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Time Periods Feel Longer When They Span More Category Boundaries: Evidence from the Lab and the Field

Kristin Donnelly, Giovanni Compiani, and Ellen R.K. Evers

Abstract

Seven experiments (total N = 3,509) and a large field data set (N = 1,820,671) demonstrate that time periods of equal duration are not always perceived as equivalent. The authors find that periods feel longer when they span more time categories (e.g., hour, month). For example, periods like 1:45 P.M.–3:15 P.M. and March 31–April 6 (boundary-expanded) feel longer than, say, 1:15 P.M.–2:45 P.M. and April 2–April 8 (boundary-compressed). Reflecting this, participants anticipated completing more work during boundary-expanded periods than during equivalent boundary-compressed periods. This effect appears to result from the salience and placement of time boundaries. Consequently, participants preferred scheduling pleasant activities for boundary-expanded periods and unpleasant activities for boundary-compressed periods. Moreover, participants were willing to pay more to avoid—and required more money to endure—a long wait when that wait was presented as boundary-expanded. Finally, data from more than 1.8 million rideshare trips suggest that consumers are more likely to choose independent rides (e.g., UberX) when they are boundary-compressed when the alternative shared option (e.g., UberPool) is boundary-expanded. Together, our studies reveal that time periods feel longer when they span more boundaries and that this phenomenon shapes consumers' scheduling and purchasing decisions.

Keywords

time perception, scheduling, categories, estimation, field data

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Many consumer decisions involve estimating duration. For example, consumers predict how long a vacation will be enjoyable, whether they will arrive on time to a flight when using public transit, or how long to diet to reach their target weight. Although time is a continuous variable, we know consumers do not always experience time as linear. For example, supporting the adage "time flies when you're having fun," consumers feel like fun events pass faster than less enjoyable events of equivalent length (Droit-Volet and Gil 2009). In this article, we investigate how consumers anticipate duration and explore its consequences for consumer decisions. Specifically, we examine how consumers estimate the duration of equivalent lengths of time that cross more or fewer (natural) boundaries. We find that consumers reliably estimate time periods that cross more of such boundaries to last longer than equivalent periods that cross fewer. For example, consumers believe 1:45 P.M. to 2:15 P.M. (which crosses the 2 P.M. boundary) feels longer than the equivalent 1:15 P.M. to 1:45 P.M. does (which does not cross a whole hour boundary).

In what follows, we develop a framework for time estimation embracing insights from the categorization literature. This framework assumes a top-down influence of naturally salient category boundaries on estimates of duration. This contrasts with dominant paradigms offering bottom-up attentional accounts of consumer misestimation. Although this article examines only time estimation, our categorization-based framework may provide a parsimonious explanation for a variety of findings related to consumer estimation more broadly. Our findings also have clear practical implications, as the estimation of duration plays an implicit role in a wide range of routine choices —including but not limited to scheduling and time/money trade-offs for deliveries and waiting times. Knowing when

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and why consumers may over- or underestimate duration can help us better predict their decisions and will inform marketing professionals on how to position different options to maximize consumer and company welfare.

Theoretical Background

Time Perception

Although time is a continuous variable, a large body of research finds that consumers do not always treat it as such. For example, consumers experience time as passing more quickly during events that are enjoyable compared with equal-duration events that are not (Droit-Volet and Gil 2009). Time also seems to pass faster when it is uninterrupted compared with when it involves several disruptions (Thomas and Brown 1974). These studies specifically examine the experience of time, but many decisions involve its *anticipation*—a consumer must schedule a dentist appointment, forecast the disutility from a long layover instead of a more expensive direct flight, decide whether to cook or order takeout, and much more. All these decisions involve some sort of estimation or prediction of how much time different events will take. Here as well, the current literature suggests that such estimates may be biased. For example, recent work finds that consumers anticipate positive, enjoyable events to feel shorter than equally distant, equal-duration negative events (Tonietto et al. 2021). Similarly, other research finds that an interval of time is anticipated to feel shorter when it ends with a loss (e.g., having to move into a smaller, worse office) instead of a gain (e.g., having to move into a bigger, better office) (Bilgin and LeBoeuf 2010). In short, the existing literature in marketing suggests that experiential and contextual features can affect estimates of duration, though a cohesive framework for understanding these different effects is lacking.

The current article advances our understanding of when and why consumers over- and underestimate duration. Specifically, we propose that consumers categorize time according to salient (natural) boundaries. This premise prompts several directly testable predictions about time estimation. Furthermore, it allows us to predict and test ways in which the categorization of time affects consumer preferences, valuation, and choices. Finally, we discuss how our framework may also elucidate existing findings in the literature.

Categorization

Categorization details how information is organized and is fundamental to cognition (Medin and Schaffer 1978; Tversky and Hemenway 1984). Despite competing theories and some open questions (e.g., Barsalou 1982), the categorization literature largely agrees on the same basic principles (Vergne and Wry 2014). Categorization facilitates reasoning and communication by organizing stimuli into hierarchical groups defined by salient characteristics (Medin and Smith 1981; Rosch 1999; Rosch and Mervis 1975). For example, an iPhone belongs to the category "smartphones" (phones with internet access), which itself lies in the "mobile phones" category, a subcategory of "electronics." Consumers categorize objects (Mervis and Rosch 1981) and people (Cantor and Mischel 1979) but also abstract concepts such as events (Abelson 1981). The categorization of time can readily be seen via months, such as December and January belonging in the winter category, whereas July and August belong to the summer category.

Whether they are abstract concepts or physical entities, stimuli that share category membership are typically more similar than stimuli that do not share category membership (e.g., Eiser and Stroebe 1972). However, consumers often infer similarity from category membership even when it is not accurate to do so. As a result, two stimuli that are equally similar or share the same psychological distance are judged to be more dissimilar (distant) when they belong to different categories compared with the same category. For example, consumers estimate two locations in different states to be farther apart than equidistant locations in the same state (Irmak, Naylor, and Bearden 2010; Maki 1982), and the same people are judged to be more different when they are members of different groups rather than the same group, even when group membership is uninformative (Locksley, Ortiz, and Hepburn 1980). Even colors appear more or less similar depending on whether one's language puts them in the same category or different ones (Winawer et al. 2007). In summary, pairs of stimuli that span category boundaries are perceived to differ more than stimuli that fall within the same boundaries.

If people indeed categorize time, we should observe similar top-down effects on judgments of psychological distance for points in time and, thus, judgments of duration. For example, we would expect consumers to judge February (winter) and April (spring) to be more distant from each other than March and May (both spring). Because the distance between the former points in time feels larger than the distance between the latter, we expect consumers to also believe that the *duration* of the former period is longer.

To predict the effects of categorization on perceived duration, we first need to know how consumers actually categorize time. Research on this topic, while limited, finds that people put present and future into coarse categories of "like the present" and "not like the present" (Tu and Soman 2014) and organize the narrative of their lives into chapters (Thomsen 2009). In addition, consumer behavior often changes around salient temporal landmarks, such as a new age decade (Alter and Hershfield 2014), year, or month (Ayers et al. 2014; Dai, Milkman, and Riis 2014), in support of the idea that consumers view those landmarks as indicating a new category of time. Work on temperature estimation further suggests that months provide natural categories; consumers expect temperature to differ more between consecutive days in different months (e.g., March 31 and April 1) rather than the same month (e.g., April 1 and April 2; Krueger and Clement 1994).

Defining exactly what consumers may use to categorize time is difficult, as categories are quite flexible and often determined by goals or other features in the environment (e.g., Barsalou 1982; Kahneman and Miller 1986). Categories are also hierarchical, and stimuli are typically categorized at the most basic level at which they meaningfully differ ("the principle of cognitive economy"; see Rosch 1999). Consequently, the same stimuli can share a category in one context but not another (e.g., Evers, Imas, and Kang 2021; Graesser et al. 1980). For example, a consumer facing an assortment of wine and beer will likely automatically categorize items as either wine or beer, but if the assortment only contains wine, they are likely to group by categories such as red and white. Similarly, though some features are more readily categorized than others, this perception is also fairly malleable, and making features more or less salient changes which ones are used for categorization (e.g., Markman and Hutchinson 1984). Combined, the literature on categorization suggests that consumers typically categorize on salient features, whether they are naturally salient (Sloutsky 2003) or salient because attention has been drawn to them (Pieters and Wedel 2004; Tversky 1977).

To determine which features consumers naturally use as category boundaries, we draw on work on the fresh start effect (Dai, Milkman, and Riis 2014) and temporal focal points (Allen et al. 2017), which finds that the starts of new days, months, and years are particularly salient markers for setting and pursuing goals. Combining this with the logic of hierarchical categorization, we would expect consumers to naturally categorize time around salient landmarks on the highest differentiating level. Specifically, we expect the most differentiating category to be year for periods that start and end in different years and month when periods span distinct months (but not distinct years), and for even shorter durations, we expect whole hours to provide category boundaries. Consequently, we expect consumers to perceive time periods that span more such boundaries as longer than periods spanning fewer. We shall call the former periods "boundary-expanded" and the latter periods "boundary-compressed."

Alternative Explanations for Consumer Misestimation

We propose that categorization provides a top-down mechanism for predicting when and why estimates of time may be biased; that is, the consumer's knowledge structures affect how they evaluate stimuli. This lies in stark contrast with the explanation for a famous misestimation phenomenon in pricing, namely, left-digit bias. Left-digit bias is an attentional bias in which consumers either overweigh the leftmost digit or neglect digits that are not leftmost (Manning and Sprott 2009; Thomas and Morwitz 2005). It is thought to explain consumers' sharp drop in demand when prices increase in the leftmost digit. For example, Strulov-Shlain (2021) found that consumers were not any less likely to buy a product when it was priced at \$4.99 instead of \$4.98 but bought about 3% to 5% fewer units of that product when its price was \$5.00 instead of \$4.99.

Of course, left-digit bias and a categorization-based account would make similar predictions in certain situations. Both would predict that consumers judge, say, 1:45 P.M. to 2:15 P.M. (boundary-expanded) to feel longer than 1:15 P.M. to 1:45 P.M. (boundary-compressed). In the case of left-digit bias, a consumer would attend only to the leftmost digits—the hour marks—making the first period appear longer than the second. Under our framework, the first period starts and ends in different categories, whereas the second period does not. However, across several situations, our framework makes opposite predictions to the ones made by attention-based accounts. For example, because consumers typically use categories at the most basic level at which differences occur, our model would predict what is essentially a right-digit bias for time periods spanning years. Similarly, we assert that making alternative forms of categorization salient affects perceived duration; left-digit bias would not make this prediction. In other words, although left-digit bias cannot accommodate all our predictions, our framework based on categorization can accommodate all previous findings on left-digit bias.

Combined, this logic leads to the following hypotheses we test in this article:

> **H_1:** Time periods that cross more temporal category boundaries are estimated to last longer than equivalent time periods that cross fewer boundaries.

H_1 provides our theoretical backbone: consumers naturally categorize time, and these categories affect anticipated duration. In most of the studies that follow, we rely on natural temporal boundaries, such as the start of an hour, month, or year, as these are involved in many routine consumer decisions.

Next, because categorization is hierarchical, we expect that the categories (and thus category boundaries) that are salient will be the highest-level categories that contain meaningful differences. Therefore, we further predict the following:

> **H_2:** When periods are expressed in multiple temporal units (e.g., hour and minute, month and year), estimation is affected by boundaries at the largest possible unit of measurement (e.g., hour marks for time periods spanning minutes, year marks for periods spanning months).

Because such hierarchical organization is a critical part of categorization, H_2 allows us to test for evidence supporting the notion that categorization biases time estimation. It also allows us to contrast predictions from our categorization-based account with the predictions of bottom-up attentional accounts such as left digit bias.

Finally, categories are flexible and can be created on the spot by drawing attention to (non)shared features. As stated previously, salient categories should be the highest-level categories at which a period's start and end time differ. But salience is also determined by how the period is expressed—for example, the hour category would not be salient for a period that is expressed as 90 minutes. We predict the following:

> **H_3:** Upon introducing and expressing periods in terms of new category boundaries, time periods crossing more of

those imposed boundaries (boundary-expanded) are estimated to last longer than would equivalent periods that cross fewer of these boundaries (boundary-compressed).

Because our effects rely on category boundaries that naturally stand out (e.g., temporal focal points), making alternative boundaries more salient instead should lead consumers to overestimate time periods that cross more (rather than fewer) of those alternative boundaries. For example, if a new activity starts every hour on the half-hour (e.g., 9:30 A.M.), we expect that the half-hour mark would be more salient than the boundary provided by a new hour (e.g., 9:00 A.M.). This would also mean that 10:20 A.M.–10:40 A.M. would seem longer than, say, 9:50 A.M.–10:10 A.M.

Because many routine consumer decisions involve time/money trade-offs, we would expect these estimation biases to affect those decisions. A consumer is expected to perceive a negative event even more negatively if it is boundary-expanded and thus perceived to last longer. For that same reason, a positive experience may become more attractive when it is boundary-expanded. Our final hypotheses involve the effects of category boundaries on consumer decisions. Because they overestimate durations that cross more (vs. fewer) natural boundaries,

H_{4a}: Consumers prefer to schedule pleasant experiences during periods that cross more category boundaries but prefer periods that cross fewer category boundaries when scheduling unpleasant experiences.

H_{4b}: Consumers are willing to pay more to avoid a waiting period that crosses more category boundaries compared with an equivalent one that crosses fewer. They also require more compensation to endure that boundary-expanded wait (vs. a boundary-compressed one).

H_{4c}: When requesting a rideshare, consumers are less likely to select a shared ride when that option would cross a salient hour boundary and the (faster) independent ride would not.

Overview of Studies

We begin with Study 1, which tests H_1 using a behaviorally valid measure, and briefly summarize the numerous pilot studies exploring the basic premise. Across these studies, we reliably find that consumers estimate time periods to last longer when they are boundary-expanded compared with boundary-compressed. Studies 2a and 2b test H_2, varying the duration of periods in a way that changes which categories are salient and thus which boundaries influence the perception of duration. Study 3 complements this, testing H_3—drawing attention to a different form of categorization results in those new category boundaries affecting perceived duration.

The next three studies examine behavioral consequences. Study 4 finds that participants prefer to schedule aversive experiences during boundary-compressed periods (that feel shorter) and enjoyable experiences during boundary-expanded ones (that feel longer). Study 5 varies the boundary-expansiveness of long wait times and elicits consumer willingness to pay (WTP) to avoid the wait and the payment they require to endure it. Finally, Study 6 uses archival transportation data from Chicago to study the real-world impact of boundary-expansiveness on consumer behavior. Our analysis of more than 1.8 million rideshare trips suggests that consumers are more likely to choose an independent ride when it is boundary-compressed and the shared ride option is boundary-expanded compared with when both rides are boundary-compressed or expanded.

The design, hypotheses, sample size, and analyses of all experimental studies reported herein were preregistered. The analysis plan and treatment of the rideshare data set were prespecified. For all studies, we report all data exclusions, all manipulations, and all measures. We note here that analyzing the data without exclusions does not meaningfully affect the results of any study; those analyses are presented in the Web Appendix. In each experiment, trials were presented in random order, and for those involving choosing between time periods, the visual position of options was also randomized. Finally, because the expression of time varies in different countries, we recruited participants from the United States in every study except Study 2a (which intentionally sampled from the United Kingdom). Our preregistrations, materials, data, and code are available at https://osf.io/dav53/.

Study 1: Establishing the Effect of Boundary Expansiveness

Study 1 tests the basic effect of boundary-expansiveness using a behaviorally valid measure. Specifically, we asked Amazon Mechanical Turk workers (MTurkers) to predict numbers of human intelligence tasks (HITs) they expect to be able to complete during various time periods. We used a between-subjects design; all periods were either expanded or compressed. Per H_1, we expected that MTurkers would estimate being able to complete more HITs during boundary-expanded periods compared with boundary-compressed periods of equal length.

Method and Procedure

Our participants were 612 MTurkers (53.8% female, 44.9% male, 1.8% other; $M_{age} = 37$ years), and after our preregistered exclusions, we were left with a final sample of 576.

Participants were told to imagine they had a day completely free to do MTurk work and were asked to estimate how many HITs they could accomplish in a given period, answering on a slider from "0" to "500 or more." Following our preregistration, responses at either of these end points were eliminated. We manipulated boundary-expansiveness between subjects; that is, participants were randomly assigned to view either five

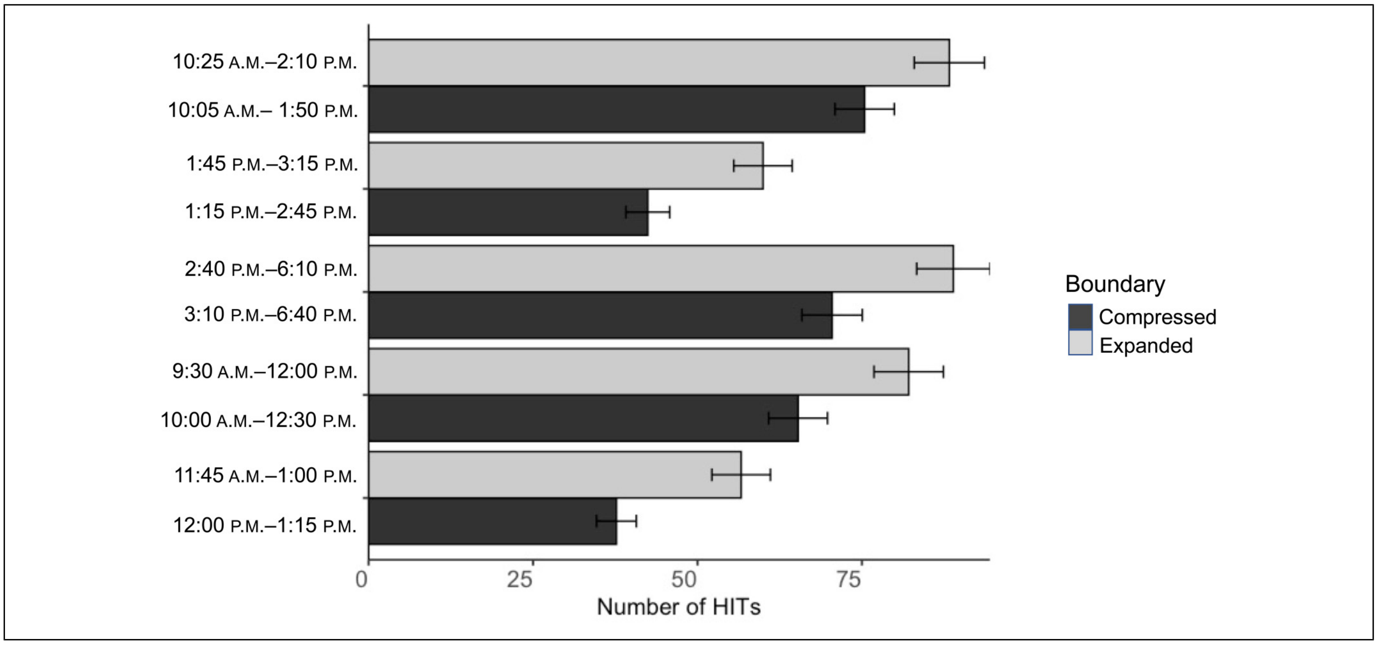


Figure 1. MTurk workers estimated that they could complete more HITs during boundary-expanded periods compared with boundary-compressed (Study 1).
Notes: Error bars represent the standard error of the mean.

boundary-expanded or five boundary-compressed periods. Figure 1 displays these periods and the results.

Results and Discussion

A mixed-effects negative binomial model regressed number of HITs on boundary condition (expanded vs. compressed) and specified random effects of question and participant. We opted to use negative binomial regression because it is far less susceptible to producing false positives than Poisson regression is (Gardner, Mulvey, and Shaw 1995; Ryan, Evers, and Moore 2021). As we predicted, participants considering boundary-expanded periods estimated that they could perform more HITs ($M = 75.13$, $SD = 84.37$) than did those considering equivalent boundary-compressed periods ($M = 58.23$, $SD = 71.44$; $z = 3.16$, $p = .002$, $d = .22$). These results support our basic premise that time periods feel longer when they span a greater number of time categories.

Providing further support, the Web Appendix reports a series of studies in which participants indicated how long different time periods felt (studies W1–W8 in the Web Appendix). We find that periods are selected (W1) and rated (W2) to feel longer when they span more hours; this holds when participants first calculate their duration (W3), when periods lend themselves to rounding (W4), and when they are shown on numberless clocks (W6) or described verbally (W7). The effect is robust to different definitions of boundary-expansiveness, such as when periods span months (W5) or seasons (W8). Given space constraints, Table 1 offers a brief overview; full details are available in the Web Appendix.

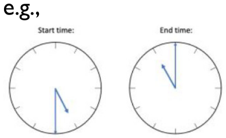
Studies 2a and 2b: Hierarchical Boundaries

Because categories are hierarchical, we expected that the effect observed in the previous studies is a consequence of hours being the most differentiating unit (rather than a consequence of left-digit bias specifically). The next two studies explore whether this is true by varying the position of the differentiating units. To do so, Study 2a employs situations in which the differentiating units occur in the middle of the expression rather than on the left. Specifically, Study 2a employs months as boundaries and uses participants in the United Kingdom, where the day-month-year format puts month in the middle position (rather than the leftmost). This design allows us to contrast predictions made by our categorization-based framework with predictions made by models assuming attentional biases.

Study 2a

Participants and procedure. Participants were 203 Prolific Academic workers (56.2% female, 39.9% male, 3.9% did not answer; $M_{age} = 37$ years) recruited from the United Kingdom. A final sample of 152 remained after our preregistered exclusions. Participants indicated how long five distinct time periods felt on a 101-point scale from 0 (“does not feel long at all”) to 100 (“feels extremely long”). Participants evaluated each period twice; it was presented once as boundary-expanded (spanning more month categories, e.g., October 30 to December 10), and once as boundary-compressed (e.g., November 1 to December 11). Thus, participants rated a total of ten periods.

Table 1. Summary of Supplemental Studies Demonstrating That Time Periods Feel Longer When They Span More Category Boundaries.

Study	Description of Stimuli	Stimuli	Dependent Measure	Result
W1	Pairs of equal-duration periods (one expanded, one compressed).	**Boundary-expanded:** 7:30–10:00, 2:45–5:00, 2:45–4:30, 10:30–1:00, 1:30–6:00 **Boundary-compressed:** 7:00–9:30, 3:00–5:15, 3:00–4:45, 10:00–12:30, 1:00–5:30	"Which period feels longer?"	Boundary-expanded periods selected more often than compressed (68% vs. 32%, respectively; $z = 4.94$, $p < .001$)
W2	Periods shown one at a time.	Same periods as W1	"How long does it feel?" (0–100)	Boundary-expanded periods ($M = 43.14$, $SD =$ 23.31felt longer than compressed ones ($M = 38.15$, $SD = 24.04$; $t = 4.84$, $p < .001$)
W3	Pairs of equal-duration periods (one expanded, one compressed).	Same periods as W1	"Which period feels longer?" – answered after calculating duration	Boundary-expanded periods selected more often than compressed (75% vs. 25%, respectively; $z = 6.93$, $p < .001$)
W4	Boundary-expanded and compressed periods shown one at a time. For half the periods, rounding would make the expanded period feel longer than the compressed.	**Unlikely to be rounded** e.g., 6:30–10:00 (boundary-expanded) 7:00–10:30 (boundary-compressed) **If rounded, expanded feels longer** e.g., 6:40–10:10 (boundary-expanded) 7:20–10:50 (boundary-compressed)	"How long does it feel?" (0–100)	Boundary-expanded periods felt longer than compressed ($Ms = 35.96$ and 31.51; $SDs = 23.97$ and 24.14; $F(1, 2308.1) = 33.14$, $p < .001$). Effect didn't differ by rounding type, $F(1, 2307.1) = .035$, $p = .85$
W5	Pairs of equal-duration periods (one expanded, one compressed). Periods spanned months.	e.g., 5/31–6/1 (boundary-expanded) 5/24–5/25 (boundary-compressed)	"Which period feels longer?"	Boundary-expanded periods selected more often than compressed (73% vs. 27%;, $z = 6.91$, $p < .001$)
W6	Boundary-expanded and compressed periods shown one at a time with numbers (digital condition) or on numberless clocks (analog condition).	**Digital condition** e.g., **Analog condition** e.g., 	"How long does it feel?" (0–100)	Boundary-expanded periods ($M = 48.63$, $SD = 24.13$) felt longer than boundary-compressed ($M = 43.04$, $SD = 24.04$), $F(1, 2505.5) = 87.17$, $p < .001$. Effect didn't differ by presentation format, $F(1, 2505.5) = .35$, $p = .556$.
W7	Boundary-expanded and compressed periods were expressed in terms of the same start and end hour. Periods shown one at a time.	e.g., Quarter to two to four o'clock (boundary-expanded) Quarter past two to half past four (boundary-compressed)	"How long does it feel?" (0–100)	Boundary-expanded periods ($\underline{M} = 45.30$, $SD = 25.21$) felt longer than boundary-compressed ($\underline{M} = 43.73$, $SD = 25.46$), $t = 3.16$, $p = .002$.
W8	Pairs of equal-duration periods (one expanded, one compressed). Periods spanned seasons.	e.g., April to June (boundary-expanded) March to May (boundary-compressed)	"Which period feels longer?"	Boundary-expanded periods selected more often than compressed (79% vs. 21%, respectively, $z = 6.91$, $p < .001$)

Notes: Full details are available in the Web Appendix.

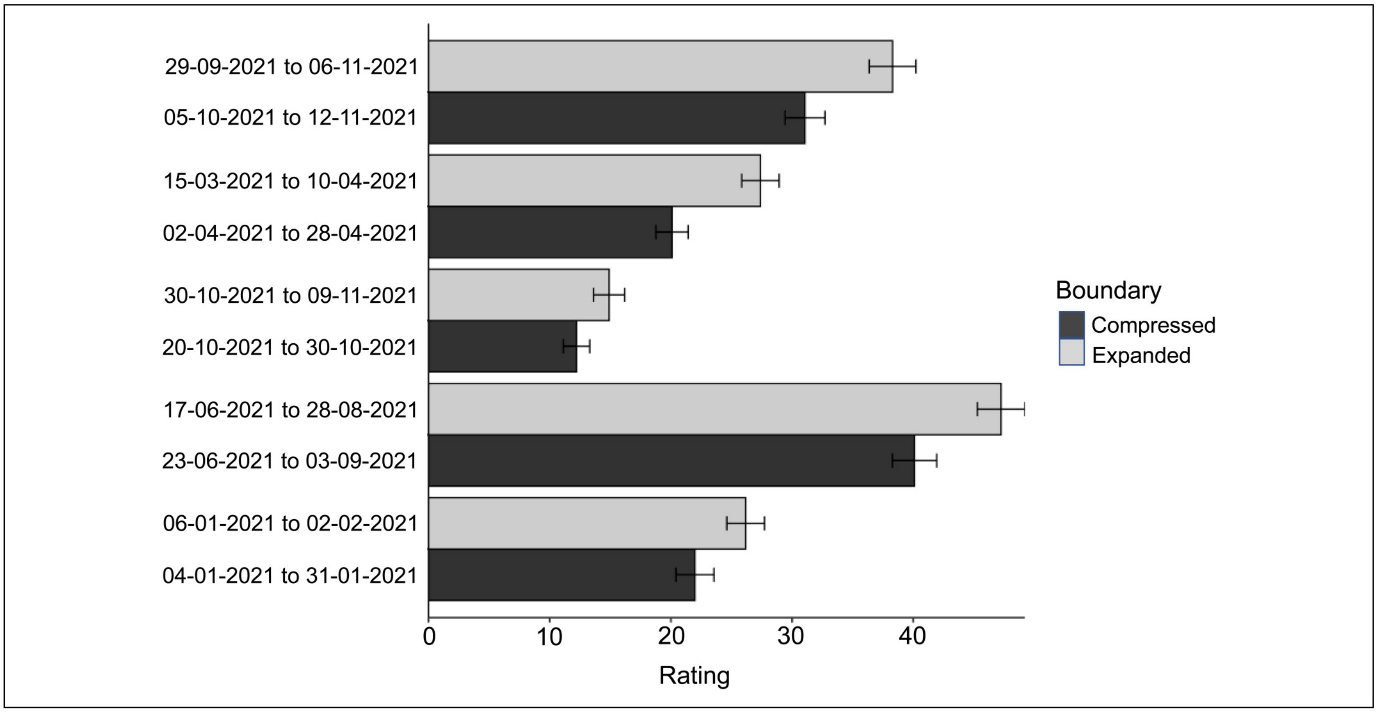


Figure 2. Time periods spanning more month categories felt longer (Study 2a).
Notes: Periods are expressed as day-month-year and rated by participants in the United Kingdom. Error bars represent the standard error of the mean.

Importantly, adopting the format used in the United Kingdom (and most countries outside the United States), dates were expressed as day-month-year (see Figure 2 for these periods and the results).

Results and discussion. A mixed-effects model regressed ratings of perceived length on boundary type (expanded vs. compressed), with random effects of participant and question. As predicted, boundary-expanded periods ($M = 30.80$, $SD = 23.38$) were rated to feel longer than equivalent compressed ones ($M = 25.09$, $SD = 20.94$; $t = 9.00$, $p < .001$, $d = .26$). These results demonstrate that judgments are influenced not by the leftmost digit but rather by the *most differentiating* unit. We report an additional study with similar logic to that of Study 2a in the Web Appendix (Study W8). Here, we use seasons as natural boundaries. Consistent with our expectations, we find that participants believe equivalent time periods to last longer when they cross (vs. do not cross) into new seasons. We continue to examine the effect of differentiating units in the next study by manipulating whether the same units are or are not the most differentiating.

Study 2b

Study 2b tests the hierarchical assumption discussed previously—that when judging duration, consumers attend to the largest unit of measurement (or highest-level category) that differentiates a period's start and end time. Specifically, we examine how the month category affects judgments when the higher-order year category does or does not differentiate start and end time. We also test generalizability by manipulating the periods' temporal location (past vs. future).

Participants and procedure. Participants were 257 Prolific Academic workers (45.1% female, 53.7% male, 1.2% did not answer; $M_{age} = 36$ years). Due to a coding error, no attention check was employed in this study.[1]

Participants rated the perceived duration of time periods on the same 101-point slider used in Study 2a. This study involved four periods of the same duration, each occurring in different times of the year; see Figure 3 for a full list of periods. Temporal location varied within subject such that two periods occurred in the past and the other two in the future, starting in a year randomly selected from the past 20 or next 20 years, respectively. Boundary-expansiveness varied in terms of months: Each period was shown once as boundary-expanded (spanning more distinct months) and once as boundary-compressed (spanning fewer) for a total of eight judgments. For half of the participants, the time periods always started and ended in the same year (e.g., 10-20-2021 to 11-30-2021), making month the largest differentiating category. For the other participants, periods were exactly one year longer, starting and ending in different (adjacent) years (e.g., 10-20-2021 to 11-30-2022), making year the largest differentiating category.

[1] This attention check was similar to that of the bus scenario, but the wait was four hours shorter. As preregistered, we removed participants who had a higher WTP here than they did on either bus trial.

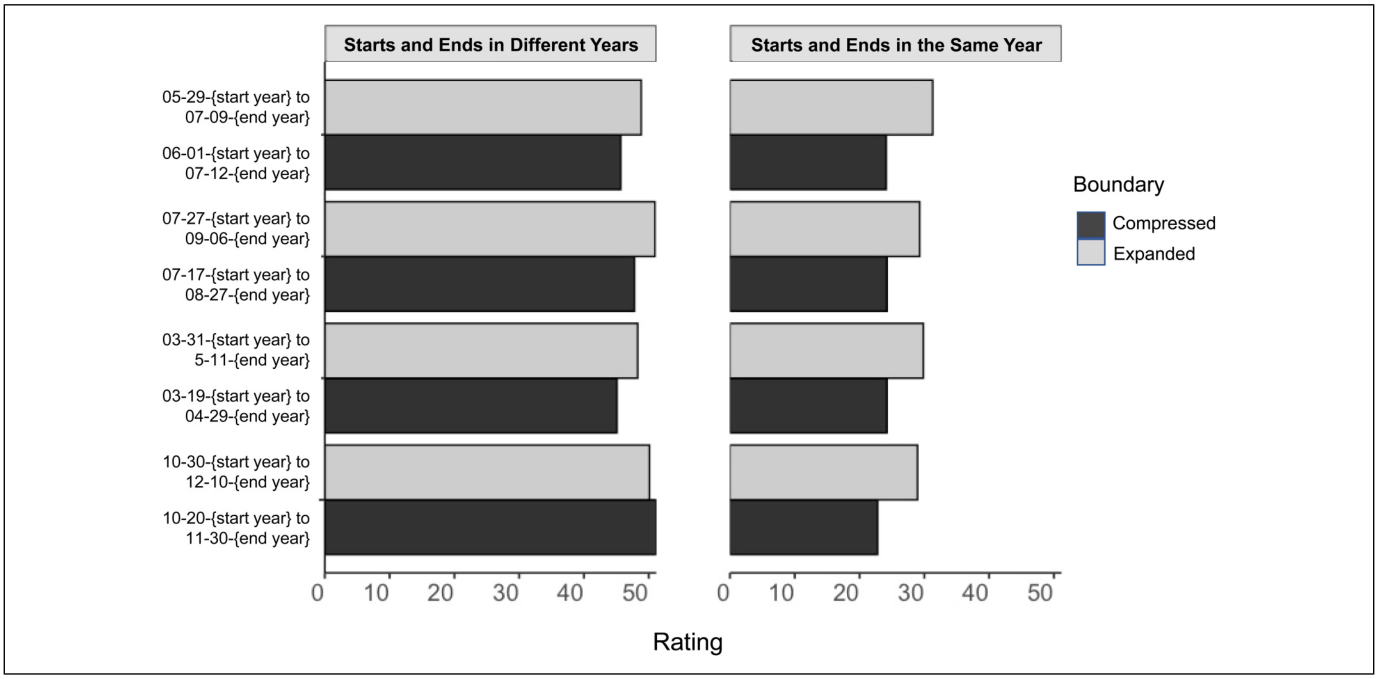


Figure 3. Time periods spanning more month categories felt longer (Study 2b).
Notes: Start and end year were either the same (right column) or different (left column).

Thus, this study used a 2 (boundary: expanded vs. compressed) × 2 (temporal location: past vs. future) × 2 (start and end year: same vs. adjacent) mixed design.

Results. A mixed-effects analysis of variance (ANOVA) regressed ratings of perceived length on boundary type (expanded vs. compressed), temporal location (past vs. future), and start and end year (same vs. adjacent), allowing for the interaction of these three factors along with random effects of participant and question. Unsurprisingly, participants assigned to see periods that span adjacent years rated them as longer (M = 48.50, SD = 25.27) than did participants who rated periods that started and ended in the same year (M = 26.84, SD = 23.52; $F(1, 255.43) = 68.67$, $p < .001$, d = .89). Once again, boundary-expanded periods (M = 39.48, SD = 26.34) felt longer than boundary-compressed did (M = 35.39, SD = 26.87; $F(1, 1{,}779.94) = 48.94$, $p < .001$, d = .15). Interestingly, periods in the future (M = 39.04, SD = 27.30) felt longer than those in the past (M = 35.65, SD = 25.86; $F(1, 1{,}787.09) = 36.11$, $p < .001$, d = .13), but the effect of boundary-expansiveness was the same for past and future periods ($F(1, 1{,}779.94) = .616$, $p = .433$).

Our primary term of interest was the interaction between boundary type and year, which was the only significant interaction in the model, $F(1, 1{,}779.94) = 11.08$, $p < .001$. The results reflect the hierarchy we anticipated. For participants who saw periods that always started and ended in the same year, boundary-expanded periods (M = 29.85, SD = 23.61) felt longer than compressed ones (M = 23.85, SD = 23.06; $t = 7.38$, $p < .001$). But for participants who saw periods that started and ended in different (adjacent) years, this difference was strongly attenuated, reaching marginal significance (Ms = 49.56 and 47.45, SDs = 25.27, 25.23; $t = 2.57$, $p = .051$). Figure 3 illustrates this finding.

Discussion. Because of the hierarchical nature of categories, we predicted that the effect of boundary expansiveness depends on whether it occurs on the most differentiating unit. Consistent with this prediction, we found that the effect of boundary expansiveness (of a period's month unit) strongly attenuated when the periods spanned multiple years, and thus, months were no longer the most differentiating factor. That is, when the higher-order year category did not differentiate start and end time, consumers attended to the month category. Conversely, they relied *less* on month category differences when every period spanned two adjacent years.

Combined, Studies 2a and 2b provide support for H_2, which posits that the effect of boundary-expansiveness results from consumers perceiving differences in the highest-order category that differentiates start and end time.

Study 3: Manipulating Categories

The previous studies rely on the natural variation of time to make equivalent periods span more or fewer categories. In Study 3, we experimentally manipulate which categories are salient. According to our third hypothesis, we should be able to evoke different kinds of time categories—beyond the "natural" ones provided by, say, hour, month, or year—and observe overestimation of periods that span more of them. To test this, Study 3 prompted participants to evaluate a period that spanned different classes in a student's schedule. That is, the period was either boundary-compressed (spanning Classes B and C) or expanded (spanning Classes A, B, and C).

Orthogonally, we manipulated the class schedule such that the period was either boundary-expanded or boundary-compressed in terms of hours. Because this study describes the period in terms of classes, not hours, classes should be more salient; thus, we predict that the period feels longer when it spans more classes, regardless of how many hour categories it spans.

Method and Procedure

Sixteen hundred ten MTurkers participated; as per our preregistration, none were excluded. We did not collect demographics for Studies 3 or 4.

In a 2 (number of class boundaries: two vs. three) × 2 (number of hour boundaries: two vs. three) between-subjects design, participants saw a hypothetical schedule for three 60-minute classes (Classes A, B, and C). Half saw a schedule in which classes started every hour on the hour:

Class A: 9:00–10:00
Class B: 10:00–11:00
Class C: 11:00–12:00

For the other half of participants, a new class started every hour on the *half-hour*:

Class A: 9:30–10:30
Class B: 10:30–11:30
Class C: 11:30–12:30

Participants were then asked to consider a 1-hour, 40-minute period, manipulated between-subject to span either all three classes or only two. Those assigned to a three-classes condition read,

> Shelly charged her cell phone for the last thirty minutes of Class A, all sixty minutes of Class B, and the first ten minutes of Class C.

Participants in the two-classes condition saw a slightly different version:

> Shelly charged her cell phone for all sixty minutes of Class A and the first forty minutes of Class B.

Just as before, participants rated the period for "How long does it feel?" on a 101-point slider.

Because in this design, participants' attention is drawn to the start and end of classes, and those boundaries are thus especially salient, we predicted that they would judge the same time period as lasting longer when it spanned three rather than two classes, reflecting H_3.

Results and Discussion

We performed a 2 (number of classes: two vs. three) × 2 (number of hours: two vs. three) ANOVA. As predicted, we observed a significant main effect of number of classes spanned. Specifically, the period was rated as feeling longer when it spanned three classes (and was thus boundary-expanded, $M = 67.94$, $SD = 23.44$) rather than two classes (boundary-compressed, $M = 64.53$, $SD = 24.56$; $F(1, 1{,}606) = 8.08$, $p < .01$, $d = .14$). Conversely, perceived duration did not differ when, in terms of the hour category, the period spanned three rather than two distinct hours ($Ms = 66.76$ and 65.73, $SDs = 24.27$ and 23.84, respectively; $F(1, 1{,}606) = .85$, $p = .357$, $d = .04$). There was no interaction between number of hours and number of classes ($F(1, 1{,}606) = .631$, $p = .427$). In short, the period felt longer when it spanned more classes, regardless of whether it spanned more or fewer hours. This suggests that when periods span categories of different types, the effect of boundary-expansiveness is limited to the category that is most salient.

These results support H_3—that boundary-expansiveness according to idiosyncratic categories (i.e., events) affects perceived duration in the same way as the standard categories provided by, say, hour or month. People appear to rely on those standard categories when no others are salient; however, when we introduced more salient, alternative categories, those provided the boundaries that affected judgments. Interestingly, participants only responded to the newly imposed categories, with no additional influence from hour categories.

Given that time periods spanning more categories are estimated to last longer, boundary-expansiveness should affect consumer decision-making across a variety of contexts. Unpleasant experiences should be even more aversive when they are boundary-expanded rather than boundary-compressed, but positive experiences may be even more desirable when boundary-expanded. The next three studies examine how boundary expansiveness affects scheduling, valuation of delays, and finally, rideshare choices within a large set of real-world transportation data.

Study 4: Scheduling

Consumers may want to minimize the amount of time taken by unpleasant activities, such as getting cavities filled or going to the Department of Motor Vehicles (DMV). Alternatively, consumers may want to maximize time spent on enjoyable activities, such as exploring a new city or taking a much-needed nap. Thus, when scheduling activities for which they want to maximize time, consumers may prefer boundary-expanded time periods, and when scheduling activities for which they want to minimize time, boundary-compressed periods may be more appealing. Study 4 tested this hypothesis.

Method and Procedure

Participants were 601 Amazon MTurkers; 600 remained after excluding one person who failed the preregistered attention check. Participants were asked to plan eight different activities in one of two equal-duration time slots, one boundary-expanded and the other boundary-compressed. Four activities were those that consumers would typically prefer to expedite (time minimizing, e.g., getting blood drawn), and four were activities that they would likely want to savor (time-maximizing, e.g., watching the finale of a favorite show); see Table B5 in the Web Appendix for a full list of activities. To confirm this, a separate group of MTurkers ($N = 40$) rated each activity from 1 ("I would want to feel like I got it over with as fast as possible") to 7 ("I would want to feel like it lasted as long as possible"). The time-minimizing activities received a much lower score ($M = 1.48$, $SD = .88$) than the time-maximizing ones ($M = 6.11$, $SD = 1.14$). For each activity, we counterbalanced whether the boundary-expanded option occurred earlier or later than the boundary-compressed one. This design choice reduced the influence of time-of-day preferences (e.g., if an activity is generally preferred earlier or later in the day). Table B5 in the Web Appendix presents the results broken down by each activity and available time periods.

Results and Discussion

We performed a mixed-effects logistic regression of choice (boundary-expanded vs. compressed) on activity type (desirable vs. undesirable), with a random effect of participant. As we predicted, participants' choice of period differed between the two types of activities ($z = -5.53$, $p < .001$). For the desirable activities, participants selected boundary-expanded periods over compressed ones (52% vs. 48%; $z = 2.27$, $p = .023$), but for the undesirable activities, participants selected the compressed periods more often (56% vs. 44%; $z = -5.35$, $p < .001$).

In summary, we found that boundary-compressed periods were more attractive for scheduling activities people may want to expedite, such as visiting the DMV. In contrast, the expanded periods were more appealing for activities people may want to savor, such as a lunch break. In Study 5, we explore consequences for how consumers value their time—specifically, how much they are willing to pay to avoid a long wait and how much compensation they require to endure it.

Study 5: The Valuation of Time

Consumers frequently decide whether they want to spend money to save time or spend time to save money. This is particularly true in the domain of transportation, which is rife with long (and often painful) waiting periods. We hypothesized that consumers would be willing to pay more to avoid a long wait when it is boundary-expanded compared with boundary-compressed and will demand more compensation to endure it. Study 5 tested these predictions.

Method and Procedure

Three hundred two Prolific Academic workers (45.1% female, 53.7% male, 1.2% did not answer; $M_{age} = 36$ years) participated. After our preregistered exclusions, 260 remained. Each participant completed four trials in this 2 (expanded vs. compressed) × 2 (WTP vs. willingness to accept) design. The scenarios eliciting WTP and willingness to accept were shown in random order. For the WTP judgments, participants read the following scenario:

> Imagine that you get to the Greyhound bus station and learn that the tickets for the next bus are sold out, so you have to buy a seat on the bus that leaves at {*departure time*}. It is {*present time*} right now. The man next to you has a ticket for the bus that leaves in half an hour. What is the MOST you'd be willing to pay to switch tickets?

Participants responded on a slider scale from $0 (I would not be willing to pay anything for the ticket) to $300 or more. Those selecting the latter option were asked to enter their required amount; as preregistered, responses larger than the scale maximum ($300) were excluded. Then participants saw a modified version of the scenario ("Now imagine that it is {*present time*} right now, and the only available seats are on a bus that leaves at {*departure time*}") and again gave their willingness to pay to switch to an earlier bus. One of the bus scenarios was boundary-expanded, whereas the other was boundary-compressed; participants evaluated these in random order.

For the willingness-to-accept judgment, participants read the following:

> Imagine that you are waiting to board a plane, and the flight is overbooked. It is {*present time*} right now. The airline is offering to pay you to take a later flight that leaves at {*departure time*}. What is the lowest amount of money you'd have to receive in order to take the later flight?

As in the bus scenario, participants responded on a slider scale, this time ranging from $0 (I would not be willing to pay anything for the ticket) to $1,000 or more. Again, responses at the scale maximum were removed. After completing the first trial, they were presented with the second trial, ensuring that they responded to both a boundary-expanded and a boundary-compressed version of the same scenario (again, versions were shown in random order).

For each scenario, we counterbalanced time of day between participants such that the boundary-expanded wait ended either earlier or later in the day than the boundary-compressed. This did not affect our results; see Web Appendix C for more information.

Results and Discussion

For the flight scenario, a mixed-effects model regressed required compensation on waiting period (expanded vs. compressed), specifying a random effect of participant and rating order. As predicted, the amount of money participants required to take a later flight was higher when the waiting period was boundary-expanded ($M = 251.98$, $SD = 188.49$) compared with compressed ($M = 234.81$, $SD = 175.78$; $t = 4.44$, $p < .001$, $d_z = .29$). We ran the same model for the WTP scenario. As we predicted, participants were willing to spend more to take an earlier bus when the waiting period was expanded (M

$= 64.70$, $SD = 51.83$) compared with compressed ($M = 60.93$, $SD = 51.83$; $t = 2.97$, $p = .003$, $d_z = .20$).

These results reveal that boundary-expansiveness may affect consumer decisions about how to trade off time and money. Participants reported being willing to pay about 6% more to avoid a waiting period when it was boundary-expanded compared with boundary-compressed. Moreover, they required about 7% more compensation to endure an expanded (vs. compressed) wait.

Thus far, our studies have explored the effects of boundary-expansiveness through experiments and hypothetical scenarios (see Table 2 for a full summary). Our final study examines the effects of boundary-expansiveness on real-world transportation choices.

Study 6: Rideshare Choices

The results of the previous experimental study suggest that boundary-expanded wait times may make consumers more likely to upgrade to a faster, more expensive option. We now investigate whether these findings can also be detected in noisier real-world settings, specifically rideshare choices. There are two primary reasons for focusing on this market. First, rideshares are pervasive. Booking a rideshare is a common consumer decision in the modern world: Uber alone provides more than 14 million rides every day (Srivastav 2019), 20% of which are UberPool, and is the most frequently expensed vendor for business travelers (Hagen 2019). Second, the availability of very large data sets on rideshare decisions provides the statistical power needed to detect our hypothesized effects on actual consumer choices.

Consumers in metropolitan areas often have a choice between two types of trips: a faster trip wherein the consumer is the only rider or a cheaper, longer trip that the consumer might share with other passengers (instead of riding alone). They are given estimated arrival times for both options, and because the shared ride is always projected to arrive later, consumers sometimes face a choice set in which the shared option crosses into a new hour but the independent does not.

If the estimated arrival time crosses into a new hour for the shared option, but not the independent, the shared trip may seem relatively longer, increasing the likelihood that the consumer selects the independent option. Thus, we propose that if consumers face a "mixed" choice set—whereby the shared ride is boundary-expanded and the independent ride is boundary-compressed—they would be less likely to request the shared option than they would if the two options did not differ in boundary-expansiveness (i.e., if neither or both rides crossed into the next hour).[2] We test this hypothesis on a large data set of trips taken in the Chicago metropolitan area. We describe the data in the next subsection and then develop and estimate a model of consumer choice.

[2] As we discuss subsequently, we only consider trips that last at most 60 minutes.

Data and Variables Construction

Our data were retrieved from the Chicago Transportation Network via the city's open data portal (https://data.cityofchicago.org). To our knowledge, Chicago is the only city that requires rideshare companies to publicly document all trips. Web Appendix D details all variables present in this data set and provides summary statistics (Table D1). Following the emerging standard procedure for analyzing large archival data, we used a split-half analysis. That is, we initially looked at roughly half the data (the exploratory half) and developed a plan for how to approach the remaining data (the confirmatory half). This approach grants researchers the flexibility needed to handle imperfect secondary data while maintaining low false-positive rates by preventing researchers from cherry-picking exclusion and inclusion criteria. After exploring the first half of the data, we preregistered a cleaning and analysis plan that we believe best reduced noise and prevented potential coding errors from affecting the subsequent analysis; this is critical given the already noisy nature of the data. This preregistration also reduced researcher degrees of freedom (Simonsohn, Simmons, and Nelson 2020): we committed to our analysis plan before seeing or interacting with the actual data on which we ran our analyses. We examined trips taken between November 2018 and May 2019 ($N = 1{,}559{,}675$) for the exploratory half and trips taken between June 2019 and December 2019 for the confirmatory half ($N = 1{,}820{,}671$).

After the exploratory phase, we preregistered various restrictions to reduce measurement error and confounds. For example, we removed the small number of trips with coding errors or missing data, and because our modeling strategy is only appropriate for trips that are less than 60 minutes, trips longer than that (.9%) were excluded (see Web Appendix D for more information). Another critical exclusion was time of day. Daytime rides present an obvious confound: one reason to select an independent ride, particularly when the shared ride is boundary-expanded, is to arrive on time to events that begin exactly on the hour (e.g., work at 9 A.M., a dinner at 7 P.M.). To mitigate this concern, we only examined trips that that began on weekdays between 1 A.M. and 5 A.M. These and other exclusion criteria are discussed in detail in Web Appendix D.

Note that we committed to these exclusion criteria before analyzing the confirmatory data, thereby preventing us from cherry-picking the criteria that "worked best" (e.g., Leamer 1983). Even so, alternate analyses in the Web Appendix show that the results are robust to all exclusions and also hold for rides taken during the day (i.e., between 8 A.M. and 6 P.M.). After all exclusions, a final sample of $N = 1{,}820{,}671$ remained (80.7% independent, 19.3% shared).

The data only record cost and expected duration for the ride type that the consumer actually chose. For instance, if a consumer chose the independent ride, we do not observe the cost and expected duration for the shared ride that they could have chosen instead. In addition, though the data set provides

Table 2. Summary of Results from Studies 1–5.

A: Average Estimated Number of HITs in Study 1 (N = 576, 54% female, M_{age} = 37 years, MTurk)				
Boundary-Expanded (N = 301)			**Boundary-Compressed (N = 275)**	
75.13 (84.37)			58.23 (71.44)	
Main finding	Demonstrates basic effect. MTurkers estimated being able to complete more HITs during boundary-expanded periods compared with boundary-compressed periods of equal length.			
B: Average Perceived Length in Study 2a (Day-Month-Year Format, U.K. Participants; N = 206, 56% female, M_{age} = 37 years, Prolific Academic)				
Boundary-Exp	30.80 (23.38)			
Boundary-Comp	25.09 (20.94)			
Main finding	Addresses left-digit bias. Even though the month unit was not leftmost, time periods felt longer when they spanned more months rather than fewer,			
C: Average Perceived Length in Study 2b (Year or Month as Highest-Level Unit; N = 257, 45% female, M_{age} = 36 years, Prolific)				
	Same Start and End Year (N = 131)		**Different Start and End Years (N = 126)**	
Past				
Boundary-Exp	26.63 (21.55)		48.58 (24.78)	
Boundary-Comp	21.69 (21.49)		46.32 (24.25)	
Future				
Boundary-Exp	32.70 (24.98)		50.46 (25.73)	
Boundary-Comp	25.74 (24.25)		48.47 (26.11)	
Main finding	Shows that participants attend to the largest unit that differentiates start and end time. When month was the largest differentiating unit, periods felt longer when they spanned more months. This effect was strongly attenuated when year was the largest differentiator. Results hold for past and future periods.			
D: Average Perceived Length of Periods in Study 3 (Classes vs. Hours; N = 1610, MTurk)				
	2 Classes, 2 Hours Future (N = 388)	**3 Classes, 3 Hours Future (N = 400)**	**3 Classes, 2 Hours Future (N = 412)**	**2 Classes, 3 Hours Future (N = 410)**
	64.46 (23.74)	68.98 (22.94)	66.93 (23.90)	64.60 (25.35)
Main finding	Demonstrates that non-numeric categories produce the same effect. A period expressed in terms of classes felt longer when it spanned more of them, regardless of how many distinct hour categories it spanned.			
E: Choice Share of Preferred Periods in Study 4 (Scheduling Activities; N = 600, MTurk)				
	Boundary-Expanded		**Boundary-Compressed**	
Pleasant	52%		48%	
Unpleasant	44%		56%	
Main finding	Participants preferred boundary-expanded periods when scheduling pleasant activities and boundary-compressed periods when scheduling unpleasant activities.			
F: Average Reported Valuation of Time Spent Waiting in Study 5 (in dollars; N = 260, 45% female, M_{age} = 36 years, Prolific)				
	WP (in \$)		**Required Compensation (in \$)**	
Boundary-Exp	64.70 (51.83)		251.98 (188.49)	
Boundary-Comp	60.93 (51.83)		234.81 (175.78)	
Main finding	Participants reported being willing to pay about more to avoid a long wait—and required more compensation to endure it—when the waiting period was boundary-expanded compared with compressed.			

precise estimates of trip duration, it rounds the start and end times for each trip to the nearest 15 minutes. Therefore, some work is needed to approximate the choice menus consumers faced when they requested their rides. We next describe the steps we took.

First, we computed an interval of possible start times for each trip. The earliest and latest possible start times were determined, respectively[3]:

$$b_k = \max(y_k - d_k, t_k) - 7.5,$$
$$B_k = \min(y_k - d_k, t_k) + 7.5,$$

where y_k is the end time provided in the data set, t_k is the provided start time, d_k is the duration of the trip in minutes, and k indexes the trip. An interval of possible start times S_k was computed for each trip as

$$S_k = \{x \in N \mid b_k \leq x \leq B_k\}.$$

We next estimated the probability that the independent and shared ride options would have been boundary-expanded. This involved the following steps. First, for each trip, we found the set of similar trips where the rider had requested a shared ride and the set of similar trips where the rider had requested to ride solo. Trips were considered "similar" if they had the same start hour and route as the trip; route was a coarse grouping of pickup and drop-off location coordinates (i.e., latitude and longitude rounded to the nearest tenth[4,5]). For each trip in the data set, we calculated the proportion of similar trips of each request type that, if they had started at the same time as the trip, would have crossed into a new hour (making the trip boundary-expanded). For example, suppose Trip X was requested at approximately 2:45 A.M. at Y location and traveled to destination Z. We found all independent trips that were also requested at Y location with Z destination sometime in the 2 A.M. hour and computed the proportion of those trips that would have arrived at or after 3 A.M. if they had started at the same time as Trip X. Because each trip had an interval of possible start times instead of a precise start time, the proportions were calculated for each minute within the trip's start time interval and averaged. The proportions served as the predicted probabilities of crossing into a new hour. This gives us the probability of the individual trip crossing into the next hour. We then repeated the same process for the shared ride.

This way, two probabilities were modeled for each trip: the probability that the shared option was boundary-expanded and the probability that the independent option was boundary-expanded. Expressed mathematically, these probabilities for trip k were constructed as follows:

$$P_{ind}(k) = \frac{1}{|S_k|}\sum_{s_k \in S_k}\left[\frac{1}{|N_k|}\sum_{n \in N_k} 1(d_n + s_k \geq 60,\ \text{ride n is ind})\right],$$

$$P_{shared}(k) = \frac{1}{|S_k|}\sum_{s_k \in S_k}\left[\frac{1}{|N_k|}\sum_{n \in N_k} 1(d_n + s_k \geq 60,\ \text{ride n is shared})\right],$$

where N_k denotes the set of trips similar to k (as defined previously), and $|N_k|$ denotes the number of elements in N_k and similarly for start time interval $|S_k|$. Because our predictions crucially entail consumers facing a shared ride crossing into a new hour while the independent option does not, we then created our variable of interest,

$$P_{diff}(k) = P_{shared}(k) - P_{ind}(k),$$

which is simply the difference between the two probabilities. This value captures the probability of a "mixed" choice set—that is, the likelihood that the consumer had chosen between a boundary-expanded shared ride and a compressed independent ride. A score of 1 indicates that for all other similar trips, the shared ride would have crossed a new hour, but the independent ride would not. Conversely, a score of 0 implies that both or neither ride would have crossed.

Next, we followed a similar procedure to approximate how much the options differed in expected duration and cost. For each trip, we found the difference between the average duration of all other shared and all other independent rides with same start hour and route as that trip:

$$D_{diff}(k) = \frac{1}{|N_k|}\sum_{n \in N_k} d_n 1(\text{ride n is shared}) - \frac{1}{|N_k|}\sum_{n \in N_k} d_n 1(\text{ride n is ind}).$$

The same procedure was performed for cost, subtracting the average cost of the analogous independent rides from the average cost of the shared ones:

$$C_{diff}(k) = \frac{1}{|N_k|}\sum_{n \in N_k} c_n 1(\text{ride n is shared}) - \frac{1}{|N_k|}\sum_{n \in N_k} c_n 1(\text{ride n is ind}),$$

where c_n is the trip's cost in dollars (the data set rounds to the nearest \$2.50). $P_{diff}(k)$, $D_{diff}(k)$ and $C_{diff}(k)$ and will be explanatory variables in our empirical model, which we now turn to.

Model

We next outline the model of consumer choice estimated on the confirmatory half of the rideshare data. We focus on a consumer in the process of booking a ride on an app who is faced with the

3 B_k is the latest possible start time because, given the provided start and end times, the trip must have started before both $t_k + 7.5$ and $y_k + 7.5 - d_k$. A similar argument applies to b_t.

4 This corresponds to partitioning the city into cells roughly of size 7 miles (north-south) by 5 miles (east-west). This yields 95 unique pickup and drop-off location pairs.

5 All results hold when coordinates are rounded to the hundredth (.7 by .5 miles); see Web Appendix Table D1 for that analysis.

choice between an independent and a shared ride.[6] We assume that the decision will be affected by how long each option is expected to take and how much it will cost, along with the date and time in which the decision is made. In addition, we consider the possibility that the consumer might be influenced by whether each ride type crosses an hour mark (boundary-expanded) or not (boundary-compressed). Accordingly, we specify the utility that a consumer gets from choosing the independent ride as

$$U_{ind} = \alpha_{ind} + \beta_{exp} ind_{expanded} + \beta_d d_{ind} \beta_c c_{ind} + \beta_{t,ind} time + \beta_{r,ind} route + e_{ind}, \quad (1)$$

where $ind_{expanded}$ denotes the dummy variable indicating that the independent ride is boundary-expanded; d_{ind}, c_{ind} are the duration and cost of the independent ride, respectively; time denotes the time when the choice is made (this term is composed of date, hour, and minute), and route denotes the pickup and drop-off locations. Any other factors (e.g., the consumer's idiosyncratic preference for independent rides) are captured by the consumer-specific term e_{ind}. Similarly, we let the utility that a consumer gets from choosing the shared ride be

$$U_{sh} = \alpha_{sh} + \beta_{exp} shared_{expanded} + \beta_d d_{sh} + \beta_c c_{sh} + \beta_{t,sh} time + \beta_{r,sh} route + e_{sh}, \quad (2)$$

Our main hypothesis is that the coefficient β_{exp} is negative (i.e. that, all else being equal, consumers prefer a boundary-compressed ride instead of a boundary-expanded one). Furthermore, β_d and β_c provide sanity checks because both should be negative, reflecting the fact that consumers tend to dislike spending time in transit and dislike spending money.

A consumer will choose the shared ride if the utility of choosing the shared ride is greater than the utility of choosing the independent one (i.e., whenever $U_{sh} > U_{ind}$); in other words, whenever

$$\alpha + \beta_{exp} mixed + \beta_d (d_{sh} - d_{ind}) + \beta_c (c_{sh} - c_{ind}) + b_t \, time + b_r route + e > 0, \quad (3)$$

where $\alpha = \alpha_{ind} - \alpha_{sh}$, $mixed = shared_{expanded} - ind_{expanded}$, $\beta_t = \beta_{t,sh} - \beta_{t,ind}$, $\beta_r = \beta_{r,sh} - \beta_{r,ind}$, and $e = e_{sh} - e_{ind}$. Note that the model allows consumer choices to be affected not just by the features of the two ride types but also by time and route.

As discussed previously, the data only contain information on the ride type that the consumer actually chose. Therefore, we do not observe some of the variables in Equation 3. To address this, for a given trip k, we approximate *mixed*, $d_{sh} - d_{ind}$, $c_{sh} - c_{ind}$ with $P_{diff}(k)$, $D_{diff}(k)$, $C_{diff}(k)$, respectively. These variables were defined in the previous subsection. The idea is that if $P_{diff}(k)$ is large, it is more likely that the consumer taking trip k faced a boundary-expanded shared ride and a boundary-compressed independent one; similarly, when $D_{diff}(k)$, $C_{diff}(k)$ are large, the differences in expected duration and cost between the two options are more likely to be large, respectively. Thus, from Equation 3 we obtain, for trip k,

$$\alpha + \beta_{exp} P_{diff}(k) + \beta_d D_{diff}(k) + \beta_c C_{diff}(k) + \beta_t time + \beta_r route + e > 0. \quad (4)$$

This is the equation we will use to estimate the effects of interest. We assume that the time and route fixed effects capture all the unobservables that are correlated with the explanatory variables; that is, we rule out issues of endogeneity. This would be violated if there were factors varying across both time and routes that influenced consumer decisions along with the probability of a mixed choice set or the duration or fare differences. For instance, a big sporting event might affect whether consumers choose the independent or the shared ride and also affect the estimated times of arrival shown on the app. Our focus on nighttime trips partially alleviates these concerns.

Results and Discussion

We estimate the coefficients in Equation 4 via logistic regression, where the dependent variable is a dummy equal to 1 when the consumer chooses the shared ride and 0 when they choose independent. As shown in Equation 4, we control for the expected differences in duration and fare; we also control for route and start time by respectively including fixed effects for each pickup/drop-off location pair[7] and day/hour (e.g., January 3 at 2 A.M.) as well as minute. Table 3 reports these results and results from a linear regression with robust standard errors; this allows the distribution of the unobservable e to vary across trips, so that, for example, the unobserved preference for the independent ride could vary more across consumers for certain trips relative to others.

As shown in the table, the coefficients for D_{diff} and C_{diff} are negative, reflecting that, all else being equal, consumers prefer shorter trips over longer ones and prefer cheaper trips over more expensive ones, respectively. Of particular interest is the negative coefficient β_{exp} on the mixed choice set probability term, P_{diff}, which indicates the following: As it became more likely that the consumers faced a boundary-expanded shared ride and a boundary-compressed independent ride, they were less likely to choose the shared ride.[8] It is important to emphasize that this analysis already accounts for any differences in each option's duration and cost. That is, the mixed choice set probability has a negative impact on the probability of choosing the shared ride that is distinct from the sheer effect of differences in cost and duration between the two rides.

[6] Note that we do not model the choice of which app to use (e.g., Uber, Lyft) or the choice between using a rideshare app and other means of transportation (driving, biking, etc.).

[7] Specifically, we partition Chicago into four roughly equally sized quadrants and include fixed effects for each combination of pickup and drop-off location (e.g., from southwest to northeast).

[8] P_{diff} improves the model likelihood from −831,721.2 to −831,609.6 ($\chi^2(1) = 223.28$, $p < .001$).

Table 3. Coefficient Estimates, Standard Errors and *p*-Values Under Logistic Regression and Linear Regression with Robust Standard Errors (Study 6).

	Logistic Regression		Linear Regression with Robust SEs	
	Estimate	SE	Estimate	SE
P_{diff}	−.525***	.035	−.068***	.005
D_{diff}	−.035***	.002	−.008***	.0003
C_{diff}	−.220***	.002	−.032***	.0003

***$p < .001$.
Notes: The dependent variable is a dummy equal to 1 if the consumer chooses the shared ride.

In the Web Appendix, we provide some additional exploratory analyses that show that the estimate of the effect of interest is robust across several alternate specifications. In particular, we estimate a least-squares linear regression based on Equation 4. The fact that our main effect continues to be statistically significant provides some reassurance regarding measurement error. Under the standard assumption that measurement error is independent of the mis-measured variable, the estimates would be biased toward zero. Thus, the fact that we still find significant effects suggests that the true coefficients might in fact be even larger.

We now turn to quantifying the effect of a mixed choice set on consumer behavior. One standard way to proceed is to consider odds ratios. Define the odds of a shared ride as the probability that a consumer chooses the shared ride divided by the probability that they choose the independent ride. Then our results imply that, all else being equal, the odds of selecting a shared ride when the choice set is mixed are 41% lower than when it is not mixed. In other words, when the shared ride crosses a new hour and the independent ride does not—compared with when neither or both options cross—consumers are 41% less likely to select the shared ride.

Another way to assess this is to look at how much consumers would be willing to pay to avoid crossing the hour boundary. To this end, we proceed in three steps. First, for every choice in the data set, we calculate the utility the consumer expects to derive from the choice set that they face. Second, we repeat that calculation for the hypothetical scenario in which neither option crosses a boundary. Given the estimated negative coefficient on boundary crossing, the expected utility computed for this hypothetical scenario will be higher than that in step one. Finally, we use the quantities in the previous two steps to compute the amount that consumers would be willing to pay to go from the status quo to the hypothetical scenario. This is a measure of their WTP to avoid crossing the hour boundary. On average, we find that the consumers would be willing to pay $.60 per trip, or around 5% of the fare for the independent ride.

This amount may scale up massively; in Chicago alone, approximately 10.9 million trips were taken in 2019 (https://data.cityofchicago.org). Therefore, platforms might substantially increase their revenues by incorporating these insights into their pricing strategy. To explore this, we consider the following change in prices. For every trip with $P_{diff} > 0$, we increase the price of the independent ride and simultaneously decrease the price of the shared ride by the same amount. This policy increases the price for the ride type that is less likely to be boundary-expanded while leaving the average price faced by each consumer constant. We consider this type of pricing policy to account for the fact that rideshare app firms might not want to increase the overall price levels for fear of losing customers to competing apps or other modes of transportation.[9] We calculate the expected revenue under this alternative pricing scheme and compare it with that obtained under the pricing policy in the data.[10] Web Appendix Figure D1 shows that for a range of price changes, expected revenue per trip would increase. In particular, by increasing the price of the independent ride by about $1.80 (and decreasing the price of the shared ride by the same amount), our estimates suggest that rideshare apps could increase their revenue per trip by more than $.30. Scaling this by the number of annual rides in Chicago yields an increase in expected revenue of more than $3.5 million. Of course, changing the pricing policy could alter consumers' choices in ways that our model does not capture (e.g., they might be more willing to bike or walk when independent rides become more expensive, even if the average price stays constant), and our estimates do not reflect this. Nonetheless, the strong evidence that boundary crossing affects consumer choice does support the notion that rideshare apps could increase their revenues by incorporating this insight in their pricing strategies.

Finally, based on a reviewer suggestion, we consider how boundary-crossing might affect tipping. If their trip was supposed to be boundary-expanded but ended up being compressed, the consumer may feel particularly satisfied and tip more. Controlling for ride duration, choice (independent vs. shared), fare, route, and time of day, we find evidence of such correlation—riders tipped more as it became more likely that the trip had been forecasted to be boundary-expanded but was actually boundary-compressed. These results suggest that expectations around boundary-expansiveness may have downstream consequences for consumer satisfaction.

General Discussion

The present research finds that time periods of equal duration do not always feel equivalent and therefore affect consumer decisions across a variety of domains. We demonstrate that time periods feel longer when they span more distinct time categories (e.g., the "3" in 3:15, the "March" in March 3). Furthermore, when a period is expressed in terms of multiple time categories (e.g., March 3, 2022–April 7, 2023), consumers attend to the largest unit that differentiates start and end time. When new categories are made salient (e.g., classes in a

[9] Our model does not capture this type of substitution, as the data do not contain information on the customers who considered booking a ride but eventually chose not to.

[10] We focus on changes in revenues as opposed to profits because assessing the latter would require a measure of the costs incurred by rideshare apps for the different types of rides, which is not in our data.

student's schedule), consumers perceive periods that span more of them to feel longer. Finally, our studies suggest various consequences for consumer decisions. We find that consumers prefer to schedule unpleasant activities during boundary-compressed times and pleasant ones during boundary-expanded and report being willing to pay more to avoid a long wait—and require more compensation to endure it—when that waiting period is boundary-expanded. Furthermore, our analysis of archival rideshare data suggests that consumers are more willing to select a faster, more expensive ride (e.g., UberX) when the alternative (e.g., UberPool) is boundary-expanded.

Theoretical Implications

Understanding how consumers broadly estimate magnitude is important given that virtually all consumer decisions involve some form of estimation (e.g., of quantity, price, duration). Theories about misestimation generally propose either top-down or bottom-up processes. During top-down processing, information is interpreted in light of one's previous knowledge, experience, or expectations (Gregory 1970). Top-down processes have been shown to affect a large variety of judgments such as recall (Loftus and Palmer 1974), estimation (Krueger and Clement 1994), and even taste (Lee, Frederick, and Ariely 2006). In contrast, bottom-up processes require no knowledge or interpretation and are stimulus based (Gibson and Carmichael 1966). Though these two processes may sometimes lead to the same predictions, they involve significantly different underlying psychology and thus imply different moderators and different debiasing interventions. Without carefully testing these circumstances, it is easy to mistake one kind of bias for the other.

We believe left-digit bias may be one of these phenomena. Despite its fame within marketing, the causes of left-digit bias are not well understood. One explanation is that consumers completely ignore the rightmost digits (Bizer and Schindler 2005). However, Strulov-Shlain's (2021) analysis of retail scanner data shows that, despite a sharp discontinuity in demand for products with .99-ending prices, shoppers do not ignore the cents component entirely. The seminal work of Thomas and Morwitz (2005) posits a slightly different attentional explanation. They argue that when comparing prices, consumers form a holistic impression of magnitude on an internal number line. Because people typically process information from left to right, the leftmost digit disproportionally affects subsequent processing. Thus, these authors also propose an attention-driven process. To our knowledge, Sokolova, Seenivasan, and Thomas (2020) offer the most direct test of bottom-up processing in left-digit bias. They find that left-digit bias is stronger when consumers evaluate prices side by side rather than retrieve prices from memory, consistent with bottom-up, stimulus-level processing. However, a sizable left-digit bias still occurs for memory-based evaluation, one that is about half the size of the bias found for stimulus-based evaluation. The fact that left-digit bias persists when evaluation is not perceptual suggests that it may at least partially result from top-down effects like the ones documented here.

Over and above left-digit bias, a categorization framework may help explain a variety of related findings in the marketing literature. For example, Tonietto, Malkoc, and Nowlis (2019) found that time periods feel shorter when they are bounded by an upcoming task (e.g., a meeting) instead of unbounded (i.e., when the time afterward is unaccounted for). The authors do not offer process evidence and speculate that the finding might be explained by factors such as devoting attention to the bounding event or construing it as a goal-relevant task. However, the result easily fits a categorization-based account. Categorization exaggerates the difference between stimuli in different categories and minimizes the difference between stimuli in the same category. Thus, if a salient event provides a category boundary, we would expect their result—that the same interval of time feels shorter when it leads up to an event compared with when it does not. To test whether a categorization account fits, one might vary the nature of the bounding event. Time may feel less contracted if a bounding event is similar to (vs. different from) the interval preceding it. For example, after working on paper X, working on paper Y (a similar activity) should be less of a category switch than, say, going to the post office (a different activity). If so, a bounding event may have a weaker effect on contracting time when that event is similar rather than different.

A categorization perspective would also predict that introducing *more* salient boundaries within a period results in its overestimation. This prediction closely relates to the unit effect, wherein the difference between time periods seems larger when, by changing their unit of measurement, those periods differ by more (smaller) units (e.g., warranties of 84 and 108 months feel like they differ more than do warranties of seven and nine years; Pandelaere, Briers, and Lembregts 2011). This aligns with what we would expect upon changing the category salience. If consumers use years when comparing seven- and nine-year periods and months when comparing 84- and 108-month periods, it makes sense that difference feels smaller between the former pair compared with the latter—seven and nine years are separated by fewer boundaries than 84 and 108 months are.

Open Questions

Herein, we focus on estimates of prospective duration given that most consumer decisions involve such estimates. Do the same biases also shape a consumer's subjective *experience* of duration? Although categorization may affect certain perceptual experiences (e.g., of color, Winawer et al. 2007), we believe that the effects shown here occur primarily for prospective (and possibly retrospective) duration estimation; boundary-expansiveness is unlikely to affect the experience of duration. This is because the effect depends on the presence of salient category boundaries. Unless these boundaries are strongly emphasized during the interval itself (e.g., a clock ticking down, or switching classes, as in Study 3), the boundaries tested here (e.g., for hours, months) may not be salient enough to affect the actual experience of duration.

A second open question is which cues provide temporal category boundaries. This paper focuses on natural boundaries from hour and month categories and introduce new salient boundaries to test for process. However, in daily life, consumers may use additional categories, such as morning, afternoon, and evening. What happens when two salient category boundaries coincide? For a consumer who ends work at 5 P.M., 5 P.M. may be a more salient boundary than, say, 4 P.M. Similarly, 12 P.M. might signal a new hour *and* a new activity, lunch. We would expect consumers to overestimate periods more when they cross such "double" boundaries.

An implication of the categorization framework is that time periods containing more naturally occurring intervening events (and thus boundaries) should be estimated as longer than equal-duration periods containing fewer such boundaries. Some periods in Web Appendix Study W4 offer an initial test of this prediction; despite having the same duration, some are in the afternoon (i.e., 11:30 A.M.–3 P.M., 12 P.M.–3:30 P.M.), and others are in the evening (i.e., 6:30 P.M.–10 P.M., 7 P.M.–10:30 P.M.). We indeed find that the afternoon period was rated to feel longer than the equal-duration evening period, $F(1, 850.55) = 45.47$, $p < .001$. However, the effect of boundary-expansiveness on perceived duration did not differ between these two periods.

Yet another open question is whether the effect differs by a period's duration. Because the difference of one extra unit is proportionally larger, boundary-expansiveness might affect shorter periods more than longer ones (e.g., spanning two categories instead of one should feel like a larger difference than would spanning eight instead of seven). Three of our studies have enough variability in duration to explore this prediction (see the Web Appendix), though they were not designed to do so, and only Study 1 has a large enough sample to offer a reliable test. That said, Study 1 does find the anticipated pattern —the perceived difference between boundary-expanded and boundary-compressed periods decreased as their duration increased ($z = -5.48$, $p < .001$). The other two studies (W2 and W4 in the Web Appendix) have roughly one-sixth the sample size of Study 1 and show (nonsignificant) patterns in the same direction.

A final open question concerns the degree to which consumers can be debiased. If consumers reliably misestimate duration, knowing how to debias them could greatly improve consumer well-being. However, debiasing may be difficult for two reasons. First, the effect of boundary-expansiveness appears to have a strong impact on various routine, familiar judgments. For example, MTurkers in Study 1 expected that they could do about 17 more HITs during boundary-expanded periods compared with boundary-compressed periods, an astronomical difference given that the two types of periods had the exact same duration and that doing HITs was a familiar task. This suggests that experience alone will not be enough to alleviate these biases. Second, we believe the bias we documented may result from overapplying what is generally a sensible strategy. After all, time categories are somewhat informative—categories like 1 P.M. and March are respectively closer to 2 P.M. and April than 3 P.M. and May—and on average, periods *are* longer when they start and end in different hours or months (vs. the same ones). Thus, relying on categories when judging duration may be an adaptive, effort-saving heuristic (Gigerenzer and Gaissmaier 2011) that is misapplied in the contexts studied here. Its strength and functionality suggest that this bias will be difficult to avoid. Any debiasing efforts should aim to help consumers recognize situations in which they could fall prey to the bias and actively correct for it.

Practical Implications

Given the ubiquity of consumer decisions that involve time estimation, our findings may easily apply to a variety of situations. As shown in Studies 4, 5, and 6, our basic finding that time periods are estimated to be longer when they cross more (vs. fewer) salient boundaries implies that negative events are disliked more, and positive events liked better, when those events cross more boundaries because they appear to last longer. Thus, whenever feasible, companies should try to present negative events (e.g., layovers, teeth cleanings, waiting times) in boundary-compressed form and positive events (e.g., theater shows, lunch breaks, massages) in expanded form. This strategic use of boundary-expansiveness may facilitate booking and improve customer satisfaction. For instance, customers may be more unhappy when delays broach a new time category and especially satisfied when a waiting period is supposed to end *after* a new category but ends *before* it instead. The exploratory tipping analysis mentioned in the rideshare study supports this idea (see Table D2 in the Web Appendix).

Of course, many companies cannot control scheduling to this degree. A flight may be slightly more attractive if it is set to arrive at 10:58 A.M. rather than 11:02 A.M., but flight times are highly constrained by numerous factors (e.g., airport infrastructure, available runways). This is where our categorization framework highlights unique opportunities. As shown in Studies 2a and 2b, categories are quite flexible, and drawing attention to another kind of temporal organization results in those categories being used. For example, including the description "take off in the morning, be in your hotel before lunch" makes lunch a salient boundary and thus likely minimizes the difference between a 10:58 and 11:02 arrival. Similarly, building on the results of Study 3, companies could even introduce new category boundaries themselves, reorganizing time to make events feel like they will last for a longer or shorter time.

Although the effects presented here may appear relatively small, their potential impact is substantial. Not only do these effects involve minor changes (manipulating a time period's placement relative to temporal categories without ever changing its duration), but their impact on consumer decisions scales up dramatically. For example, we found a 6% increase in WTP for a bus ride when it was boundary-compressed instead of boundary-expanded (Study 5). If consumers were willing to pay even 1% more for, say, boundary-compressed flights, pricing those flights at $505 (instead of $500, and assuming a 200-person capacity on a Boeing 737) would net an additional $1,000 per flight. The difference becomes quite significant given that there were more than 38 million flights in 2019 alone (Statista 2020). Similarly, our rideshare analysis found that customers were willing to pay about 5% of the cost of the independent ride—roughly $.60 more—to avoid crossing an hour boundary. Again, this amount scales up. Rideshare platforms may observe sizable gains in revenue if they consider the influence of boundary-expansiveness when pricing options. Doing so, by our estimates, would increase expected revenue by more than $3.5 million per year in Chicago alone.

Conclusion

Together, our studies suggest that time periods feel longer when they span more boundaries and that this phenomenon may shape the scheduling and purchasing decisions consumers make in everyday life. Broadly, this research provides novel insight into the ways in which consumers perceive time and anticipate the duration of future experiences.

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