsky (2023), "Cross-Period Impatience: Subjective Financial Periods Explain Time-Inconsistent Choices," *Journal of Consumer Research*, 50(4), 787–809.

Trade-offs between sooner and later benefits are fundamental to consumer decision-making. For example, by foregoing consumption now and spending less, a consumer can afford more consumption in the future. Consumers' time preferences, the degree to which they are willing to forgo smaller-sooner rewards for larger-later rewards, have been used to predict a wide array of consumer financial decisions including spending and saving (Bartels and Urminsky 2015), educational investment (Yoon, Yang and Morewedge 2022), mortgage repayment (Atlas, Johnson and Payne 2017), and retirement decisions (Bidewell, Griffin and Hesketh 2006).

Consumer time preferences have been characterized in terms of two distinct aspects: their *discount rate*, the degree to which consumers value earlier outcomes more than later outcomes in general, and their *present bias*, the degree to which they value an outcome more if it occurs in the present (see Frederick, Loewenstein and O'Donoghue 2002; Urminsky and Zauberman 2015 for reviews). The normative exponential discounting model assumes that consumers have stable time preferences, defined only by a constant discount rate, resulting in consistent preferences between options separated by a given delay, regardless of when the delay begins (Samuelson 1937). However, descriptive research has challenged the assumption that consumers discount normatively, documenting evidence of hyperbolic discounting, such that people value options with short delays less than would be predicted by the normative model, relative to options with longer delays (Ainslie 1975; Thaler 1981). Present bias explains this deviation from normative choices as an additional devaluation of options when they are delayed from the *present*, over and above exponential discounting based on the length of the delay (Laibson 1997).

The construct of present bias has been widely applied as an explanation of consumer behaviors that suggest short-run impatience across a variety of financial decisions, including paying for costly monthly memberships instead of a cheaper annual membership (DellaVigna

and Malmendier 2006), failure to stick to debt-repayment plans (Kuchler and Pagel 2021), and food stamps recipients' failure to save sufficiently for end-of-month grocery purchases (Shapiro 2005). Measures of present bias predict various apparently short-sighted consumer financial decisions, such as failure to save (Bernheim, Skinner and Weinberg 2001), credit card borrowing (Meier and Sprenger 2010), and energy consumption (Werthschulte and Löschel 2021).

Prior theories have largely assumed that present-biased consumers tend to give in to impulsivity when faced with the possibility of an immediately available "present" option (Hoch and Loewenstein 1991). However, despite widespread reliance on the present bias construct, prior work on intertemporal choice has not precisely defined the "present" that consumers treat differently and has not empirically identified it. Recent research (Hershfield and Maglio 2020) has confirmed that most people see the present as short (e.g., commonly the current day or shorter), and finds a relationship between the length of the present and general future-minded preferences, but does not investigate present bias. Research adopting the assumption that the duration treated as the present is as brief as the current day (or briefer) has found quite mixed results when directly testing for present bias over this interval (Scholten and Read 2010).

We propose and test a novel and falsifiable account of time-inconsistent preferences, based on consumers' own subjective mental categorization of financial periods (Heath and Soll 1996; Henderson and Peterson 1992; Sussman and O'Brien 2016). In our *cross-period discounting* framework, consumers are more impatient specifically when choosing between two options that each occur in *different* subjective financial periods (e.g., compared to equivalent choices between options that both occur within the *same* subjective financial period), or in other words, exhibit *cross-period impatience*.

THEORETICAL BACKGROUND

Time Discounting, Time Inconsistency, and Present Bias

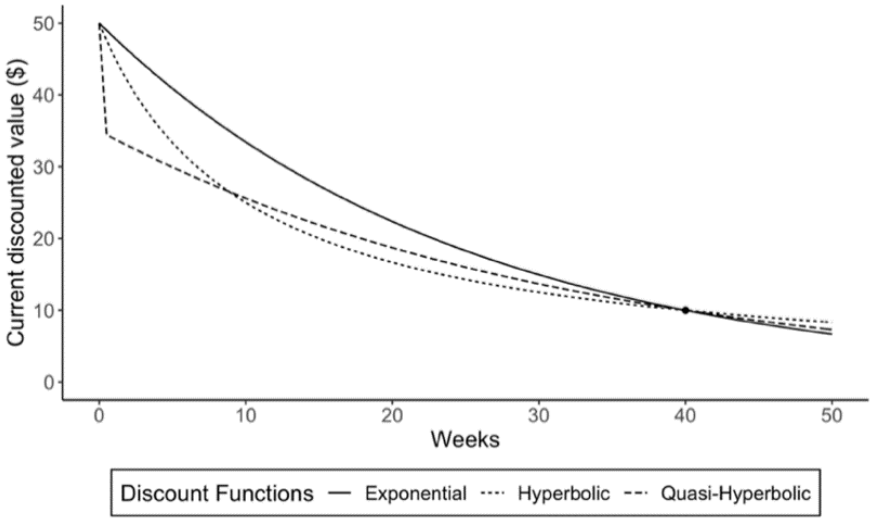
Positive financial outcomes that occur further in the future are objectively less valuable (e.g., due to opportunity costs, such as foregone interest earned). The normative exponential model predicts that the loss of value for an outcome due to a given delay should be the same regardless of when the delay occurs (Samuelson 1937). In this view, a person choosing whether to select a larger-later reward over a smaller-sooner one simply assesses whether the extra reward amount constitutes sufficient compensation for the additional delay, relative to the person's personal interest rate.

Descriptive research has instead found that people exhibit diminishing impatience, such that delayed outcomes lose less value per unit of time for longer delays (Ainslie 1975; Loewenstein and Prelec 1992; Thaler 1981). The tendency to more strongly favor a sooner over a later option the earlier the sooner option occurs has been explained as "a bias for the 'present' over the 'future'" (O'Donoghue and Rabin 1999), i.e., *present bias*. In this view, an outcome in the present is especially valued, and therefore greater value is lost when the outcome is delayed from the present than from other times. Present bias has been used as an explanation of self-control failure, such that temptations in the present are over-valued relative to the delayed consequences, resulting in impulsive behaviors that contradict consumers' own intentions to be far-sighted in the future (Ainslie 1975; Hoch and Loewenstein 1991; O'Donoghue and Rabin 1999).

The degree to which an outcome loses value due to a delay of length t can be expressed

as a discount factor, f(t), which is multiplied by the non-delayed value to compute the net present value of the delayed option (see Urminsky and Zauberman 2015 for a review). Early researchers proposed replacing the exponential discount factor, $f(t) = \delta^t$, with an entirely different, *hyperbolic* function, $f(t) = 1/(1+kt)$, based on prior descriptive research in animal behavior (Ainslie 1975; Mazur 1987). While highly influential in psychology, this approach confounds present bias with discount rates and cannot capture the possibility of normative exponential discounting. As a result, some researchers instead use the *quasi-hyperbolic* discounting model (Laibson 1997), which can be defined as $f(t) = \beta\delta^t$ when $t > 0$ and $f(0) = 1$ when $t = 0$ (i.e., in the present), to accommodate present bias. In this model, the parameter $\beta < 1$ captures the degree of present bias (i.e., the degree of departure from exponential discounting). Figure 1-1 illustrates the difference in the present value of $50 in t weeks, depending on the assumed model.

Figure 1-1. Discount Functions



NOTE. For a hypothetical individual indifferent between $50 in 40 weeks and $10 today.

Measuring Present Bias

While time discounting has been estimated in various ways, the most direct test of present bias specifically, as opposed to time inconsistency in general, is to compare people's choices between a smaller present option and a larger delayed option with their choices in another scenario, in which a "common delay" has been added to both options (i.e., making it a choice between a relatively less delayed and more delayed option). For example, present-biased consumers would be more likely to choose the smaller-sooner option when facing a choice between $100 now or $110 in 4 weeks, than they would when instead choosing between $100 in 26 weeks or $110 in 30 weeks (i.e., both options moved forward by a "common delay" of 26 weeks; Keren and Roelofsma 1995). This test of the *common delay effect* has found evidence for present bias in multiple studies (Coller and Williams 1999; Green, Fristoe and Myerson 1994; Keren and Roelofsma 1995; Kirby and Herrnstein 1995).

The common delay test also reveals an unresolved question in the existing models–how long must the common delay be before present bias for the sooner option is transcended and people's choices become more patient? Some theories suggest that present bias involves non-linear gradual change over time in psychological factors, such as subjective perceptions of time (Zauberman et al. 2009), the concreteness of the mental representation (Fujita et al. 2006), and connectedness to the future self (Bartels and Rips 2010). Other theories, however, have argued that present bias is due to unique psychological properties of the current moment, including certainty of immediate outcomes (Keren and Roelofsma 1995) and greater affective temptation for immediate rewards (Loewenstein 1996; Metcalfe and Mischel 1999). This stream of research suggests that even brief delays from the present should result in a one-time drop in subjective

value for the outcome, a view increasingly adopted in economic theories of present bias (Direr 2020; Harris and Laibson 2013; O'Donoghue and Rabin 2015).

The empirical evidence has not resolved the question of when the "present" period ends. Scholten and Read (2010) report mixed evidence for present bias in the prior literature, with some studies failing to find evidence of the common delay effect. In fact, studies that failed to detect present bias have been interpreted as providing support for normative time-consistent preferences for monetary rewards (Andreoni and Sprenger 2012a; Augenblick, Niederle and Sprenger 2015; Holcomb and Nelson 1992).

Most recently, Hershfield and Maglio (2020) directly examined the mental construct of "the present," that is, when people perceive that the present moment ends and the future begins in general. For the majority of their study participants, the present ended in less than a day, which is consistent with the assumption of some previous tests of present bias (e.g., treating "today" vs. "tomorrow" as in the present vs. future period). However, while they find that a shorter subjective present predicts generally far-sighted behaviors, they did not test whether rewards are more valued when occurring in the subjective present (vs. after the present). Other studies have found the common delay effect between choices involving only delayed options (Green et al. 1994; Green, Myerson and Macaux 2005; Scholten and Read 2006), which cannot be explained by present bias, defining the present period based on Hershfield and Maglio (2020).

Mental Accounting and the Categorization of Time

Our account begins from the premise that time inconsistency may be better understood in terms of how consumers mentally account for time. Consumers use categorization to manage

their financial activities, organizing their income and expenditures into "mental accounts" (Heath and Soll 1996; Thaler 1999) and proactively using budget categories when making future financial plans (Zhang et al. 2022). Thinking in terms of categories allows consumers to consider a narrower set of aggregate outcomes, reducing cognitive burden (Henderson and Peterson 1992). In a variety of domains, people have been found to narrowly-bracket outcomes, assessing costs and benefits within a temporal category, as opposed to interchangeably across time periods (Camerer et al. 1997; Lambrecht and Tucker 2012; Zhang 2017).

Research on memory has found evidence of spontaneous use of temporal categories, such that people can recall a broader temporal unit to which past events belong, even when they fail to precisely recall the exact timing of the event (Huttenlocher, Hedges and Prohaska 1988; Robinson 1986). Consistent with the view that people think categorically about time, financial outcomes that co-occur are more likely to be categorized in the same mental account than events that are temporally distinct (Thaler and Johnson 1990), and conversely, people prefer similar events to be in the same temporal category (Evers, Imas and Kang 2022).

Such categorization can be shaped by salient external markers (e.g., the end of the hour or the month, or one's birthday), with consequences for consumer preferences and decisions (Dai, Milkman and Riis 2014; Donnelly, Compiani and Evers 2022; May 2017; Peetz and Wilson 2013, 2014; Soster, Monga and Bearden 2010; Tu and Soman 2014). Research on categorization shows that, in addition to externally defined categories, a category structure can be initially constructed based on salient goals (Barsalou 1983), and then established in memory, remaining stable over time (Barsalou 1995) and influencing consumer decisions (Reinholtz, Bartels and Parker 2015). This suggests that consumers managing their finances may learn what temporal categorization fits their goals, adopt that categorization, and reliably apply the categorization to

their decisions.

Furthermore, the temporal categories people apply to financial decisions may vary across individuals. Indeed, survey-based research has found that people differ in their long-term subjective financial planning horizons (between several months to several years) and that longer planning horizons predict a range of "farsighted" financial behaviors (for a review, see Hong and Hanna 2014). However, long-term financial planning (e.g., saving and investment plans over the period of several years) is distinct from shorter-term financial planning (e.g., managing one's expenses each month), which focuses on cash-flow and credit management (Hilgert, Hogarth and Beverly 2003). Accordingly, different financial management tasks may motivate different financial planning horizons. Lynch et al. (2010) find that some consumers endorse multiple planning horizons, in terms of days, months, and years, with distinct behavioral correlates.

Subjective Financial Periods and Cross-Period Discounting

Building on these insights from categorization and mental accounting research, we propose an alternative account of time inconsistency, based on consumers' mental accounting of outcomes into different time periods, specifically in financial planning (i.e., as opposed to a general sense of the present, as in Hershfield and Maglio 2020). Assuming that consumers prefer sooner outcomes to later outcomes, we posit that individual consumers making intertemporal choices on positive financial outcomes will additionally rely on their own subjective categorization of time into financial periods that aid in managing relevant financial affairs (e.g., their cash flow).

We define a subjective financial period as a type of mental account defined over a

specific period of time. A key insight from mental accounting research is that people treat resources in different accounts as non-fungible. People set goals specific to a mental account, such as their earning target or budget for category-specific expenditures (Camerer et al. 1997; Soman and Cheema 2011). Therefore, the categorization of resources can affect people's budgeting and tracking of their progress toward their financial goals. Such mental budgeting can also affect spending decisions. For example, people are reluctant to incur an additional expense in a category when doing so would exceed their mental budget for their category (Heath and Soll 1996) or when they perceive the expense to be made out of a smaller account or lower total balance (Morewedge, Holtzman and Epley 2007; Soster, Gershoff and Bearden 2014).

Similarly, people may group financial outcomes within each subjective financial period together and set period-specific financial goals. To the extent that they group and aggregate financial outcomes occurring at different times into the same period, the precise timing of the individual outcomes may be less relevant for their mental accounting. On the other hand, when people face a trade-off between benefits in different periods, they may consider them to be non-interchangeable, perceiving delaying a reward to a different financial period as having a larger impact on their financial planning and spending decisions than the reward being delayed by the same amount of time but remaining within the same financial period.

The novel insight in our account is that time-inconsistent choices can therefore be explained by *cross-period discounting*, an incremental discrete devaluation of the outcomes that occur in a later (vs. sooner) financial period, *over and above* any continuous discounting based on delay. Cross-period discounting implies that consumers will be less willing to wait for an outcome, holding objective delay constant, when it occurs in a later subjective financial period, and will therefore exhibit *cross-period impatience*. Contrary to the standard view that time-

inconsistent preferences are caused by a present bias defined by immediacy, we propose that inconsistent preferences are instead explained by people's current subjective financial period, the time horizon most immediately relevant for managing their financial matters. Specifically, we predict that the common delay effect will be observed when the common delay is long enough for the smaller-sooner outcome to no longer be perceived as in the current financial period.

We test our account in six pre-registered studies (N=4,540). We first demonstrate that the shift in preference from making a more impatient choice (preferring the smaller-sooner option) to a more patient choice (preferring the larger-later option) is only reliably observed when comparing present-future choices to future-future choices with a sufficiently long common delay (Study 1). These results are not predicted by either normative exponential discounting, which assumes time-consistent preferences, or the standard behavioral accounts (hyperbolic and quasi-hyperbolic discounting). These results confirm that while discounting is inconsistent over time, the pattern of inconsistency cannot be simply explained by present bias.

Next, we test for *cross-period impatience*: greater impatience when choosing between two options that occur in different (vs. the same) individual-specific time periods. In Study 2, we measure consumers' categorization of each option as belonging to either their current or future financial period and measure the degree of cross-period impatience, over and above present bias and stable time preferences. We confirm the predicted cross-period effect in Study 3, by eliciting each person's boundary between the current and future financial periods and using a repeated measures design. We further distinguish cross-period impatience from calendar-based categorization effects on time preference (Study 4). Then, we test cross-period impatience using experimentally manipulated subjective financial periods in budgeting, between a current and future period (Study 5) as well as among different future periods (Study 6).

To test our proposed process account, we examine the perception of non-fungibility of options across different financial periods as potentially underlying the cross-period effect. In Studies 3 and 5, we ask consumers about the impact of the option timing on how they manage their finances and on their spending decisions. We likewise test other psychological processes that may depend on the financial period categorization and contribute to the cross-period effect, including perceived duration (Donnelly et al. 2022; May 2017; Zauberman et al. 2009), resource slack (Zauberman and Lynch 2005) and time-varying utility of money (Sharma, Tully and Wang 2019; Strotz 1955). We find both non-fungibility and perceived duration consistently contribute to cross-period discounting.

All studies were pre-registered, including exclusions based on incomplete responses, duplicate IP addresses, and failing the attention check in all studies. Full data, study materials, and analysis codes are available on the OSF repository: https://osf.io/pnzsw. Links to the pre-registrations and additional details of the studies and analyses are provided in the Web Appendix, which is also available on the OSF repository.

STUDY 1: VARYING COMMON DELAYS TO TEST TIME-INCONSISTENT PREFERENCES

To test for time inconsistency, we used the common delay paradigm (Green et al. 1994; Loewenstein and Prelec 1992). In a choice between two monetary rewards, we tested the effect of varying the timing for the smaller-sooner reward ("common delay"), with the larger-later reward always one month later, thereby holding the delay between the rewards ("inter-reward delay") fixed.

If consumers have time-consistent preferences (e.g., exponential discounting), their willingness to wait should be consistent regardless of the common delay. Present-biased preferences would instead imply a steep increase in preferences for the larger-later option when the timing of the smaller-sooner option initially changes from present to future, and either consistent preferences (quasi-hyperbolic) or smaller preference changes (hyperbolic) as the smaller-sooner option is further delayed into the future. In particular, based on an additional assumption that the present is a very short time (Hershfield and Maglio 2020; O'Donoghue and Rabin 2015), present bias would imply fewer choices of the larger-later option when the smaller-sooner option is available *today*, compared to when the smaller-sooner option is delayed.

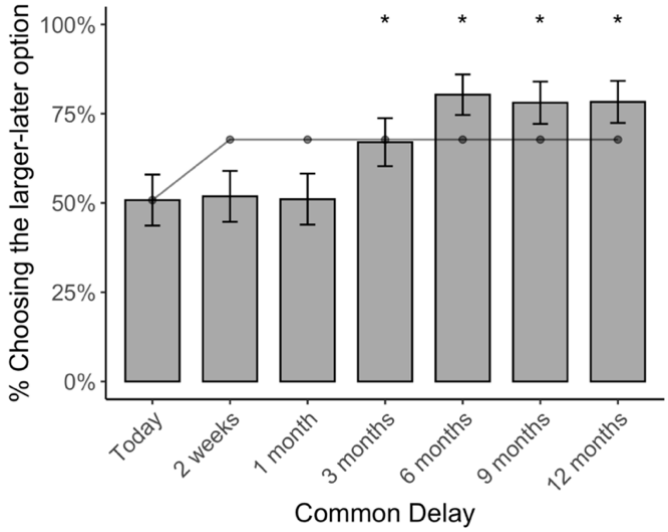
Method

We analyzed data from 1,318 online participants via Amazon Mechanical Turk, after pre-registered exclusions (for incomplete responses, duplicate IP addresses, and failing the attention check). We informed participants that they would be choosing between two monetary rewards that would be received at different times, and that some participants would receive one of the choices they had made as a bonus. Each participant chose between a smaller-sooner reward and a larger-later reward. The smaller-sooner reward was either $35, $40, or $45 (randomly assigned), to be received at the time determined by the randomly assigned common delay, either today (i.e., no common delay; baseline condition), in 2 weeks, or in 1, 3, 6, 9 or 12 months ("delayed" conditions). The larger-later reward was $50, to be received one month later than the smaller-sooner reward.

Results

In the baseline condition, when the smaller-sooner option was to be received today, 51% of participants chose the larger-later option (Figure 1-2). Choices of the larger-later option did not differ significantly from the baseline condition in the two-week (52% choosing the larger-later option, Fisher's exact test, OR = 1.04, p = .92) or one-month common delay condition (51%, OR = 1.01, p = 1). Thus, for common delays of one month or less, we fail to find the common delay effect implied by models of time preference involving present bias.

Figure 1-2. Choice Proportions by Common Delay Conditions (Study 1)



NOTE. Aggregated over smaller-sooner amount conditions. *: significantly different from the baseline ("today") condition (p < .05, pairwise Fisher's exact tests). The line shows the best-fit prediction from the quasi-hyperbolic model. Error bars show 95% confidence intervals.

However, we observed a significant increase in the preference for the larger-later option in conditions with three-month or longer common delays, relative to the baseline condition (3

months: 67%, OR = 1.97, $p = .002$; 6 months: 80%, OR = 3.94, $p < .001$; 9 months: 78%, OR = 3.44, $p < .001$; 12 months: 78%, OR = 3.48, $p < .001$). Because preferences for the larger-later option increased above 50% with longer common delays, these results cannot be explained by reversion to indifference when the common delay is longer (Franco-Watkins, Pashler and Rickard 2006). In addition, the pattern of results was consistent for both lower and higher magnitudes of the smaller-sooner reward amount (Web Appendix C).

Discussion

Theories of time-inconsistent preferences predict a higher preference for larger-later rewards when the smaller-sooner reward is delayed beyond the "present." While the length of the present period has been left unspecified in the quasi-hyperbolic model (Laibson 1997; O'Donoghue and Rabin 1999), most empirical research has operationalized the present as the day of the choice (Ahlbrecht and Weber 1997; Coller and Williams 1999; Green et al. 1994; Read and Roelofsma 2003). This assumption is consistent with recent evidence on individuals' perception of the present (Hershfield and Maglio 2020) and theories of impulsivity which posit psychological differences when making choices specifically for the here-and-now (Keren and Roelofsma 1995; Loewenstein 1996; Metcalfe and Mischel 1999).

In contrast to this prediction, our participants were no more impatient for a smaller-sooner reward today, on average, than when both options were delayed by two weeks or even one month. Some previous research has concluded that people have time-consistent preferences based on similar empirical evidence using short common delays (0, 7 or 35-day common delays, Andreoni and Sprenger 2012; 0, 1 or 7 days, Holcomb and Nelson 1992; 0 or 60 days, Kable and

Glimcher 2010). However, our results also contradict time-consistent preferences, based on the significant common delay effect when choices were sufficiently delayed (i.e., for more than one month). Nevertheless, both our results and these prior results are inconsistent with *present* bias, unless the present is defined to extend over a month.

These results suggest that consumers have time-inconsistent preferences that are not well-explained by the constructs of present bias and impulsivity. In the remaining studies, we test our alternative account of time inconsistency based on cross-period discounting. This approach can explain how consumers might be both relatively insensitive to short-term common delays and be more patient over longer common delays, as observed in Study 1, based on how they subjectively categorize the timing of financial outcomes.

STUDY 2: CATEGORIZATION OF OPTIONS INTO CURRENT VS. FUTURE FINANCIAL PERIOD AND CROSS-PERIOD DISCOUNTING

Our cross-period discounting framework predicts that people will exhibit cross-period impatience, making more impatient choices between a sooner option in the current period and a later option in the future period, compared to an otherwise equivalent choice between two options that are both in the same period. To directly test this, we replicated and extended Study 1 by eliciting people's subjective categorization of the options as belonging to their current or future financial period.

Method

We analyzed data from 1,338 valid participants from MTurk after pre-registered exclusions (as in Study 1). As in Study 1, participants made a potentially consequential choice between a smaller-sooner reward and a larger-later reward. They were randomly assigned a smaller-sooner reward amount ($35, $40, or $45) and timing (i.e., common delay; today (baseline), 2 weeks, 1, 3, 6, 9, or 12 months). The larger-later option was $50, to be received one month later.

After making their choice, participants categorized each option into either a current or a future financial period. Specifically, they read, "*We are interested in how people manage financial matters over time. Please think about what your current financial period is when you manage your financial matters (i.e., planning and budgeting), such as your income and expenditure.*" For each smaller-sooner option and larger-later option that appeared in the choice they had previously made, they were further asked, "*Do you consider receiving **[amount]** in **[timing]** to be in your current financial period or in a future financial period?"* and selected either current financial period or future (next or subsequent) financial period. For exploratory analysis, we also directly asked participants whether they considered the options to be in the same financial period or in different financial periods.

Results

Choice. We replicated the findings from Study 1. Choices of the larger-later option did not differ significantly from the baseline "today" condition in the two-week (53% vs. 46%, OR =

0.76, $p = .19$) or one-month common delay condition (56%, OR = 1.15, $p = .54$). Preferences for the larger-later option were significantly higher for three-month or longer common delays, relative to the baseline condition (3 months: 70%, OR = 2.06, $p < .001$; 6 months: 76%, OR = 2.84, $p < .001$; 9 months: 81%, OR = 3.81, $p < 001$; 12 months: 84%, OR = 4.76, $p < .001$).

Financial Period Categorization. A majority of the participants still considered the smaller-sooner option to be in their current financial period even with a two-week or one-month delay, but not for longer delays (smaller-sooner today: 93%, 2 weeks: 68%, 1 month: 61% vs. 3 months: 25%, 6 months: 17%, 9 months: 13%, 12 months: 13%). Choices were coded as *cross-period* if the participant considered only the smaller-sooner option to be in their current financial period and not the larger-later option and were coded as same-period otherwise. More participants indicated that the choice crossed their current financial period in the today, two-week, and one-month common delay conditions (74%, 55%, 46% respectively) than in the longer common delay conditions (3 months: 13%; 6 months: 8%; 9 months: 7%; 12 months: 8%).

To test for present bias and cross-period impatience, we conducted a linear regression (i.e., a linear probability model, Heckman and Snyder 1997) on the choice of the larger-later option (Table 1-1). We first applied the test for present bias used in the prior literature by predicting choices based on a variable coded as 1 for the baseline "today" condition only and 0 otherwise (i.e., no common delay; *Present*), controlling for the amounts of the smaller-sooner option. Participants were, on average, more patient when both options were delayed, compared to when the smaller-sooner option was "today" (i.e., $B_{\text{Present}} = -0.16$, SE = 0.034, $t(1334) = -4.67$, $p < .001$; Model 1 in Table 1-1), which has previously been interpreted as evidence of present bias.

Table 1-1. Test of Present Bias and Cross-Period Impatience (Study 2)

Variable	Model 1	Model 2	Model 3
(Intercept)	0.87 (0.022)***	0.93 (0.022)***	0.79 (0.028)***
Present	-0.16 (0.034)***	-0.018 (0.036)	0.061 (0.037)
CrossPeriod		-0.28 (0.028)***	-0.20 (0.029)***
CommonDelay (in years)			0.27 (0.038)***
SS amount FE	Yes	Yes	Yes

NOTE. Standard errors are in parentheses. ***: $p < .001$

Next, we tested for cross-period impatience by adding the indicator variable for cross-period choices to the regression (Model 2 in Table 1-1). Participants were less likely to choose the larger-later option when the two options were categorized as in different financial periods ($B_{\text{CrossPeriod}}$ = -0.28, SE = 0.028, $t(1333) = -9.92$, $p < .001$), controlling for present bias, which was no longer significant (B_{Present} = -0.018, SE = 0.036, $t(1333) = -0.51$, $p = .61$). This suggests that the seeming evidence of present bias (with present defined as "today") in Model 1 was in fact confounded with and explained by the cross-period effect. These results are robust to also controlling for the length of the common delay (Model 3), showing that the cross-period effect does not merely reflect impatience linearly diminishing with common delay.

Discussion

This study provides initial evidence for cross-period discounting. The effect of common delays we documented in Study 1 and replicated in Study 2 is clearly incompatible with exponential discounting, which would predict no differences across the conditions, given that the inter-reward delay was held constant at one month. The results are also incompatible with the typical understanding of present bias, in which the present is defined to be short, as we observed a significant reversal only with a longer common delay (3 months).

Our results suggest that people's idiosyncratic categorization of options into either current or future financial periods partially explains this pattern. The subjective financial periods, by identifying which choices involved options that spanned across current and future financial periods, better explained time inconsistency in participants' choices than did present bias.

While we mainly focus on the current financial period (vs. any future period) in the current study and studies that follow (Studies 3-5), it is possible that people also plan their finances across multiple future financial periods. In an exploratory analysis, among the participants in the delayed conditions who indicated that both options were in a future financial period (n=756), we tested the sensitivity of their choices to whether they considered the two future-period options to be in the same or in different future periods (when asked directly in a follow-up question). We found significantly lower patience among those who categorized the options into different future periods compared to those who categorized the options into the same future period (72% vs. 81% preferring the larger-later option; OR = 0.60, p = .005). This provides suggestive evidence that the cross-period effect is not limited to the current subjective financial period but instead applies to perceived differences across any two subjective financial periods. We return to this question in Study 6.

STUDY 3: CURRENT-FUTURE PERIOD BOUNDARY AND POTENTIAL PROCESSES

In the studies thus far, different participants had been assigned different choice options. In Study 3, we expanded the scope of the delays and asked all participants to make the same set of choices in a repeated measures design. Then, instead of asking participants to categorize each of the specific choice options into the current or future financial period, we elicited their boundary

between their current and future subjective financial periods. Based on each person's identified boundary for the current financial period, we classified each choice for that person as involving same-period or cross-period options. We used this coded variable to again test whether people were more impatient when a choice involved options they viewed as in different periods and whether that explained what would otherwise be interpreted as evidence of present bias.

Further, we explored the potential reasons for the cross-period effect. First, based on our theorizing that consumers mentally account for resources over time using subjective financial periods, we tested whether the cross-period effect on choice can be partly accounted for by perceiving the options in different subjective periods to be less fungible for managing their financial resources. We also tested three additional constructs that have been proposed to contribute to time inconsistency in prior literature: perceived duration between the timing of the options, usefulness of money, and perceived resource slack.

Prior research suggests that perceiving the time between the options to be longer can explain higher impatience in intertemporal choice (Zauberman et al. 2009). Furthermore, people perceive a duration to be longer when it is presented as spanning different fixed categories (e.g., hours; Donnelly et al. 2021) or punctuated by a larger number of events (May 2017). Correspondingly, people might perceive the time interval between the options to be longer when the options span across a subjective financial period boundary.

The last two constructs specifically pertain to people's idiosyncratic beliefs about their needs for extra resources at different times (as opposed to a more general sense of non-fungibility resulting from mental accounting). People may have a salient consumption occasion on a specific date (Sharma et al. 2019; Strotz 1955). If subjective financial periods correlate with such salient needs, people may report that having extra money in the earlier subjective period

would be more useful than in the later period. In addition, consumers' tendency to believe that they will have more "slack" in the future than in the present (i.e., fewer financial resources and more financial constraints in the present), has been found to underlie people's present-biased preferences (Zauberman and Lynch 2005). If consumers believe that they have fewer available resources and more financial constraints specifically throughout the current financial period as compared to during future financial periods, this could also contribute to the cross-period effect.

Method

We analyzed 519 valid responses from MTurk. In addition to the exclusion criteria in prior studies, only the participants who provided valid responses to the current-future period categorization, such that their current period boundary could be identified, were included (see Web Appendix A for details).

Participants were first informed that one out of 100 participants would be selected at random to be paid out one of their choices. Each participant made 30 intertemporal choices, between $10 sooner and $20 later, in randomized order. The delays associated with each option were constructed by crossing six different timings for the smaller-sooner option (common delay; today, one week, one month, six months, one year, or five years) with five different intervals between the smaller-sooner and larger-later options (inter-reward delay: one week, one month, six months, one year, or five years). For example, when the common delay was one month and the inter-reward delay was six months, participants chose between $10 in one month and $20 in seven months.

After making their choices, participants classified seven different times (today, one week,

one month, three months, six months, one year, and five years) as either in the current or future financial period. We used the latest time categorized as the current financial period by a given participant, such that the subsequent time was categorized as a future financial period, as an approximate measure of the boundary between the current and future financial periods for that participant.

To examine potential mechanisms underlying the cross-period effect, we focused on one of two subsets of choices for each participant: six choices with one-month inter-reward delays and six choices with six-month inter-reward delays (with common delays varying within each subset). We selected the participants who exhibited time inconsistency (e.g., switching between choosing the smaller-sooner and choosing the larger-later option) within either of these subsets of choices (n=403). For those who demonstrated inconsistency in both one-month choices and six-month choices, one subset was randomly chosen. To avoid respondent fatigue, we collected five potential process measures, in random order, only for each of these six choices (rather than for the entire set of 30 choices) from each participant. This data enables testing whether the process measures partly explain the cross-period effect on this subset of choices.

First, we have posited that people may see financial resources received at different times as less fungible with each other when they occur in different (vs. same) financial periods. As a consequence, they would perceive that the timing of the options matters more when the options span across a subjective period boundary. Therefore, we asked how much it would make a difference for their spending ("impact on spending") or for managing their finances and meeting their financial goals ("impact on managing finances") if they were to receive an extra $20 at one or the other of the two different times in each of the six focal choices (1: makes no difference, 10: makes a big difference; see Web Appendix B for the full wording).

Next, using a scale of subjective time from prior research (Donnelly et al. 2022; May 2017; Zauberman et al. 2009), we elicited the perceived duration of the time between the smaller-sooner and larger-later options, separately for the pairs of times used in the options in each of the six focal choices, on an unnumbered slider (0: very short, 100: very long).

Lastly, we measured two variables pertaining to the subjective value of extra money. First, we directly asked which of two times having extra money would be more useful (-5: more useful at the smaller-sooner timing, 5: more useful at the larger-later timing; for all six focal choices). Second, we measured at which of two times they anticipated having more "slack" in their financial resources (-5: more money available at the smaller-sooner timing, 5: more money available at the larger-later timing; for all six focal choices).

In the questions asking about the impact of extra money (impact on spending, impact on finances, and usefulness), we kept the amount constant, since our goal was to measure the effect of timing independently of magnitude. We used the larger-later option amount since it should be able to also satisfy any need that could have also been fulfilled by the smaller dollar amount.

Results

Cross-Period Impatience. The median of the longest time still considered to be in the current financial period was one month, chosen by 38% of the participants for whom the boundary between current and future financial periods could be identified. Only 8% of the participants indicated that their current period ended in less than one week. After coding each of the thirty intertemporal choice questions as presenting a same-period or cross-period choice to the participant based on that participant's own definition of the current vs. future period, about

40% of the choices were cross-period for the median participant.

We conducted the same linear regression analyses as in Study 2 on the choice of the larger-later option, except that we clustered standard errors at the participant level to account for repeated measures. In our initial test for present bias, participants were, on average, more patient when both options were delayed (controlling for inter-reward delay), compared to when the smaller-sooner option was "today" (B_{Present} = -0.10, SE = 0.0071, t(15567) = -14.51, $p < .001$; Model 1 in Table 1-2). Consistent with Study 2, participants were less likely to choose the larger-later option when the two options spanned different financial periods ($B_{\text{CrossPeriod}}$ = -0.23, SE = 0.0091, t(15566) = -25.05, $p < .001$), controlling for inter-reward delay and present bias (Model 2). In fact, once we account for this cross-period effect, the test of present bias was substantially reduced (B_{Present} = -0.026, SE = 0.0069, t(15566) = -3.77, $p < .001$), suggesting that present bias in Model 1 was in fact confounded with and partially explained by the cross-period effect. The cross-period effect was robust to additionally controlling for the length of common delay, while present bias was no longer significant (Model 3).

Lastly, the cross-period effect was robust to controlling for the length of the subjective current financial period for each participant (Model 4). This result suggests that the cross-period effect was not due to an overall higher impatience (i.e., across all choice options) among those with a shorter current period. Instead, consistent with our account, having a different length of the current period predicts greater patience for some choices (those that would be in the same period for the person) but greater impatience for other choices (those in different periods).[2] We

[2] For all regression analyses involving within-subjects repeated measures reported in the article, we find consistent results when using linear regression with random intercepts, which accounts for potential aggregation bias, instead of clustered standard errors (reported in Web Appendix C).

also replicated these results in an additional study with the same design (N=285, Web Appendix D), in which we find no effect of present bias when accounting for cross-period.

Table 1-2. Test of Cross-Period Impatience (Study 3)

Variable	Model 1	Model 2	Model 3	Model 4
(Intercept)	0.71 (0.0095)***	0.76 (0.0097)***	0.73 (0.01)***	0.71 (0.013)***
Present	-0.10 (0.0071)***	-0.026 (0.0069)***	-0.0082 (0.0066)	-0.0073 (0.0066)
CrossPeriod		-0.23 (0.0091)***	-0.20 (0.0094)***	-0.20 (0.009)***
CommonDelay (in years)			0.021 (0.0018)***	0.021 (0.0018)***
Length of current period				0.0024 (0.00091)**
InterrewardDelay (in years)	-0.13 (0.0022)***	-0.12 (0.0022)***	-0.13 (0.0022)***	-0.12 (0.0022)***

NOTE. Standard errors are in parentheses. **: $p < .01$, ***: $p < .001$

Potential Processes. The internal consistency between the two non-fungibility measures was high (Cronbach's alpha = .77) and we averaged these measures into an index of perceived non-fungibility.[3] Overall, the variance inflation factors (VIFs) of the non-fungibility index (1.11), perceived duration (1.14), usefulness (1.08), and slack (1.03) when jointly entered into a regression predicting choice were all close to 1, confirming that these variables are non-redundant and explain distinct variation (see Web Appendix C for bivariate correlations and comparison of a one- vs. multi-factor model).

We first confirmed that the significant cross-period effect replicated in the subset of six choices per participant (either the choices with one-month inter-reward delay or six-month inter-reward delay, depending on the participant) for which we measured process variables. Applying the same regression as Model 3 to this smaller subset of choices, we confirmed a significant cross-period effect ($B_{\text{Cross-Period}} = -0.21$, SE = 0.024, $t(2413) = -8.71$, $p < .001$), controlling for

[3] While this was not part of our pre-registered plan, we combined the measures for simplicity and for consistency with Study 5. The pre-registered analysis using the two questions separately is reported in Web Appendix C and supports the same conclusions.

present bias, common delay, and inter-reward delay.

Using the same regression specification, we also found a significant effect of crossing financial periods on three of the potential process measures: perceived non-fungibility ($B_{\text{Cross-Period}}$ = 0.87, SE = 0.11, $t(2413) = 7.96$, $p < .001$), perceived duration ($B_{\text{Cross-Period}}$ = 6.86, SE = 1.09, $t(2413) = 6.30$, $p < .001$), and usefulness of money ($B_{\text{Cross-Period}}$ = -0.78, SE = 0.12, $t(2413)$ = -6.50, $p < .001$). We did not find a significant effect of period-crossing on perceived slack in financial resources ($B_{\text{Cross-Period}}$ = 0.093, SE = 0.10, $t(2413) = 0.91$, $p = .36$), so we excluded this variable from subsequent analyses.

Exploratory mediation analyses using the significant measures confirmed that each measure significantly mediated the cross-period effect on its own, each explaining between 7-8% of the total effect (Table 1-3(a)).[4] Jointly including all three measures in the regression cumulatively explained about 18% of the cross-period effect on choice, which suggests that each of these measures independently accounts for some of the effect. Indeed, each measure had a significant indirect effect, controlling for each of the other process measures or both jointly (Table 1-3 (b)-(d)).

Table 1-3. Mediation Analyses Results (Study 3)

		Non-fungibility (combined)	**Perceived duration**	**Usefulness**
(a) Without control	Indirect effect (% mediated) 95% Bootstrap CI	-0.017 (8.0%) [-.025, -0.009]	-0.017 (8.1%) [-0.024, -0.009]	-0.015 (7.1%) [-0.022, -0.007]
(b) Controlling for perceived duration	Indirect effect (% mediated) 95% Bootstrap CI	-0.013 (6.7%) [-0.020, -0.005]		-0.013 (6.2%) [-0.020, -0.005]
(c) Controlling for non-fungibility (combined)	Indirect effect (% mediated) 95% Bootstrap CI		-0.015 (7.7%) [-0.022, -0.008]	-0.014 (7.1%) [-0.021, -0.005]
(d) Controlling for usefulness	Indirect effect (% mediated) 95% Bootstrap CI	-0.015 (7.9%) [-0.023, -0.007]	-0.016 (8.0%) [-0.022, -0.008]	
(d) Controlling for two other variables together	Indirect effect (% mediated) 95% Bootstrap CI	-0.012 (6.5%) [-0.019, -0.004]	-0.014 (7.6%) [-0.020, -0.007]	-0.012 (6.7%) [-0.019, -0.004]

[4] We used 5,000 bootstrapped samples in all mediation analyses reported.

Discussion

Using a within-subject design with more extensive delay lengths than in the prior studies, we again find that subjective financial periods better explain time inconsistency in participants' choices than does present bias, via identification of the cross-period choice options, even controlling for length of the common delay.

This study also provides initial support for the role of perceived fungibility in time-inconsistent intertemporal choice. When the options spanned across different financial periods, people perceived the difference in timing to have a larger impact on their finances, which partly accounts for the cross-period effect. In particular, perceived non-fungibility does not seem to necessarily rely on perceptions of the length of time or beliefs about different needs for money at different times. Nonetheless, multiple factors—perceptions of fungibility, usefulness, and perceived duration— all mediated the cross-period effect. These results suggest that the relationship between financial period categorization and patience is likely multiply determined. We further examine these potential mechanisms when we experimentally test for the causal effects of subjective financial periods in Study 5.

STUDY 4: DURATION VS. DATES AND SPONTANEOUS VS. PRESENTATION-DEPENDENT CATEGORIZATION

In everyday life, intertemporal choices often involve trade-offs between options represented in terms of dates, and previous research has found that using dates (vs. durations)

affects discounting (i.e., higher patience, Leboeuf 2006; reduced hyperbolic discounting, Read et al. 2005). Thus, in Study 4, we varied how the delays are represented, either as durations or dates, to test the robustness of the cross-period effect to date formats.

In addition, we test whether consumers' subjective financial periods underlying the cross-period effect can be better explained as a relatively stable individual difference, or as constructed using salient categorization cues. People may use features of the stimulus in a "bottom-up" manner to create context-dependent categorizations (Kaplan and Murphy 2000). As the date format makes the calendar-based category cues (i.e., the boundary between months) more salient, compared to duration descriptions, people will be more likely to make decisions based on month categorization when shown dates, predicting a cross-month effect (additional discounting over month boundaries). If the cross-period effect relies on the subjective periods that are constructed based on these cues, the cross-month effect will coincide with the cross-period effect. Therefore, we test whether cross-period impatience, based on subjective financial periods, predicts intertemporal choices, over and above any effect of the month-boundary.

Method

We analyzed 345 valid surveys from two non-overlapping Prolific (prolific.co) samples, one recruited early in the month (August 4th, N=175) and the other late in the month (August 21st, N=170), to experimentally vary whether some choices involve crossing month boundaries, keeping the relative delay from survey date ("today") constant. We used the same exclusion criteria as in Study 3, only including those whose current-future period boundary could be identified.

Participants in each wave of the survey were randomly assigned to either the duration condition or the date condition. In the duration condition, as in the prior studies, the timing of each choice option was presented as the duration of time from today (e.g., "in 1 month"). In the date condition, the same time was instead presented as the date on which the outcome would occur (e.g., "on September 4, 2020"). This resulted in a 2 (survey date: early vs. late in the month) × 2 (presentation format: duration vs. date) between-subjects design.

Each participant made 33 choices, between $15 at an earlier date and $20 at a later date. Thirty pairs of choice options were created by crossing five timings of the sooner ($15) reward (i.e., common delays; today, 3 days, 1 week, 2 weeks, and 1 month) and six inter-reward delays (3 days, 1 week, 10 days, 2 weeks, 3 weeks, and 1 month). Three additional choices were constructed specifically so that both options were within the same month in one of the survey waves but were in different months in the other wave of the survey (details in Web Appendix B). As in the prior study, we elicited participants' subjective financial time periods by having them categorize a list of twenty different times, displayed in the same format as the times in the intertemporal choices (i.e., duration or date), into either the current or future financial period.

This design enabled us to distinguish between *cross-period* effects (based on subjective financial periods, as in the prior studies) and *cross-month* effects. Consider a participant taking the survey on August 4th, who reports having a two-week current financial period. A choice between $15 in a week (August 11th) and $20 in two weeks (August 18th) would be a cross-period choice based on the self-reported subjective periods but not a cross-month choice because both options are in the same month. Conversely, for a participant on August 21st whose current period was longer than two weeks, choosing between $15 in a week (August 28th) and $20 in

two weeks (September 4th) would be a cross-month choice (based on crossing from August into September) but would not be a cross-period choice based on their subjective financial periods.

Results

Overall Differences Based on Presentation Format and Survey Timing. Overall, we found more choices of the larger-later options in the date (vs. duration) conditions (proportion of larger-later options per person, averaged over participants: $M_{\text{Date}} = 0.76$ vs. $M_{\text{Duration}} = 0.57$, Welch's t-test, $t(341.73) = 5.59$, $p < .001$), replicating prior research on duration vs. date asymmetry (Leboeuf 2006; Read et al. 2005). There was no significant main effect of survey timing ($M_{\text{Early Month}} = 0.65$ vs. $M_{\text{Late Month}} = 0.67$, $t(342.14) = -0.57$, $p = .57$).

The majority of participants (73%) reported subjective current financial periods that differed from the salient calendar period (end of the month). While subjective financial periods matched the end of the month more in the date condition (44%) than in the duration condition (10%, $\chi^2(1) = 47.05$, $p < .001$; see Web Appendix C for details), even in the date condition, the majority of the participants (56%) reported a current period different from the end of the month.

Cross-Period and Cross-Month Effects. As our main tests, we compared choices that did vs. did not span relevant boundaries, based on either participants' self-reported categorization or the end of the month, separately for the duration and date conditions, using linear regression predicting choices of the larger later option with standard errors clustered at the participant level. We coded two variables: *CrossPeriod* to indicate choices between options that were in different (vs. the same) subjective financial periods for the person, and *CrossMonth,* indicating choices

between options in different (vs. the same) months. We first separately tested the cross-period effect (Model 1 in Table 1-4) and then the cross-month effect (Model 2), controlling for whether the choice involved a present option (*Present*), as well as the length of the common delay and inter-reward delay, and survey date. We then tested both cross-period and cross-month effects in a single regression (Model 3).

In the duration condition, we replicate our prior findings of a cross-period effect ($B_{\text{CrossPeriod}}$ = -0.091, SE = 0.024, $t(5769)$ = -3.75, $p < .001$), with no additional effect of present bias (B_{Present} = 0.0082, SE = 0.011, $t(5769)$ = 0.74, p = .46; Model 1). By contrast, there was no detectable cross-month effect ($B_{\text{CrossMonth}}$ = -0.0016, SE = 0.013, $t(5769)$ = -0.13, p = .90; Model 2). The cross-period effect persists ($B_{\text{CrossPeriod}}$ = -0.091, SE = 0.024, $t(5768)$ = -3.74, $p < .001$; Model 3) controlling for the non-significant cross-month effect, consistent with most participants not using month-ends as their current financial period when timings were expressed as delays.

In the date condition, we again replicated a significant cross-period effect ($B_{\text{CrossPeriod}}$ = -0.064, SE = 0.022, $t(5604)$ = -2.92, p = .004; Model 1). Additionally, we found a significant cross-month effect ($B_{\text{CrossMonth}}$ = -0.039, SE = 0.014, $t(5604)$ = -2.87, p = .004; Model 2), suggesting that when outcome timing was presented as dates, people were less likely to choose the larger-later option when it crossed into a different month, all else equal.

Because the end of the month often coincided with the end of the subjective financial period in this condition, we included both cross-month and cross-period in a joint regression. We find a strongly significant effect of cross-period controlling for cross-month ($B_{\text{CrossPeriod}}$ = -0.061, SE = 0.023, $t(5603)$ = -2.69, p = .007) while the cross-month effect controlling for cross-period was marginally significant ($B_{\text{CrossMonth}}$ = -0.026, SE = 0.014, $t(5603)$ = -1.91, p = .057; Model 3). This result suggests that subjective financial period categorization and month boundaries had

parallel but largely distinct effects on intertemporal choice when people were prompted to think in calendar terms by presenting outcomes as dates.[5]

Table 1-4. Test of Cross-Period and Cross-Month Effects (Study 4)

	Duration Condition			Date Condition		
Variable	**Model 1**	**Model 2**	**Model 3**	**Model 1**	**Model 2**	**Model 3**
(Intercept)	0.80 (0.035)***	0.79 (0.035)***	0.80 (0.035)***	0.92 (0.033)***	0.91 (0.033)***	0.92 (0.033)***
Present	0.0082 (0.011)	0.016 (0.011)	0.0085 (0.011)	-0.011 (0.012)	-0.01 (0.011)	-0.014 (0.012)
CrossPeriod	-0.091 (0.024)***		-0.091 (0.024)***	-0.064 (0.022)**		-0.061 (0.023)**
CrossMonth		-0.0016 (0.013)	0.002 (0.013)		-0.039 (0.014)**	-0.026 (0.014)+
CommonDelay (in years)	0.85 (0.25)***	1.30 (0.22)***	0.85 (0.25)***	-0.43 (0.18)*	-0.26 (0.16)	-0.46 (0.18)*
InterrewardDelay (in years)	-6.21 (0.46)***	-6.91 (0.42)***	-6.23 (0.47)***	-4.05 (0.45)***	-4.09 (0.46)***	-3.76 (0.45)***
Late (vs. Early) Month	-0.001 (0.051)	0.00001 (0.05)	-0.0018 (0.05)	0.045 (0.047)	0.056 (0.047)	0.055 (0.046)

NOTE. Standard errors are in parentheses. +: $p < 0.1$, *: $p < 0.05$, **: $p < 0.01$, ***: $p < 0.001$

Discussion

This study demonstrates the robustness of the cross-period effect. We consistently replicate the cross-period effect on intertemporal choices, regardless of salient cues (timing of outcomes presented as durations vs. dates) and when controlling for cross-month effects and the time of the month the survey was conducted. Overall, these results suggest that the cross-period effect we have documented thus far reflects sensitivity to a relatively stable categorization of subjective financial periods and does not merely reflect the effect of calendar boundaries. We do find sensitivity to contextual cues: making calendar timing more salient does increase the overlap between subjective financial periods and calendar boundaries and makes choices directionally more sensitive to whether the options cross calendar boundaries. Nevertheless, we still find dissociable separate cross-month and cross-period effects. This is consistent with the notion that

[5] In a pooled analysis, using both conditions in the same regression and interacting all variables from Model 3 in Table 1-4 with duration vs. date conditions, we did not find significant differences between the conditions in the magnitude of either the cross-period effect ($p = .36$) or the cross-month effect ($p = .13$).

both stable construal of categories (e.g., subjective financial periods) and salient context-specific factors (e.g., end of the month) are jointly relevant to categorization-related reasoning (Isaac and Schindler 2014; Medin et al. 2003).

Thus far, we have tested the cross-period effect relative to participants' actual self-reported subjective financial periods. In the final two studies, we employ a hypothetical scenario that enables us to experimentally manipulate the length of the current period, and thereby test the causal effect of differences in subjective financial periods on intertemporal choices.

STUDY 5: THE CAUSAL EFFECT OF FINANCIAL PERIOD CATEGORIZATION

To test for a causal effect of subjective financial period categorization on intertemporal choice, we presented participants with a novel scenario and instructed them to assume different hypothetical budget periods (2 vs. 6 weeks remaining). Participants then made scenario-specific intertemporal choices. According to cross-period discounting, we would expect people to discount differently when making choices for which manipulating the period boundary changes whether the options are cross-period (i.e., the choice options are in different periods in one condition but not the other). However, we would not expect the manipulation to impact intertemporal preferences in those choices where the manipulation does not affect whether the options are in different periods. As a result, the manipulation would not necessarily make people more or less patient overall. For example, in our account, when making a choice between a smaller reward in one week and a larger reward in four weeks, participants who had been instructed that there are two weeks remaining in the current period would be less willing to wait (because the choice options are cross-period) than participants who were instead instructed that

there are six weeks remaining (because both choice options are in the same period). However, a choice between rewards today or in one week would not be affected by the manipulation.

Method

We analyzed 601 valid complete surveys from Prolific after the pre-registered exclusions (the same exclusion criteria as Study 1, as well as an additional stimuli-based attention check). Participants were randomly assigned to one of two between-subjects conditions: a two-week-remaining or six-week-remaining current budget period. Participants first read the following, accompanied by a visual aid (Figure 1-3)*: "Imagine that you are using a budget planner that has eight weeks per page. For convenience, you balance the books every eight weeks in accordance with the planner's organization. [Six weeks/Two weeks] have already passed since you started the current budget period. Hence, the current budget period will end exactly [two weeks/six weeks] from today, as depicted in the picture below."*

Participants then reported the number of weeks remaining in the current budget period as an attention check. We held constant the total budget period at eight weeks in both conditions to avoid a potential confound (i.e., a longer total budget period signaling a longer time horizon in general).

Figure 1-3. Hypothetical Budget Periods in Study 5

NOTE. Two-week current period condition (left) and six-week condition (right).

Choices. All participants answered 28 intertemporal choices, in a randomized order, each between $40 at a sooner time and $50 at a later time, with varying common delays and inter-reward delays (see Table 1-5 for the full set of choices). Fourteen of the choices served as *test choices* of the cross-period effect. Seven of these choices were designed so that the options crossed the two-week period boundary, but not the six-week period boundary ("cross-period in two-week condition"). Specifically, in each of these choices, the $40 option would be received at a specified time sooner than in two weeks, and the $50 option would be received at a specific later time, between two and six weeks from now. In a similar manner, another seven choices were instead designed so that the options crossed the six-week period boundary, but not the two-week period boundary ("cross-period in six-week condition").

The remaining 14 choices constituted *control choices*, in which the options did not cross either a two-week or six-week boundary. Specifically, in five of the choices both options were always in the current period (in less than two weeks; "current period in both conditions"), in another five choices both options were in the current period in the six-week condition but in the next period in the two-week condition (between the two weeks and six weeks from now; "next

period in two-week condition and current period in six-week condition"), and both options were always in the next period (more than six weeks from now) in the remaining four choices ("next period in both conditions").

Table 1-5. List of Choices in Study 5

	Test Choices			Control Choices	
	Smaller-sooner option (SS) time ($40)	**Larger-later option (LL) time ($50)**		**Smaller-sooner option (SS) time ($40)**	**Larger-later option (LL) time ($50)**
Cross-period in 2-week condition	today	in 2 weeks and 1 day	Current period in both conditions	today	in 1 week
	today	in 3 weeks		today	in 1 week and 3 days
	in 3 days	in 2 weeks and 4 days		in 3 days	in 1 week and 3 days
	in 3 days	in 3 weeks and 3 days		in 3 days	in 1 week and 6 days
	* in 1 week	in 2 weeks and 3 days		in 1 week	in 2 weeks
	* in 1 week	in 3 weeks and 1 day	Next period in 2-week / current period in 6-week condition	in 4 weeks	in 5 weeks
	in 1 week	in 4 weeks		in 4 weeks	in 5 weeks and 3 days
Cross-period in 6-week condition	* in 4 weeks	in 6 weeks and 1 day		in 4 weeks and 3 days	in 5 weeks and 3 days
	in 4 weeks	in 7 weeks		in 4 weeks and 3 days	in 5 weeks and 6 days
	in 4 weeks and 3 days	in 6 weeks and 4 days		in 4 weeks and 5 days	in 5 weeks and 5 days
	in 4 weeks and 3 days	in 7 weeks and 3 days	Next period in both conditions	in 6 weeks and 1 day	in 7 weeks and 1 day
	* in 4 weeks and 5 days	in 6 weeks and 1 day		in 6 weeks and 1 day	in 7 weeks and 4 days
	in 4 weeks and 5 days	in 6 weeks and 6 days		in 6 weeks and 1 day	in 8 weeks and 2 days
	in 4 weeks and 5 days	in 7 weeks and 5 days		in 6 weeks and 1 day	in 9 weeks and 1 day

NOTE. Total 28 choices. *: Choices used for the analyses of process measures

Process Measures. As in Study 3, we collected process measures for a targeted subset of the choices. We selected two choices that would be cross-period only in the two-week condition ($40 in 1 week vs. $50 in 2 weeks and 3 days, $40 in 1 week vs. $50 in 3 weeks and 1 day) and two that would be cross-period only in the six-week condition ($40 in 4 weeks vs. $50 in 6 weeks and 1 day, $40 in 4 weeks and 5 days vs. $50 in 6 weeks and 1 day).

For each of these four choices, participants answered four of the potential process measures from Study 3: two measures of perceived fungibility (impact of timing on spending and impact on managing finances), perceived duration of the interval between the options, and usefulness of money (excluding perceived resource slack, because we found no effect of period-crossing in Study 3). The measures were nearly identical to Study 3, except that we asked

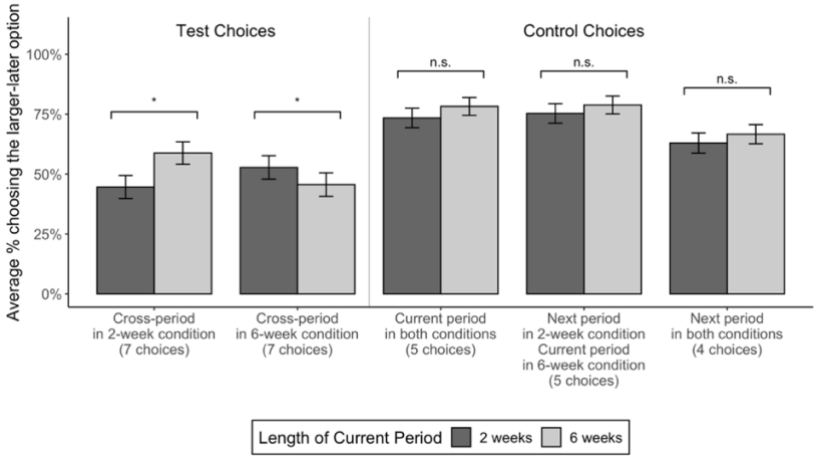
participants to think about receiving an extra $50 in the perceived fungibility and usefulness measures.

Results

Cross-Period Effect. We tested for the overall cross-period effect using regression analysis. We predicted participants' choices by whether the choice was cross-period in that participant's randomly assigned condition, controlling for the main effect of conditions, fixed effects for choices (to account for the different delays associated with each choice), and clustered standard errors at the participant level. We again found a significant cross-period effect ($B_{\text{CrossPeriod}}$ = -0.11, SE = 0.013, $t(16798)$ = -8.11, $p < .001$), suggesting that preference for the larger-later option in a choice was on average 11% lower in the condition in which that choice's options were in different (vs. the same) periods. There was no significant effect of condition ($B_{6 \text{ week (vs. 2 week)}}$ = 0.038, SE = 0.027, $t(16798)$ = 1.39, p = .16), suggesting that the manipulated remaining length of the current period did not make participants substantially more or less impatient overall, but affected choices only by changing whether the options were viewed as cross-period or not.

Specifically, as shown in Figure 1-4, people were less likely to choose the larger-later option in the two-week-remaining condition than in the six-week-remaining condition for choices that only crossed the two-week period ($M_{2 \text{ weeks}}$ = 0.45 vs. $M_{6 \text{ weeks}}$ = 0.59, $t(597.48)$ = -4.15, $p < .001$). The opposite pattern was observed for choice options that only crossed the six-week period, with greater patience in the two-week-remaining condition ($M_{2 \text{ weeks}}$ = 0.53 vs. $M_{6 \text{ weeks}}$ = 0.46, $t(598.82)$ = 2.037, p = .042).

Figure 1-4. Choice Proportions Based On the Length of Current Period (Study 5)



NOTE. *: $p < .05$ (t-test). Error bars show 95% confidence intervals.

By contrast, there was no significant difference between the conditions in any of the three sets of control choices (both options in the current period in both conditions: 0.73 vs. 0.78, $t(591.24) = -1.72$, $p = .085$; both options in the next period in the two-week condition but in the current period in the six-week condition: 0.75 vs. 0.79, $t(590.85) = -1.25$, $p = .21$; both options in the next period in both conditions: 0.63 vs. 0.67, $t(596.17) = -1.25$, $p = .21$). We found similar results in an exact replication study (N=532, Web Appendix D).

Mediation. The internal consistency between the two measures of perceived fungibility (perceived impact on spending and impact on managing finances) was high (Cronbach's alpha = .89). As pre-registered, we averaged these measures into a single index of perceived nonfungibility. In a regression predicting choices, the variance inflation factors (VIFs) of the composite non-fungibility variable (1.36), perceived duration (1.39), and usefulness (1.04) were close to 1, confirming that these variables have independent explanatory variance, as in Study 3 (bivariate correlations and factor analyses results are available in Web Appendix C).

We first confirmed the significant cross-period effect on the four focal choices that we used to test the potential mechanism ($B_{\text{CrossPeriod}}$ = -0.11, SE = 0.016, t(2398) = -6.98, p < .001). Further, we found that period-crossing also significantly affected perceived fungibility, such that consumers perceived a larger impact on their spending and finances when the choices spanned across different financial periods compared to when they were within the same period ($B_{\text{CrossPeriod}}$ = 0.51, SE = 0.072, t(2398) = 7.18, p < .001). A follow-up mediation analysis confirmed that perceived fungibility significantly mediated the cross-period effect on choice, explaining about 30% of the total effect (Table 1-6 (a)).

Similarly, participants also reported perceiving the duration between the options to be longer when they occurred in different budget periods ($B_{\text{CrossPeriod}}$ = 2.87, SE = 0.69, t(2398) = 4.14, p < .001). Perceived duration, in turn, also significantly mediated the cross-period effect on choice, explaining about 20% of the total effect. We did not find a significant cross-period effect on the usefulness of money ($B_{\text{CrossPeriod}}$ = -0.095, SE = 0.08, t(2398) = -1.18, p = .24) and found no significant indirect effect of usefulness of money.

To test the extent to which perceived fungibility explained the cross-period effect beyond what is accounted for by perceived duration, we conducted additional mediation analyses for each of the measures, controlling for the other measure (Table 1-6 (b)-(c)). Perceived fungibility had a significant indirect effect, controlling for perceived duration, and conversely, perceived duration also had a significant indirect effect, controlling for perceived fungibility. These results suggest that the cross-period effect is multiply determined, with cross-period differences in both perceived fungibility and perceived duration independently contributing to the effect.

Table 1-6. Mediation Analyses Results (Study 5)

		Non-fungibility (combined)	**Perceived duration**	**Usefulness**
(a) Without control	Indirect effect (% mediated)	-0.034 (30%)	-0.022 (19.3%)	-0.004 (3.1%)
	95% Bootstrap CI	[-0.048, -0.019]	[-0.038, -0.006]	[-0.011, 0.005]
(b) Controlling for perceived duration	Indirect effect (% mediated)	-0.019 (20%)		-0.002 (2.4%)
	95% Bootstrap CI	[-0.027, -0.009]		[-0.007, 0.003]
(c) Controlling for perceived non-fungibility	Indirect effect (% mediated)		-0.017 (21%)	-0.003 (3.6%)
	95% Bootstrap CI		[-0.029, -0.004]	[-0.009, 0.004]

Discussion

Study 5 presents a precise causal test of the proposed cross-period effect. Manipulating the relevant financial period, we find that intertemporal choices differ by condition only in the test trials in which the manipulation shifted the cross-period timing, but not in the control trials. In particular, prompting people to adopt a longer current period for the task did not make them more or less patient overall. This potentially contrasts with some predictions, that when people believe that the "present" ends sooner, they will be more likely to make future-oriented choices in general, such as saving (Hershfield and Maglio 2020).

This study also provides additional evidence for mental accounting as a causal mechanism underlying time discounting and contributing to time-inconsistent choices: Different current budget periods influenced perceived fungibility of money across the options and explained the cross-period effect, without necessarily affecting the perception of the usefulness of money at different times (c.f., Study 3), beyond what can be explained by differences in the perceived duration between the options.

STUDY 6: BEYOND THE CURRENT BUDGET PERIOD

Thus far, in the studies that measured (studies 2-4) and manipulated (study 5) the subjective current financial period, our primary analyses distinguished between the current period and a future period, with all outcomes not in the current period treated as if they occur in the *same* future financial period. We made this simplifying assumption because we expected the duration of the current financial budget period to be particularly salient and relevant for financial decision-making. However, people may budget for more than one period ahead, distinguishing not only between the current period and the subsequent period but also between a subsequent period and a period one or more after that. To the degree that people think about their finances in terms of multiple periods, boundaries between different future subjective financial periods could similarly reduce patience for choices in which the options are on opposite sides of the boundary. Our exploratory result in Study 2 offered initial evidence of a multi-period effect, as participants who considered the options to be in different future periods were more impatient than those who considered them to be in the same future period.

In Study 6, we extended the design of Study 5 to test the generalizability of the cross-period effect to different future financial periods. As a conservative test, we used the same type of manipulation of the boundary between current and subsequent financial periods as in Study 5 but included intertemporal choice questions such that options involved times beyond the first (current) and second budget periods. This allows us to test whether people spontaneously extrapolate and are sensitive to the implied boundaries between future financial periods (e.g., the boundary between the first and second future budget periods).

Method

We collected 419 valid complete surveys from MTurk. We pre-registered to use the same exclusion criteria as in Study 5, but decided not to use the stimuli-based attention check to avoid a potential selective attrition bias (Zhou and Fishbach 2016). However, the results were similar when excluding based on all the pre-registered attention checks (reported in Web Appendix C).[6] Participants were randomly assigned to one of two conditions, either two-weeks-remaining or six-weeks-remaining in the current budget period. They were presented with the same instructions as in Study 5, except that the budget planner had six weeks per page in total. They made 64 intertemporal choices in randomized order.

As in Study 5, we employed a mix of intertemporal choices that varied in whether the options crossed a budget period boundary (and for which period) in a given condition (see Web Appendix B for the full list). Some choice options only crossed a boundary in the two-week condition ("cross-period in two-week condition," 15 choices), others in the six-week condition only ("cross-period in six-week condition," 5 choices), and others did not cross a boundary in either condition ("same period in both conditions," 28 choices). Extending Study 5, we predict higher impatience in the conditions in which a set of choices are categorized as cross-period, compared to the other condition, in which the same set of choices are categorized as same-period (test choices). By contrast, we predict no effect of condition for the sets of choices for which both options were in the same period in both conditions (control choices). Additionally, we

[6] Our original pre-registration did not clearly specify the role of future budget period boundaries and only addressed the distinction between current and future budget periods. An analysis that only coded for the current and future period distinction would be confounded by different future budget periods. The analyses we report here account for the future budget periods, and we therefore consider them to be more correct.

included 16 choices that constitute a second type of control choice, different from those in Study 5, where the two options in each choice were in different periods consistently in both conditions ("cross-period in both conditions").

A subset of these choices allows us to evaluate participants' sensitivity to crossing specifically *future* financial boundaries. For three choices, the options crossed the boundary between the second and third periods in the two-week condition and did not cross any boundary in the six-week condition ("future-crossing in two-week condition and non-crossing in six-week condition"). Conversely, for six other choices, the options crossed a future-period boundary in the two-week condition (i.e., between the second and third periods) but crossed the current-period boundary in the six-week condition ("future-crossing in two-week condition and current-crossing in six-week condition").

Our general cross-period discounting framework predicts more impatience in the two-week condition for the three future-crossing vs. non-crossing choices (i.e., because a future boundary is crossed in the two-week condition but not in the six-week condition). By contrast, our account predicts no difference in patience between conditions for the six future-crossing vs. current-crossing choices. However, if people are only sensitive to crossing the current period boundary but are not sensitive to future period boundaries (e.g., as in an account of present bias that defines the current period as the present), we should observe the exact opposite effects. Specifically, we should see no differences in the three future-crossing vs. non-crossing choices because the choices do not differ in terms of crossing the *current* period boundary across the two-week and six-week conditions. By contrast, participants in the six-week condition should be more impatient in the six future-crossing vs. current-crossing choices because the choices are

cross-period relative to the current period boundary in the six-week condition, but not in the two-week condition.

Results

Using a similar regression framework as in Study 5, we replicated the cross-period effect based on crossing any (either current or future) period boundaries ($B_{\text{CrossPeriod}}$ = -0.099, SE = 0.015, $t(26750) = -6.77$, $p < .001$), representing a 10% lower preference for the larger-later option in cross-period choices on average. We found no overall effect of conditions ($B_{\text{6 week (vs. 2 week)}}$ = 0.021, SE = 0.027, $t(26750) = 0.77$, $p = .44$).

To test whether the cross-period effect extends to crossing future period boundaries, we repeated the regression analysis, separately defining one variable for crossing the current period boundary only (*CrossCurrentPeriod)* and another for crossing the boundary between any two future periods (*CrossFuturePeriod*). We find a significant effect of both types of cross-period effects ($B_{\text{CrossCurrentPeriod}}$ = -0.10, SE = 0.015, $t(26749) = -6.70$, $p < .001$; $B_{\text{CrossFuturePeriod}}$ = -0.087, SE = 0.023, $t(26749) = -3.73$, $p < .001$). Adding *CrossFuturePeriod* significantly improved the fit of the baseline model with only *CrossCurrentPeriod* ($\chi^2(1) = 24.26$, $p < .001$). These results suggest that the cross-period effect is not limited to crossing the current period but extends to boundaries between subsequent periods.

We also investigated the specific choices which provide a direct test of sensitivity to future financial period boundaries. For the three future-crossing vs. non-crossing choices, participants in the two-week condition, for whom the choices crossed a future period boundary, were significantly less likely to choose the larger-later option than those in the six-week

condition, for whom both options were in the second period (0.43 vs. 0.53, $t(416.62) = -2.40$, $p = .017$). This result is consistent with a general definition of cross-period discounting, in which people are sensitive to future financial period boundaries.

By contrast, there was no significant difference between the conditions in the six future-crossing vs. current-crossing choices (0.28 vs. 0.30, $t(416.81) = -0.44$, $p = .66$), consistent with similar sensitivity to both current and future boundaries in cross-period discounting. This pattern of results is the opposite of what would be predicted if people specifically valued outcomes in the current period more (i.e., if they were "present-biased" with regards to the entire *present period*) but were not sensitive to differences between subsequent periods.

Discussion

We replicated the causal current vs. future cross-period effect from Study 5 and extended the findings to a further cross-period effect across boundaries between future periods. The additional discounting over future periods cannot be explained by present bias (which assumes additional discounting only after the present period) or other existing accounts of non-stationary time discounting. Our findings suggest that cross-period impatience, as we have theorized, is not only relevant to correcting our understanding of "present" bias but can more broadly explain discontinuities in people's intertemporal preferences.

GENERAL DISCUSSION

Trading off benefits that occur at different times is a fundamental feature of many consumer financial decisions. By foregoing a smaller benefit that would occur sooner, consumers are often able to receive a larger benefit later. For example, choosing an advanced tax refund incurs fees or interest payments, reducing the total amount, as opposed to waiting to receive the full amount later. When consumers are time-inconsistent in these intertemporal choices, such that they make different choices about trading off a fixed delay depending on how far off the options are in the future, their preferences at the time of choice may not represent their general preferences, leading to short-sighted behavior and subsequent regret.

Time-inconsistent preferences have typically been attributed to present bias. In this interpretation, which has been widely used as a model of a more general self-control failure (Ainslie 1975; Hoch and Loewenstein 1991), people have an impulsive preference for present outcomes. Present bias has been widely proposed as a model of many decisions consequential for consumers' well-being, such as home financing, credit card debt, investment in education, and retirement savings. Some tests of the common delay effect have provided support for this view, by showing that people are more likely to choose a sooner outcome when it is in the present (e.g., as opposed to an equivalent trade-off between two options that are both in the future). However, the prior literature, including formal models, has left the timing of a “present” outcome undefined, typically assuming that outcomes involving even a brief delay (e.g., after a few hours, or the next day) are no longer favored as being in the present.

The Cross-Period Effect and Mental Accounting of Time

We find that people's intertemporal preferences are not well explained by prior theories involving impulsivity and present bias. We find no significant evidence for shifts in preference when adding moderate delays to both options (i.e., common delays) that would be predicted by present bias (e.g., higher discounting for outcomes delayed from the present). Instead, we find a reliable increase in patience only for the longer common delays (e.g., more than a month) that better correspond to differences in people's subjective financial planning periods (Studies 1-2).

We propose and find evidence for *cross-period impatience*, in which decision makers are more impatient specifically when choice options fall on different sides of the boundary between their own subjective financial periods (Studies 2-3), which is robust to time of month and presentation mode (duration vs. date, Study 4). Further, we find causal effects of shifting the boundary between financial periods, experimentally manipulating financial budgeting periods in a decision scenario (Studies 5-6). We find that the effect is partially mediated by consumer perceptions of cross-period options as less fungible with each other, even controlling for perceived time, which also contributes to the effect (Studies 3, 5).

Implications for Short-Sighted Consumer Decision-Making

Intertemporal Choice. Our findings, including that people have heightened impatience when choosing between options that span two future financial periods (Studies 2 and 6) and that people are also sensitive to timing within a financial period, contradict widely used models of present bias, such as the quasi-hyperbolic model (see Web Appendix F for a more detailed

discussion). The cross-period effect may also help account for other prior findings that contradict standard models, such as the lack of a common delay effect in some studies with short delays and even instances of *reverse* time inconsistency (greater impatience with a common delay; Read 2001), depending on the timing of people's subjective financial periods (see additional results from Study 5 in Web Appendix C for an example).

Our cross-period discounting framework may also be relevant to prior findings of heterogeneity across participants in their present bias. For example, differences across people in the common delay until a preference reversal (Kirby and Herrnstein 1995) may be explained by heterogeneity in the subjective current financial period. More generally, heterogeneity in discount rates may confound differences in patience with differences in financial periods, particularly when using a single item or a limited set of items that do not sufficiently vary in timing. Additional research would be needed to develop a fully detailed framework for predicting intertemporal preferences, including extending the findings to other kinds of choices (e.g., including losses), a more limited form of present bias (e.g., impatience for "as short as possible" delays, such as the end of the experiment vs. end of the day; Balakrishnan, Haushofer and Jakiela 2020; Imai, Rutter and Camerer 2021), and identifying whether people are differentially sensitive to different financial period boundaries (e.g., current vs. future period boundaries, or multiple boundaries).

Impulsivity and Self-control. Present bias has often been described as a failure of self-control, occurring due to a variety of factors, including greater temptation and emotional processing of immediate outcomes and undervaluing future outcomes. Our research suggests that, instead of consistently undervaluing future outcomes, consumers behave as if outcomes that

are in different periods are less fungible, resulting in a lower valuation when the future outcome is in another subjective period. This suggests the need for future research to move beyond the use of time-discounting as a metaphor for self-control and instead distinguish between these psychological constructs and their potentially distinct consequences for consumer decisions.

In particular, attempts to correct consumers' present-biased preferences have focused on reducing impulsivity and shortsightedness, and on putting the future on an "equal footing" psychologically with the present (e.g., via mental construal of outcomes, Zhao, Hoeffler and Zauberman 2007; salience of future preferences, Hershfield et al. 2011). Our findings suggest a different set of approaches, such as shifting how time is categorized, reducing the reliance on categorization in intertemporal decisions, or changing the salience of time-period boundaries.

Consumer Budgeting. Inconsistent time preferences may also be a consequence of an otherwise beneficial heuristic, with consumers consistently using subjective periods as mental accounts to simplify managing their finances. These subjective periods may be relatively stable goal-derived categories, rather than ad hoc categorizations that are constructed as needed. Consistent with this view, the cross-period effect is largely robust to contextual cues (e.g., salient month boundary in date formats, Study 4) and framing or salience manipulations (additional studies described in Web Appendix G).

Thus, our findings suggest a need to better understand how people mentally budget across time periods and the factors that determine people's subjective financial periods. Survey evidence from Zhang et al. (2022) shows that people vary in their budget period, and further finds a correlation between their budget period and pay frequencies, suggesting that financial periods may be determined in part by fixed timing aspects of the consumers' financial situation.

However, in a supplementary study (Web Appendix E), we find that subjective financial periods are largely stable over time for many people (two-week apart test-retest $r = .80$), consistent with many consumers reporting their subjective financial periods as the same length of time from the current day, despite time having passed. The possibility that the subjective current period may often be *rolling* (i.e., having approximately the same length regardless of the current date, as opposed to ending at a *fixed* point in time) is consistent with Lynch et al. (2010)'s finding that consumers' propensity to plan for a given time horizon remains largely consistent over time. Future research should investigate the causes of heterogeneity in the length and type of consumers' subjective financial periods.

Consumer Behavior. Consumers' mental accounting of time can have broad consequences for their financial behavior (De La Rosa and Tully 2022; Donnelly et al. 2022; Zhang 2017). Our findings have important implications for firms and policymakers facing trade-offs between consumers' impatience and other factors. Viewing consumers through the lens of present bias may create a mistaken belief that providing immediacy will be disproportionately valued by consumers. This may lead firms to over-value the benefit of providing financial resources (e.g., rebates, refunds, incentives) to consumers immediately, when consumers may in fact be relatively patient as long as the benefits are received sometime during their current financial period. Firms and policymakers may be able to leverage this, based on an understanding of the length of consumers' current period, by incorporating it into modeling and predicting consumers' valuations to schedule benefits late in the current period but payments early in the subsequent period. This may require testing the implications of our framework in a broader range of settings, including those involving losses as well as gains.

The current research has focused specifically on financial choices in the domain of gains, and future research could further explore whether the use of categorization of time in intertemporal choice extends beyond the financial domain. While the construct of present bias has been widely applied across financial and non-financial contexts, prior research has neither precisely defined the present nor considered the possibility of a domain-specific present period. It is possible that people use a different, domain-specific categorization scheme for other domains. In the context of consumer goods, expediting delivery can be nonlinearly costly, such that further reducing delivery times becomes disproportionately more expensive. While faster shipping may be a competitive advantage overall, it is notable that the "immediate gratification" business model (e.g., Kozmo.com, Bensinger 2012) has not proven viable. To the degree that our cross-period discounting framework extends to the timing of non-financial tangible goods, it would suggest that consumers may have a "current period" during which they are less sensitive to the precise timing of when goods are received. Firms might be better off providing "just-in-time" delivery (e.g., "Amazon Day Delivery" that includes a feature allowing customers to choose their delivery date), rather than expediting delivery across the board.

Our research provides a new perspective on intertemporal choices, based on the mental accounting of time, explaining choices that seem like present bias as instead due to cross-period differences in evaluations of delayed rewards. Our results suggest that people are particularly likely to make more impatient choices when one option is seen as in an earlier financial period than another. In effect, consumers are often quite willing to wait, as long as doing so doesn't relegate a desirable outcome to an entirely different financial period. One key to understanding and addressing short-sighted consumer behaviors may lie in identifying how consumers idiosyncratically partition time into financial periods.

CHAPTER 2.

PREFERENCE FOR EARLY PROGRESS SIGNALS AS A SOURCE OF IMPATIENCE IN INTERTEMPORAL CHOICE

ABSTRACT

Intertemporal choice is generally studied using the *time discounting* framework, which assumes that people's choices depend on their subjective valuation of future rewards (e.g., money) based on their timing (e.g., when one receives the money). Thus, in modeling people's choices, time discounting research has focused solely on the timing of the final rewards (thus often dubbed temporal *reward* discounting), assuming that the entire utility is realized upon the timing of the reward. Impatience in intertemporal choice is therefore interpreted as reflecting the devaluation of delayed rewards. However, delayed rewards are often accompanied or preceded by one or more events that are informative of the progress toward the rewards, which can provide additional psychological benefit by resolving uncertainties about the reward. Conflating the timing of these "progress signals" and the timing of the reward can confound the impatience from discounting the reward with the impatience for receiving progress signals that reduce uncertainty about the reward. The current research teases apart the impatience for the progress signals from the discounting of the final reward by experimentally separating the timing of the two. We propose improved models of intertemporal choice and demonstrate the difference when estimating discount factors.

Keywords: time discounting, intertemporal decisions, uncertainty, information

People face many decisions where they are required to make trade-offs between outcomes that occur at different times. Research on intertemporal choice has shown that people are often unwilling to wait, even for an objectively larger amount of benefit, more than what is normatively justified (Frederick et al. 2002; Urminsky and Zauberman 2015).

Intertemporal choice has been primarily modeled with the *time discounting* framework, which assumes that people engage in subjective discounting of the value of the future rewards based on the length of the delay, and then compare the discounted "net present value" of the choice options to make their decision. When a decision-maker opts for a smaller reward sooner (e.g., $100 today) over a larger reward later (e.g., $150 in one year), for example, time discounting assumes that the decision-maker's relative subjective valuation of the larger-later reward, based on its timing (i.e., one year), is lower than that of the smaller-sooner reward.

The underlying assumption in the time discounting framework is that utility is derived solely from the consumption of the reward, upon receiving the reward. However, rewards rarely occur in isolation. Upon receiving the reward, people may also experience other types of utilities, beyond that of consuming the reward.

In particular, rewards are often accompanied by or preceded by one or more actions and events that convey some information about the *progress* toward the reward. These may be actions taken by the recipient, such as investing an effort or completing a task, or by the issuer of the reward, such as processing the reward and providing information about its status to the recipient. In all cases, through these events, the recipients receive some affirmation about the likelihood that they will receive the reward at the promised time. For example, in a loyalty program, efforts result in earning reward points, before earning the material reward (e.g., a free drink), which may be in the form of a coupon that can only be used later, toward a future

purchase. To receive a tax refund, the person must first file the tax returns, the returns must be processed and approved by the tax authority, and often some intermediate currency, such as a check, is then received, all before the actual financial rewards are received.

These events, or what we refer to as *progress signals*, can be rewarding on their own. Online shoppers like to track the status of the delivery of their orders, even when doing so does not affect the delivery date. Customers in a rewards program accelerate their purchases to get another stamp on their reward card, even when they are not going to claim the reward (e.g., free coffee) immediately (Kivetz, Urminsky and Zheng 2006). Even though the delivery status of a product or a coupon does not offer any consumption utility, it may provide psychological benefits, such as a sense of progress, achievement, or reduced uncertainty.

In the current research, we adopt a broader perspective that the attainment of the reward in an intertemporal choice is a complex event that can be rewarding in multiple aspects, beyond what is assessed by consumption utility. In particular, we focus on the psychological benefits people may attain from receiving progress signals, such as due to uncertainty reduction. Prior research on time discounting has typically conflated these potential benefits from progress signals with the value of the reward, potentially resulting in misestimates of the degree of reward discounting. Evidence of impatience in intertemporal choice could instead be driven, at least partly, from the desire for these progress signals and their associated benefits, rather than entirely from impatience for the reward itself. We resolve this confound by experimentally separating the timing of the progress signals from the timing of the rewards.

THEORETICAL BACKGROUND

Intertemporal Choice and Time Discounting

Many decisions involve outcomes that occur at different points in time. Delayed rewards are often objectively more valuable (e.g., a larger sum of money due to interest earned from saving) than more immediate rewards (e.g., spending the money now), which poses a trade-off between the value of the reward and the timing of the reward's receipt. Research on intertemporal choice has suggested that people are generally *impatient*, often preferring the smaller-sooner reward in exchange for the larger-later reward more than normatively justified (for reviews, see Frederick, Loewenstein and O'Donoghue 2002; Urminsky and Zauberman 2015).

The prevalent framework for studying intertemporal choice has been *time discounting* since Samuelson (1937) proposed a model of intertemporal choice where an individual is considered to maximize the sum of all future utilities, which are appropriately "discounted" based on the timing on which they are realized. Time discounting assumes that the subjective value of a reward decreases the longer one has to wait for it. For example, a decision-maker would prefer to receive $100 now rather than in 6 months, because the subjective value of $100 in 6 months is less than that of the same amount now.

In time discounting, the value of an option that promises a reward of magnitude V at time t_P is modeled as the subjective value of the reward to be received from that option, $V * f(t_P)$, where *f* is a *discount factor* that quantifies the degree of devaluation by a given delay in time and is generally assumed to be a decreasing function of t_P. From this perspective, the degree of an

individual's impatience in intertemporal choice (i.e., willingness to forgo the larger reward in exchange for a smaller but sooner reward) reflects the individual's discounting of the *consequential* reward, or *reward discounting*—the rate at which they devalue delayed rewards, based on the amount of the delay to the final reward. Steeper discounting of delayed rewards (i.e., smaller discount factor) indicates delayed rewards lose subjective value faster, resulting in higher impatience. Thus, to understand individuals' impatience, time discounting research has sought to specify the reward discount factor, $f(t_P)$.

Time discounting has been widely adopted to study intertemporal choice across various contexts, not limited to human decisions but also non-human animal behaviors, by economics and psychologists alike. The standard normative economic model of time discounting, the exponential discounting model (Samuelson 1937), assumes that the value of future outcomes declines monotonically with the delay, based on a per-period fixed discount rate, representing the subjective interest rate. In this model, a single, constant discount factor is applied to the delayed payment, adjusted by the length of the delay: $f(t_P) = \delta_P^{t_P}$, where δ_P represents the degree of discounting per unit of delay.

Prior empirical research has documented numerous ways in which people's choices diverge from normative exponential discounting. For instance, people make more patient choices when the same two-option tradeoff is further in the future (e.g., the "common delay" effect, Coller and Williams 1999; Green, Fristoe and Myerson 1994; Keren and Roelofsma 1995; Loewenstein and Prelec 1992). Such inconsistent choices over time have been considered as arising from people being "short-sighted" or "impulsive" (Ainslie 1975; Hoch and Loewenstein 1991; but see Jang and Urminsky 2023 for a contrary view).

Hyperbolic discounting (Ainslie 1975; Mazur 1987) proposes a different functional form of discounting—a hyperbolic function (e.g., $f(t_P) = 1/(1 + kt_P)$), which can accommodate time-inconsistent choices. Beyond accommodating deviations from the standard economic model, hyperbolic discounting also offers an interpretation for non-normatively short-sighted choices in terms of reward discounting. Ainslie (1975) specifically characterized the phenomenon of impulsiveness as the reward decreasingly effective with delay: Self-control failures and impulsive choices are due to overvaluing immediate rewards. Under this view, impulsivity and generally shortsighted behaviors are indexed by steeper discounting of delayed rewards.

Some researchers have proposed that discounting of the rewards is represented at the neural level, predicting a decrease in the activation of the reward-processing regions of the brain when the rewards are delayed (Kable and Glimcher 2007; Scheres, De Water and Mies 2013).

Time preferences are commonly measured by estimating how much money an individual requires for a delay in receiving that amount (Frederick et al. 2002). This approach assumes that intertemporal decisions about monetary rewards capture people's discounting of rewards, based on the utility they can potentially gain from consuming the money. This assumption is consistent with the psychological assumption that monetary rewards are motivating due to their association with primary rewards, as they enable the acquisition of primary rewards (i.e., consumption).

Indeed, empirical findings suggest that discount rates measured from intertemporal choices over financial rewards cannot be explained by economic calculation of the opportunity cost of delayed financial gains (e.g., an arbitrage opportunity or interest rate). If time preferences merely reflected people's consideration of investment opportunities, discount rates above the lowest interest rate available to the decision-maker should not be observed (Chabris, Laibson and

Schuldt 2016). Although there is evidence that people in immediate need of money tend to exhibit higher discounting of future rewards (Noor 2009), people generally exhibit impatience that is substantially higher than could be explained by available market interest rates. While this is especially true for small amounts (Chapman and Elstein 1995; Loewenstein and Thaler 1989; Thaler 1981), people exhibit high discount rates even for high-stakes real choices. For example, preferences for lump-sum payments over high-yield annuities among retiring military members suggest higher-than-market-rate discount rates (Warner and Pleeter 2001).

Testing reward discounting models with choices between time-specific monetary amounts assumes that the additional utility, relative to what would have been experienced without a reward, is realized at the time the reward is received (t_p), under the assumption that the utility is derived from consuming the reward as soon as it is received ("consume-on-receipt"; Cohen et al. 2020; Ericson and Laibson 2019).

However, studies that measured when participants consume the reward suggest that impatience in choices can be inconsistent with impatience in consumption. Reuben et al. (2015) found that among the presumably impatient participants who chose to receive a smaller amount immediately over a larger amount two weeks later, the majority of the participants had still not cashed the check one week later. Nearly half of the impatiently-choosing participants (53.5%) had still not cashed the check two weeks later, which was after the timing of the larger-amount option they had previously rejected.

People do not necessarily consume an earned reward immediately. Consumers in loyalty programs often pass up opportunities to redeem their points even when there is no incentive to stockpile them (Stourm, Bradlow and Fader 2015). People also procrastinate in using resources and consuming experiences (Shu and Gneezy 2010), including items like gift cards that are

steeply discounted when people make prospective trade-offs, particularly when they have low connectedness to their future selves (Bartels and Urminsky 2011).

The case of impatience in intertemporal choice even when consumption does not necessarily occur upon receiving the reward suggests that there are other sources of impatience, beyond the discounted subjective value of the reward, that are not captured in the time discounting framework.

Impatience for Progress Signals and Uncertainty Resolution (vs. Consequential Reward)

The limitation of the reward-focused discounting approach in accommodating other motives influencing intertemporal choices suggests the need for an alternative, broader perspective on intertemporal decisions. In particular, the reward-focused time discounting framework takes a *consequentialist* approach, assuming people make choices in a way that maximizes the value of the final rewards, inheriting the assumption from expected utility theory. Rather than simply relying on the value of the consequential reward, however, people may construe the attainment of a delayed reward as a psychologically complex event, involving different sources of utility that may occur at different times than the timing of the reward, varying by the specific reward circumstances. Impatience in intertemporal choices can reflect not just discounting of the consequential reward, but time preferences for these other utilities that may arise upon attaining the reward.

In particular, we propose that people derive utility from receiving *progress signals*, which we broadly define as *any actions or events that are informative of the progress made toward the rewards in the context of the intertemporal choice.* Progress signals may be simply received only

upon receiving the reward (as receiving the reward is identical to making full progress toward the reward), or in any intermediate stages leading up to the reward, even over multiple stages, with each stage conveying a different type of progress information. For example, before receiving compensation for completing a job, a worker may receive a notification that their job has been approved for payment, soon after they finish the job but long before receiving the compensation. Separately, the worker may also receive a gift card in the mail, which can be used to redeem the reward, further affirming the attainability of the reward. Even though these progress signals do not affect the outcome, such as the size or timing of the final reward (e.g., monetary payment), if people find the progress signals rewarding by themselves, it could contribute to their time preferences, depending on when progress signals occur.

Some evidence for the utility of progress signals comes from research on goal pursuit. The level of progress toward the reward affects motivation in goal pursuit (i.e., the *goal-gradient* hypothesis; Hull, 1932; Kivetz et al., 2006). Receiving information on how fast they are making progress toward their goals (Huang and Zhang 2011), or inferences from subgoal completion or other indicators of goal progress (Fishbach, Koo and Finkelstein 2014; Harkin et al. 2016; Rai et al. 2023) can promote goal-pursuit behaviors as well. People may desire progress information because goal completion is uncertain, and progress information helps to resolve uncertainty about the attainability of the goal.

People may seek progress information in the context of an intertemporal choice, even if it does not inform their actions or affect the attainability of the reward, because delayed rewards are inherently uncertain as well. In fact, Mischel and Grusec (1967) described intertemporal choice (for both monetary rewards and products) in terms of expectancy. Some time discounting research has proposed uncertainty as an explanation for why delayed rewards are discounted

more than immediate rewards. Uncertainty about the reward can be seen as inherently increasing with delay (Andreoni and Sprenger 2012b; Frederick et al. 2002; Halevy 2008), contributing to more "impatient" choices (Epper, Fehr-Duda and Bruhin 2011). For instance, people may see delayed rewards, even those presented as guaranteed to occur, as involving more risk of non-payment (Chabris et al. 2016). This perspective suggests that uncertainty is the explanation for reward discounting. That is, the subjective value of delayed rewards is reduced because more delayed rewards are more uncertain. On the other hand, other findings suggest that time discounting and discounting by probability and uncertainty are two different processes (Green and Myerson 2004), suggesting time discounting does not simply reflect reduced subjective value due to perceived delayed rewards as less probable. Further, experimental studies that have attempted to control for the risk of non-payment found a robust effect of delay discounting (Andreoni and Sprenger 2012a). Importantly, the uncertainty-discounting account assumes uncertainty affects intertemporal choice via discounting of the subjective value of the rewards and does not consider the possibility that people have time preference over uncertainty resolution, which can be rewarding in and of itself.

Progress signals can provide additional psychological benefits by alleviating uncertainties associated with the rewards. People generally seek to resolve uncertainty. Feelings of uncertainty about the future have been linked to negative affect, such as anxiety and worry (MacLeod, Williams and Bekerian 1991). People also avoid risk or uncertainty, at times with little normative justification (Kahneman and Tversky 1979), even valuing uncertain prospects less than the worst possible realization of the outcome (Gneezy, List and Wu 2006).

Beyond eliminating the inherently uncomfortable state, the resolution of uncertainty itself may serve as a reward by satisfying curiosity or information needs (Loewenstein 1994).

Uncertainty reduction can also be intrinsically rewarding via association with the consequential reward, even when such knowledge is not instrumental (Blain and Sharot 2021). As a result, people seek to resolve uncertainty even at the cost of unpleasant experiences (Hsee and Ruan 2016) or monetary fees (Eliaz and Schotter 2007). Neural evidence suggests that preference for information is encoded in the same neural systems as reward for both humans and non-human animals, further suggesting that information is rewarding independently of extrinsic, tangible rewards such as money or goods (Kang et al. 2009).

Early work has looked at people's general preference over the timing of uncertainty resolution (Ahlbrecht and Weber 1996; Chew and Ho 1994), along with offering theoretical frameworks (Kreps and Porteus 1978; Wu 1999). Even if the information is non-instrumental, people prefer uncertainty resolution sooner (e.g., resolving a lottery, holding constant the payoff timing, though people may prefer to postpone information in case of negative information; Ganguly and Tasoff 2017). Some of these theories predict that preference for information is proportionate to the size of the reward (Ganguly and Tasoff 2017) or the probability (Abdellaoui et al. 2022; Chew and Ho 1994). These investigations were primarily interested in the timing of information (e.g., lottery drawing). Therefore, they generally held fixed the timing of the reward (e.g., when the payment is to be made). It is unclear whether preferences for sooner uncertainty resolution play a role in intertemporal "reward" choices where people trade off the timing of the consequential reward.

Because time preferences for uncertainty resolution and rewards have been generally studied separately, fundamental theoretical questions remain unresolved that can be answered by testing whether there is a potentially separable effect of the delay in progress signals from the effect of delay in the consequential rewards. Conventional time-discounting characterization of

intertemporal choice does not accommodate a separable utility from progress signals. Therefore, studies designed to measure reward discounting did not control for when people receive progress signals. As the receipt of the reward itself can also serve as a progress signal, tests that only account for the timing of the reward cannot identify whether impatience in intertemporal choice can be partly attributed to the desire for progress signals, separately from the valuation of the final reward. Preference for the smaller-sooner monetary reward in an intertemporal choice can be entirely or partly due to the sooner reward also providing the benefits from resolving the various uncertainties associated with the reward earlier. If such is the case, we expect higher patience when the larger-later reward can provide a progress signal in advance of the reward timing.

The Proposed Model

We propose a comprehensive account of impatient choices, which can capture the motivation to pursue options that offer sooner progress signals, thereby sooner uncertainty reduction, separately from discounting of the consequential reward. We do this by expanding standard models of monetary time discounting. This approach allows us to test either sensitivity only to the timing of the progress signal or only to the timing of receiving rewards as special cases.

We begin with the standard exponential discounting model (Samuelson 1937), which assumes a single, constant discount factor applying to the delayed payment, adjusted by the length of the delay: $f(t_P) = \delta_P^{t_P}$, where δ_P represents the degree of discounting per unit of delay. We can similarly define impatience for progress, represented as discounting by delays until

receiving progress signal: $f(t_G) = \delta_G^{t_G}$, where t_G is the timing of the progress signal and δ_G represents the degree of devaluation due to delay in the progress signal per unit of delay.

In sum, our extended discounting model can be written as a multiplicative function of discounting due to a delay in the progress signal and delay in payment:

$$f(t_G, t_P) = \delta_G^{t_G} \delta_P^{t_P}$$

This model assumes separate time-consistent preferences for progress signals and payments.

One common finding in the time discounting literature is that people have time-inconsistent preferences (Thaler 1981), commonly observed as higher discounting in the short term than in the long term. To explore whether people have time-inconsistent preferences in the dimension of progress signals or payments, we extend the widely used and more general quasi-hyperbolic discounting model (Laibson 1997) in the same approach as our main model:

$$f(t_G, t_P) = \beta_G^{GD} \delta_G^{t_G} \beta_P^{PD} \delta_P^{t_P}$$

In this model, GD = 0 if t_G = 0 (i.e., in the "present" period), and GD = 1 if t_G > 0, while PD = 0 if t_P= 0, and PD = 1 if t_P > 0. In this model, β_G^{GD} and β_P^{PD} each represents an additional preference for the present-period progress signal and payment (often called present bias), respectively.

The Current Research

We test our account across four studies. To disassociate uncertainty reduction from the reward, we vary the timing of progress signals separately from payment timing. While the uncertainty about the reward may not be fully resolved until the reward is received, we assume

people gain psychological benefits from receiving information that reduces a particular source of uncertainty about receiving the reward.

First, in an incentive-compatible experiment, we demonstrate that people have a preference for a sooner progress signal, operationalized as having their effort reviewed and approved on an online worker platform (Study 1). In an intertemporal choice involving options with different approval dates, people generally prefer an earlier approval-date option, holding constant the date when the compensation is paid out (i.e., payment made).

We then test whether it generalizes to different types of progress signals, each reducing different types of uncertainty to different degrees. First, we use the context of lotteries, where the attainability of the reward remains uncertain until the lottery drawing resolves the uncertainty. We find that preference for the lottery option with a smaller payoff is stronger not only when it offers an earlier payment date (consistent with time discounting), but also when it offers an earlier drawing date (i.e., lottery tickets; Study 2). We further replicate our finding in another context, when a reward will be delivered as a pre-paid debit card. The receipt of a physical card in the mail serves as a progress signal, while the actual payment may be received later when the funds on the card become available (Study 3).

Further, we elicit people's valuation of the rewards jointly based on the progress signal delay and payment delay. Using the elicited valuations, we estimate the degree of impatience for progress signal separately from the degree of preference for the sooner reward (a standard payment-based discount factor) using our extended discounting model (Study 4). We find that people discount based on both delays to the payment and progress signal.

Full data, study materials, and code for analyses are available on the OSF repository: https://osf.io/pnzsw/.

STUDY 1: PREFERENCE FOR EARLY TASK APPROVAL IN INTERTEMPORAL CHOICE

We first tested whether people exhibit a preference for early progress signals, in the context of receiving a monetary reward for working on a short task. We recruited workers on Amazon Mechanical Turk (MTurk) as participants. MTurk workers regularly engage in short tasks or ad hoc jobs for payments that are contingent on their successful performance on the tasks. We operationalized progress signal as getting completion of the task reviewed and approved, which would resolve the uncertainty about whether they qualified for the payment as well as how much payment they qualified for. We asked participants to make consequential choices between doing a task with an early approval date (i.e., the date on which their performance on the task will be reviewed and payment approved) or a task with a later approval date. Importantly, the timing of receiving compensation for the task varied independently of the approval date.

Method

We recruited 170 U.S. participants from Amazon Mechanical Turk, based on a pre-registered (https://aspredicted.org/X3X_9YK) target sample size of 150 participants, after exclusions. We received 168 completed responses from unique IP addresses. We excluded three participants who failed our instructional attention check and analyzed a final sample of 165 participants (*M*age = 42.88, SD = 12.95; 54% women). This study was sufficiently powered to detect a 15% deviation from 50% in binary choice.

Participants made a series of five consequential choices. In each choice, they chose between two tasks with three components: the range of bonus payment amount (as compensation for completing the task), the "approval date" (when the task would be reviewed and the participants would be notified of their bonus amount, i.e., progress signal timing), and the "payment date" on which the bonus would be issued (see Table 2-1). Both tasks in each choice were described as promising at least a $0.20 bonus payment and as involving short data-entry and data-checking tasks. We informed participants that one of their five task choices would be randomly chosen and they would be doing that task.

In the first choice, the two tasks had exactly the same bonus amounts and payment dates ("same monetary amount and payment date"), but different approval dates (choice #1, Table 2-1). One task was to be reviewed on the day of the study and the other was to be reviewed eight days after the survey date (i.e., March 22). This first choice thus tested whether people prefer to do a task that provides a sooner progress signal, all else equal.

In the subsequent four choices, we presented participants with options involving different trade-offs, in randomized order. In the "monetary difference" choices (choices #2 and #3), the task options had different bonus amounts (either 90 cents or one dollar). In one choice (#2), only the bonus amounts differed, and both the approval date (8 days later) and payment date (11 days later) were the same in both task options. In the other choice (#3), the task with the smaller maximum bonus had an earlier approval date (i.e., reviewed on the same day, as opposed to 8 days later) than the other option. This pair of choices allows us to test whether offering a sooner progress signal can increase the willingness to accept a smaller expected bonus amount because it reduces the uncertainty about the compensation sooner.

Similarly, in the "payment date difference" choices (choices #4 and #5), the options had the same maximum bonus amount ($1) but different payment dates (in 11 days vs. in 9 days). In one choice (#4), both options had the same approval date (in 8 days). In the other choice ($5), the option with a later payment date (in 11 days, as opposed to in 9 days) offered an earlier approval date (same day, as opposed to in 8 days). This pair of choices allows us to test whether offering sooner progress signal can increase willingness to accept later compensation.

After making these five choices, participants completed short demographic questions (gender and age). Then, as per our instruction, we randomly picked one task from each participant's five selected tasks and informed them of the result. They then completed a short spelling check task for the chosen compensation.

Table 2-1. Task Choices and Results (Study 1)

	Same monetary amount and payment date	
	Task 1	Task 2
Bonus	Up to $1.00	Up to $1.00
Approval date	Same day	In 8 days
Payment date	In 11 days	In 11 days
% choosing Task 1	90% (148/165, $\chi^2(1) = 102.42$, $p < .001$)	

	Monetary difference				**Payment date difference**			
	2. Same Approval Date		3. Different Approval Date		4. Same Approval Date		5. Different Approval Date	
	Task 1	Task 2	Task 1	Task 2	Task 1	Task 2	Task 1	Task 2
Bonus	Up to $0.90	Up to $1.00	Up to $0.90	Up to $1.00	Up to $1.00	Up to $1.00	Up to $1.00	Up to $1.00
Approval date	In 8 days	In 8 days	Same day	In 8 days	In 8 days	In 8 days	Same day	In 8 days
Payment date	In 11 days	In 11 days	In 11 days	In 11 days	In 11 days	In 9 days	In 11 days	In 9 days
% Task 1	13% (21/165)		33% (55/165)		15% (24/165)		52% (86/165)	
McNemar's test	$\chi^2(1) = 28.66$, $p < .001$				$\chi^2(1) = 53.16$, $p < .001$			

NOTE. Approval and payment dates were described to the participants both as calendar dates (e.g., March 22) and as days from the day of the survey (e.g., in 8 days).

Results

In the first choice ("same monetary amount and payment date"), the majority of the participants (90%, 148/165, compared to 50%, $\chi^2(1) = 102.42$, $p < .001$) chose the task with an earlier approval date, exhibiting a strong preference for the option with a sooner progress signal, even though there was no difference in when they would actually receive the payment from completing the task.

Next, we examined the two "monetary difference" choices to test whether participants preferred early approval date even when the task options differed in the amount of the maximum bonus amount. When the options only differed in the maximum bonus amount (choice #2), the majority of the participants chose the task with a higher maximum bonus, with only 13% (21/165) choosing the task with a smaller maximum bonus. However, the task with the smaller maximum bonus payment was chosen significantly more when the task had an earlier approval date (choice #3; 33%, 55/165, vs. 13%, $\chi^2(1) = 28.66$, $p < .001$), despite the payment timing remaining the same. This result suggests that people are willing to sacrifice some anticipated compensation to receive a progress signal sooner.

Lastly, we examined the two "payment date difference" choices and found similar results when the choices presented a trade-off between the payment date and the approval date. When the approval date was the same in both options (choice #4), only 15% (24/165) chose the task with a later payment. However, the preference for the later-payment option significantly increased when it offered an earlier approval date (choice #5: 52%, 86/165, vs. 15%; $\chi^2(1) = 53.16$, $p < .001$).

Discussion

Time discounting assumes that people make intertemporal choice based on how much they devalue rewards that occur in the future. The reward that will be received farther in the future is preferred less than a reward equivalent in objective value but closer in time. Thus, time discounting predicts that our participants would have based their decisions solely on the amount and timing of the payments.

However, we found that people preferred an option that offers a progress signal earlier, even when the timings of the payments were constant across the options. Some participants were even willing to accept costs (by going with a smaller expected bonus) or a delay to the payment for early approval of the task. This finding cannot be accommodated solely by the time discounting account. Instead, the participants' preference for a sooner progress signal suggests that people preferr to reduce some uncertainty about the reward sooner rather than later, with some people even willing to accept smaller payment or receive the payment later in time.

When a reward is contingent on investing effort to finish a task as in this study, in general, progress signals may involve other benefits, such as a sense of accomplishment, self-enhancement, or anticipating no more future effort. To focus on the role of uncertainty reduction, we next examine intertemporal choice contexts that do not involve effort provision.

STUDY 2: LOTTERY DRAWING AS A PROGRESS SIGNAL

In Study 2, we focus on the uncertainty-reduction aspect of progress signals by using a different context—lotteries. Lotteries inherently entail uncertainty about the reward—a person

receives a reward only if and after they are selected as the winner. This uncertainty can be resolved upon the drawing of the lottery. Based on this idea, we manipulated the progress signal timing as the drawing date of the lotteries (i.e., the date on which the winner will be drawn and announced), independently of the payment date (i.e., when the lottery prize will be paid to the winner). We tested people's choices between a lottery with a smaller reward but a sooner drawing or payment date and a lottery with a larger amount but a later drawing or payment date.

Method

We recruited 218 U.S. participants from MTurk, aiming for a target sample size of 200. We received 208 complete responses from unique IP addresses. We excluded 7 participants who failed our instructional attention check and analyzed a final sample of 201 valid responses (M_{age} = 42.14, SD = 13.10; 57% women). This study was pre-registered (https://aspredicted.org/HZS_8YD).

Each participant made twenty-one choices (in randomized order) between a smaller prize lottery ($30) and a larger prize lottery ($40), with different lottery drawing dates (i.e., as progress signal timing) and payment dates. Across the choices, the delays between the options ranged between one day and four weeks, for both drawing and payment (see Table 2-2). Small and large payment delays were matched with different lengths of progress signal delays to construct specific tests. For example, in three choices where the payment delays differed by one day between the options, one choice involved a one-day difference in progress signal delay and the other two a two-week difference (choices #1-3 in Table 2-2). Likewise, in four choices where the payment delays differed by two weeks, the difference in progress signal delays varied between

one day, one week, and two weeks (choices #14-17). Similarly, differences in payment delays were also paired with a variety of differences in progress signal delays. This design allows for identifying sensitivity to the timing of uncertainty reduction from progress signal and consequential reward separately.

We also varied whether the soonest delay in the stimuli was tomorrow or in one week, between-subjects, to explore the effect of a "common delay." The delays in the one-week condition were created by approximately shifting the timings in the tomorrow condition by one week into the future (i.e., by adding a one-week common delay). The test of whether preferences are consistent when a common delay is added tests a deviation from the standard exponential discounting (Prelec and Loewenstein 1991; Thaler 1981). Specifically, the exponential discounting model predicts that people's time preferences only depend on the delay between the rewards (i.e., the additional waiting time required to receive the larger-later reward in exchange for the smaller-sooner reward), and not when the delay begins (e.g., the timing of the earliest reward). Therefore, under the normative model, adding a common delay should not affect preferences. On the other hand, some accounts have suggested that people have higher impatience for rewards that are closer in time (e.g., hyperbolic discounting or quasi-hyperbolic discounting models). Prior research has found the common delay effect consistent with diminishing impatience (i.e., more patience when options are delayed to the future; Coller and Williams 1999; Green, Fristoe and Myerson 1994; Keren and Roelofsma 1995; Kirby and Herrnstein 1995).

Table 2-2. Timings in Studies 2 and 3

	"Tomorrow" condition				"One-week" condition			
	Smaller option (Study 2: $30, Study 3: $35)		**Larger option** ($40 in both Studies 2-3)		**Smaller option** (Study 2: $30, Study 3: $35)		**Larger option** ($40 in both Studies 2-3)	
	Progress Signal	Reward	Progress Signal	Reward	Progress Signal	Reward	Progress Signal	Reward
1	tomorrow	tomorrow	in 2 days	in 2 days	in 1 week	in 1 week	in 1 week and 1 day	in 1 week and 1 day
2	tomorrow	in 2 weeks	in 2 weeks	in 2 weeks and 1 day	in 1 week	in 3 weeks	in 3 weeks	in 3 weeks and 1 day
3	tomorrow	in 3 weeks	in 2 weeks	in 3 weeks and 1 day	in 1 week	in 4 weeks	in 3 weeks	in 4 weeks and 1 day
4	tomorrow	tomorrow	in 1 week	in 1 week	in 1 week	in 1 week	in 2 weeks	in 2 weeks
5	tomorrow	in 3 weeks	in 1 week	in 4 weeks	in 1 week	in 4 weeks	in 2 weeks	in 5 weeks
6	tomorrow	in 3 weeks	in 2 weeks	in 4 weeks	in 1 week	in 4 weeks	in 3 weeks	in 5 weeks
7	tomorrow	in 3 weeks	in 3 weeks	in 4 weeks	in 1 week	in 4 weeks	in 4 weeks	in 5 weeks
8	tomorrow	in 3 weeks	in 4 weeks	in 4 weeks	in 1 week	in 4 weeks	in 5 weeks	in 5 weeks
9	tomorrow	in 4 weeks	in 1 week	in 5 weeks	in 1 week	in 5 weeks	in 2 weeks	in 6 weeks
10	tomorrow	in 4 weeks	in 2 weeks	in 5 weeks	in 1 week	in 5 weeks	in 3 weeks	in 6 weeks
11	tomorrow	in 4 weeks	in 3 weeks	in 5 weeks	in 1 week	in 5 weeks	in 4 weeks	in 6 weeks
12	tomorrow	in 4 weeks	in 4 weeks	in 5 weeks	in 1 week	in 5 weeks	in 5 weeks	in 6 weeks
13	tomorrow	tomorrow	in 1 week	in 2 weeks	in 1 week	in 1 week	in 2 weeks	in 3 weeks
14	tomorrow	tomorrow	in 2 days	in 2 weeks and 1 day	in 1 week	in 1 week	in 1 week and 1 day	in 3 weeks
15	tomorrow	in 2 weeks	in 2 weeks	in 4 weeks	in 1 week	in 3 weeks	in 3 weeks	in 5 weeks
16	tomorrow	in 3 weeks	in 2 weeks	in 5 weeks	in 1 week	in 4 weeks	in 3 weeks	in 6 weeks
17	tomorrow	in 1 week	in 2 weeks and 1 day	in 3 weeks and 1 day	in 1 week	in 2 weeks	in 3 weeks and 1 day	in 4 weeks and 1 day
18	tomorrow	tomorrow	in 1 week	in 3 weeks	in 1 week	in 1 week	in 2 weeks	in 4 weeks
19	tomorrow	in 3 weeks	in 3 weeks	in 6 weeks	in 1 week	in 4 weeks	in 4 weeks	in 7 weeks
20	tomorrow	in 4 weeks	in 3 weeks	in 7 weeks	in 1 week	in 5 weeks	in 4 weeks	in 8 weeks
21	tomorrow	tomorrow	in 1 week	in 4 weeks	in 1 week	in 1 week	in 2 weeks	in 5 weeks

Results

We first fitted participants' choices (1: choosing the larger prize lottery, 0: choosing the smaller prize lottery) on the difference in lottery drawing dates between the options and the difference in payment dates, controlling for the common delay conditions (e.g., drawing for the smaller lottery is tomorrow vs. adding one week to all the timing) using a linear probability regression model.

We found a significant effect of payment delays ($B_{Payment}$ = -0.11, SE = 0.0091, $t(4217)$ = -12.25, $p < .001$). Consistent with time discounting, participants were less willing to wait for the larger prize lottery the longer the payment delay from the smaller prize lottery.

Importantly, we also found a significant effect of delay in progress signal (B_{Signal} = -0.036, SE = 0.0063, $t(4217) = -5.73$, $p < .001$), albeit significantly smaller than the effect of payment delays ($B_{Payment} - B_{Signal} = -0.076$, $p < .001$). Participants were less willing to wait for the larger prize lottery, the longer the delay in learning whether they had won, relative to the smaller prize lottery. Given that all choices in the study involved payment delays, this shows that participants were still sensitive to the timing of the progress signal, even when there was a meaningful difference in the timing of actually receiving the monetary reward.

In addition, there was a significant effect of the common delay conditions ($B_{\text{One-week (vs. tomorrow)}} = 0.13$, SE = 0.051, $t(4217) = 2.59$, $p = .010$). Overall, participants in the one-week condition were more patient than in the tomorrow condition, choosing the larger-later option more in general, consistent with prior work on time-inconsistent preferences.

In an exploratory analysis, we tested the interaction between progress signal delays and payment delays, controlling for the common delay conditions. The interaction was not significant ($B_{\text{Payment Delay X Signal Delay}} = 0.0006$, SE = 0.005, $t(4216) = 1.18$, $p = .24$), suggesting the sensitivity to delays in either dimension, progress signal or payment, was not moderated by extent of delay in the other dimension.

In another exploratory analysis, we tested the interactions between the common delay conditions and each of the progress signal delays and payment delays. The common delay did not significantly moderate the effect of either type of delay ($B_{\text{Payment Delay X One-week (vs. tomorrow)}} = 0.031$, SE = 0.018, $t(4215) = 1.75$, $p = .08$; $B_{\text{Signal Delay X One-week (vs. tomorrow)}} = -0.005$, SE = 0.013, $t(4215) = -0.37$, $p = .71$). In separate analyses for each of the common delay conditions, we found consistently significant effects of payment delays and progress signal delays (soonest date is tomorrow: $B_{\text{Payment Delay}} = -0.13$, SE = 0.013, $t(2160) = -9.46$, $p < .001$; $B_{\text{Signal Delay}} = -0.034$, SE

= 0.0092, $t(2160) = -3.67$, $p < .001$; soonest date is one week: $B_{\text{Payment Delay}} = -0.096$, SE = 0.012, $t(2055) = -7.88$, $p < .001$; $B_{\text{Signal Delay}} = -0.039$, SE = 0.0087, $t(2055) = -4.42$, $p < .001$).

Discussion

We found that our participants were impatient for the progress signal delays when making intertemporal choices. Specifically, they preferred the smaller-sooner option in terms of the payment timing more when it offered an earlier lottery drawing date.

We also did not find a significant interaction between progress signal delays and payment delays. That is, the sensitivity to progress signal delays did not significantly differ whether the payment delays were relatively small (e.g., one day) or large (e.g., four weeks). The effect of progress signal delays was generally smaller than the effect of payment delays, suggesting people avoid payment delays more than comparable progress signal delays.

STUDY 3: RECEIVING A PHYSICAL CARD AS A PROGRESS SIGNAL

In Study 3, we introduced a third context: the issuance of a physical debit card with cash benefits to be loaded on the card. We operationalized progress signals as receiving the physical card, separately from receiving access to the funds on the card. We assumed that people would perceive receiving the physical card as reducing uncertainty about whether they will eventually receive the funds, which is the consequential reward. We varied the date on which the debit card arrives in the mail as the progress signal timing and the date on which the funds on the card can be used as the payment timing.

Method

We recruited 225 U.S. participants from MTurk, aiming for a target sample size of 200 after exclusions. We received 225 complete responses from unique IP addresses. After excluding 4 participants for failing our instructional attention check and 20 additional participants for failing our comprehension check about the debit card timing (described below), we analyzed a final sample of 201 valid responses (M_{age} = 41.95, SD = 12.48; 55% women, 2 indicated 'other'). This study was pre-registered (https://aspredicted.org/Z3M_ZC5).

Participants were asked to assume that they were eligible to receive some cash benefits that were to be issued on a prepaid debit card. There was to be no cost to receiving and using the card. Participants made twenty-one choices, each between a smaller-amount debit card option ($35) or a larger-amount debit card option ($40). Across the choices, the options differed in the dates of receiving the debit card in the mail ("delivery date", representing progress signal timing), and when the card would be activated, making the funds accessible ("activation date," representing payment timing). The timings were the same as in Study 2 (Table 2-2).

After making all choices, participants completed a short comprehension check, asking about their understanding of the activation date. Only those who correctly answered that the money could be used after the card was activated (instead of as soon as the card was received) were included in the analyses.

Results

We conducted the same linear regression analysis as in Study 2. Replicating the prior results, we again found a significant negative effect of the payment delays, which were the delays in the activation date of the card in this case ($B_{Payment}$ = -0.15, SE = 0.010, t(4217) = -14.75, $p < .001$). This reflects impatience for the reward, as time discounting suggests.

Importantly, we also found a significant negative effect of progress signal delays (i.e., delays in the delivery of the debit card; B_{Signal} = -0.014, SE = 0.0054, t(4217) = -2.56, p = .01), albeit again significantly smaller than the effect of payment delays ($B_{Payment}$ - B_{Signal} = -0.14, $p < .001$). People preferred to receive the debit card earlier, controlling for when the funds would be made accessible. This is consistent with a preference for a sooner progress signal. The sensitivity to progress signal timing was significant even though the payment timing differed across the options in all choices. Unlike in Study 2, there was no significant overall effect of the common delay (B = 0.064, SE = 0.051, t(4217) = 1.26, p = .21).

In the same exploratory analysis as Study 2, we tested the interaction between progress signal delays and payment delays, controlling for the common delay conditions. The interaction was not significant ($B_{\text{Payment Delay X Signal Delay}}$ = 0.0014, SE = 0.0045, t(4216) = 0.33, p = .74), suggesting the sensitivity to delays in either dimension (payment or progress signal) was not moderated by extent of delay in the other dimension.

Further, we found no significant interaction between common delay and each of payment delays and progress signal delays ($B_{\text{Payment X Common Delay}}$ = 0.029, SE = 0.021, t(4215) = 1.38, p = .17; $B_{\text{Signal X Common Delay}}$ = -0.015, SE = 0.011, t(4215) = -1.41, p = .16). However, examining each common delay condition separately, the results were more nuanced. In the tomorrow

condition, only the effect of payment delays was significant, and not the effect of progress signal timing in the tomorrow condition (B_{Payment} = -0.17, SE = 0.015, t(2055) = -11.60, p < .001; B_{Signal} = -0.006, SE = 0.0089, t(2055) = -0.64, p = .52). In the one-week condition, both effects were significant (B_{Payment} = -0.14, SE = 0.015, t(2160) = -9.46, p < .001; B_{Signal} = -0.021, SE = 0.0063, t(2160) = -3.33, p < .001).

Discussion

Replicating our findings from Study 2, we found our participants were impatient for progress signals, in addition to being impatient for the rewards. Specifically, even when the progress signal did not resolve explicit uncertainties (compared to the lottery drawing scenario in Study 2), people preferred to receive it early, presumably because it reduces implicit uncertainty about receiving the consequential reward (i.e., funds).

Next, we test and measure the joint effect of progress signal and payment by fitting our extended discounting model.

STUDY 4: ESTIMATION OF DISCOUNT FACTORS

Our results thus far show that people not only prefer to receive payments earlier but also prefer sooner progress signals, which reduces the uncertainty associated with receiving the payments. To better relate our findings to the literature on time discounting, we quantify these effects in terms of discount factors, using our proposed extended discounting model. To do so, we employ titration tasks (Green et al. 1994), a method commonly used to elicit people's

indifference points between sooner and delayed rewards. In a typical titration task, participants make a series of intertemporal choices between two options that differ in the timing of the outcomes. Across the choices, the experimenter varies the payment amount of one timing option, keeping the payment amount of the other timing option the same. Experimenters can estimate the participant's indifference point by identifying at what amount of the first timing option the participant switches their preference to the other timing option. The indifference point allows for modeling intertemporal preferences and calculating the preference parameters as specified in the model of the researcher's choice (Urminsky and Zauberman 2015) without making additional functional-form assumptions about choices.

We fit our extended discounting model, encompassing discounting over both payment delays and progress signal delays, to estimate separate discount factors for the respective delays (i.e., the degree to which an outcome is devalued by the delay in payment timing, and separately by the delay in progress signal timing).

We return to the context of getting paid for completing a job, similar to Study 1. Using the titration method, we elicited the indifference value for the amount of compensation for a one-time job, based on the schedule for job approval and payment. All trade-offs involved payment delays, and we varied the extent of the trade-off in progress signal delays between the options for the given payment delay. Using this design, we further illustrate how preference for sooner progress signals can confound time discounting, including testing a case where the later-payment option offers a sooner progress signal than the sooner-payment option.

Method

We recruited 225 U.S. participants from MTurk, aiming for a pre-registered (https://aspredicted.org/3K6_NGJ) target sample size of 200. We received 225 complete responses from unique IP addresses. After excluding four participants for failing the instructional attention check and 19 additional participants for inconsistency in one or more of the titration tasks, we analyzed a final sample of 202 responses (M_{age} = 41.78, SD = 11.74; 53% women; 3 indicated 'other').

Participants read a scenario about receiving compensation for completing a one-time job and evaluated different payment schedules, which specified when the job would be reviewed and approved (approval date, representing progress signal) and when the payment of the compensation would be made if the job is approved (payment date). Each participant was presented with eight titration tasks (Table 2-3). In each titration task, participants were made a series of choices between two payment schedule options: a sooner-payment option and a later-payment option (see Figure 2-1 for an example). The trade-off in the approval date between the options differed across the titration tasks.

Table 2-3. Progress Signal and Payment Timings for Titration Tasks and Results (Study 4)

			Sooner Option (Titrated)		Later Option (Fixed, $500)			
Task	Payment timing trade-off	Larger-later option progress signal timing	Progress Signal (Approval date)	Payment (Payment date)	Progress Signal (Approval date)	Payment (Payment date)	Mean indifference value	Payment discount factor (monthly; ignoring progress signal timing)
1	tomorrow vs. 2 months	Baseline (Simultaneous as payment)	tomorrow	tomorrow	2 months	2 months	$374.21	0.865
2		Expedited by 1 month	tomorrow	tomorrow	1 month	2 months	$380.35	0.872
3		Expedited by 2 months	tomorrow	tomorrow	tomorrow	2 months	$390.00	0.883
4	1 month vs. 2 months	Baseline (Simultaneous as payment)	1 month	1 month	2 months	2 months	$415.15	0.830
5		Expedited by 1 month	1 month	1 month	1 month	2 months	$423.22	0.846
6		Expedited by 2 months	1 month	1 month	tomorrow	2 months	$439.60	0.879
7	Additional choices	Progress signal for both options expedited by 1 month from Choice 4	tomorrow	1 month	1 month	2 months	$413.96	0.828
8		Payment for both options expedited by 1 month from Choice 7	tomorrow	tomorrow	1 month	1 month	$402.87	0.806

Figure 2-1. Sample Titration Task in Study 4

We counterbalanced whether the titrated amounts were presented in an increasing or decreasing order. In each titration task, participants first viewed a list of choices where the sooner payment option ranged between $260 and $500, in $20 increments, while the later payment options were kept constant at $500. If a participant chose the sooner payment option in all choices (i.e., their indifference point was not elicited within the $260 and $500 range), they were shown a second list of choices, where options ranged from $0 to $240.[1]

As baselines, in two tasks (tasks 1 and 4 in Table 2-3), the approval date coincided with the payment date in both options ("simultaneous delay"). The trade-offs in these tasks represented intertemporal choices between a smaller-sooner option and a larger-later option, in which progress signal co-occurs with payment of the reward.

We designed four other tasks to test sensitivity to progress signal timing. In these tasks, the later-payment option offered an "expedited" approval date by either one month (tasks 2, 5) or two months (tasks 3, 6) compared to the baseline tasks (keeping the approval date of the sooner-payment option the same). For example, in tasks 3 and 6, by expediting the approval date of the later-payment option by two months, the approval date was brought up to *tomorrow*.

In addition, we designed two tasks (tasks 7, 8) to further explore time inconsistency in preferences for progress signal and payment timing. Keeping the delays between the options the same, both in terms of approval dates and payment dates, we shifted the timing of one component in both options. This allows us to test the *common delay effect*, i.e., decreasing impatience when the same amount of delay is added to both the sooner and larger options. Specifically, in task 7, the approval dates in both options were expedited by one month relative to

[1] This was done to limit the number of choices participants would have to make to minimize fatigue. The initial range ($260-$500) is sufficient to capture even excessive discounting in the current design (monthly payment discount factor of about 0.7).

task 4. Comparing these two tasks allows us to test the common delay effect over progress signal timing. In task 8, the payment dates in both options were expedited by one month, relative to task 7. Comparing these two tasks allows us to test the common delay effect over payment timing.

Discounting Model

For each titration task, we calculated a ratio of the indifference value (A) and the later option's payment amount, which was fixed (B): r = A/B. First, based on time discounting only, the ratio can be expressed as a function of the monthly payment-based discount factor δ_P and the timing of the payments for the lotteries, t_P^A (sooner lottery's payment timing) and t_P^B (later lottery's payment timing):

$$r = \frac{A}{B} = \delta_P^{(t_P^B - t_P^A)} \quad \text{(Eq. 1)}$$

The parameter δ_P measures people's degree of impatience specifically for the payment, i.e., how much they devalue the monetary reward based on payment delay. A value of 1 represents indifference to the timing of the payments, and values below but close to 1 are interpreted as time discounting with high patience. In contrast, a low value signals a strong devaluation of future payments and impatience to receive payments sooner. The smaller δ_P is, the more impatient people are to receive payment early. (The weekly discount factor of $\delta_P = 0.98$, for example, commensurate with requiring an approximate 2% weekly interest rate, which surmounts to 180% annual interest rate when compounded). This estimates a standard exponential time-discounting model.

Our proposed extended model similarly accommodates people's impatience for progress signals, thus enabling us to quantify devaluation specifically based on progress signal delays

(e.g., based on the approval dates, t_G^A for the sooner approval option and t_G^B for the later approval option) in terms of a discount factor, δ_G, such that:

$$r = \frac{A}{B} = \delta_G^{(t_G^B - t_G^A)} \quad \text{(Eq. 2)}$$

The progress signal discount factor, δ_G, measures how much people are willing to sacrifice in terms of the amount to attain sooner progress signals. As with the payment discount factor δ_P, a value of 1 represents indifference to progress signal delays. A value below 1 can be interpreted as a preference for sooner progress signals, with a lower value representing high impatience for progress signals. Quantifying the impatience for progress signals using this approach allows us to incorporate it into a time discounting model. Specifically, jointly accounting for both progress signal and payment timings, we can estimate the following model, i.e., our extended discounting model:

$$r = \frac{A}{B} = \delta_G^{(t_G^B - t_G^A)} \delta_P^{(t_P^B - t_P^A)} \quad \text{(Eq. 3)}$$

Results

We first examined the tasks where the trade-off in the payment dates was tomorrow versus in two months (tasks 1-3). For each task, we calculated a payment discount factor, ignoring progress signal timing (i.e., Eq. 1). In the baseline task where the progress signal and payment delays were simultaneous (task 1), the estimated (monthly) payment discount factor (δ_{P1}) was 0.865. Compared to this baseline, expediting the approval date of the later-payment option led to significantly higher discount factors (by one month, task 2: $\delta_{P2} = 0.872$; difference from task 1: $\delta_{P2} - \delta_{P1} = 0.011$, SE = 0.0042, $t(402) = 2.61$, $p = .010$; by two months, task 3: $\delta_{P3} =$

0.883; δ_{P3} - δ_{P1} = = 0.018, SE = 0.0042, $t(402) = -4.30$, $p < .001$), which can be attributed to the difference in progress signal timing.

We found similar results in tasks 4-6, where the payment dates were one month versus two months. In the baseline simultaneous-delay scenario (task 4), the monthly payment discount factor (δ_{P4}) was estimated as 0.830. Expediting the approval date of the later-payment option led to a significantly higher discount factor (by one month, task 5: $\delta_{P5} = 0.846$; difference from task 4: δ_{P5} - $\delta_{P4} = 0.016$, SE = 0.0057, $t(402) = 2.85$, $p = .005$; by two months, task 6: $\delta_{P6} = 0.879$; δ_{P6} - δ_{P4} = 0.049, SE = 0.0093, $t(402) = 5.26$, $p < .001$).

These results illustrate how estimates of the payment discount factor can be confounded by a separable preference for early progress signal. Specifically, the discounting of delayed rewards could be partly due to the later reward often having more of a delay in progress signal. While keeping the payment date the same, allowing for sooner progress signal led to a greater "patience", represented by a higher overall discount factor.

Next, using all titration tasks, we fitted our extended exponential discounting model to estimate separate discount factors for progress signals and for payments (Eq. 3), using non-linear regression. We used clustered standard errors to account for the correlated errors due to the within-subjects design. We found significant discounting based on both payment ($\delta_P = 0.871$, SE = 0.0094, $t(1614) = 13.73$, $p < .001$) and progress signal delays ($\delta_G = 0.974$, SE = 0.0042, $t(1614) = 6.07$, $p < .001$). The progress signal discount factor was significantly larger than the payment discount factor (δ_P - $\delta_G = -0.10$, SE = 0.0094, $t(1614) = -10.96$, $p < .001$).

Discussion

In the context of getting paid for completing a job, we again found that people are also willing to forgo part of the compensation not just to receive the payment earlier, but also to get the progress signal sooner, which was to get approved for the compensation.

We calculated the degree of impatience separately for the payment and progress signal using a model-fitting approach. We estimated people's valuation of the monetary reward that is delayed in terms of progress signal or payment by measuring the amount that they are willing to receive instead in order to receive progress signal or payment sooner. We found results consistent with our prior findings: People are not only sensitive to the timing of the payments but also to the timing of progress signal, i.e., the time when the uncertainty about achieving the reward is reduced.

The modeling approach specifically allowed us to quantify the degree of impatience for progress signals in terms of discounting the value of the reward, as has commonly been done in prior research to quantify the degree of time discounting. Using this approach, our results suggest that people are willing to forgo some future benefits in exchange for a sooner progress signal, similar to how time discounting describes people as devaluing future rewards based on payment delays. Further, using the implied discount factor specific to progress signal timing, we could quantify the impatience for progress signals relative to the time discounting of the payment. The progress signal discount factor was consistently smaller than the payment discount factor, suggesting people are relatively more patient for delayed progress signals than for delayed payments (while still overall impatient). Further, we demonstrate that failure to account for

people's impatience for progress signals can lead to a biased estimate of their discounting of delayed payments, suggesting greater impatience in monetary discounting than is the case.

Compared to when the progress signal coincided with payment (i.e., simultaneous delays), decoupling the progress signal from payment by offering an earlier approval date in the later-payment option led to a higher valuation of that option, represented by higher estimated discount factors, i.e., greater patience for delay of payment when the progress signal is sooner.

Exploratory Analyses: Time-Inconsistent Discounting

Additionally, as an exploratory analysis, we tested for the possibility of deviation from the exponential discounting model, which assumes a constant discount factor across time. Specifically, we test time-inconsistent discounting (i.e., the possibility of a time-varying discount factor) in the short term versus the long term, using the quasi-hyperbolic discounting model (Laibson 1997). The quasi-hyperbolic discounting model extends the standard exponential discounting model by incorporating an additional short-term discount factor, β, that applies to the "present period." The full discount factor is therefore defined as $f(t) = \beta\delta^t$ if $t > 0$ and $f(0) = 1$. With $\beta < 0$, this model predicts higher impatience for rewards that are closer in time, as posited by the hyperbolic discounting model (Ainslie 1975), or *present-biased* preferences (O'Donoghue and Rabin 2015). However, unlike the hyperbolic discounting model (which assumes a completely different functional form), the quasi-hyperbolic discount function accommodates the possibility of normative exponential discounting (e.g., when $\beta = 1$).

Based on the payment timing only, we can separately identify a long-term discount factor δ_P and a separate short-term discounting parameter β_P, which results in the following notation:

$$r = \frac{A}{B} = \beta_P^{(1[t_P^B=0]-1[t_P^A=0])}\delta_P^{(t_P^B-t_P^A)} \text{ (Eq. 4)}$$

where the indicator function, $1[t = 0]$, has a value of 1 if time *t* is in the present period and 0 otherwise. Consistent with common practices, we apply the short-term discount factors (βs) to the soonest timing available in the stimuli, which was the "tomorrow" in Study 4.[2] Extending this model to separately account for impatience for progress signals:

$$r = \frac{A}{B} = \beta_G^{(1[t_G^B=0]-1[t_G^A=0])}\beta_P^{(1[t_P^B=0]-1[t_P^A=0])}\delta_G^{(t_G^B-t_G^A)}\delta_P^{(t_P^B-t_P^A)} \text{ (Eq. 5)}$$

Here, β_G and β_P denote the additional discounting that applies only in the short term (i.e., for rewards that are closer in time) for progress signals and payments, respectively.

We fitted this extended quasi-hyperbolic discounting model (Eq. 5) to the indifference ratios, using non-linear regression and clustered standard errors at the participant level. We found a short-term discount factor for payments that is larger than 1, the opposite of diminishing impatience predicted by hyperbolic discounting (β_P = 1.032, SE = 0.012, $t(1612)$ = -2.61, p = .009), but significant long-term discounting (δ_P = 0.861, SE = 0.011, $t(1612)$ = 12.92, p < .001). For progress signals, we found significant short-term discounting (β_G = 0.978, SE = 0.0057, $t(1612)$ = 3.80, p < .001), consistent with diminishing impatience, as well as long-term discounting (δ_G = 0.985, SE = 0.0048, $t(1612)$ = 3.05, p = .002).

However, our exploratory tests of time inconsistency using the specific pairs of tasks designed to test the common delay effect (i.e., changing the progress signal or payment timing

[2] The precise timing of when the short-term discount factor, β, applies (i.e., the definition of the "present" period) has not been clearly established in the literature. A common assumption is that it applies to immediate rewards (e.g., within a day) but in practice, it is commonly applied to the most immediate time period available in the data. In our exercises, we are interested in whether the rate of discounting in the short term (for short delays) and in the long term differs in general. For our purposes, we use the soonest time available in the stimuli. See the first chapter of this dissertation (also the published article, Jang and Urminsky 2023) for a more detailed discussion and evidence that time-inconsistency is most likely to be observed with longer delays than previously assumed.

for *both* options by one month) showed inconsistent results with the "present bias" implied by the model-fit findings. Compared to task 7, we did observe a significantly smaller discount factor in task 8 where the payment timing in both options was expedited by one month, holding everything else constant (again based on payment timing only, Eq. 1; $\delta_{P7} = 0.828$ vs. $\delta_{P8} = 0.806$; $\delta_{P7} - \delta_{P8} = 0.022$, SE = 0.0093, $t(402) = -2.38$, $p = .018$), suggesting diminishing impatience with regard to payment delays that was not captured in the model parameter β_P. However, compared to task 4, we found no significant difference in task 7 where both options' progress signal timings were expedited by one month ($\delta_{P4} = 0.830$ vs. $\delta_{P7} = 0.828$; $\delta_{P4} - \delta_{P7} = 0.0024$, SE = 0.0053, $t(402) = 0.45$, $p = .65$), which suggests constant discounting with regard to progress signal delays, again inconsistent with the significant β_G parameter in the model.

These inconsistent results suggest that the short-term discounting factor for progress signal estimated from the quasi-hyperbolic model may be due to participants having a lower monthly discount factor for a one-month interval (1 month vs. 2 months) than for a two-month interval (tomorrow vs. 2 months), i.e., subadditive discounting (Read 2001; Read and Roelofsma 2003), rather than diminishing impatience. On the other hand, we found the opposite pattern for payment delays. Whereas the quasi-hyperbolic model estimate of the β_R was greater than 1, indicating higher patience in the short term, we found a significant common delay effect consistent with diminishing impatience, which suggests the opposite. This seemingly conflicting pattern may even reflect *super*additive discounting described in Scholten and Read (2006).

CONCLUSION

Time discounting assumes that people make trade-offs between a smaller-sooner reward and a larger-later reward based on how much they discount the value of the rewards, based on the timing at which they would receive the rewards. In other words, higher impatience in intertemporal choice reflects the degree to which people value the delayed reward less. However, delayed rewards inherently involve uncertainty. Upon receiving a delayed reward, people not only attain the reward but also the psychological benefit from uncertainties about the rewards resolved. By manipulating the timing of *progress signals* that offer information about the progress of the delayed reward, thereby reducing some of the uncertainties associated with the delayed rewards, this research examined the individual effects of preferring sooner uncertainty reduction and discounting of the rewards on intertemporal choice. The findings demonstrate that people's decisions are jointly sensitive to both the timing of progress signals and the timing of receiving rewards. We offer improved models for intertemporal choices that take into account both of these effects.

CHAPTER 3.

WHO LIKES IT MORE? INFERRING OTHERS' PREFERENCES FROM CONSIDERATION SET SIZE

ABSTRACT

This research explores how individuals make inferences about others' preferences based on the *size of others' consideration sets*—the set of options people have a liking for and deem sufficient for consumption, and thus will consider for choice. People tend to infer that individuals with smaller consideration sets have a higher liking for options within their set compared to those with larger consideration sets. This result was replicated even with marketing professionals. However, when it comes to category-liking, the opposite judgment is made—individuals infer that others with larger consideration sets have a stronger preference for the category of the options in their consideration set. Highlighting the superordinate category structure of preferences (by making multiple categories salient) reconciles these two inferences and attenuates the inference of stronger option-liking from a smaller consideration set. These inferences have downstream consequences for how people choose to allocate scarce resources across different people, including delegated choice or gift-giving, and their predictions on how much others are willing to pay for an option. For example, sellers may erroneously set a higher price for people with smaller consideration sets.

Keywords: consideration set, social inference, preferences, valuation, prediction

Consider two chocolate consumers, Alex and Casey. Alex likes only three brands: Godiva, Hershey's, and Lindt. Casey likes not only those three brands but also several other brands like Toblerone, Ferrero Rocher, and Cadbury. Given this information, how would an observer determine who likes Godiva chocolates more?

Consumers are inundated with a variety of options in the market. Whether it be drinks, chocolates, Netflix shows, or songs, consumers are rarely loyal to one brand. They have affinities for multiple brands and enjoy diverse movies and songs from a variety of artists. Given this wealth of options, people may form *consideration sets*—a range of different products and services that they like or would consider when making a choice (Shocker et al. 1991). Information on consumers' consideration sets is increasingly available. Consumers share their likes and favorite items on their social media. Data on consumers' purchase histories are becoming more thorough and accessible to marketers. Marketers can now more easily observe what people consider before purchasing by examining the options they browse, add to their wish list, and abandon in the shopping cart. Further, in everyday social interactions and exchanges, people communicate their consideration sets as they discuss their everyday consumptions and choices (e.g., movies they watched or consider watching).

Given this information, consumers and marketers often attempt to gauge how *different individuals hold different degrees of liking for a specific product*. Marketers want to target the most relevant segment of consumers for their products based on which group places the highest value on them. Further, marketers may want to apply an individualized pricing strategy, which requires knowing a potential customer's preferences. For example, in an individualized promotion, marketers may want to decide whether to offer a higher or lower price discount to those who seem to consider more options. Consumers may want to decipher the preferences of

those around them to inform their social interactions when making delegated choices or choosing for joint consumption. They may need to make decisions not only about what to give but also about who the intended recipient of an option should be, particularly when managing scarcity, based on who is deemed to value that specific option more. However, this is not a straightforward task because it is impossible to directly access others' subjective states.

Even with knowledge of people's consideration sets, this inference task remains a challenge. In my example, it remains unclear who has a stronger liking for Godiva chocolate, as both Alex and Casey expressed a liking for it. It is not fully understood what people learn from others' consideration sets and how people evaluate different individuals' liking of the same option.

This research uncovers an intriguing phenomenon: Despite lacking the information to accurately compare how much different people subjectively value an item, when comparing others' liking of a specific option, people make strong inferences that the person with a smaller consideration set has a greater liking for the option compared to another person with a larger consideration set that includes the same option.

In contrast, when comparing others' liking of the *superordinate category* of options (e.g., chocolates as opposed to Godiva), people infer that the person with a larger consideration set likes the category more compared to another person with a smaller consideration set, even when the larger consideration set contains the smaller consideration set. These divergent inferences can lead to contradictory conclusions, depending on whether the item or the category is the basis of the inference.

My research expands the existing literature in two ways. First, I investigate how people make inferences about the extent of others' liking for a known favored option (i.e., one that is

already in their consideration set). Second, I focus on how people make interpersonal comparisons, inferring the relative liking for products across different individuals based on their respective consideration sets. My research differs from previous investigations, which primarily focused on predicting whether a given individual will like or dislike a specific option based on their preferences for other options (e.g., Barasz, Kim, and John 2016; Baum and Nelson 2022). Specifically, I explore a novel factor that influences these inferences: the size of an individual's consideration set.

THEORETICAL BACKGROUND

Consideration Sets

Every consumer decision involves the consideration of numerous alternatives. Due to constraints in resources and time, it is impractical, if not impossible, to deliberate on all available options (Smaldino and Richerson 2012). For example, when choosing a snack, a consumer might recall a set of snacks they like from memory when making a choice or select a group of snacks to consider more carefully after some initial screening, instead of considering all possible snacks.

Reflecting this notion, a consideration set refers to the subset of options that a consumer actively considers when making a decision (Howard and Sheth 1969; Nedungadi, Chattopadhyay and Muthukrishnan 2001; Shocker et al. 1991). It consists of the alternatives that are deemed relevant and feasible for the consumer's specific goals or needs. While consideration sets are often discussed in the context of consumers making decisions among options stored in memory, a similar notion can be found in decisions where people choose from the options in front of them.

Narrowing down the number of options helps people reduce cognitive workload, and consider a smaller subset of select options more carefully (Payne 1976).

Understanding consumers' consideration sets has been of interest to both researchers and marketers because it strongly predicts consumer choice (Hauser and Wernerfelt 1990; Nedungadi 1990). Brands or products that are excluded early in the decision-making process are less likely to be chosen. Inclusion in the consideration set is a crucial factor in determining which brands and products consumers ultimately select. Research thus far suggests consideration sets are determined by evaluations of cost and benefit, including the costs associated with mental processing and the anticipated utility or benefits derived from the alternatives (Hauser and Wernerfelt 1990; Roberts and Lattin 1991). People may also employ heuristics and rule-based strategies that reduce cognitive effort (Hauser 2014; Tversky 1972).

Therefore, consumers' consideration sets can provide valuable insights into their preferences and decision processes (Roberts and Lattin 1997). Marketers often elicit consumers' consideration sets in surveys to assess how well their brand measures up against competitors. More broadly, both marketers and consumers are interested not only in what others choose but also in the potential alternatives they might consider, as this understanding enhances knowledge of their preferences.

Importantly, marketers can access information about consumers' consideration sets through a variety of sources beyond marketing surveys. Consideration sets may be inferred from the purchase data (Van Nierop et al. 2010). Consumers often communicate their preferences about multiple options in casual conversations (as in the earlier scenario) or through social media platforms. Interpersonal communication of preferences is valuable not only for consumers as they navigate their future social interactions but also for marketers. Word-of-mouth

communication serves as a channel through which information about products and brands can spread, potentially influencing people's attitudes (Berger 2014).

Whether a consumer making a choice involving other people or a marketer, people may have insight into another person's consideration set, even when they don't know the other person's preferences.

Inferring Others' Preferences

People frequently encounter situations where they need to predict the preferences of others. Consumers attempt to select appropriate gifts for their friends. Marketers want to understand their customers' preferences to provide suitable recommendations or target them with promotions for specific products and services.

However, accurately inferring others' preferences can be challenging due to the inherent difficulty in gauging others' subjective states accurately. Nevertheless, even with limited information, people spontaneously generate predictions about other people's experiences. These predictions are often erroneous (Frederick 2012; Jung, Moon and Nelson 2020).

Previous research on predicting others' preferences has primarily focused on how people extrapolate from observing other's liking of a single option to predict either their liking of other options or their underlying preferences (Barasz, Kim and Evangelidis 2019; Barasz et al. 2016; Baum and Nelson 2022). Such insights are valuable as they assist consumers and marketers in understanding *what* others like and guiding their decisions. For instance, if I know that someone prefers books over chocolates, it would be wise to consider gifting them books.

However, two questions still remain unclear. How do people infer preferences from

observing multiple products in a consideration set rather than a single choice? Further, how do people determine *who* likes a particular option more?

Relevant to the first question, some previous research has examined what people infer from others' consistency versus variety-seeking behavior. More consistent preferences can be attributed to a more stable factor within the individual (Kelley 1973). While some research has examined the effect of variety-seeking on social perceptions, they have primarily focused on the perception of the choosers' traits, rather than the choosers' preferences (Kim and Drolet 2003; Ratner and Kahn 2002; Sela et al. 2019). For instance, Sela et al. (2019) examined how variety in the assortment choices (e.g., how to compose a box of twenty chocolates) affects the perception of expertise in a domain but this research does not address whether higher variety signals a stronger or weaker liking of a specific option.

In the context of choosing for joint consumption, Kim et al. (2023) examined what inferences people make about someone expressing indifference ("I have no preference"). Importantly, in their context, indifference was expressed over the entire set of available options rather than a subset of the available options (e.g., a consideration set). In the context of gift-giving, Cheng et al. (2021) found that gift-givers exert more effort in selecting gifts for "picky" individuals, defined as those who have narrower and unpredictable preferences. However, smaller consideration sets may indicate narrower preferences but still express a clear preference. Neither of these investigations examined inferences about how much another person likes a specific option.

Further, to the second question, how do people judge the liking of a specific option across different individuals? Determining the strength of preferences across individuals is crucial in various contexts. Economists have long recognized the comparison of utilities across individuals

as a fundamental challenge (Binmore 2009; Harsanyi 1955; Hausman 1995). In everyday life, however, people and marketers frequently evaluate how individuals value the same option differently. This judgment becomes particularly important when there is a scarcity of resources, such that misallocation of resources due to misunderstanding people's preferences can lead to inefficiencies. In marketing decisions, understanding which customers value a product more is vital for maximizing profit. It allows marketers to target their efforts more efficiently within the constraints of a limited budget and supply of products. Such information can assist marketers in implementing differentiated pricing strategies or individualized promotions. Further, people generally want to allocate goods and services based on the desires and needs of individuals, giving them to those who value them the most (Deutsch 1975; Shaddy and Shah 2018).

While prior research has investigated how people tackle the problem of identifying an individual's likes versus dislikes, it has not examined how people evaluate preferences across individuals, especially about options that they generally like (i.e., their consideration set).

Current Research: Consideration Set Size Effect on Inference of Others' Preferences

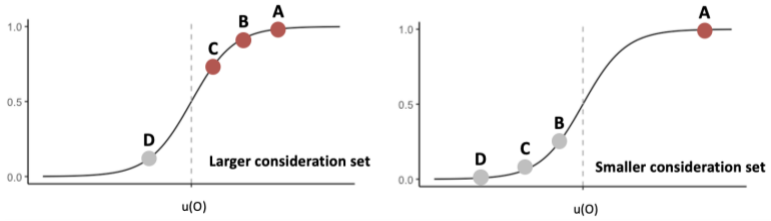
In the current research, I define a consideration set as a narrower set of options that a chooser is considering or willing to accept in service of a specific goal. In other words, the consideration sets would include options that are sufficient to serve the person's goal and that they would be willing to choose over an outside option (e.g., no purchase or non-consumption). These may be a set of options people explicitly consider prior to choice after going through the initial screening stages or a set that they generally hold in their minds (e.g., brands that they like) that they can pull from memory when making future choices.

I first examine if people make inferences about the strength of others' liking for a particular option based on the size of their consideration sets. Specifically, I am interested in understanding what inferences people make when they judge the strength of preference for an option that is included commonly in the consideration sets of different individuals.

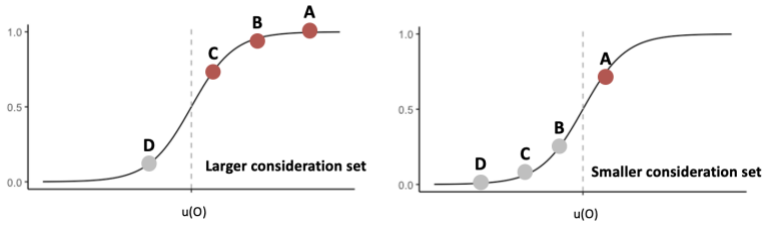
It is important to note that consideration sets alone do not provide sufficient information to confidently judge which option within an individual's consideration set they value more, or which individual values the same option more. Figure 3-1 depicts how the size of a consideration set can vary independently of the liking of a specific option that is consistently included in the consideration set. I assume that options are included in a consideration set when their utility for the individual exceeds a threshold (such as the utility of the outside option, denoted as u(O)). Even if we assume that the utilities of the two individuals can be compared on an objective scale, we cannot determine the precise location of each alternative on their utility curves based solely on the consideration sets.

Figure 3-1. Illustration of the Missing Information Problem With Consideration Sets

(a) Scenario where option A is liked more by the person with a smaller consideration set



(b) Scenario where option A is liked more by the person with a larger consideration set



What we *can* learn from consideration sets is individuals' *intrapersonal* preferences. That is, we can conclude that the options within a consideration set are preferred to those that are not in the consideration set. As the smaller consideration set for a given individual is a subset of a larger one after going through further screening, the options within the smaller consideration set are likely to be valued more highly on average than those in the larger consideration set from the same person.

I propose that people generalize this correspondence between the consideration set size and intrapersonal preferences (preferences between options by a given chooser, e.g., the strength of liking for a chocolate brand over another brand by the same person) to the relationship between consideration set size and objective preferences that can be used to make interpersonal comparisons (e.g., the strength of liking for a chocolate brand by two different people). This is also consistent with the idea of attribute substitution, where individuals substitute a difficult question with a simpler one that they can answer more easily, using an attribute that is related and more readily assessed (Kahneman and Frederick 2002). When there is insufficient information to make *inter*personal comparisons, individuals may substitute a rule that works for *intra*personal comparisons. Further, because they have an easily accessible heuristic, people may not recognize that they have insufficient information (Kardes, Posavac and Cronley 2004).

As a result, I predict individuals will infer that a person with a smaller consideration set has a stronger liking for the options within their set compared to another with a larger consideration set. Therefore, this is an instance of (erroneously) generalizing the correlation between consideration set size and *intra*personal preferences to situations involving *inter*personal comparisons.

Liking of an Option versus Liking of a Category

The size of consideration sets may lead to different inferences when observers shift their focus from others' liking of a specific option (e.g., Godiva chocolates) to the same people's preference for the category of the options (e.g., chocolates overall). People may have a lay theory that individuals with a stronger liking for a category are likely to list or retrieve a larger number of items they like within that category. Further, support theory (Tversky and Koehler 1994) suggests that people's subjective probability of an event increases as more supporting evidence is presented. The presence of additional options within the consideration set can serve as additional supporting evidence for valuing the category more.

Earlier, I predicted that people infer that an individual with a smaller consideration set likes a particular option more than another person with a larger consideration set containing the same option. While this inference may be unwarranted, it may not be inaccurate. In my previous example, it is possible that Alex has a deep affinity for the three chocolate brands they mentioned and enjoys them significantly more than Casey enjoys each of the six brands they generally like. In this scenario, it is plausible to assume that Alex would enjoy Godiva chocolate more than Casey does (similar to the illustration in Figure 3-1(a)).

However, if people simultaneously make the opposite inference regarding others' liking of the superordinate category of options (i.e., the person with a larger (vs. smaller) consideration set likes the category more), they may reach the opposite conclusion. Casey may enjoy Godiva chocolates more than Alex does because Casey generally has a greater liking for chocolates as a whole, while Alex does not possess a strong affinity for chocolates in general (similar to Figure 3-1(b)) because Alex only finds three brands satisfactory.

I propose that when making inferences about others' liking of a specific option, people initially neglect to consider this just-as-likely alternative scenario, and therefore will generally infer stronger option-liking from a smaller consideration set. This could be due to failure to consider that the consideration set over a category is part of a larger preference structure held by others, including other categories and goals. This tendency may stem from a general neglect of background information and a narrow focus on the current consideration set or the category to which the options belong (Chakravarti, Janiszewski and Ülkümen 2006; Leclerc, Hsee and Nunes 2005). For example, in the context of predicting intrapersonal preferences, Baum and Nelson (2022) find that observers neglect the fact that a person's preference for an option (e.g., liking Coca-Cola) also reflects liking the category (e.g., cola products in general, including Pepsi).

I further predict that increasing the salience of the hierarchical category structure of preferences could increase the salience of the alternative scenario, where a larger consideration set reflects higher category-liking and therefore higher liking of the specific option. Highlighting that a given consideration set represents one category among a larger set of categories for which individuals hold preferences would lead to a greater focus on evaluating their liking of the category. By making this evaluation more salient, observers would be more likely to then deduce from category-liking their prediction of option-liking. Consequently, I anticipate that when multiple categories are made salient, the inference of stronger option-liking from a smaller consideration set will be attenuated.

Overview of Studies

I test my predictions across eight studies. Throughout my studies, I examine inferences and decisions people make (as "observers") regarding the option that is common to the consideration sets of two other people ("choosers"). I refer to this option as the "focal option."

To motivate my question, I first examine how people make allocation decisions when there is a scarcity of goods (e.g., only one item of an option remaining), based on others' consideration set sizes (study 1). I find that people are more likely to give the focal option to the chooser who expresses a smaller consideration set, suggesting consideration set size may influence the perception of others' preferences. I confirm this conjecture: People infer a stronger liking of a given option from the person who considers fewer options for consumption (studies 2a-b). I further replicate this result with a sample of current and former marketing professionals (study 2b).

I find that this effect is robust to making the preference ordering of the options within the consideration set explicit (i.e., by providing the ranking of the options by each chooser; study 3) and providing information on the frequency of purchasing each item (i.e., the person with the smaller consideration set purchases less of the category overall; study 4). These results allow us to rule out the possibility that the belief that larger consideration sets reflect less-defined preferences or greater uncertainty about the individual's preference for an option within a larger consideration set (due to a larger number of items in the set).

Further, I test my prediction that people make a reverse inference when evaluating others' liking of the *superordinate category* of options (versus liking of the individual option; study 5). People infer a higher category-liking from a larger consideration set, the exact opposite of the

inference they make when judging option-liking. Leveraging this result, I manipulate the salience of the category structure by presenting multiple categories (thus highlighting that the category is one of many categories; study 6). People's tendency to infer greater liking of a given option from smaller consideration sets is significantly reduced when they also view other categories (vs. only the category of the consideration set). This also affects people's allocation decisions (to whom to give a gift).

Lastly, to illustrate the degree to which these inferences can result in prediction errors and inefficiencies, I elicit consideration sets (all the items they are willing to purchase) and willingness to pay for each item in the set from participants serving as *buyers*. I compare these to pricing decisions by another group of participants as *sellers*, in an incentivized "selling" game (measuring what prices they would offer to the buyers who indicate a smaller vs. larger consideration set when participants can receive the profit as a bonus; study 7). I find that while the sellers charge a higher price to the buyer with a smaller consideration set, buyers do not differ in their willingness to pay for an item based on the size of their consideration set.

All studies (sample size, participant exclusions, and analyses) were pre-registered. The web appendix, full data, study materials, and code for analyses are available on the OSF repository: https://osf.io/pnzsw.

STUDY 1: CONSIDERATION SET AND DELEGATED CHOICE

In study 1, I examine how people make allocation decisions based on others' consideration sets in a simple delegated choice scenario. When delegating a choice to someone else (a surrogate), people have the incentive to express their willingness to accept multiple

options to provide flexibility for the surrogate, especially in the event that some of the options are not available. This flexibility may allow the surrogate to maximize the utility across multiple people and find a way to give everyone what they are willing to accept. However, when resources are scarce, not everyone can receive the options that they want.

In this study, I ask participants to decide between giving a focal option (wanted by both choosers) to a chooser who wants only one option and another chooser willing to accept one of two options. In the scenario, only one unit of the focal option is available, forcing the participant to give the option to only one of the recipients. Specifically, the other option in the second recipient's consideration set was not available. Therefore, only one of the choosers could receive an item indicated in their consideration set.

Method

I recruited 165 U.S. participants from Amazon Mechanical Turk, targeting 150 participants after exclusions. In all studies, I only considered complete responses from unique IP addresses and excluded participants on the instructional attention check, all as pre-registered. I received 164 completed responses from unique IP addresses. I further excluded 12 participants who failed the instructional attention check and analyzed a final sample of 152 participants (pre-registration: https://aspredicted.org/LBP_4QV; M_{age} = 43.32, SD = 13.07; 54.6% women, 44.7% men, 0.7% indicated 'other').

Participants read a scenario where they were to purchase beverages for two coworkers and asked about their preferences. They read a short dialogue in which each person made requests. One coworker indicated a willingness to accept only one type of juice ("I'd like a bottle

of orange juice. If they don't have it, I'll just have a bottle of water"; henceforth the "smaller-set chooser"). The other coworker indicated a willingness to accept two types of juice ("I'd like a bottle of orange juice or grapefruit juice. If they don't have either, I'll just have a bottle of water."; the "larger-set chooser"). Participants were told that there is only one orange juice left and chose which coworker they would give the orange juice (and give water to the other).

Results and Discussion

I found that the majority of the participants chose to give the orange juice to the smaller-set chooser (orange juice only; 71%, 108/152; against 50%, $\chi^2(1) = 26.11$, $p < .001$), which meant giving water to the larger-set chooser (orange juice or grapefruit juice).

Is giving the orange juice to the smaller-set chooser an optimal choice that maximizes utility across the two coworkers? Based only on the information provided, it is impossible to know. From the perspective of the coworker who expressed a larger consideration set, despite being willing to accept one of two different juices, they do not get to receive either. Based on the information provided, it is plausible that the larger-set chooser strongly wanted any juice over water, while the smaller-set chooser was more willing to settle for water.

Why do people show such a skewed preference to give the juice to the smaller-set chooser? I surmise that this may reflect people's inference that having a smaller consideration set indicates a stronger liking of the option, relative to that of another person with a larger consideration set. I directly test this conjecture in the following studies.

STUDIES 2A-B: INFERRING LIKING OF AN OPTION FROM CONSIDERATION SETS

In Studies 2a-b, I test whether people infer that a chooser with a smaller consideration set has a stronger liking of the focal option compared to the larger-set chooser.

I use two different contexts in which a consideration set is formed and expressed—as communicating the options that the choosers are considering in the process of making a choice for a specific consumption (i.e., choosing dinner menu; study 2a), and as listing the products that they would consider purchasing as they come to mind (i.e., the beverage products they would consider if they were to purchase one; study 2b).

Further, in Study 2b, I test the generalizability of my prediction. Misprediction of consumers' preferences might be particularly more costly to marketers. Given the importance, marketing professionals who have more experience in having to understand consumers' preferences may make different inferences. One possibility is that they are more aware of the insufficient information they have from consideration sets and are therefore less likely to rely on the consideration set size. To examine this, I recruited two separate samples: one with general online survey participants and another sample specifically with marketing experience.

Study 2a: Consideration Set From Stimulus-Based Choice

In study 2a, I operationalize the consideration set as the narrowed-down set of options during the process of choosing what to have for dinner, from which the decision maker will eventually choose one (but has not done so yet).

Method. I recruited 160 U.S. participants from MTurk. As in Study 1, I considered 160 completed responses from unique IP addresses and further excluded five participants who failed the instructional attention check, based on the pre-registered criteria. I analyzed a final sample of 155 valid responses (pre-registration: https://aspredicted.org/RV1_MDK; M_{age} = 41.09, SD = 12.36; 54.2% women, 44.5% men, 1.3% indicated 'other').

Participants read a short dialogue about two people choosing what to order for dinner from among different types of cuisines—Italian, Mexican, Chinese, Indian, American, and Thai. One person stated they were considering two cuisines (e.g., Mexican and Chinese; smaller consideration set) while the other person was considering the same two cuisines as well as two other cuisines (e.g., Mexican, Chinese, Indian, and American; larger consideration set). The four cuisines mentioned in the dialogue were randomly selected from the six types of cuisines for each participant.

For generalizability, I counterbalanced how the consideration sets were characterized within the dialogue using two different versions of the wording, between-subjects. In one version, each person stated, "*I am debating between [consideration set].*" In the other version, they stated, *"I've narrowed it down to [consideration set]."*

In my measurement of predicted relative liking, I focused on one of the two options that were common in both person's consideration sets as the focal option (always presented first in each person's list). Further, I employed a bipolar scale to allow participants to easily indicate indifference by selecting the midpoint. Specifically, participants answered, "Who do you think likes [*focal option*] more?" and "Who do you think would enjoy [*focal option*] more?" on a scale between -3 and 3 with the midpoint at 0, with the endpoints labeled with each person in the

scenario. In this study and all other studies using this bipolar scale, which endpoint corresponded to the smaller-set chooser was counterbalanced across participants. For convenience, I present the results from this scale re-coded as -3: larger-set chooser and 3: smaller-set chooser throughout.

Results. The two measures of relative liking had high internal consistency (Cronbach's α = .77) and were averaged into a single rating. Participants rated the person with the smaller consideration set, considering two options (versus four), as liking the focal option more overall (M = 1.25, SD = 1.01; against 0, $t(154) = 15.35$, $p < .001$). There was no significant difference based on the wording ($p = .92$).

These results suggest that people infer a stronger liking of an option from someone who is considering fewer options (i.e., has a smaller consideration set). However, I have argued that the consideration set sizes do not provide sufficient information to make this judgment. Furthermore, this effect was not driven by a minority of participants: the majority of the participants (73% in the linking question and 68% in the enjoyment question) made an inference in favor of the smaller-set chooser (i.e., away from the midpoint).

Study 2b: Consideration Set From Memory

In study 2b, I examine the consideration set size effect in the context of observing a set of products or brands people would consider buying in general (rather than in a specific choice context immediately before consumption as in study 2a). Based on the prior literature's characterization of the consideration set as being constructed from the memory (Lynch,

Marmorstein and Weigold 1988; Shocker et al. 1991), I presented participants with the items that two customers generated as they "come to mind" when they are considering making a specific purchase (i.e., buying beverages). This method of eliciting consideration sets, often referred to as the unaided recall method, is commonly used in research (e.g., Brown and Wildt 1992) and in market research (Nielsen 2023). Specifically, participants were told that these were responses to a consumer survey.

Given that my stimuli were in the form of a consumer survey, in addition to a sample of regular online participants, I also replicated my test with a separate sample with marketing experience to examine whether marketing professionals would be less prone to drawing conclusions from the consideration set data or make a different inference.

Method. I recruited 160 U.S. participants from MTurk. After applying the same pre-registered exclusion criteria as Study 2a, I analyzed 156 responses from MTurk (pre-registration: https://aspredicted.org/YCN_2BS; M_{age} = 41.51, SD = 12.75; 51.3% women, 47.4% men, 1.2% indicated 'other'). As a separate sample, I recruited 110 participants from Prolific who were screened based on their decision responsibilities involving 'Marketing/Sales' or 'Customer/Clients'. After applying the same pre-registered exclusion criteria, I analyzed 103 responses (pre-registration: https://aspredicted.org/624_VSF; M_{age} = 35.18, SD = 11.44; 48.5% women, 48.5% women, 3% indicated 'other'). 94% of participants in this second sample indicated that they are either currently or have been in the past in a profession related to marketing/sales or customers/clients, and 82% indicated they have at least some experience surveying customers. Further, they reported that it is generally important for them to consider their customers' preferences (M = 5.33, on a scale of 1-7).

Participants read that a group of consumers had participated in a survey about their preferences and purchase decisions and one question in the survey asked, "*Think about what you would get from a store to drink. Please list all the items that come to your mind. If possible, please name specific brands or products.*"

Participants then viewed answers from two hypothetical respondents from this survey. One respondent listed three different items while the other respondent listed the same three items and three additional items. These six items were randomly selected from the pool of 30 beverage products for each participant (e.g., Coca-Cola, Mountain Dew, Vitamin Water; full stimuli available on the OSF repository). Participants were further informed that the order of the items was randomized before being presented to them. However, I kept constant the position of the focal option as the second in both lists.

I elicited inferred likings in two ways. Participants first separately rated each survey respondent (i.e., chooser)'s liking of the focal product ("How much do you think [*Respondent #14 / Respondent #29*] likes [*focal product*]?"; 1: not at all, 9: a lot). Second, as in study 2a, they explicitly compared which respondent likes the focal option more on a bipolar scale (-3: larger-set chooser, 3: smaller-set chooser), as a measure of relative liking.

Results. I first examined the results from MTurk consumer sample. I compared participants' separate evaluations of each respondent's liking of the focal product. The inferred liking of the focal option was significantly higher for the respondent with the smaller consideration set than the one with the larger consideration set ($M_{\text{Smaller}} = 6.92$, SD = 1.47, $M_{\text{Larger}} = 6.23$, SD = 1.49, Paired t-test $d = 0.69$, $t(155) = 5.01$, $p < .001$). The result from the bipolar scale was consistent with this result ($M = 0.99$, SD = 1.48; compared to 0, $t(155) = 8.33$, p

< .001), with 60% (94/155) choosing on the scale in favor of the person with the smaller consideration set and 29% (46/110) indicating ambivalence.

Consistent results were found in the sample of marketing professionals recruited from Prolific. The inferred liking of the focal option was significantly higher for the respondent with the smaller (vs. larger) consideration set ($M_{Smaller}$ = 7.02, SD = 1.14, M_{Larger} = 6.50, SD = 1.31, Paired t-test d = 0.51, $t(102) = 3.57$, $p < .001$). The result from the bipolar scale was again consistent (M = 0.78, SD = 1.52, $t(102) = 5.18$, $p < .001$), with 55% (57/103) choosing in favor of the person with the smaller consideration set and 31% (32/103) indicating ambivalence.

Discussion For Studies 2a-b

Studies 2a-b provided an initial demonstration of the consideration set size effect on the inference of others' preferences. Participants perceived people who consider fewer options as liking the focal option more than people who consider a larger number of options. I found this to be the case when people were just about to make a choice (Study 2a) and when considering products they would purchase in general (Study 2b). Further, in Study 2b where the stimuli took the format of a common marketing research survey, I replicated this result among marketing professionals.

In the next set of studies, I further test the generalizability of this effect by using different contexts. I also explore the psychological basis of this effect by examining alternative explanations.

STUDY 3: PREFERENCE ORDERING INFORMATION

In Study 3, I test if the effect of the consideration set size is robust to having information about the preference ordering of each chooser. In addition to learning about the choosers' consideration sets, I further manipulate whether people also learn about the rank order of the options in their consideration sets. This manipulation allows us to examine two alternative explanations for my prior results. In the previous studies, the participants may have perceived that smaller consideration sets reflect clearer and more well-defined intrapersonal preferences. They may also have inferred that having more options in the consideration set means that a given option has a potentially wider range of placement within the person's consideration set (it could be the most or least preferred option for that person or fall somewhere in the middle). Explicitly providing rankings of options within the consideration sets addresses both issues. These alternative explanations predict that the consideration set size effect will be eliminated when I provide ranking information. On the other hand, I expect the effect to be robust if it results from a heuristic based on their mental model of the correspondence between consideration set size and strength of preference, as I theorized.

Further, I use another common context in which consideration sets are elicited: surveys in which participants can select multiple options.

Method

I recruited 220 participants from Prolific and analyzed 203 valid responses after applying pre-registered exclusion criteria (pre-registration: https://aspredicted.org/34D_GVY; M_{age} =

35.70, SD = 12.52; 50.7% women, 47.8% men, 1.5% indicated 'other'). Participants were assigned to one of two (elicited information: choice vs. ranking) between-subjects conditions.

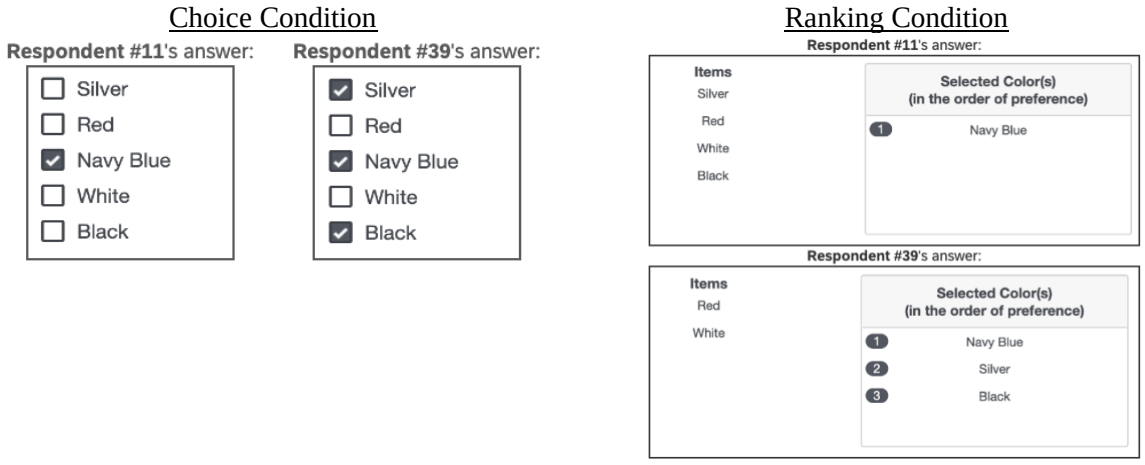
Participants read that a group of consumers had been surveyed about their preferences for a gift, which they were to receive as a thank you from the surveyors. Participants read brief instructions that were supposedly given to the survey respondents. In the choice condition, the instructions in the hypothetical survey read as follows:

"*We would like to send you a water bottle as a gift. Please choose all the color options for the water bottle you like. You will receive only one from the colors you select, based on availability. NOTE: If none of the colors you chose is available, you may receive a color that you did not select or an alternative gift with an equivalent price (a specialty coffee mug).*"

The instructions in the ranking condition were similar except that the hypothetical survey respondents had been asked to place the chosen options in the order of their preference.

Participants then viewed responses from two survey respondents (Figure 3-2), one who had selected one out of the five options (smaller-set chooser) and the other who had selected three options (larger-set chooser). In the choice condition, these responses were displayed in the form of multiple-choice checkboxes. In the ranking condition, the selected options were presented in a box, ordered by the respondent's preference (based on Qualtrics' tool for eliciting choice and rankings of multiple options). Importantly, the focal option (in both choosers' consideration sets; navy blue) was ranked first by the larger-set chooser.

Figure 3-2. Study 3 Stimuli



Participants rated which respondent would like the navy blue water bottle more on a bipolar scale as in studies 2a-b (e.g., -3: larger-set chooser, 3: smaller-set chooser).

Results and Discussion

Consistent with the earlier findings, in the choice condition, participants inferred that the smaller-set chooser would like the focal option (navy blue water bottle) more than the larger-set chooser (M = 2.51, SD = 0.94, against 0: $t(101) = 26.93$, $p < .001$). This reflected 95% (97/102) answering on the scale in favor of the smaller-set chooser.

In the ranking condition, where preference ordering information had also been provided, participants also inferred a stronger liking for the smaller-set chooser (M = 2.19, SD = 1.20, $t(100) = 18.36$, $p < .001$), reflecting 89% (90/101) of the participants choosing the smaller-set chooser on the scale.

While the inferred greater liking for the chooser with the smaller consideration set was significantly reduced in the ranking condition, compared to the choice condition ($t(189.54)$ =

2.13, $p = .035$), suggesting that learning about the choosers' preference orderings may have reduced some participants' reliance on consideration set size in their inference, participants in the ranking condition still inferred a stronger liking from a smaller consideration set overall. This suggests that it is unlikely that the prior results are driven primarily by perceiving the larger-set chooser's preferences to be less well-ordered than the smaller-set chooser's, or by expecting the focal option to rank lower within the larger-set chooser's preferences.

STUDY 4: OBSERVING PURCHASE HISTORY

Thus far, I have considered situations in which consideration sets have been expressed by the choosers. In domains where consumers hold relatively stable consideration sets, their consideration sets may also be inferred by observing their repeated purchases and consumption. In Study 4, I test whether the consideration set size effect holds even when people learn about the choosers' consideration sets by observing their past purchases (rather than their expressed consideration sets), either sequentially or simultaneously. Further, the frequency of each option in the consideration set appearing in the prior purchases would convey information about people's preferences across the different options (i.e., a more frequently purchased item is likely to be a more preferred option within a person's consideration set). Therefore, Study 4 further addresses the alternative explanations discussed in Study 3, by providing relevant, albeit imperfect, information about preferences.

Importantly, past purchases also reveal the overall rate of consumption in the category, which is informative about the chooser's category-liking. Thus, I test whether people readily incorporate this relevant information when judging option-liking. Lastly, leveraging the fact that

the stimuli involve choice data, I compare people's inferences to the predictions from a multinomial logit model as a baseline, which is widely used to model choices from observational data sets in quantitative marketing research (e.g., Guadagni and Little 1983). In addition to measuring inferences about liking of the focal option in the same way as prior studies, I also asked participants which of the choosers they would give the focal option as a gift.

Method

I recruited 260 U.S. participants from MTurk and analyzed 248 responses after applying pre-registered exclusion criteria (pre-registration: https://aspredicted.org/TJJ_W6D; M_{age} = 44.71, SD = 13.74; 52% women, 46.8% men, 1.2% indicated 'other'). Participants were randomly assigned to one of two (presentation of choices: sequential vs. simultaneous) between-subjects conditions.

Participants were told that they would be viewing two customers' purchases over ten visits to the store, one visit per week. In the sequential condition, people viewed one customer's purchases one at a time, and then similarly viewed the other customer's purchases (see Web Appendix A for the stimuli). In the simultaneous presentation condition, participants viewed the two customers' purchases over ten weeks side-by-side. The sequential presentation was designed to mimic real-world situations where people may observe other people's choices across time, while the simultaneous presentation mimics how marketers may view a consumer's purchase history in a bird's-eye view.

In the stimuli, both customers purchased the focal option four times during the ten-week period (purchase probability of 40%). One customer—the smaller-set chooser—did not purchase

any other chocolates in the other weeks. The other customer—the larger-set chooser—purchased two different chocolates, two times each (each purchase probability of 20%).

To control for order effects (i.e., recency and primacy effects), I held constant the first, second, and last purchase for both choosers. The first choice (i.e., in week 1) was no purchase, and the second and last purchases were the focal option for both choosers.

Participants answered which of the two choosers they would give the focal product to if they only had one to distribute. Then, participants evaluated each chooser's liking of the focal product separately (1: not at all, 9: a lot), and then the relative liking on a bipolar scale (e.g., -3: larger-set chooser, 3: smaller-set chooser).

Results and Discussion

First pooling across the conditions, 83% of participants overall (207/248) chose to give the focal option to the smaller-set chooser. In both measures of liking, participants rated the smaller-set chooser as liking the focal option more: The separate evaluation of liking was higher for the smaller-set chooser (M_{Smaller} = 7.75, SD = 1.39; M_{Larger} = 6.36, SD = 1.27; d = 1.39, $t(247)$ = 12.01, $p < .001$). The relative liking evaluation from the bipolar measure was consistent with this result (M = 1.52, SD = 1.73; $t(247)$ = 13.87, $p <.001$). These results are consistent with the prior findings.

The results were not significantly different across the conditions with respect to any of the three measures. The majority of the participants, 80% (97/12) in the sequential condition and 87% (110/127) in the simultaneous condition, chose to gift the focal option to the smaller-set chooser (OR = 1.60, p = .23). Similarly, inferred liking of the focal option did not significantly

differ across conditions, either for the separate evaluations of liking ($d_{\text{Sequential}} = 1.54$, $d_{\text{Simultaneous}} = 1.24$; Welch's t-test, $t(236.46) = 1.27$, $p = .21$; there was no main effect of presentation conditions, $p = .2$) or for the evaluation of relative liking ($M_{\text{Sequential}} = 1.54$, SD = 1.86; $M_{\text{Simultaneous}} = 1.51$, SD = 1.60, $t(236.83) = .11$, $p = .91$).

Even though they observed the past frequency of the chooser purchasing the focal option, which was held constant between the two choosers, participants inferred a stronger liking of that option from the smaller-set chooser. This inference was erroneous relative to the logit model as the standard, which predicts that the smaller-set chooser has a lower utility for the focal option than the larger-set chooser. Based on the parameter values, the larger-set chooser's utility of the focal option is $\log(2) \approx 0.69$, while the smaller-set chooser's utility is $\log(2/3) \approx -0.41$ (note that utility here can scale from negative infinity to infinity, with the utility of no purchase defined as 0 for both choosers; see Web Appendix B for the utility calculations).

The consistent results across both presentation methods suggest that the effect is a robust phenomenon that generalizes across the different ways that people may observe others' consideration sets: make decisions and inferences from memory about others' choices or with information in front of them in a bird's-eye view. As the focal option was the most frequently purchased by both choosers, this result again helps rule out the alternative explanations based on the perception of how well-defined each person's preferences are and the relative position of the focal option within each person's preferences (as in study 3).

Did the participants recognize that the larger-set chooser (more frequent consumer of chocolate) is likely to like chocolates more overall, that is, has a higher *category-liking*? As exploratory measures, participants also rated each chooser's liking of chocolates in general, using both separate and relative liking evaluations again. Participants indeed inferred that the

larger-set chooser, who purchased chocolates more frequently in this case, likes chocolates more overall than the smaller-set chooser (separate evaluation: $M_{Smaller}$ = 5.93, SD = 1.60; M_{Larger} = 8.00, SD = 1.18; d = -2.06, $t(247)$ = -17.20, p <.001; relative evaluation: M = -1.82, SD = 1.49, $t(247)$ = -19.28, p < .001; results are consistent across presentation conditions).

Given that the focal option is the most preferred option in the category (study 3) or the most frequently purchased (thus also likely to be most preferred; study 4) by both choosers, it is even more plausible that the chooser who holds a stronger liking of the category would value that focal option more. Nevertheless, the participants do not seem to incorporate this alternative scenario when evaluating option-liking. In the following studies, I test if people infer stronger category-liking from larger consideration sets even without purchase frequency information, and further examine the divergence between evaluating option-liking and category-liking.

STUDY 5: OPTION-LIKING VERSUS CATEGORY-LIKING

In Study 5, I measure inferences of both option-liking and category-liking in a between-subjects design. I predicted that people would evaluate the larger-set chooser to have a higher liking of the *category* of the options in the consideration set, opposite of what I predict in terms of option-liking. I further explore the possibility that evaluating either of these evaluations affects people's allocation decisions (to whom to give the focal option).

Method

I recruited 320 U.S. participants from Prolific and analyzed 297 valid responses after

applying pre-registered exclusion criteria (pre-registration: https://aspredicted.org/YDN_HKD; M_{age} = 39.52, SD = 13.72; 47.1% women, 52.5% men, 1.3% indicated 'other'). Participants were assigned to one of three (evaluation: option-liking, category-liking, choice) between-subjects conditions.

Study 5 had a similar format as Study 2a where participants read two choosers expressing their consideration sets in a dialogue, except that the choosers were not in the process of making a choice and simply expressed their liking of different chocolate brands (i.e., what they would likely consider for future chocolate consumption). Two choosers, Jordan and Mei, each named two or five chocolate brands they like (the brands in the scenario were randomly selected for each participant from ten popular chocolate brands, e.g., Godiva, Lindt, Hershey's).

Participants answered one of three questions, based on the condition. In the option-liking condition, they were asked which of the two choosers likes the focal option more (the brand both Jordan and Mei mention) on a bipolar scale (i.e., -3: larger-set chooser, 3: smaller-set chooser), as in the prior studies. In the category-liking condition, I instead asked which person likes chocolates more, using the same scale. In the choice condition, participants were told that they only have one box of the chocolate brand both Jordan and Mei mentioned and decided which of the two choosers they would give the chocolate to as a gift (without evaluating either option-liking or category-liking).

Lastly, to explore whether making different evaluations can influence people's choices, I additionally asked participants in the option-liking and category-liking conditions to make the gift decision as well, after answering the respective liking evaluation.

Results and Discussion

I first examined the gift decisions by participants in the choice condition. The majority (87%, 89/102) chose to give the focal option to the smaller-set chooser, consistent with the prior results. In the option-liking conditions, participants again inferred that the smaller-set chooser liked the focal option more (M = 1.35, SD = 1.37, $t(99) = 9.88$, $p < .001$). On the contrary, in the category-liking condition, participants inferred that the larger-set chooser liked the category more (M = -1.85, SD = 1.22, $t(94) = -14.70$, $p < .001$). The two inferences (option-liking vs. category-liking) were significantly different ($p < .001$).

Intriguingly, the choice results differed when people had first made either an option-liking or category-liking evaluation. In the option-liking condition, 94% (94/100) of the participants chose to gift the focal option to the smaller-set chooser, consistent with the result from the unprompted choice condition (OR = 2.28, $p = .15$) where they made their choice without first being asked to evaluate either option-liking or category-liking. On the other hand, in the category-liking condition, a smaller proportion (67%, 64/95) of participants chose the smaller-set chooser, significantly different from either the unprompted choice (OR = 0.30, $p = .001$) or choice after evaluating option-liking (OR = 0.13, $p < .001$). This suggests that people are more likely to spontaneously make inferences from the consideration set about option-liking and make allocation decisions accordingly. However, when prompted to evaluate each chooser's category-liking, they inferred that the larger-set chooser likes the category more and became more likely to allocate the focal option to the larger-set chooser as a result.

These exploratory results suggest that one possible intervention that can guide people's evaluations is emphasizing the choosers' liking of the category. I test this idea in the next study

by manipulating the salience of category-liking in a less direct manner, by structuring the stimuli to emphasize the superordinate category structure.

STUDY 6: SALIENCE OF SUPERORDINATE CATEGORY STRUCTURE AND GIFT CHOICE

In Study 6, I test if the salience of the superordinate category structure in others' preferences affects gift decisions and inferences of option-liking. I make the category structure salient by displaying other categories (e.g., Pop, Hip Hop) in the higher superordinate category (e.g., music) to which the category of the focal consideration set belongs (e.g., K-Pop music). I test the impact of salience in the context of viewing other people's music preferences on their social media pages.

I expected the prior results—inferring the smaller-set chooser to like the artist more, and more likely to decide to give them that artist's album—to replicate when people view consideration sets (artists people like) only for a specific category (i.e., K-Pop). However, the proposed account predicts that both the inference and choice would be attenuated (shifting towards the larger-set chooser) when other categories are salient.

Method

I recruited 270 U.S. participants from MTurk and analyzed 255 valid responses after applying pre-registered exclusion criteria (pre-registration: https://aspredicted.org/L8N_9C8; M_{age} = 44.36, SD = 13.58; 57.3% women, 41.2% men, 1.6% indicated 'other'). Participants were

randomly assigned to one of two (decision context: single-category vs. multiple-category) between-subjects conditions.

Participants read that they would be viewing two social media users' pages about their music preferences. In the single-category condition, it was further specified that they would be viewing the K-Pop (Korean popular music) artists the users liked.

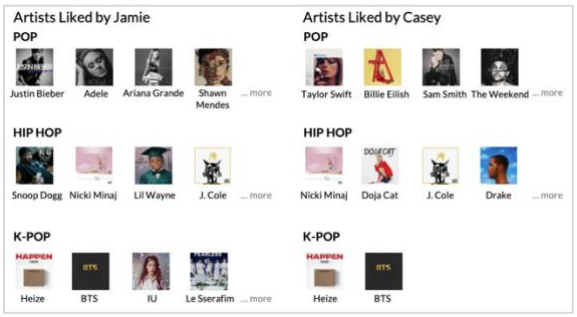
The single-category condition resembled the stimuli in prior studies, such that only the options within the category of interest (K-Pop music) were shown. Specifically, participants viewed the lists of K-Pop artists liked by two users—Jamie and Casey (names were counterbalanced). The smaller-set chooser's page showed two artists, Heize and BTS, while the larger-set chooser's page showed four artists including the same two artists, as well as a "more" sign that indicated there were more artists liked by the user that are not displayed on the page (Figure 3-3).

In the multiple-category condition, participants viewed the same information as in the single-category condition, and additionally, the artists the users liked in other genres, Pop and Hip Hop (four in each category for each person). Crucially, the number of other-category artists liked by each person was the same (the set of other-category artists was also counterbalanced across participants).

Figure 3-3. Study 6 Stimuli

Single Category Condition

Multiple Categories Condition



Participants then chose which of the two users they would gift a new album that neither user had yet from Heize, one of the K-Pop artists that were liked by both users. Next, on a separate page, participants evaluated how much each user liked Heize (1: not at all, 9: a lot), and evaluated the relative liking of Heize between the two users (e.g., -3: larger-set chooser, 3: smaller-set chooser).

Results and Discussion

Gift Choice. In the single-category condition, 70% (91/130) of the participants chose to give the gift to the smaller-set chooser, consistent with the prior results. In comparison, this preference was significantly reduced and fully eliminated in the multiple-category condition, with 51% (64/125) choosing to give the gift to the smaller-set chooser (vs. single-category condition, OR = 0.45, p = .003). The results were consistent with the prediction in the proposed account that being presented with multiple categories would increase the salience of the category

structure of preferences, and debias the preference to give the focal option to the smaller-set chooser.

Option-Liking Evaluation and Mediation. Participants' evaluations of option-liking were consistent with the gift choices, as well as with the results from prior studies. In the single-category condition, participants believed that the smaller-set chooser likes Heize more, both in separate liking measures (M_{Small} = 7.26, SD = 1.71 vs. M_{Large} = 6.75, SD = 1.40, d = 0.52, $t(129)$ = 3.32, p = .001) and the relative liking measure (M = 0.75, SD = 1.70; $t(129)$ = 4.99, p < .001). On the other hand, in the multiple-category condition, on average, participants did not anticipate either person to have a stronger preference, in either measure (separate: M_{Small} = 6.82, SD = 1.45 vs. M_{Large} = 6.75, SD = 1.40, d = 0.064, $t(124)$ = 0.48, p = .63; relative: M = -0.14, SD = 1.77, $t(124)$ = -0.86, p = .39).

Comparing these evaluations across the conditions showed that in the multiple-category condition, the inference of stronger liking from the smaller-set chooser is significantly attenuated compared to the single-category condition (separate: d_{Single} = 0.51, SD = 1.77 vs. $d_{Multiple}$ = 0.064, SD = 1.49, $t(248.45)$ = 2.21, p = .028; relative: $t(251.49)$ = 4.05, p < .001).

The relative liking evaluation significantly mediated the effect of category salience on gift choice (using 'R' mediation package, using linear probability model for predicting choice and 5,000 bootstrapped samples: indirect effect, a × b = -0.12, 95% CI: [-0.19, -0.06]), explaining about 66% of the total effect. The direct effect was not significant (c' = -0.063, 95% CI = [-0.17, 0.04]).

Overall, the findings are consistent with my theorizing: People infer a stronger liking of a focal option from a smaller-set chooser because they neglect to consider their liking of the

category. When the superordinate category structure is made salient, however, people incorporate their judgment of category-liking into their evaluation of option-liking, resulting in a shift in not only inferences about option-liking but also their allocation (gift) choice.

Further, these results are unlikely to be simply driven by people becoming less confident and more indifferent in their judgments. There were no significant differences across conditions in the proportion of participants choosing the midpoint ($p = 1$) and in an exploratory confidence rating about their option-liking evaluation ($M_{\text{Single}} = 5.38$ vs. $M_{\text{Multiple}} = 5.14$, $p = .43$). Further, I confirmed that the manipulation did not make a significant difference in the evaluation of relative category-liking (see Web Appendix C for details and discussion on the interpretation of the results).

STUDY 7: WILLINGNESS-TO-PAY VERSUS PRICING DECISION

Lastly, I test a potential marketplace implication of my findings. In the first part, I elicit people's preferences over a set of items as well as their consideration sets (the subset of options that they are willing to purchase) and examine whether there is a relationship between consideration set size and willingness to pay among buyers. In the second part, I asked another group of participants to act as a seller and set prices for the smaller-set chooser and larger-set chooser respectively, in an incentivized setting where they would receive the profits as a bonus. As a result, if the sellers mispredicted the prices at which the buyers would be willing to buy based on the consideration set size, they would earn less of a bonus. I measure the degree of error when sellers predict preferences and then set prices based on the observed consideration set sizes.

Consideration Set and Preference Elicitation

Method — Buyer's Study. I recruited 270 U.S. participants from MTurk and analyzed a final sample of 260 participants after applying the pre-registered exclusion criteria (pre-registration: https://aspredicted.org/FT9_2PR; M_{age} = 41.32, SD = 12.88; 40.3% women, 57.7% men, 2% indicated 'other').

Participants were asked to consider eleven different chocolate candy products. Each chocolate candy was presented with an image of the package of the candy and brief nutritional information (e.g., calories and fat, sugar, and protein content). Further, for exploratory purposes, half of the participants were additionally asked to assume that they would receive one item from their consideration set. This manipulation did not affect the key results relevant to my purposes. Thus, I present the results collapsed across these two versions below (additional details in the Web Appendix D).

For each candy product, I elicited two measures of liking. First, participants rated how much they like the candy on a 7-point scale (1: not at all, 7: a lot). Next, I elicited their willingness to pay (WTP) for the given candy in a two-step process (similar to Evangelidis, Jung, and Moon 2022). Participants first clicked one of two options: a zero-WTP option: "*$0 (I am not interested in this candy and am not willing to pay anything for it)"* and a positive-WTP option ("*I would like this candy and be willing to pay $___ (enter amounts in dollars in the box, using only numbers)*". Participants who clicked the positive-WTP option entered a positive amount of up to $3.00. Any option for which the buyer indicated a positive WTP was classified as in their consideration set.

After evaluating all eleven candy products, participants answered three questions about their category-liking (liking of chocolates, candies, and candy bars; 1: not at all, 7: a lot).

Results — Buyer's Study. On average, participants indicated that 6.35 of the eleven candy products (median: 6) were in their consideration set (i.e., items they are interested in purchasing). However, the consideration set sizes varied widely across respondents (SD = 3.12; see Figure 3-4). Further, there was a significant positive relationship between consideration set size and category-liking (averaged across three measures: $B = 0.19$, SE = 0.025, $t(258) = 7.51$, $p < .001$): Those who liked chocolates more overall (higher category-liking) expressed larger consideration sets.

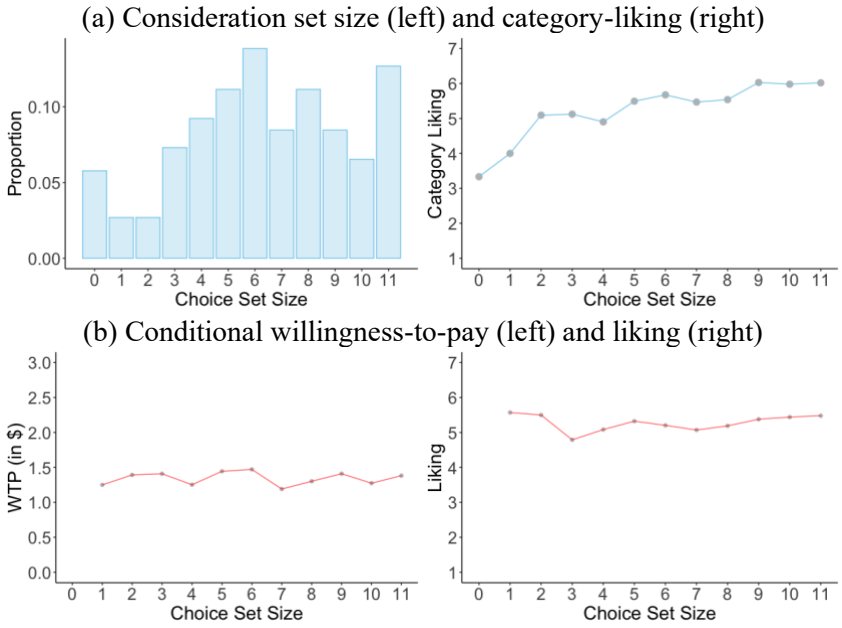
Next, I analyzed the liking and WTP for items that were included in each participant's consideration set (i.e., only the items for which the participant indicated they are willing to pay more than $0).

I first examined if there was any systematic linear relationship between the size of a buyer's consideration set and their liking for the individual items in the consideration set, measured on a 7-point Likert scale (Figure 3-4(b)). I found a significant positive relationship between consideration set size and liking of each item in the consideration set ($B = 0.064$, SE = 0.021, $t(1640) = 2.98$, $p = .003$; controlling for fixed effects for different candy products and clustering standard errors at the participant level), suggesting people with larger consideration sets provided a higher rating of the individual items they chose as well, potentially reflecting their category-liking.

However, there was no significant linear relationship between the consideration set size and the average WTP across options in the consideration sets (i.e., averaged over items for which

the WTP was above zero for each participant; B = -0.001, SE = 0.011, $t(243)$ = -0.119, p = .91). Accounting for the different items in the consideration set (by controlling for item fixed effects), there was still no significant linear relationship between the consideration set size and WTP for individual options in the consideration set (B = -0.0007, SE = 0.013, $t(1640)$ = -0.054, p = .96; clustering standard errors at the participant level).

Figure 3-4. Study 7 Consideration Set and Preference Elicitation



The discrepancy between the liking measure and WTP may reflect differences due to the elicitation method. For the purpose of comparing these results to people's predictions, I rely on my results on WTP, as it is more relevant for marketing decisions and may be more reflective of tradeoff-based thinking that occurs in actual consumer choices.[1]

[1] Using WTP (vs. ratings) constitutes a conservative test, as the relationship between liking ratings and consideration sets elicited here (stronger liking with a larger consideration set) is the opposite of what I predict the sellers/observers would infer.

Method — Seller's Study. I recruited 280 U.S. participants from MTurk and analyzed 210 valid responses after applying pre-registered exclusion criteria, which included the same criteria as in prior studies as well as excluding participants based on a comprehension check (pre-registration: https://aspredicted.org/5YR_PLS; M_{age} = 41.71, SD = 12.33; 50% women, 49% men, 1% indicated 'other').

Participants in the pricing phase (the "sellers") were told that they would be playing a "selling game" where they would be offering selling prices to different buyers. They were further informed that they would receive any profit they made as part of the game as their bonus payment. A transaction (sale) would occur if they offered a price that was less than or equal to the buyer's willingness to pay for the item. The mechanism for what leads to a successful transaction and how the profit is determined was described to the participants, accompanied by a comprehension check.

Next, the sellers viewed two buyers' consideration sets (the items the buyers were willing to purchase, as indicated by a non-zero WTP). The stimuli were constructed based on the same eleven chocolate candy products from the buyer's survey. One buyer represented the smaller-set chooser, indicating a purchase interest in two, three, four, five, or six (counterbalanced across participants) different candy bars. The other buyer, the larger-set chooser, indicated a purchase interest in five additional options (i.e., seven, eight, nine, ten, or eleven), in addition to the options selected by the smaller-set chooser. The different consideration set size conditions did not moderate the results and report the collapsed results below as pre-registered.

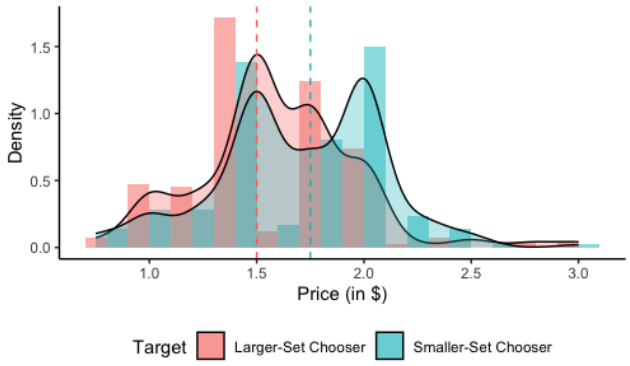
As the focal option, I used Hershey's Milk Chocolate, which was included in 72% of the participants' consideration sets from the preference elicitation phase and had an average WTP comparable to the overall average across items included in consideration sets ($1.36).

Results — Seller's Study. The average price the sellers offered, across the two buyers, was $1.65, which is higher than the average WTP for Hershey's from the buyer's survey ($1.36), reflecting a general seller-buyer asymmetry (e.g., Frederick 2012).

Importantly, the sellers set a significantly higher price for the smaller-set chooser than for the larger-set chooser, on average (M_{Small} = 1.70, SD = 0.38, M_{Large} = 1.60, SD = 0.37, paired t-test, d = .10, $t(209) = 4.19$, $p < .001$), charging about 10 cents more (a 6.3% mark-up) to the smaller-set chooser on average. In terms of the proportion of the sellers, 48% (100/210) offered a higher selling price to the smaller-set chooser, while 27% (57/210) offered the same price to both buyers and only 25% (53/210) offered a higher price to the larger-set chooser. The median price was $1.75 for the smaller-set chooser and $1.50 for the larger-set chooser (see Figure 3-5 for the distributions).

The results suggest that the consideration set size effect on inferred liking extends to inferences about willingness to pay and resulting pricing decisions. Consistent with my theoretical prediction that the sellers would infer a greater liking of Hershey's and therefore a higher willingness to pay by the smaller-set chooser compared to the larger-set chooser, the participants set a higher price for the smaller-set choosers. Given that I did not find a systematic difference in people's willingness to pay across consideration set sizes, this indicates a misprediction by the sellers.

Figure 3-5. Study 7 Distribution of Offer Prices



NOTE. The vertical lines show the median of each distribution.

Given that there was no systematic difference in WTP across consideration set sizes in the preference elicitation phase, I use the average WTP of Hershey's ($1.36) as the expected WTP, regardless of the consideration set size. As the offer prices were on average higher than the expected WTP, only 18% of the offers would lead to a sale; 16% for the smaller-set chooser and 21% for the larger-set chooser. While this difference was not statistically significant (p = .21), these results illustrate the potential inefficiencies that can arise, not simply from generally misestimating the buyers' willingness to pay (i.e., overestimating them overall), but also from erroneously predicting one group of consumers are willing to pay more than the others. This resulted in a $0.096 average profit per seller earned from the smaller-set choosers (across sales and non-sales), 25% lower than the average profit from the larger-set choosers ($0.128).

GENERAL DISCUSSION

I document how people make inferences about other people's preferences based on the sizes of their consideration sets. Specifically, people infer that a chooser who expresses a smaller consideration set (e.g., considering, liking, or being willing to pay for fewer products) has a *higher* liking of an option in the consideration set, compared to another person with a larger consideration set (studies 2-6). This effect emerged even among marketing professionals with experience surveying customers and relying on their knowledge of consumers' preferences (study 2b).

Across the studies, I communicated consideration sets in a variety of ways, ranging from interpersonal communication (studies 1, 2a, 5), marketing surveys (study 2b, 3), purchase data (study 4), and likes on social media (study 6). The consideration set size effect was also consistently observed across different characterizations of the consideration set, whether as a narrowed-down list of options (study 2a, 3), options constructed from memory (study 2b), or simply expressing liking of multiple options (study 5, 6) as well as across when the consideration sets are directly expressed by the choosers (studies 1-3) or can be inferred by observers from learning about the choosers' liking of multiple options or their past choices (studies 4-6).

My results cannot be explained by people's belief that the larger-set chooser's preferences are less well-defined and thus weaker or that there is more room in the larger consideration set for an option to rank lower within the person's preferences. The effect persisted when people had information about the rank order of the options within each person's consideration set (study 3) or prior frequency of purchase (study 4).

Based on the same consideration set information, people also make a contradictory

inference, that the smaller-set chooser has a *lower* liking of the overall category, which is the opposite of their specific option-liking evaluation (studies 4, 5). Once participants are prompted to consider the superordinate category of the consideration sets (i.e., by viewing the options in the focal category alongside other categories), people incorporate this judgment into their option-liking evaluation. This leads their inferences of option-liking to shift toward the larger-set chooser (study 6).

In my studies, inferring stronger liking from smaller consideration sets resulted in erroneous inferences and inefficient decisions in comparison to some standards. Based on the purchase history, people's inferences about who held a greater liking of the focal option were the reverse of the prediction from a multinomial logit model (study 4). Further, based on actual liking and willingness to pay elicited from consumers (study 7), people set a higher price for the smaller-set chooser, while buyers' willingness to pay did not differ based on their consideration set sizes, and liking was higher among large-set choosers.

Implications

Inference of Other's Preferences. My findings contribute to the literature on how people make inferences about other people's liking and preferences (Barasz and Kim 2022; Barasz et al. 2016; Orhun and Urminsky 2013), a key input into a variety of interpersonal consumer decisions. Predicting other people's preferences is challenging due to the fundamental lack of access to other people's subjective states. Unlike past research, which focused on inferences based on observing one-shot choices, I document how people infer other people's preferences based on observing the set of options they like or have purchased. Further, I examine how people compare

the value of an option across different individuals, which customers and marketers use to answer the question of whom to give scarce resources to, and what price to charge to whom.

I argued that the results arise from overgeneralizing a mental model that people hold about the relationship between consideration set sizes and strength of liking *within* an individual to that *across* individuals. This is another example where people apply intuitive but unwarranted heuristics to evaluate others' preferences in the face of insufficient information (Jung et al. 2020).

Marketers' and Sellers' Decisions. My findings are relevant to how sellers may infer consumers' liking of products and make strategic decisions. For example, a car dealer for a specific brand observes a prospective car owner considering several options. As they are negotiating with the buyer, they may stick to a higher price for the customer who is considering fewer car options, believing that the customer must like those cars very much and would accept a higher price, relative to other customers who consider a larger number of options. However, the dealer may be neglecting the fact that the customer may not value the brand as much overall. To avoid this bias, sellers may need to account for the customers' consideration of other categories—different car brands in this case. These takeaways extend to scalable marketing strategies. Marketers may conduct price discounts or promotions, erroneously differentiating consumers based on their observed considerations set, either based on examining their purchase histories, likes on social media, or products visited in online shopping malls.

Overall, the results highlight the importance of marketers carefully considering the full set of information available to them about their consumers. While algorithmic marketing practices that rely on quantitative models are becoming popular, many marketing decisions are

made by humans. Further, customer-to-customer (C2C) sales are also becoming more common with the development of online environments that enable them (e.g., Poshmark). My results show that both laypeople and people with marketing experience are prone to making judgments about consumers' preferences based on consideration set sizes. An intervention to debias people from spontaneously reaching conclusions based on the sizes of consideration sets only (based on the findings of Study 6, for example) may help marketers and sellers to more effectively price and promote their products.

Implications for Consumers. My results have implications for when people make choices for other consumers (Liu, Dallas and Fitzsimons 2019; Yang and Urminsky 2018). As illustrated in the studies, people may choose to give a specific gift to a smaller-set chooser, inferring that they may like it better than a larger-set chooser. Moreover, limited resources in delegated choice may result in individuals with larger consideration sets not receiving their desired options, despite expressing more flexibility in their preferences.

Further, observers' inferences from others' consideration set sizes may even affect the observers' own choices. Consumers often solicit recommendations from others. In interpersonal contexts, it is common for recommenders to simply express what they themselves liked, rather than giving recommendations based on what others may like. For instance, a friend may tell another friend about the movies they enjoyed. If one friend names multiple movies they liked, while the second friend names fewer movies, the listener may conclude that the second friend enjoyed the movies more and is more likely to choose one to watch from their recommendations.

Lastly, astute consumers who are aware of this tendency may be able to leverage the sellers' reliance on consideration set size when making inferences and strategically express their

consideration set accordingly. For example, in the earlier context of car purchases, people may intentionally express a larger consideration set to understate their interest in purchasing a specific vehicle and negotiate a lower price from the seller. However, this strategy may backfire in other settings where there are limited resources. For instance, if there is only one unit of the model the customer wants, the seller may prioritize offering it to another customer expressing a smaller consideration set.

Remaining Questions and Directions for Future Research

In my studies, people's tendency to infer stronger liking from smaller consideration sets is strong, highly consistent, and robust to further information such as people's ranking of options and prior purchase frequency. However, I found that this inference can be attenuated when the superordinate category structure of a consideration set is made salient (e.g., by presenting multiple categories), likely because it draws people's attention to the category-liking of the choosers. Future research can examine whether there are other moderators or boundary conditions of the effect. For example, I did not examine how the composition of the consideration set (i.e., the options that comprise the consideration set) impacts inferences. The specific options in the consideration set may matter, if some options are more diagnostic than others in signaling preferences. If the extra items in the smaller-set chooser's consideration set consist of generic items that are generally popular (e.g., movies that everybody watched), while a larger-set chooser's consideration set includes more unique and curated items (e.g., niche movies), people may infer the larger-set chooser has a stronger liking of the options, overriding their inferences based on the size of the consideration sets. Such situations where the specific

options convey information about people's preferences may serve as a boundary condition for the observed effect.

There are also more questions to be explored about what people infer from others' consideration sets. For example, how do people infer which person would like an option that is in neither chooser's consideration set? One possibility is that people infer the larger-set chooser would like that option more, based on the inferred category-liking, because considering an option outside the chooser's consideration sets draws attention to its category belonging.

Lastly, future research can examine in what contexts people's inferences from consideration set sizes lead to larger or smaller errors in predicting others' preferences. In some situations, it may be correct to judge that people with smaller consideration sets indeed place higher values on their options. This may be the case when everyone must take an option from the consideration set, or when people do not have any outside option. Everyone needs a place to live but people often may only consider apartments within a certain radius of their work. When the smaller consideration set reflects more specific and unique needs that must be fulfilled, smaller options in the consideration set may represent higher value from that individual compared to another individual with more options in the set. Field research where individual sellers make these decisions every day (e.g., car dealerships or real estate as in my examples) can further test if the effect generalizes in real transaction contexts and quantify the consequences of the mistakes (as I did in study 7).

Conclusion

I document the effect of consideration set sizes on how people infer others' preferences. People infer that others with smaller consideration sets have a stronger liking for an option than those with larger consideration sets. They simultaneously perceive those with larger consideration sets as having a greater preference for the overall category. When the superordinate category structure of the consideration set is more salient, however, people are more likely to incorporate this judgment on category-liking to deduce their prediction of others' option-liking. Compared to standards from a common choice model or people's actual willingness to pay, these inferences can lead to mispredictions and inefficient choices.

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