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Enjoy It Again: Repeat Experiences Are Less Repetitive Than People Think

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What would it be like to revisit a museum, restaurant, or city you just visited? To rewatch a movie you just watched? To replay a game you just played? People often have opportunities to repeat hedonic activities. Seven studies (total $N = 3{,}356$) suggest that such opportunities may be undervalued: Many repeat experiences are not as dull as they appear. Studies 1–3 documented the basic effect. All participants first completed a real-world activity once in full (Study 1, museum exhibit; Study 2, movie; Study 3, video game). Then, some predicted their reactions to repeating it whereas others actually repeated it. Predictors underestimated Experiencers' enjoyment, even when experienced enjoyment indeed declined. Studies 4 and 5 compared mechanisms: neglecting the pleasurable byproduct of continued exposure to the same content (e.g., fluency) versus neglecting the new content that manifests by virtue of continued exposure (e.g., discovery), both of which might dilute uniform dullness. We found stronger support for the latter: The misprediction was moderated by stimulus complexity (Studies 4 and 5) and mediated by the amount of novelty discovered within the stimulus (Study 5), holding exposure constant. Doing something once may engender an inflated sense that one has now seen "it," leaving people naïve to the missed nuances remaining to enjoy. Studies 6 and 7 highlighted consequences: Participants incurred costs to avoid repeats so to maximize enjoyment, in specific contexts for which repetition would have been as enjoyable (Study 6) or more enjoyable (Study 7) as the provided novel alternative. These findings warrant a new look at traditional assumptions about hedonic adaptation and novelty preferences. Repetition too could add an unforeseen spice to life.

Keywords: enjoyment, hedonic adaptation, novelty, prediction, repeat experiences

You and your friends decide to visit a museum. In discussing options, your friends push for a museum that you yourself recently just visited. How excited would you be? Most of us presumably would feel less than enthused, even if we enjoyed our trip just fine the first time around. We may fight for a different pick. We may spend the next hour researching alternate plans. Eventually, we may succumb but buy ourselves the pricier ticket with access to a new temporary exhibit, despite having no real interest in its content.

This example illustrates a common tradeoff in everyday life: whether to fill our leisure with novel experiences or familiar experiences. Many studies document a strong intuitive preference for novelty. When people have the goal to maximize consumption enjoyment, they intuitively seek out something *new* as opposed to something they have consumed or done in the recent past (Kahn & Ratner, 2005; McAlister & Pessemier, 1982). Throughout this research is an assumption that people are actively drawn to the novel option, and for good reason: Filling leisure with novel experiences indeed disrupts adaptation and promotes discovery, supporting the idea that "variety is the spice of life" (Lyubomirsky, Sheldon, & Schkade, 2005; Sheldon, Boehm, & Lyubomirsky, 2012). Time spent repeating the same old activity means time not spent on limitless other novel possibilities, and missing out on novelty presents a real opportunity cost for enjoyment.

The current research highlights a different perspective: People are also actively drawn *away* from the *familiar* option. As in the opening example, people might prefer novelty simply because repeat experiences seem like a waste of one's time, absent any obviously superior alternative. Seven studies (six preregistered; $N = 3{,}356$) document this intuitive aversion to repetition and reveal that it is miscalibrated. A repeat experience may be less intensely enjoyable than one's first exposure—hedonic adaptation seems to pervade nearly everything (Frederick & Loewenstein, 1999)—but the current studies reveal that many repeat experiences are more enjoyable than people think. Further, we find that this miscalibration occurs not because literal repetition is surprisingly fun, but because literal repetition is surprisingly fictional. For certain activities, repetition unveils a similar spice as provided by new entities—while revisiting a sprawling museum, for example,

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one may discover new attractions that were inevitably missed or forgotten from the first trip—in effect rendering real-time repetition "newer," and thus more enjoyable, than what had played in one's mind. Sometimes, the bigger opportunity cost may be in choosing whatever looks new on the surface, over repeating an activity that can provide a similar, or even superior, hedonic experience (especially if the newer option comes with other costs in money, search time, or generating consumption waste, as is often the case). As will be shown, the current studies warrant a new look at hedonic adaptation and other classic exposure effects, highlighting the yet-underemphasized value of repetition for uncovering novelty and building depth. If people avoid a repeat because they assume that the familiar has nothing left to offer, enjoyable surplus often may be left untapped.

Unpacking Novelty Preferences

People generally prefer novelty when seeking to maximize their enjoyment for many experiences (Kahn & Ratner, 2005; McAlister & Pessemier, 1982; O'Brien & Smith, 2019; Ratner, Kahn, & Kahneman, 1999; Read & Loewenstein, 1995; Simonson, 1990)—consuming or doing something *new* rather than something they have consumed or done in the recent past.

In studies that document this intuitive novelty preference, researchers typically ask participants to choose which experiences they would prefer to consume at some later point, and find that people choose options that are different than whatever they usually consume or have recently consumed. For example, choice bracketing studies show that people overchoose novelty when preselecting their choices (e.g., in planning desserts for the next three weekends, people buy chocolate, vanilla, and strawberry; but as each weekend arrives, they just want chocolate). When experiences seem like a single set (e.g., one future window of time), novel options become more salient, and the robust finding is that people then incorporate them (Read & Loewenstein, 1995; Simonson, 1990). To concretely illustrate this typical design, we conducted a national survey of 385 American adults and replicated the robust finding.[1] When asked what they would prefer to do after completing a leisurely activity and finding it very enjoyable, a full 61.04% (235 of 385) nonetheless reported that they would pursue a comparable but *new* activity at their next chance for leisure, whereas only 23.12% (89 of 385) preferred to seek the same experience again; the remaining 15.84% (61 of 385) had no preference. This preference for novelty was held by a significant majority of respondents, $\chi^2(2, N = 385) = 136.04$, $p < .001$.

Such effects can be interpreted in two ways. On the one hand, the prevailing interpretation is that people indeed put weight on the novel option, assuming the new addition or update will be worthwhile (Fennis & Stroebe, 2016; Tormala, Jia, & Norton, 2012; Wänke, Bless, & Schwarz, 1998). After all, experiencing novelty does provide real hedonic benefits, such as slowing hedonic adaptation and promoting enjoyable discovery, leading positive psychologists to recommend acquiring novel entities to boost enjoyment (Berlyne, 1970; Fredrickson, 2001; Lyubomirsky et al., 2005; Quoidbach & Dunn, 2013; Sheldon et al., 2012). On the other hand, one could also interpret these findings as an aversion to repetition: People may choose novelty not because they expect exceptionally positive reactions to the new option, but because they expect exceptionally dull reactions to the old option. Because the typical design tests absolute choice, past studies obfuscate the possibility of this other path. As initial evidence for this distinction, we conducted another survey using the same prompt as before, with 501 new respondents.[2] First, we replicated the robust preference for novelty: 55.09% (276 of 501) chose novelty, 21.36% (107 of 501) chose repetition, and 23.55% (118 of 501) reported equal interest, $\chi^2(2, N = 501) = 107.08$, $p < .001$). Next, we asked this majority to indicate their primary reason for intuitively choosing novelty, by choosing between high expectations (e.g., anticipated excitement) about the novel option or low expectations (e.g., anticipated boredom) about the repeated option. In fact, most chose the latter: 61.59% (170 of 276) of novelty-seekers reported that they would seek novelty simply because the thought of repetition seemed overly dull, $\chi^2(1, N = 276) = 14.84$, $p < .001$). These findings suggest a more nuanced psychology underlies novelty preferences beyond what is currently assumed.

The current research was designed to better understand this second path: people's intuitions about repeat experiences. Many repeats likely *are* less intense than their initial exposures, but how much less? People may understand direction but misunderstand magnitude, but small differences in magnitude matter for affecting choices and behaviors (Mellers & McGraw, 2001). There is reason to believe that people exaggerate dullness: Repeat experiences may be less dull than people think, in part because repetition may prove not so repetitive. If people pursued novelty purely because of the perceived benefits of the new option, such a discrepancy may not be so problematic, all else equal. However, as shown above, people may also pursue novelty because of expected dullness of the repeat. In such cases, a discrepancy between expected versus actual dullness may lead people astray (especially when all else is *not* equal, such as when new options have added costs).

[1] These data are original to this paper. They come from an item we added to a survey administered by Qualtrics Panels, a national adult panel (N = 385 at $5.00 per respondent; $M_{age} = 41.39$, $SD_{age} = 13.14$; 51.17% female; 72.47% Caucasian American/White, 8.05% African American/Black, 7.01% Asian American/Asian, 12.47% Mixed/Other Ethnicity):

"This question pertains to your general attitudes towards repeating the same experiences versus choosing new experiences in terms of your leisure preferences. People make lots of choices for their leisure—which meals to eat, places to travel to, movies and shows to watch, books to read, museums to visit, and so on. Imagine you have a leisurely experience like this and it's very enjoyable. For your next choice for such a leisurely experience . . .: As a general rule of thumb, would you prefer to repeat this same exact thing right away (e.g., to eat, visit, watch, or read the thing you just ate, visited, watched, or read), or to go for something also enjoyable but one that you haven't experienced before? (e.g., a new food, place, movie/show, book, museum, etc.)."

1. "In general, I'm someone who prefers to repeat the same exact thing right away for the next one".
2. "In general, I'm someone who prefers to try out something also enjoyable but new for the next one".
3. "I don't care either way; flip a coin between the same thing vs. a new thing and that's what I'll do for the next one".

[2] We requested 500 participants from Amazon's Mechanical Turk, yielding 501 participants ($M_{age} = 36.74$, $SD_{age} = 10.52$; 49.30% female; 79.84% Caucasian American/White, 6.99% African American/Black, 8.18% Asian American/Asian, 4.99% Mixed/Other Ethnicity) who completed the survey for $0.25.

Thinking About Repetition

Are repeat experiences less dull than they seem? As reviewed, past studies shed little light on this question because they assess absolute choice, leaving both predictions and experiences open to interpretation. Nonetheless, a handful of studies have assessed continuous ratings of hedonic repetition. As a representative example, Kahneman and Snell (1992) asked participants to eat a sample of yogurt and predict their liking if eaten every day for the next seven days. Then, participants reported their real-time liking over the week. Participants generally believed the yogurt would grow less enjoyable, but in reality most reported that the yogurt stayed more similarly enjoyable from day to day.

To date, such findings have largely been understood as a misperception of *time*. Even strong states of satiation (e.g., feeling stuffed) subside relatively quickly (Groves & Thompson, 1970), and so intuiting tomorrow's reactions (and the next day, and so on) requires an accurate understanding of the time course of current feelings. As the logic goes, because long intervals are difficult to mentally simulate (e.g., one's hunger over a week), people err because they draw on easier-to-imagine short intervals when making predictions ("How much would I enjoy it again *right now*?"), insufficiently adjusting for the "resetting" effect of longer periods of time in between consumption (Galak, Kruger, & Loewenstein, 2011; Kahneman & Frederick, 2002; Read & Loewenstein, 1995).

This research provides initial support for the hypothesis that people generally may underestimate their enjoyment for repeat experiences, although the evidence is limited in domain; these past studies mostly involve eating plain snacks or listening to short audio clips (following the precedent of Kahneman & Snell, 1992; see Kahn & Ratner, 2005). A more substantive limitation is the emphasis on time. By focusing so much on time as the driver, existing studies imply that eliminating the time in between exposures should eliminate the error. As the logic goes, if people expect repetition to be dull right now—having just had the experience—their expectations should be well calibrated because immediate repetition is presumably as dull as they imagine (Galak et al., 2011; Read & Loewenstein, 1995; Simonson, 1990); after all, there is no time to reset to baseline. As Galak et al. (2011) summarize, "As the inter-consumption interval decreases, predicted preference for variety should align with in-experience (and thus actual) preferences" (p. 231). However, there is reason to hypothesize that even *immediate* repetition—when the interconsumption interval is essentially zero—may be less dull than expected, raising the possibility of a broader psychological block beyond the difficulties of perceiving time.

The Static Simulator

Experience is not static. By virtue of continued exposure, people's perspective on what they are experiencing can *change*, in particular for information-rich activities. If people do not account for this dynamic beforehand—if predicted repetition is more static than actual repetition—expected dullness for repeats may be prone to exaggeration.

In what unforeseen ways might repeat experiences end up being different than initial experiences? On the one hand, repeat experiences come with a sense of familiarity that initial experiences do not offer, and a sense of familiarity may combat the uniform dullness of otherwise unchanged stimuli. For example, one might feel overwhelmed or confused during the first time at a museum, but can confidently stride around during a return visit knowing exactly what and where everything is. Despite still consuming the same exact content, thus triggering hedonic adaptation, this state of "knowing" may be pleasurable in itself and dilute how quickly one adapts. This possibility is consistent with research on phenomena such as the mere exposure effect (Bornstein, 1989; Zajonc, 1980) and processing fluency (O'Brien, 2013; Reber, Schwarz, & Winkielman, 2004). Theories of mere exposure posit that initially neutral stimuli (e.g., unknown foreign letters) are liked more the more that people view them (typically presented via subliminally or split-second exposures), which has been attributed to the broader evolutionary rationale that if one has successfully encountered a stimulus in the past, the stimulus must not be harmful and can be approached (see Zajonc, 2001). Likewise, theories of fluency posit that if a stimulus is easy to process (which presumably is more likely upon repetition), people overgeneralize their experience of ease to other judgments (e.g., the same recipe seems easier to cook when printed in easy-to-read font vs. hard-to-read font: Song & Schwarz, 2008). To our knowledge, only one study has tested whether people accurately *predict* such effects, finding that at best people have moderate awareness of the mere exposure effect (Snell, Gibbs, & Varey, 1995). Thus, one potential block is that such dynamics are too subtle to fully appreciate beforehand: Repeat experiences may be less dull than they seem because people do not account for the added pleasure of knowing what to expect and experiencing it as expected. Literal repetition likely *does* desensitize one to the content being repeated, but people's simulations may not incorporate how they as perceivers will change upon repeating it—that fluency and mere exposure coincide with repeating the same content and may dilute how quickly one adapts to that content.

On the other hand, the current research goes beyond existing studies to explore how people think about richer, real-world activities, which do not start out as hedonically neutral nor involve mere split-seconds of exposure. It is possible that the basic processes of fluency and mere exposure still underlie reactions to such activities, with people still failing to fully appreciate these contributions. However, we propose a more pervasive, yet-untested dynamic. In addition to the possibility that people literally process the same exact content across repetition (and thus experience fluency and mere exposure), people might also notice new content altogether (and thus experience the pleasures of novelty). For example, during one's return visit to a museum, one might also discover completely new hallways and exhibits—and mixing new content into repeat visits rather than literally repeating the same content may dilute how quickly one adapts to the museum overall. Of course, this is not always possible—watching paint dry will not unveil new colors—but many activities do possess such qualities. Some simply contain too much information to encode at first pass, from sprawling museums to lengthy movies. Others contain later information that alters earlier information, such as movies with plot twists. Still others require time to unfold, such as acquired tastes for some artwork and warming up to some social events. The current research focuses on these kinds of experiences, what we refer to as "complex" hedonic activities. For complex hedonic activities, repetition may unveil new content within the entity (i.e., not just more fluently processing the same content at later exposures, but also attending to other content altogether at later expo-

sures)—and, as outlined, experiencing novelty really does help. Critically, people's simulations may not fully incorporate how the content itself changes in this way upon repeating it, imperfectly anticipating which activities will be more or less complex—leading to our hypothesis that people may underestimate the remaining novel enjoyment that repetition can uncover.

Why? Various literatures highlight the fact that perception is limited. There is far too much information to process at each step of daily life, and so people simplify: In one-shot (nonrepeated) encounters people notice only bits and pieces (Chabris & Simons, 2010; Rensink, 2002), and then mentally represent those experiences in even smaller understandable units (Gigerenzer, 2008; Higgins, 1998; Kahneman & Frederick, 2002; Klein & O'Brien, 2018; Redelmeier & Kahneman, 1996). The fact that perception is limited is a welcome adaptation for enabling action (Fiske & Taylor, 1991). However, problems can emerge because people notoriously fail to *realize* that perception is limited. Kahneman (2011)'s WYSIATI principle—"what you see is all there is"—captures much of this research. For example, test-takers often do not account for the knowledge that they lack and thus overestimate their scores (e.g., omission neglect: Frederick, 2005; Kardes, 2013; Kruger & Dunning, 1999); "cold" performers often do not account for the "hot" emotions that arise in execution and thus overestimate how gracefully they will perform (e.g., empathy gaps: Kardas & O'Brien, 2018; Van Boven, Loewenstein, Welch, & Dunning, 2012); and forecasters often do not account for the mundane events that will inevitably still occur following major changes in circumstances (e.g., focalism: O'Brien, Kristal, Ellsworth, & Schwarz, 2018; O'Brien & Roney, 2017; Wilson & Gilbert, 2005). More generally, people tend to have the sense "by default" that they have a more complete understanding of things than they really do (Camerer, Loewenstein, & Weber, 1989; Nisbett & Wilson, 1977; Ross, 1989). Such misperceptions may be most miscalibrated when thinking about truly complex entities, as in the Illusion of Explanatory Depth (e.g., in a typical study, participants assume that they possess a deeper knowledge of the inner workings of mechanical objects like helicopters or toilets than they display once pressed: Keil, 2003).

Putting these findings together suggests a yet-untested block in how people think about repeat hedonic activities, a domain that none of the research above has examined yet is common to everyday decisions. Experiencing complex activities just once may leave people with an inflated sense that they have now seen "it"—a welcome adaptation for easing cognitive load but leaving people naïve to the emergence of novel features left within the familiar, those details and nuances that they almost certainly missed the first time around. This novelty should be more likely to manifest by virtue of having more exposure time, which should dilute uniform dullness in the moment of consumption—all dynamics that people may not easily intuit. The very nature of the intuition may reinforce this gap: As people avoid repeat experiences, they rarely learn about any enjoyment left remaining to be reaped. In short: The mere *thought* of completing a complex hedonic activity over and over may play in one's mind as a tedious overdose of the same already-seen content, whereas the *actual experience* of repetition may prove less monotonous.

The Current Research

Seven studies tested the hypothesis that repeat experiences of complex hedonic activities may be less dull than people think.

First, Studies 1–3 documented the basic effect. All participants were exposed to a real-world enjoyable activity once and reported their hedonic reactions (e.g., enjoyment, interest, excitement). Some participants were instructed to imagine repeating the activity a set number of times and predicted each reaction ("Predictor" condition). Others were instructed to actually repeat the activity that number of times and reported each reaction ("Experiencer" condition). We hypothesized that, even if repetition may eventually (perhaps inevitably) elicit adaptation even to these complex hedonic activities, it will do so to a lesser, slower degree than people think. Our study designs advance past research by assessing continuous ratings of repeated hedonic exposure (vs. absolute choices open to interpretation), testing more involved domains and activities (vs. isolated tastes and noises), and testing the conservative window of *immediate* repetition, which should promote *actual* dullness (working against the hypothesis). Note also another conservative feature of these designs (in Studies 1–3 and in all studies): All participants first have the experience once in full. In our studies, it cannot be that Predictors simply have distorted or insufficient information about the experience compared with Experiencers (a feature that may apply to many existing forecasting errors, which by design have involved predictions about future events that have not yet occurred: see Wilson & Gilbert, 2005), because our Predictors get the full bottom-up experience before making predictions about *repeating* it. This feature renders any subsequent mispredictions especially informative.

Next, Studies 4 and 5 tested mechanisms, representing the first documentation of the role of discovering novel content via repetition beyond known effects of literal repetition (e.g., fluency and mere exposure). In Study 4, we took a moderation-based approach by manipulating stimulus complexity while holding exposure (and thus effects like fluency) constant. We tested whether, somewhat paradoxically, the gap between prediction and experience is bigger for *complex* hedonic activities (which indeed offer new things to uncover) than for simple stimuli (since people may "see all there is to see" after just one exposure). In Study 5, we took a mediation-based approach by testing whether explicit reports of noticing new content mediate differences in predicted versus actual enjoyment, and furthermore, we tested another potential moderator derived from this proposed mechanism: Cues that bring to mind an activity's missed complexities should boost (and therefore calibrate) people's dulled expectations, again holding exposure constant.

Finally, Studies 6 and 7 tested consequences. Our findings so far suggest that, when seeking to maximize enjoyment, people may avoid repeat experiences that in reality would be more enjoyable than expected. As discussed, however, such a choice must be understood in the context of opportunity costs: Avoiding repetition is an "error" only to the extent that the novelty that people choose instead proves to be the less enjoyable experience, when maximizing enjoyment was one's goal. In Study 6, we tested whether participants (with the goal to maximize enjoyment in that moment) were willing to pay a premium to watch a new video rather than rewatch the same video—in a case when both options would have been equally enjoyable. In Study 7, we tested whether participants (with the goal to maximize enjoyment in that moment) chose to

spend the remainder of an enjoyable study on their phones rather than repeat the same study tasks twice in a row—and in doing so, would end up having a *less* enjoyable overall study experience.

Study 1: Revisiting the Same Museum Exhibit

Study 1 was conducted at a city museum. Some visitors imagined going through an exhibit twice in a row, whereas others actually did. We hypothesized that experiencing the exhibit again, despite having just seen it, may be more enjoyable than people think.

Method

Sample size was predetermined for this and all studies to comprise at least 50 participants per experimental cell, comparable with past similar designs (e.g., 20–30 per cell in Morewedge, Gilbert, Myrseth, Kassam, & Wilson, 2010; Wilson, Wheatley, Meyers, Gilbert, & Axsom, 2000; 50–60 per cell in Campbell, O'Brien, Van Boven, Schwarz, & Ubel, 2014; Hsee & Rottenstreich, 2004). We doubled this number or more for online studies, and still more depending on available resources. All measures, manipulations, and exclusions are reported. All studies except Study 1 were preregistered. All data, materials, and preregistrations are available at http://osf.io/up2qn/.

Participants. We recruited 110 visitors at the Museum of Science and Industry (the MSI) in Chicago (M_{age} = 40.17, SD_{age} = 14.01; 33.94% female; 67.89% Caucasian American/White, 8.26% African American/Black, 9.17% Asian American/Asian, 15.45% Mixed/Other Ethnicity) to complete the study for candy and a $5.00 gift card.

Procedure. The MSI is one of the largest science museums in the world with more than one million visitors annually. With museum approval, research assistants set up outside the Genetics exhibit.[3] Genetics was chosen for its many enjoyable experiences, and its high-traffic location is ideal for managing participants. Passersby can be tracked before entering and upon exiting, and they can easily find and return to the recruitment location.

Participants were randomly assigned to one of two conditions. Predictors (n = 56) were recruited upon exiting Genetics, having gone by their own volition. All participants confirmed in the consent process that they just went to Genetics. Predictors were asked to imagine they had yet to visit Genetics and completing a study on rating two exhibits. For the first exhibit, Predictors imagined being asked to go through Genetics, without the experimenter and at their own pace, exploring however they wished; when done, they were to return to rate their experience. They imagined being asked at this point, "Overall, how much did you enjoy your experience of the Genetics exhibit during this Task 1?", and predicted their rating from 1 (*not at all enjoyable*) to 7 (*extremely enjoyable*).

This "prediction" captures the actual reaction that Predictors just had. Having Predictors make a "prediction" for Time 1 rather than simply report their reactions was by design, to help rule out some incidental difference that may explain underestimations of subsequent exposures (i.e., Predictors and Experiencers should also differ at Time 1 if this is the case). For purposes of generalizing across a variety of methods, Study 3 also uses this backtracking method (i.e., Predictors have the experience, then imagine having it for the first time + n more times), and Study 2 and Studies 4–7 use a strictly real-time method (i.e., Predictors report their first reactions, then predict the repeated exposures).

For the second exhibit, Predictors imagined being asked to go through Genetics again, having learned that the hypothetical study was actually on repeating the exhibit. The only stipulation was that they had to imagine going with the same party. They read:

> Imagine that after you finish going through Genetics, coming back, and rating the experience, we inform you that the study is really about repeated experiences: We want you to go through Genetics for a second time right then and there (you can do whatever you want the second time through, keeping the same requirements as your first time). Now: Imagine you just finished and returned to us again.

Predictors imagined being asked, "Overall, how much did you enjoy your experience of the Genetics exhibit during this Task 2?" and predicted their rating on the same scale.

Experiencers (n = 54) were recruited as they were entering Genetics by their own volition, but before entering. Right at the consent process, they were told that the study involved rating two exhibits to reduce attrition and selection effects. After consenting, Experiencers were informed that their first exhibit was Genetics, and only those who confirmed that they had not yet been to Genetics proceeded. All procedures and measures were identical to the Predictors condition, except Experiencers actually completed and rated each visit one after the other (following exactly what was described to Predictors).

Finally, all participants reported demographic information. The surveys also displayed checklists of some of the individual things to see within the exhibit (identical across conditions and at both time points), but included no other critical measures.

Results and Discussion

For this and all studies, our analysis plan was as follows: For studies with only two exposures (such as the current study), we conducted a Repeated Measures GLM with Condition (between-subjects factor: Predictors, Experiencers), Time (within-subjects factor: reaction at Time 1, reaction at Time 2), and the Condition × Time interaction term entered as predictors.

For studies with three or more exposures, we conducted a repeated-measures GLM in the same way, as well as two supplementary change analyses. First, we report simple difference scores between the first exposure and final exposure. Second, we report the results of growth curve modeling, which tests for changes in reactions within each condition while accounting for the nonindependence (nesting) across all of the multiple ratings within each individual (this analysis can only be conducted with data comprising three or more exposures). We specified a multilevel random coefficient model using the SPSS Mixed command (Peugh &

[3] Per the official MSI website (https://msichicago.org), Genetics offers the following:

"Discover how the tiniest genetic variations define traits that make us distinct from animals and each other. Frogs with glowing eyes, mutated fruit flies and cloned mice are just a few ways you'll see genetic engineering up close. Polls and interactive scenarios let you weigh issues of ethics and privacy. Enjoy one of the Museum's most beloved experiences: watching as baby chicks peck out of their shells and take first steps into the world."

Enders, 2005). Condition, Time, and the Condition × Time interaction term were entered as predictors of reactions, which varied over time.

In terms of the basic effect, was no main effect of Condition, $F(1, 108) = 1.73$, $p = .192$, $\eta^2 = .02$, and a main effect of Time such that the exhibit was rated as growing less enjoyable over time, $F(1, 108) = 11.61$, $p = .001$, $\eta^2 = .10$. However, this was qualified by the critical interaction, $F(1, 108) = 6.39$, $p = .013$, $\eta^2 = .06$ (see Figure 1).

Pairwise comparisons reveal that participants *imagined* a decline: Predictors believed that going through again right away ($M = 4.80$, $SD = 1.51$) would be less enjoyable than their original experience ($M = 5.43$, $SD = 1.23$), $F(1, 108) = 17.94$, $p < .001$, $\eta_p^2 = .14$ ($d = 0.62$), 95% CI_{diff} [−0.92, −0.33]. However, this was not the case in reality: for Experiencers, the return visit was just as enjoyable ($M = 5.37$, $SD = 1.39$) as the first time through ($M = 5.46$, $SD = 1.11$), $F(1, 108) = 0.38$, $p = .539$, $\eta_p^2 = .004$ ($d = 0.08$), 95% CI_{diff} [−0.39, 0.21]. The significant interaction indicates a significant difference between predicted decline ($M = -0.63$, $SD = 1.05$) and actual decline ($M = -0.09$, $SD = 1.15$).

These findings support the hypothesis in a naturalistic real-world setting: Repeat experiences (here in terms of immediately revisiting the same museum exhibit) were surprisingly enjoyable. Next, we sought to replicate these findings in more controlled settings while still testing a common hedonic activity: watching a movie. Moreover, we utilized stimulus sampling (Wells & Windschitl, 1999) such that each participant freely chose what to watch, circumventing idiosyncratic effects of any one specific movie.

Study 2: Rewatching the Same Movie

In Study 2, all participants first watched a movie of their choosing. Then, some participants imagined rewatching that movie for a second night in a row, while others actually did. We hypothesized that rewatches may be more enjoyable than people think.

Method

Participants. We recruited 130 subject pool participants to complete a two-night study for $20.00, yielding 120 participants who completed all procedures ($M_{age} = 20.86$, $SD_{age} = 3.39$; 56.67% female; 35.00% Caucasian American/White, 14.17% African American/Black, 35.00% Asian American/Asian, 15.83% Mixed/Other Ethnicity).

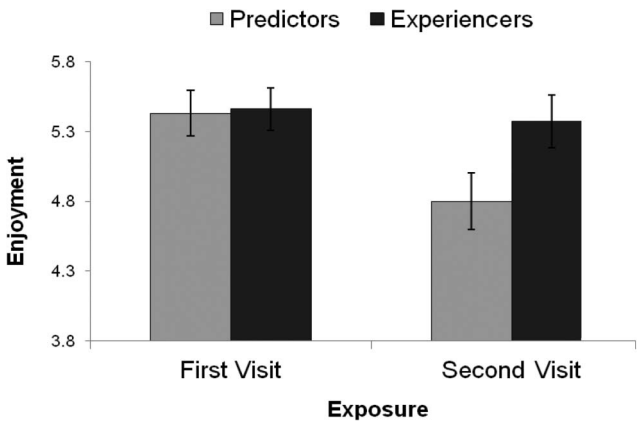

Figure 1. Enjoyment for the exhibit at first and second visit (Study 1). $M \pm 1$ *SE*.

Procedure. Participants were recruited in-person where an experimenter guided them through all study procedures via private individual sessions. The study was advertised as the "At-Home Netflix Study." Right at the consent process, all participants were informed that the study required them to watch a movie that night plus a movie the following night, at the same time and in the same way (e.g., if alone, to watch alone both nights), using their own Netflix account; and that each night they would receive a personalized e-mail containing a survey about their experience. The experimenter carefully detailed this information up front to help reduce attrition and selection effects.

After consenting and confirming that they could complete all tasks as described, all participants were given identical instructions for Night 1: their task that evening was to watch any full-length movie (e.g., no comedy specials or TV shows) of their choosing on Netflix, with the only rules being to pick a movie that (a) they had never seen before, and that (b) they thought they would enjoy a lot. Otherwise they could simply watch at home at their leisure. Participants were told that all information about Night 2 would be included at the end of their Night 1 survey. Participants then provided their e-mail address, reported demographic information, and were asked to summarize their Night 1 task via an open-ended box. They left the laboratory and completed Night 1.

In the Night 1 survey that evening, all participants were asked, "So: How much did you enjoy the movie today?", and made their rating from 1 (*not at all enjoyable*) to 7 (*extremely enjoyable*). For thoroughness, they were then asked to report the name of the movie (open-ended text box, with an N/A option for privacy) and the following details: genre (check as many as appropriate: *documentary*; *comedy*; *drama*; *action*; *horror*; *other*), why they chose it (forced-choice: *just found it*; *friend recommended it*; *searched online*; *wanting to see it for a while*; *other*), approximate length (forced-choice: *less than 90 min*; *about 90 min*; *over 90 min*), what time they started (open-ended text box), who they watched with (forced-choice: *by myself*; *with 1 other person*; *with more than 1 other person*; *other*), what they did while watching (forced-choice: *basically just sat back and watched*; *did some other things*; *did lots of other things*; *other*), and whether they had ever seen the movie before (forced-choice: *yes*; *no*).

Upon completing this Night 1 survey, participants were randomly assigned to one of two conditions. Predictors ($n = 58$) read the following:

> For tomorrow you will watch a new movie. First, however, please imagine what it would be like to rewatch this movie as part of the study. That is, imagine that at this point in the study you learn that your task for Night 2 tomorrow will be to rewatch this movie. Ok: Imagine it's tomorrow and you just completed this task.

Predictors imagined being asked, "So: How much did you enjoy the movie today?", and predicted their Night 2 rating on the same scale. At this point, Predictors had finished all critical measures. Our rationale for informing Predictors about a new movie before they made predictions about rewatching was to make a more conservative test: We hoped that holding constant their general knowledge of Night 2 may allow for a cleaner assessment of the

movie itself, rather than any incidental inferences about how their ratings could or would affect the instructions to be unveiled (see Galak & Meyvis, 2011). Predictors then proceeded to watch any movie of their choosing for Night 2, and their Night 2 survey was identical to Night 1. We preregistered their Night 2 survey as incidental with no obvious hypothesis, simply included for thoroughness of design.

Experiencers ($n = 62$) were informed that their task for Night 2 was to rewatch the movie. All procedures and measures were identical to what Predictors were given, except Experiencers actually completed the task as described and rated their rewatch experience on the same scale. In their Night 2 survey, Experiencers also reported the name of the movie, start time, who they watched with, and what they did while watching, via the same items from Night 1. Reassessing these items simply allowed us to compare how Experiencers watched from night to night, serving as a general compliance check.

At the very end of the Night 2 survey, all participants completed an attention check regarding what they did in the study (forced-choice: *watched different movies*; *watched the same movie*; *played the same game*; *completed different math problems*).

Results and Discussion

The basic effect. There was no main effect of Condition, $F(1, 118) = 5.87$, $p = .017$, $\eta^2 = .05$, and a main effect of Time such that the movie was rated as growing less enjoyable over time, $F(1, 118) = 128.70$, $p < .001$, $\eta^2 = .52$. However, this was qualified by the critical interaction, $F(1, 118) = 15.45$, $p < .001$, $\eta^2 = .12$ (see Figure 2).

Replicating Study 1, pairwise comparisons reveal that participants *imagined* a steep decline: Predictors believed rewatching their movie right away ($M = 3.47$, $SD = 1.67$) would be significantly less enjoyable than their original viewing ($M = 5.29$, $SD = 1.48$), $F(1, 118) = 112.89$, $p < .001$, $\eta_p^2 = .49$ ($d = 1.53$), 95% CI_{diff} [−2.17, −1.49]. In reality, this was exaggerated: for Experiencers, rewatches were less enjoyable ($M = 4.52$, $SD = 1.45$) than original viewings ($M = 5.40$, $SD = 1.26$), but this decline was less dramatic than imagined, $F(1, 118) = 28.43$, $p < .001$, $\eta_p^2 = .19$ ($d = 0.63$), 95% CI_{diff} [−1.22, −0.56]. The significant interaction indicates a significant difference between predicted decline ($M = -1.83$, $SD = 1.20$) and actual decline ($M = -0.89$, $SD = 1.40$).

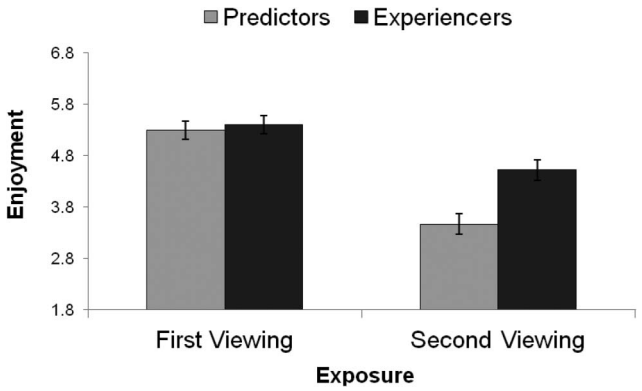

Figure 2. Enjoyment for the movie at first and second viewing (Study 2). $M \pm 1$ *SE*.

Other variables. In total, there were 85 unique movies chosen at Night 1 (as well as 11 participants who chose not to disclose). For genre, a quarter of participants chose a mix (26.70%, 32 of 120), with most others choosing either a drama (29.50%, 26 of 88) or comedy (28.40%, 25 of 88). Most movies were chosen because participants had wanted to see their choice for a while (49.20%, 59 of 120). Most movies were more than 90 min (55.80%, 67 of 120), were started between 6:00 p.m. and 10:00 p.m. (77.50%, 93 of 120), were watched alone (72.50%, 87 of 120), and were watched with full focus (60.80%, 73 of 120). All participants confirmed that they had never seen the movie before (100.00%, 120 of 120), and nearly all passed the attention check (98.30%, 118 of 120). Likewise, Experiencers maintained their watching conditions from Night 1 to Night 2 as instructed: They reported similar start times, $r = .86$, $p < .001$, watching groups, $r = .67$, $p < .001$, and focus, $r = .38$, $p = .002$, and nearly all typed the same title (96.77%, 60 of 62).[4]

Study 2 provides further support for the hypothesis: Rewatching the same movie two nights in a row proved to be surprisingly enjoyable. Next, we conducted a third test to establish this basic effect, moving to the domain of playing video games. We extended Studies 1 and 2 by assessing longer repetition cycles and more thorough measures of hedonic reactions, and also by manipulating the number of ratings to address incidental demand.

Study 3: Replaying the Same Video Game

In Study 3, participants played an interactive video game. We hypothesized that participants may assume they would grow tired of replaying the game more quickly than they indeed would if they actually played repeatedly.

Method

Participants. We requested and yielded 400 participants from Amazon's Mechanical Turk ($M_{age} = 36.47$, $SD_{age} = 11.35$; 49.50% female; 76.50% Caucasian American/White, 7.50% African American/Black, 8.50% Asian American/Asian, 7.50% Mixed/Other Ethnicity) to complete the study for $1.00.

Procedure. Participants played a video game that involves creating free-form art. We hired a programmer to design the game for our research. In the game, players are presented with a blank "canvas" window and a toolbar of different shapes and colors. After selecting a tool, players can click and drag anywhere on the canvas to "paint." Otherwise players can explore however they wish, changing shapes and colors as many times as desired. The game was embedded within their Qualtrics survey window. A copy of the game is included in our shared study materials.

To begin, all participants were informed that they would play the art game across five 1-min "Playing Sessions," with each Playing Session starting from scratch with a blank canvas. Then, they played Playing Session #1. The phrase "You are now in a

[4] Eliminating these two participants does not affect any result (interaction: $F[1, 116] = 14.58$, $p < .001$, $\eta^2 = .11$). This is true for all studies with such variables (e.g., eliminating participants who report technical errors). We retain all participants to maximize power.

Playing Session" was displayed above the canvas, and a 1-min countdown timer was shown underneath. The page automatically continued when the timer expired.

Having now experienced the stimulus once in full, at this point participants were randomly assigned to one cell of a 2 (Role: Predictors, Experiencers) × 2 (Rating: Rate 5 times, Rate 2 times) between-subjects design.

Participants in the Predictor-Rate 5 condition (n = 99) were asked to imagine that they had not yet seen or played the art game and were starting the study anew, and at that moment they played each of the five Playing Sessions as described. Specifically, they read, "Imagine you click to the next page to begin, and you play the game for Playing Session #1 of 5. After playing for one minute, imagine we ask you to rate your reactions during this Playing Session #1 of 5," and they predicted how *excited*, *bored* [reverse coded], *interested*, *amazed*, and *shocked* they would be each from 1 (*not at all*) to 7 (*extremely*). They repeated this ratings procedure for all five Playing Sessions.

Participants in the Experiencer-Rate 5 condition (n = 99) immediately rated their actual Playing Session #1 via the same scale. They then actually played each remaining Playing Session, filled in with corresponding labels, and rated their reactions after each.

For comparison, we also included Predictor-Rate 2 (n = 102) and Experiencer-Rate 2 (n = 100) conditions, in which all procedures were identical except participants made ratings only after Playing Session #1 and Playing Session #5. We included these conditions to rule out incidental demand: Being asked to make five sets of ratings may artificially inflate expectations about decline (and this may be especially true among predictors, whose ratings are not spaced out by the actual playing experiences). A strong test of our hypothesis predicts the effect should still emerge between these conditions.

Finally, all participants reported demographic information, reported any technical difficulties (forced-choice: *yes*; *no*), and completed attention checks regarding their Role condition (forced-choice: *imagined playing the later rounds*; *played the later rounds*) and their Rating condition (forced-choice: *made 5 sets of ratings*; *made 2 sets of ratings*).

Results and Discussion

For each Playing Session, the five items were collapsed into a hedonic reaction scale (αs ≥ .84). First, we report the analyses including only the Rate-5 conditions, reflecting a 2 (between-subjects, Predictors vs. Experiencers) × 5 (within-subjects, each of 5 sessions of ratings) Repeated Measures GLM. This tests for a replication of the basic effect before considering any differences across number of ratings (we cannot run a single analysis with all conditions because the Rate-2 counterparts lack data for Playing Sessions #2–#4).

The basic effect. There was a main effect of Role, $F(1, 196) = 8.03$, $p = .005$, $\eta^2 = .04$, and a main effect of Time such that the game was rated as growing less intense over time, $F(4, 193) = 53.42$, $p < .001$, $\eta^2 = .53$. However, this was qualified by the critical interaction, $F(4, 193) = 7.19$, $p < .001$, $\eta^2 = .13$ (see Figure 3).

Pairwise comparisons reveal no differences in reactions at Playing Session #1 (Predictors M = 4.48, SD = 1.29; Experiencers M = 4.49, SD = 1.24), $F(1, 196) = 0.01$, $p = .929$, $\eta_p^2 < .001$ (d = 0.01), 95% CI_{diff} [−0.37, 0.34], or Playing Session #2 (Predictors M = 4.03, SD = 1.31; Experiencers M = 4.29, SD = 1.32), $F(1, 196) = 1.94$, $p = .166$, $\eta_p^2 = .01$ (d = 0.20), 95% CI_{diff} [−0.63, 0.11]—but participants significantly underestimated their reactions at Playing Session #3 (Predictors M = 3.51, SD = 1.43; Experiencers M = 4.08, SD = 1.42), $F(1, 196) = 7.93$, $p = .005$, $\eta_p^2 = .04$ (d = 0.40), 95% CI_{diff} [−0.97, −0.17]; Playing Session #4 (Predictors M = 2.99, SD = 1.44; Experiencers M = 3.84, SD = 1.57), $F(1, 196) = 15.90$, $p < .001$, $\eta_p^2 = .08$ (d = 0.56), 95% CI_{diff} [−1.28, −0.43]; and Playing Session #5 (Predictors M = 2.72, SD = 1.52; Experiencers M = 3.66, SD = 1.64), $F(1, 196) = 17.24$, $p < .001$, $\eta_p^2 = .08$ (d = 0.59), 95% CI_{diff} [−1.38, −0.49].

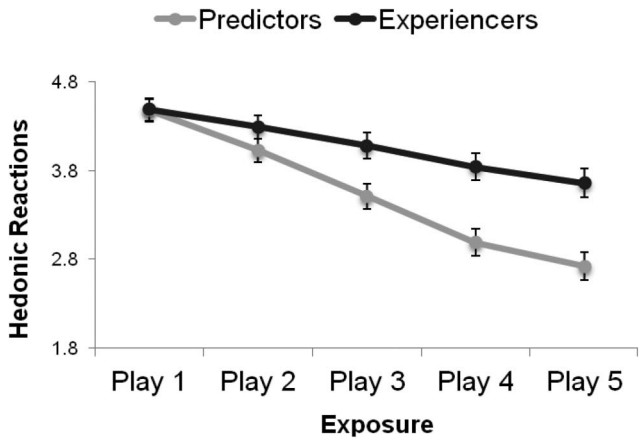

Figure 3. Hedonic reactions to replaying the same game (Study 3). $M \pm 1$ *SE*.

These results are echoed in our supplementary change analyses: The overall difference (Playing Session #5 - Playing Session #1) in predicted decline (M = −1.76, SD = 1.32) and actual decline (M = −0.84, SD = 1.28), $t(196) = -4.96$, $p < .001$, $d = 0.71$, 95% CI_{diff} [−1.28, −0.55] was significant. We also observed an interaction via growth curve analyses, $F(1, 949.22) = 30.89$, $p < .001$, indicating that Predictors declined at a steeper rate (B = −0.44, SE = .03, $p < .001$) than Experiencers (B = −0.21, SE = .03, $p < .001$).

No influence of number of ratings. When rerunning these analyses including the Rating variable, all effects above remain significant: the main effect of Role, $F(1, 396) = 15.70$, $p < .001$, $\eta^2 = .04$, the main effect of Time, $F(1, 396) = 245.83$, $p < .001$, $\eta^2 = .38$, and the critical Role × Time interaction, $F(1, 396) = 77.60$, $p < .001$, $\eta^2 = .16$.

Otherwise there were no substantive effects of Rating. There was no main effect of Rating, $F(1, 396) = 1.32$, $p = .251$, $\eta^2 = .003$, a Rating × Time interaction, $F(1, 396) = 4.81$, $p = .029$, $\eta^2 = .01$, and no Rating × Role interaction, $F(1, 396) = 0.15$, $p = .696$, $\eta^2 < .001$. These effects were qualified by a three-way interaction, $F(1, 396) = 6.11$, $p = .014$, $\eta^2 = .02$, but again, this was incidental: maintaining the basic effect, Predictors who made only 2 ratings reported significant decline from Playing Session #1 (M = 4.60, SD = 1.47) to Playing Session #5 (M = 2.80, SD = 1.61), $F(1, 396) = 156.45$, $p < .001$, $\eta_p^2 = .28$ (d = 0.95), 95% CI_{diff} [−2.08, −1.51], whereas Experiencers who made only two

ratings did not actually decline (M = 4.36, SD = 1.49 vs. M = 4.20, SD = 1.79), $F(1, 396) = 1.22$, $p = .271$, $\eta_p^2 = .003$ (d = 0.14), 95% CI_{diff} [−0.45, 0.13]. Thus, the three-way interaction reflects the fact that making only two ratings made the game better for Experiencers as compared with having to rate it each and every time; but number of ratings did not affect Predictors, nor affect the key discrepancy between Predictors and Experiencers (see Figure 4).

Other variables. Finally, nearly all participants reported that the game loaded with no technical issues (99.25%, 397 of 400), passed the attention check for Role (98.25%, 393 of 400), and also the attention check for Rating (95.75%, 383 of 400).

Study 3 provides a third replication of the basic effect, while also extending to another activity (playing games), using more thorough dependent measures, using longer repetition cycles, and accounting for incidental demand in making multiple ratings.

Next, Studies 4 and 5 move beyond documenting the basic effect to also unpacking why it occurs. We examined the yet-untested role of discovering new content (e.g., missed or forgotten details) in contributing to the basic effect. That is, repetition may help to *uncover* novelty, and this novelty may make actual repetition less uniformly dull as compared to people's simulations of literally repeating the same already-viewed content. We sought to establish this mechanism beyond the possibility that people overlook the pure effects of fluency and mere exposure, which would predict that people should still underappreciate repeat experiences to the same degree even in the absence of noticing anything new, so long as exposure is held constant; if so, literally repeating the same exact content should also be surprisingly enjoyable to the same mispredicted extent. We disentangle these possibilities by testing for the unique role of novel discovery via both moderation-based (Studies 4 and 5) and mediation-based (Study 5) approaches.

Study 4: Manipulating Complexity

If repeat experiences are surprisingly enjoyable solely because of a neglect of fluency and mere exposure, the misprediction should emerge to the same degree even for repeating barren experiences with low experiential value. However, if the misprediction also comprises a neglect of remaining newness, it should emerge even more strongly for repeating for rich experiences that *also* contain layers for people to miss at first glance.

Study 4 tested these possibilities. Participants viewed one of three collages of enjoyable images, which varied in complexity. Participants either predicted or reported their reactions to repeat viewings. We hypothesized that the gap between prediction and experience may be largest for the collage that could actually offer the most newness if repeated. Somewhat paradoxically, our framework suggests it is exactly those stimuli that promise *high* richness that people may tend to overlook the most, because of static simulation.

Method

Participants. We requested 1,000 participants from Amazon's Mechanical Turk, yielding 1,011 participants (M_{age} = 36.66, SD_{age} = 11.66; 50.94% female; 70.03% Caucasian American/White, 9.10% African American/Black, 11.97% Asian American/Asian, 8.90% Mixed/Other Ethnicity) who completed the study for $0.75.

Procedure. Participants were randomly assigned to one cell of a 2 (Role: Predictors, Experiencers) × 3 (Collage: Rich, Barren, Piece) between-subjects design.

Participants were informed that they would view the same image across five consecutive "Viewing Sessions" and report their reactions each time. A single Viewing Session entailed the image flashing on the screen for 10 seconds (with a countdown timer shown underneath), then disappearing and being replaced with a ratings screen. For all participants, the image was a collage containing nine individual square images combined into one large square. Based on random assignment, some participants viewed a Rich collage containing nine enjoyable animal photographs that offered many things to notice (e.g., a real puppy hiding in a pile of puppy dolls; the intricate details of a turtle's shell; a bug camouflaged as a leaf). In principle, participants should be able to realize that they likely missed various complexities given the short exposure time, but a strong test of our hypothesis suggests they may not. For a control comparison, we used a collage that was also enjoyable, but could be entirely encoded in a single viewing: These participants viewed a Barren collage of nine identical glowing blue orbs. However, because these two collages also vary in content aside from the intended variation in richness, we included a third control comparison that might fall somewhere in the middle: In the Piece condition, participants first viewed the Rich collage and

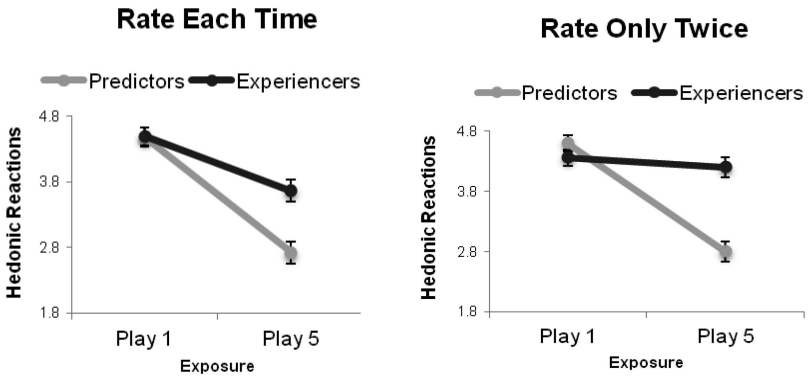

Figure 4. Repetition as a function of number of ratings (Study 3). $M \pm 1$ *SE*.

selected one enjoyable image from the nine individual options. Then, in their Viewing Sessions, they viewed the Rich collage with all squares whited out except their choice. All collages were designed for this study and were presented the same way (e.g., as identical sizes, at identical parts of the screen).

To begin, all participants completed their first Viewing Session. After, they were asked, "What were your reactions during this period of Viewing #1 of 5?", and they rated three items: "enjoyed looking at the image," "the image was stimulating," and "the image kept my interest," each from 1 (*not at all*) to 7 (*extremely*).

Participants then repeated this process for all remaining Viewing Sessions. For these Viewing Sessions #2–#5, participants in the Predictor-Rich condition ($n = 160$), Predictor-Barren condition ($n = 167$), and Predictor-Piece condition ($n = 165$) viewed a placeholder of the same size that read: "Imagine the image is being displayed here, right now." In their rating screens, they were asked, "What do you think your reactions would be during this period of Viewing #X of 5? (if the preceding screen contained the actual image, not the placeholder you just saw)." Participants in the Experiencer-Rich condition ($n = 166$), Experiencer-Barren condition ($n = 172$), and Experiencer-Piece condition ($n = 181$) actually viewed their collage and reported each reaction. Note that this design further rules out procedural artifacts by maintaining the same exact structural procedures for all participants (e.g., the same window of time between each exposure and rating).

Following all Viewing Sessions, all participants completed manipulation checks: They rated the complexity of their image from 1 (*not many different things going on*) to 7 (*a ton of different things going on*), and the valence of their image from 1 (*it was very negative/bad*) to 7 (*it was very positive/good*). These items were prefaced with the phrase, "Forget about your repetition part; assess the objective image." All Predictors also rated how difficult it felt to use their imagination, from 1 (*easy enough*) to 7 (*quite difficult*).

Finally, all participants reported demographic information, reported any technical difficulties (forced-choice: *yes*; *no*), and completed attention checks regarding their Role condition (forced-choice: *imagined viewing*; *actually viewed*) and their Collage condition (forced-choice: *images of animals/nature*; *images of blue circles*). In addition, for Predictor-Piece participants only, we asked what they had imagined when they viewed the placeholder (forced-choice: *my single chosen image*; *the full original collage*). We included this item to ensure they brought to mind what we had intended.

Results and Discussion

For each Viewing Session, the three items were collapsed into an enjoyment scale ($\alpha s \geq .90$). A replication of the basic effect observed throughout Studies 1–3 would be reflected in an interaction between Role and Time. A test of the moderation hypothesis for this specific study would be reflected in a further three-way interaction with Collage.

The basic effect. There was a main effect of Role, $F(1, 1{,}005) = 100.89$, $p < .001$, $\eta^2 = .09$, and a main effect of Time such that the collages were rated as growing less enjoyable over time, $F(4, 1{,}002) = 261.86$, $p < .001$, $\eta^2 = .51$. Replicating the basic effect, this was qualified by a Role × Time interaction, $F(4, 1{,}002) = 52.18$, $p < .001$, $\eta^2 = .17$.

As it turns out, pairwise comparisons reveal that *all* participants underappreciated their enjoyment of repetition to a significant extent (see Table 1 for descriptive statistics):

First, there were no differences between Predictors and Experiencers at Viewing Session #1 (Rich: $F(1, 1{,}005) = 0.17$, $p = .678$, $\eta_p^2 < .001$ ($d = 0.06$), 95% CI_{diff} [−0.21, 0.32]; Piece: $F(1, 1{,}005) = 1.24$, $p = .266$, $\eta_p^2 = .001$ ($d = 0.14$), 95% CI_{diff} [−0.11, 0.40]); Barren: $F(1, 1{,}005) = 0.08$, $p = .780$, $\eta_p^2 < .001$ ($d = 0.02$), 95% CI_{diff} [−0.22, 0.30]. These null effects are unsurprising because Viewing Session #1 preceded any Role manipulation.

However, all Predictors significantly *under*estimated the enjoyment of their corresponding Experiencers for Viewing Session #2 (Rich: $F(1, 1{,}005) = 57.69$, $p < .001$, $\eta_p^2 = .05$ ($d = 0.85$), 95% CI_{diff} [−1.64, −0.96]; Piece: $F(1, 1{,}005) = 48.19$, $p < .001$, $\eta_p^2 = .05$ ($d = 0.76$), 95% CI_{diff} [−1.48, −0.83]); Barren: $F(1, 1{,}005) = 14.31$, $p < .001$, $\eta_p^2 = .01$ ($d = 0.39$), 95% CI_{diff} [−0.96, −0.31]); Viewing Session #3 (Rich: $F(1, 1{,}005) = 67.86$, $p < .001$, $\eta_p^2 = .06$ ($d = 0.94$), 95% CI_{diff} [−1.74, −1.07]; Piece: $F(1, 1{,}005) = 49.25$, $p < .001$, $\eta_p^2 = .05$ ($d = 0.78$), 95% CI_{diff} [−1.49, −0.84]); Barren: $F(1, 1{,}005) = 15.17$, $p < .001$, $\eta_p^2 = .02$ ($d = 0.40$), 95% CI_{diff} [−0.98, −0.33]); Viewing Session #4 (Rich: $F(1, 1{,}005) = 68.12$, $p < .001$, $\eta_p^2 = .06$ ($d = 0.92$), 95% CI_{diff} [−1.82, −1.12]; Piece: $F(1, 1{,}005) = 32.41$, $p < .001$, $\eta_p^2 = .03$ ($d = 0.63$), 95% CI_{diff} [−1.32, −0.65]); Barren: $F(1, 1{,}005) = 13.62$, $p < .001$, $\eta_p^2 = .01$ ($d = 0.39$), 95% CI_{diff} [−0.99, −0.30]); and Viewing Session #5 (Rich: $F(1, 1{,}005) = 55.91$, $p < .001$, $\eta_p^2 = .05$ ($d = 0.81$), 95% CI_{diff} [−1.81, −1.06]; Piece: $F(1, 1{,}005) = 28.65$, $p < .001$, $\eta_p^2 = .03$ ($d = 0.57$), 95% CI_{diff} [−1.36, −0.63]); Barren: $F(1, 1{,}005) = 12.46$, $p < .001$, $\eta_p^2 = .01$ ($d = 0.39$), 95% CI_{diff} [−1.03, −0.29]). Across all stimuli, actual repeat viewings were more enjoyable than participants imagined.

Table 1
Mean Hedonic Reactions in Study 4

	Rich collage		Piece collage		Barren collage	
View	Predicted	Experienced	Predicted	Experienced	Predicted	Experienced
View #1	5.94 (.94)	5.88 (1.09)	5.60 (1.13)	5.75 (1.07)	4.33 (1.49)	4.36 (1.47)
View #2	4.55 (1.84)	5.85 (1.12)	4.39 (1.80)	5.54 (1.14)	3.44 (1.64)	4.07 (1.60)
View #3	4.15 (1.70)	5.56 (1.27)	4.04 (1.63)	5.21 (1.37)	3.04 (1.56)	3.69 (1.67)
View #4	3.87 (1.70)	5.34 (1.49)	3.80 (1.58)	4.79 (1.55)	2.72 (1.56)	3.36 (1.75)
View #5	3.56 (1.78)	4.99 (1.73)	3.53 (1.70)	4.52 (1.79)	2.47 (1.52)	3.13 (1.82)

Note. *SD*s are shown in parentheses.

Again, these results are echoed in our supplementary change analyses: The overall differences (Viewing Session #5 – Viewing Session #1) show that predicted decline was significantly greater than actual decline, each for the Rich collage ($M = -2.38$, $SD = 1.69$ vs. $M = -0.89$, $SD = 1.51$), $F(1, 1{,}005) = 70.29$, $p < .001$, $\eta_p^2 = .07$ ($d = 0.93$), 95% CI_{diff} [−1.84, −1.14]; the Piece collage ($M = -2.07$, $SD = 1.71$ vs. $M = -1.22$, $SD = 1.60$), $F(1, 1{,}005) = 24.31$, $p < .001$, $\eta_p^2 = .02$ ($d = 0.51$), 95% CI_{diff} [−1.19, −0.51]; and the Barren collage ($M = -1.86$, $SD = 1.61$ vs. $M = -1.23$, $SD = 1.50$), $F(1, 1{,}005) = 12.93$, $p < .001$, $\eta_p^2 = .01$ ($d = 0.40$), 95% CI_{diff} [−0.97, −0.28]. Likewise, we observed the same Role × Time interaction via growth curve analyses, $F(1, 5035.58) = 85.92$, $p < .001$, indicating that Predictors declined at a steeper rate than Experiencers each for the Rich collage ($B = -0.59$, $SE = .04$, $p < .001$ vs. $B = -0.22$, $SE = .02$, $p < .001$), the Piece collage ($B = -0.51$, $SE = .04$, $p < .001$ vs. $B = -0.31$, $SE = .02$, $p < .001$), and the Barren collage ($B = -0.46$, $SE = .04$, $p < .001$ vs. $B = -0.31$, $SE = .03$, $p < .001$).

Moderation by stimulus. In terms of the other remaining effects in the output, we report Roy's Largest Root where applicable, but all significance levels remain the same regardless of test. There was a main effect of Collage, $F(2, 1{,}005) = 123.74$, $p < .001$, $\eta^2 = .20$, no Collage × Time interaction, $F(4, 1{,}003) = 1.40$, $p = .231$, $\eta^2 = .01$, and a Collage × Role interaction, $F(2, 1{,}005) = 4.09$, $p = .017$, $\eta^2 = .01$. These are incidental. Last and most relevant to our hypothesis, the basic effect was indeed qualified by a significant three-way interaction, $F(4, 1{,}003) = 6.07$, $p < .001$, $\eta^2 = .02$ (growth curve: $F[2, 5035.58] = 5.88$, $p = .003$).

Despite all participants showing a significant misprediction, the three-way interaction here indicates that the size of the misprediction differed depending on what was repeated. Figure 5 visualizes the means that were reported in Table 1. As can be seen, the size of the discrepancy tracked with stimulus complexity: The biggest gap emerged for the Rich collage, then the Piece collage, then the Barren collage. These results are consistent with our hypothesis: Dulled expectations about repetition may be most problematic when there is much to see, but serve people better if they *can* "see all there is" the first time around.

Further, we sought to discern whether this three-way interaction reflects the sole fact that the Barren collage had the (visibly) smallest gap, but perhaps no statistical difference in the gaps between the Rich versus Piece collages. We reran our main repeated-measures GLM analyses excluding the Barren conditions, and all critical results held: the Role × Time interaction, $F(4, 665) = 47.14$, $p < .001$, $\eta^2 = .22$ (growth curve: $F(1, 3333.15) = 78.23$, $p < .001$), qualified by the three-way interaction with Collage (i.e., Rich vs. Piece), $F(4, 665) = 2.77$, $p = .027$, $\eta^2 = .02$ (growth curve: $F[1, 3333.15] = 6.08$, $p = .014$).

Other variables. Finally, nearly all participants reported that the stimuli loaded with no technical issues (99.41%, 1,005 of 1,011), and passed the attention checks for Role (95.45%, 965 of 1,011) and for Collage (99.41%, 1,005 of 1,011). Nearly all Predictor-Piece participants passed their specific condition check (94.55%, 156 of 165).

For the manipulation check for complexity, there was a significant effect of Collage, $F(2, 1{,}005) = 383.38$, $p < .001$, $\eta^2 = .43$. The manipulation worked as intended: the Rich collage was rated more complex ($M = 6.03$, $SD = 1.29$) than the Piece collage ($M = 4.14$, $SD = 1.62$), $p < .001$, $d = 1.29$, 95% CI_{diff} [1.65, 2.12], and the Piece collage was rated as more complex than the Barren collage ($M = 2.75$, $SD = 1.65$), $p < .001$, $d = 0.85$, 95% CI_{diff} [1.16, 1.62] (in turn, the Rich collage was also rated as more complex than the Barren collage, $p < .001$, $d = 2.21$, 95% CI_{diff} [3.04, 3.51]). There were no other substantive effects on complexity (Role: $F[1, 1{,}005] = 1.35$, $p = .246$, $\eta^2 = .001$; interaction, $F[2, 1{,}005] = 1.24$, $p = .291$, $\eta^2 = .002$).

For the manipulation check for valence, we also found an (unintended) significant effect of Collage, $F(2, 1{,}005) = 13.76$, $p < .001$, $\eta^2 = .03$, such that the Barren collage was rated as the least positive: the Rich collage ($M = 6.55$, $SD = 0.94$) was more positive than the Barren collage ($M = 6.19$, $SD = 1.31$), $p < .001$, $d = 0.31$, 95% CI_{diff} [0.20, 0.52], as was the Piece collage ($M = 6.56$, $SD = 0.86$), $p < .001$, $d = 0.33$, 95% CI_{diff} [0.22, 0.53]. However, the Rich collage and the Piece collage were similarly positive, $p = .836$, $d = 0.01$, 95% CI_{diff} [−0.18, 0.14] (i.e., moderation of the basic effect cannot purely be a function of differences in valence, since the basic effect still emerged between Rich vs. Piece conditions). There were no other substantive effects on valence (Role: $F[1, 1{,}005] = 4.52$, $p = .034$, $\eta^2 = .004$; interaction, $F[2, 1{,}005] = 0.14$, $p = .869$, $\eta^2 < .001$).

For the manipulation check for difficulty of imagination (Predictor conditions), there was a significant effect of Collage, $F(2, 489) = 4.66$, $p = .010$, $\eta^2 = .02$. This was driven by the Rich collage ($M = 2.98$, $SD = 1.69$) being harder to imagine than the

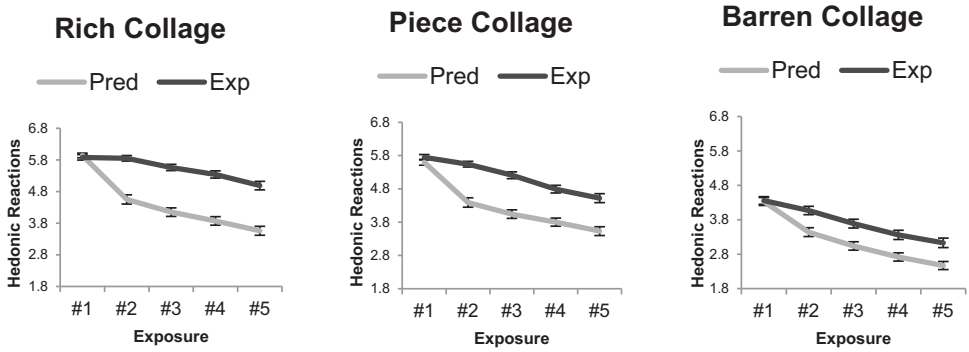

Figure 5. Repetition as a function of stimulus complexity (Study 4). $M \pm 1$ *SE*.

Barren collage ($M = 2.41$, $SD = 1.67$), $p = .002$, $d = 0.36$, 95% CI_{diff} [0.20, 0.94] (the Piece collage [$M = 2.71$, $SD = 1.75$] did not statistically differ from the Barren collage, $p = .150$, $d = 0.17$, 95% CI_{diff} [−0.07, 0.67]). However, there were no differences between the Rich collage and the Piece collage, $p = .129$, $d = 0.16$, 95% CI_{diff} [−0.10, 0.64] (i.e., moderation of the basic effect cannot purely be a function of differences in difficulty).

Study 4 again replicates the basic effect—people underappreciate enjoyment for repeat experiences—while also suggesting why. First, all Predictors underestimated their enjoyment for repetition regardless of stimulus, suggestive of the unforeseen pleasures of mere exposure and fluency. Second, the effect cannot purely be attributable to this mechanism, because exposure was held constant across stimuli and we still observed differences in the size of the misprediction. Experiences that promise the most layers upon repetition may go most underappreciated, as people may not fully realize the limits of what they extracted in just a single exposure.

Next, Study 5 sought to establish more direct evidence for this process. Rather than manipulating different stimuli, all participants experienced the same "rich" stimulus and reported the amount of new information that they did (or thought they would) notice from exposure to exposure. We tested whether discovering newness indeed varies by condition, and serves as an explicit mediator of the basic effect on enjoyment. Moreover, we assessed other moderation evidence: By identifying what Predictors might be missing, it follows that *giving people* this piece should help reignite (and thus calibrate) expected dullness. If a neglect of new features is a unique missing piece over and above a neglect of mere exposure, then cues that bring to mind novelty should help improve predictions in this context.

Study 5: Uncovering Novelty in Repetition

Method

In Study 5, Experiencers repeatedly played the same video game. For comparison, we assessed the accuracy of three different Predictor conditions, all of whom first played one round of the game before predicting their reactions to the repeated rounds of playing (as in previous studies): Control Predictors (who participated as in previous studies), High-Novelty Predictors (who were given a cue to infer that they had not yet experienced much of the game), and Low-Novelty Predictors (who, via the same manipulation, were given a cue to infer that they had already experienced much of the game).

Our hypothesis was threefold: (a) Control Predictors should replicate the basic effect from previous studies, such that Experiencers' actual experience of replaying the game may be more enjoyable than Control Predictors expect; (b) this effect should be driven by estimates corresponding misperceptions in the amount of new things noticed; and (c) after being given a cue designed to raise these estimates, High-Novelty Predictors should become more accurate and thus look more like Experiencers (whereas, conversely, Low-Novelty Predictors should remain looking like Control Predictors).

Participants. We requested 400 participants from Amazon's Mechanical Turk, yielding 401 participants ($M_{age} = 38.44$, $SD_{age} = 12.28$; 48.38% female; 77.31% Caucasian American/White, 9.48% African American/Black, 5.74% Asian American/Asian, 7.48% Mixed/Other Ethnicity) who completed the study for $1.00.

Procedure. We used the same game from Study 3. All participants began by first playing one round of the game. After this one-minute playing session, all participants rated this Playing Session #1 via three items: "enjoyable," "pleasurable," and "liked it," each from 1 (*not at all*) to 7 (*extremely*). Participants were then randomly assigned to one of four Role conditions.

Experiencers ($n = 107$) actually played each remaining playing session (five in total), but only rated the game again after Playing Session #5 (i.e., the design resembles the Rate 2 conditions from Study 3). Changes in enjoyment in this condition mark the benchmark for accuracy. In counterbalanced order, after Playing Session #5 participants also rated, "As you replayed this game more and more, what best describes what you did? For me, this involved . . ." from 1 (*basically doing the same stuff over and over*) to 7 (*discovering new, different things to do in the game*). This item served as our proposed mediator, assessing the extent to which repeat experiences entailed novel discovery.

Other participants were assigned to one of three different kinds of Predictor conditions. Control Predictors ($n = 100$) imagined completing Playing Sessions #2–5 and predicted their reactions after Playing Session #5, on the same enjoyment block and the mediator item. We hypothesized that the basic effect should replicate between Experiencers and Control Predictors (i.e., Control Predictors may underestimate enjoyment for repetition, and this may be driven by corresponding underestimations of novel discovery).

High-Novelty Predictors ($n = 95$) had identical prompts and procedures. However, right after rating Playing Session #1 (i.e., before they imagined Playing Sessions #2–#5, and made their predictions for Playing Session #5), they completed an additional task that involved listing things they had discovered in the game so far. They read:

> Before moving on: Please list 3 of the things you did in the game (e.g., a tool you used, a color you made, a movement you made, etcetera). Please just write blurbs! No need for full sentences. This task is simply to help get your mind going thinking about the things players can do in the game. You've discovered things by virtue of playing for the past minute. Note: If you played for more rounds (i.e., more minutes than you just played now), past participants like you have reported about 20–30 new details they ended up discovering, all told, after playing beyond this one initial minute. You've discovered a handful of these things so far.

Underneath, they saw three open-ended text boxes labeled "Thing #1 I did in the game," "Thing #2 I did in the game," and "Thing #3 I did in the game," and freely typed their responses to each. After completing this listing task, they completed the remainder of the study exactly as Control Predictors. We hypothesized that, by affecting the mediator item, this task should affect the basic effect on enjoyment: The note serves as a cue that there are more new things to discover in the game, and thus should boost (and calibrate) their predicted enjoyment for repetition if novel discovery is indeed a critical piece.

Low-Novelty Predictors ($n = 99$) completed this listing task in identical ways to High-Novelty Predictors, except their prompt excluded the "Note:" text. This condition can rule out incidental effects of the listing task that may boost predictions (e.g., perhaps

spending additional time to reflect on specific fun experiences may increase excitement for replaying the game, rather than the fact that those fun experiences represent a sliver of remaining experiences to enjoy). We hypothesized that, because our framework posits that perceived novelty is critical, this manipulation should not lead Low-Novelty Predictors to be more calibrated in their predictions as compared to Experiencers.

At the end of the study, High-Novelty Predictors and Low-Novelty Predictors completed manipulation checks regarding their listing task: They rated how the listing task made them feel about the amount of things they had noticed from 1 (*like I got as much out of Playing Session #1 as I could've gotten*) to 7 (*like I could've gotten more out of Playing Session #1 than I did*), and how difficult it was to recall 3 things from 1 (*easy enough*) to 7 (*very difficult*).

Finally, all Predictors (including Control Predictors) then rated how difficult it was to imagine the game from 1 (*easy enough*) to 7 (*very difficult*), and all participants (including Experiencers) rated how confusing they found the overall study from 1 (*not at all*) to 7 (*extremely*). All participants reported demographic information, reported any technical difficulties (forced-choice: *yes*; *no*), and completed an attention check regarding their Role condition (forced-choice: *imagined playing the later rounds*; *played the later rounds*). All Predictors also completed an attention check regarding whether they were given a listing task (forced-choice: *yes*; *no*).

Results and Discussion

For each playing session, the three dependent measures were collapsed into an enjoyment scale (αs $\geq$ .97).

The basic effect, moderated. There was a main effect of Role, $F(3, 397) = 2.97$, $p = .032$, $\eta^2 = .02$, and a main effect of Time such that the game was rated as growing less enjoyable over time, $F(1, 397) = 34.90$, $p < .001$, $\eta^2 = .08$. However, this was qualified by the critical interaction, $F(3, 397) = 13.23$, $p < .001$, $\eta^2 = .09$ (see Figure 6).

Teasing apart this interaction, pairwise comparisons first reveal a replication of the basic effect between Control Predictors and Experiencers, as in all previous studies: Control Predictors imagined a significant decline from Playing Session #1 ($M = 5.20$, $SD = 1.46$) to Playing Session #5 ($M = 4.56$, $SD = 1.90$), $F(1, 397) = 20.68$, $p < .001$, $\eta_p^2 = .05$ ($d = 0.46$), 95% CI_{diff} [−0.92, −0.36], but in reality Experiencers' enjoyment stayed high from Playing Session #1 ($M = 5.31$, $SD = 1.57$) to Playing Session #5 ($M = 5.44$, $SD = 1.61$), $F(1, 397) = 0.84$, $p = .360$, $\eta_p^2 = .002$ ($d = 0.10$), 95% CI_{diff} [−0.14, 0.39].

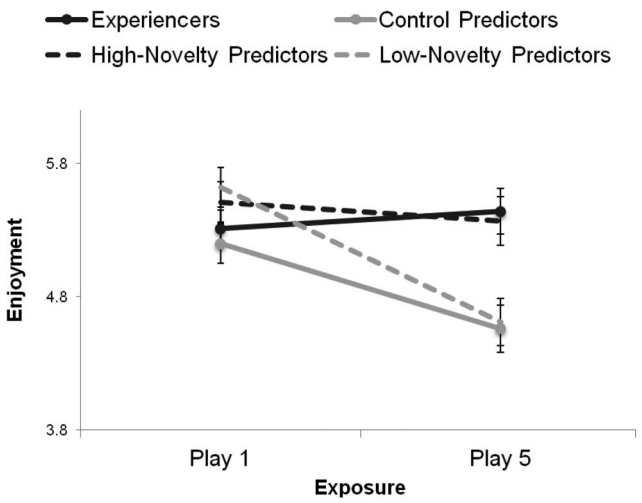

Figure 6. Prediction accuracy for replaying the same game (Study 5). $M \pm 1$ *SE*.

Did the other Predictors do better? It depended on the kind of listing task they completed beforehand. High-Novelty Predictors resembled Experiencers: They predicted enjoyment would stay high from Playing Session #1 ($M = 5.51$, $SD = 1.56$) to Playing Session #5 ($M = 5.37$, $SD = 1.66$), $F(1, 397) = 0.90$, $p = .344$, $\eta_p^2 = .002$ ($d = 0.11$), 95% CI_{diff} [−0.42, 0.15]. In contrast, Low-Novelty Predictors imagined a significant decline from Playing Session #1 ($M = 5.62$, $SD = 1.31$) to Playing Session #5 ($M = 4.61$, $SD = 1.90$), $F(1, 397) = 50.99$, $p < .001$, $\eta_p^2 = .11$ ($d = 0.63$), 95% CI_{diff} [−1.29, −0.73].

To assess accuracy more precisely, we calculated difference scores for each condition and conducted a one-way ANOVA with condition as the independent variable and these difference scores as the dependent variable. There was an omnibus effect of condition, $F(3, 397) = 13.23$, $p < .001$, $\eta^2 = .09$. Planned contrasts confirm a number of insights. First, predicted decline among Control Predictors ($M = -0.64$, $SD = 1.39$) was indeed greater than actual change among Experiencers ($M = 0.12$, $SD = 1.34$), $t(397) = 3.91$, $p < .001$, $d = 0.56$. This replicates the basic effect. Second, predicted change among High-Novelty Predictors ($M = -0.14$, $SD = 1.29$) did not differ from Experiencers, $t(397) = 1.32$, $p = .188$, $d = 0.20$. This indicates that High-Novelty Predictors more accurately predicted enjoyment (High-Novelty Predictors also differed from Control Predictors, $t[397] = 2.50$, $p = .013$, $d = 0.37$). Third, unexpectedly, predicted decline among Low-Novelty Predictors ($M = -1.01$, $SD = 1.59$) marginally differed from Control Predictors, $t(397) = 1.86$, $p = .064$, $d = 0.25$—that is, Low-Novelty Predictors became marginally *more* convinced that repetition would be dull (in turn, Low-Novelty Predictors indeed differed from Experiencers, $t[397] = 5.78$, $p < .001$, $d = 0.77$, and from High-Novelty Predictors, $t[397] = 4.32$, $p < .001$, $d = 0.60$).

The same results are attained when rerunning our main repeated-measures GLM analyses excluding all but two conditions at a time. The critical Role × Time interaction remained significant when including Experiencers and Control Predictors, $F(1, 205) = 16.23$, $p < .001$, $\eta^2 = .07$ (and likewise High-Novelty Predictors and Control Predictors, $F[1, 193] = 6.85$, $p = .010$, $\eta^2 = .03$), and when including Experiencers and Low-Novelty Predictors, $F(1, 204) = 30.76$, $p < .001$, $\eta^2 = .13$ (and likewise High-Novelty Predictors and Low-Novelty Predictors, $F[1, 192] = 17.54$, $p < .001$, $\eta^2 = .08$); but the interaction was *not* significant when including Experiencers and High-Novelty Predictors, $F(1, 200) = 1.98$, $p = .161$, $\eta^2 = .01$, and was marginal when including Control Predictors and Low-Novelty Predictors, $F(1, 197) = 3.06$, $p = .082$, $\eta^2 = .02$.

Mediation. Next, we assessed whether the effects above were driven by corresponding differences in the amount of novel discovery within the act of repetition. We conducted a one-way ANOVA with condition as the independent variable and the mediator item as the dependent variable.

There was an omnibus effect of condition, $F(3, 397) = 7.34$, $p < .001$, $\eta^2 = .05$. First and foremost, planned contrasts teasing apart the basic effect reveal that Experiencers actually discovered more novelty during repetition ($M = 5.20$, $SD = 1.93$) than Control Predictors imagined they would discover ($M = 4.47$, $SD = 2.25$), $t(397) = 2.65$, $p = .008$, $d = 0.35$. A mediation analysis using these conditions (Experiencers, Control Predictors) as the independent variable, change in enjoyment as the dependent variable, and discovery as the mediator (SPSS PROCESS Model 4, 5,000 iterations: Hayes, 2013) confirmed that the indirect effect of condition on enjoyment, via discovery, was significant, Indirect Effect = 0.23, $SE = .10$; 95% CI_{boot} [0.06, 0.43] (the same results emerge when using only Rating 2 enjoyment rather than a difference score, Indirect Effect = 0.42, $SE = .17$; 95% CI_{boot} [0.10, 0.75]). These results add to our previous studies by replicating the basic effect and also providing direct evidence that it is associated with differences in predicted versus actual novel discovery during repetition.

In addition, the listing task affected the mediator in corresponding ways. High-Novelty Predictors predicted relatively high discovery ($M = 5.64$, $SD = 1.51$), which did not statistically differ from Experiencers' actual high discovery, $t(397) = 1.60$, $p = .110$, $d = 0.25$ (in turn, High-Novelty Predictors too were higher than Control Predictors, $t[397] = 4.14$, $p < .001$, $d = 0.61$). Low-Novelty Predictors predicted relatively low discovery ($M = 4.62$, $SD = 2.12$), which did not statistically differ from Control Predictors' low predictions, $t(397) = 0.52$, $p = .602$, $d = 0.07$ (in turn, Low-Novelty Predictors too were lower than Experiencers, $t[397] = 2.11$, $p = .036$, $d = 0.29$, and lower than High-Novelty Predictors, $t(397) = 3.62$, $p < .001$, $d = 0.55$). When running the same mediation model as above but collapsing across the statistically comparable conditions, the same results emerge, Indirect Effect = 0.27, $SE = .07$; 95% CI_{boot} [0.15, 0.41] (when using only Rating 2 enjoyment: Indirect Effect = 0.50, $SE = .12$; 95% CI_{boot} [0.28, 0.75]).

Other variables. For the manipulation checks, High-Novelty Predictors and Low-Novelty Predictors differed as intended: the listing task left High-Novelty Predictors feeling like they had missed more after Playing Session #1 ($M = 5.24$, $SD = 1.54$) than Low-Novelty Predictors felt ($M = 4.71$, $SD = 1.62$), $t(192) = 2.36$, $p = .019$, $d = 0.34$, 95% CI_{diff} [0.09, 0.98]. There were no incidental differences in the difficulty of the listing task between High-Novelty Predictors ($M = 2.59$, $SD = 1.76$) versus Low-Novelty Predictors ($M = 2.72$, $SD = 1.73$), $t(192) = 0.51$, $p = .611$, $d = 0.07$, 95% $CI_{diff} = [-0.62, 0.37]$. There was no omnibus effect of condition for Predictor ratings of difficulty of imagination (all Predictor conditions; $M = 2.65$, $SD = 1.62$), $F(2, 291) = 0.90$, $p = .407$, $\eta^2 = .01$, nor for ratings of study confusion (all conditions; $M = 1.77$, $SD = 1.36$), $F(3, 397) = 0.26$, $p = .851$, $\eta^2 = .002$.

Finally, nearly all participants reported that the game loaded with no technical issues (99.75%, 400 of 401), and passed the attention check for Role (96.01%, 385 of 401). Nearly all Predictors passed their specific attention check for having or not having a listing task (96.26%, 283 of 294). Returning to the main analyses, all critical results hold when entering question order as a covariate into the models (interaction for enjoyment, $F(3, 396) = 12.99$, $p < .001$, $\eta^2 = .09$; omnibus effect for discovery, $F(3, 397) = 7.08$, $p < .001$, $\eta^2 = .05$; mediation, Indirect Effect = 0.22, $SE = .09$; 95% CI_{boot} [0.06, 0.42]).

Study 5 replicates the basic effect and provides direct evidence for mechanism. Participants underestimated enjoyment for replaying the game because they underestimated the extent to which repetition of the game would entail novel discovery. Likewise, the misprediction was moderated by expectations of novel discovery, such that giving participants cues to infer that they may not have "seen all there is" after one exposure calibrated their predicted enjoyment for repeated playing.

Finally, Studies 6 and 7 examine consequences. The basic effect implies that people underutilize opportunities to reconsume things that they already own or have already experienced, leaving enjoyable surplus on the table. Although perhaps lamentable on its own, an underutilization of repetition is not an "error" per se if the novelty that people choose instead provides even more enjoyment, when enjoyment is one's goal, all else is equal between the options (e.g., acquisition costs). Thus, in examining consequences, Studies 6 and 7 maintain conditions that are needed for the basic effect to objectively lead people astray, and test whether it still emerges under these conditions.

Study 6: (Mistakenly) Buying Novelty

In Study 6, we identified a stimulus that likely retains high entertainment value even as exposure increases: Participants watched a travel video of an immersive city tour, utilizing a technology that allows viewers to move the camera 360 degrees while the video is playing as if they were really there and able to look around however they wished (thus, each time the video plays, viewers can experience the tour in various new ways). We hypothesized that rewatching a video of the same city may be just as enjoyable as watching two videos of different cities—but that participants (a) may not realize this and instead underestimate their enjoyment for rewatching the same city (Phase I, standard paradigm of Predictors vs. Experiencers), and (b) bid real money to see a new city for the second viewing, with the goal of maximizing experienced enjoyment, despite being able to achieve that goal for free by rewatching (Phase II, Real Choosers).

Phase I Method (Standard paradigm).

Participants. First, we requested 800 participants from Amazon's Mechanical Turk, yielding 803 participants ($M_{age} = 36.54$, $SD_{age} = 10.88$; 46.82% female; 76.84% Caucasian American/White, 11.71% African American/Black, 4.73% Asian American/Asian, 6.72% Mixed/Other Ethnicity) who completed the study for $2.00.

Procedure. Participants were invited to "take a city tour" involving 10 min of total watching time. We utilized 360-degree video technology to increase realism and make for a complex hedonic activity. In the video, a tour guide walks viewers through the city while providing facts and figures exactly as in a real city tour, and viewers can rotate the camera at any angle while the video is playing (including looking behind, above, and so on) as if they were on the tour. All footage is real (i.e., not computerized).

All videos in this study were made by VR Gorilla's *Virtual Reality Productions* (http://www.vr-gorilla.com). We acquired their entire set of six city tour videos all shot in the same style and

running approximately the same length (each between 4 and 5 min): Amsterdam, Bangkok, Cancun, London, Madeira, and Salvador. Like Study 2, this allows us to use stimulus sampling to circumvent idiosyncratic effects of any one specific video.

To begin, all participants completed a technology check and a practice round with an unrelated animal video. Then, they completed Viewing Session #1: They watched one of the city videos selected at random, embedded in their Qualtrics survey window with a countdown timer displayed underneath (at this point, participants had no knowledge of the other cities beyond whatever was assigned to them). Upon completion, having now experienced the stimulus once in full, participants were randomly assigned to one cell of a 2 (Role: Predictors, Experiencers) × 2 (Video: Repeat, New) between-subjects design.

We matched all text and phrasings across conditions, and all participants rated enjoyment only once: at the very end of the study as an evaluation of overall watching.

Participants in the Predictor-Repeat condition (n = 205) were asked to imagine that, for Viewing Session #2, they rewatched their original video. Specifically, they read, "Imagine you now enter Viewing Session #2. We show you the same 360-video you just watched about [name of city]. That is, imagine spending the next 4–5 min re-watching this same 360-video for a second time giving you a tour of [name of city]. Imagine doing this now." Then, they were imagined being asked "Overall, how'd you like this study?", having just finished watching all watching (i.e., 10 min total), and predicted their reactions via three items: "enjoyable," "pleasurable," and "liked it," each from 1 (*not at all*) to 7 (*extremely*). Their counterparts, participants in the Experiencer-Repeat condition (n = 198), read the same text adapted for real-time, then actually rewatched the video. After watching, they rated their actual reactions to the study overall via the same items. These two conditions test for a replication of the basic effect from previous studies: whether Predictors underestimate enjoyment for repetition compared to Experiencers.

For comparison, this study also included novel counterparts. Participants in the Predictor-New condition (n = 196) were asked to imagine that they saw a new video. Specifically, they read, "Imagine you now enter Viewing Session #2. We show you a new 360-video about [name of 1 of the remaining 5 cities, selected at random]. That is, imagine spending the next 4–5 min watching a second 360-video, this time giving you a tour of [the city name from first bracket]. Imagine doing this now." Likewise, participants in the Experiencer-New condition (n = 204) read the same text adapted for real-time, then actually watched the new video. Participants in these two conditions then predicted or reported their reactions in the same way as above. These conditions allow us to assess prediction accuracy for novelty (we did not have specific a priori hypotheses here, but presumably participants believe that novelty will be enjoyable and it is enjoyable). More critical, these conditions allow for Experiencer comparisons between repeat enjoyment versus novel enjoyment. We expected that rewatching may be just as enjoyable as novel watching in this context.

Finally, all Predictors then rated how difficult it was to use their imagination from 1 (*easy enough*) to 7 (*very difficult*), and all participants (including Experiencers) rated how confusing they found the overall study from 1 (*not at all*) to 7 (*extremely*). All participants reported demographic information, reported whether all videos successfully played (forced-choice: *yes*; *no*), whether they heard sound in all videos (forced-choice: *yes*; *no*), whether they used the 360-degree feature in all videos (forced-choice: *yes*; *no*), and reported any other technical difficulties (forced-choice: *yes*; *no*). This study also included three attention checks, regarding: which city or cities participants encountered (forced-choice to check as many as eligible from a list of the 6 cities); their Role condition (forced-choice: *imagined watching for Viewing Session #2*; *actually watched for Viewing Session #2*); and their Video condition (forced-choice: *same travel video about the same city*; *two different travel videos about different cities*).

Phase I Results and Discussion

The three dependent measures were collapsed into an enjoyment scale (α = .96). This design differs from previous studies in that it assesses a single dependent rating (i.e., no repeated measures). Thus, data were submitted to a Univariate GLM with Role (between-subjects factor: Predictors, Experiencers), Video (between-subjects factor: Repeat, New), and the Role × Video interaction term entered as fixed factors, and the enjoyment scale entered as the dependent variable.

There was no main effect of Role, $F(1, 799) = 1.97$, $p = .161$, $\eta^2 = .002$, and a main effect of Video such that repeat videos were rated as less enjoyable than new videos, $F(1, 799) = 12.03$, $p = .001$, $\eta^2 = .02$. However, this was qualified by the critical interaction, $F(1, 799) = 8.74$, $p = .003$, $\eta^2 = .01$ (see Figure 7).

Teasing apart this interaction, pairwise comparisons reveal that repeat videos were worse than new videos *only for predictors*. Predictors thought rewatching the same travel video twice in a row would make for a less enjoyable study ($M = 5.92$, $SD = 1.20$) than watching two different travel videos ($M = 6.40$, $SD = 0.93$), $F(1, 799) = 20.60$, $p < .001$, $\eta_p^2 = .03$ ($d = 0.45$), 95% CI_{diff} [0.28, 0.69]. In reality, Experiencers enjoyed rewatching the same travel video ($M = 6.24$, $SD = 1.02$) as much as they enjoyed watching two different travel videos ($M = 6.28$, $SD = 1.10$), $F(1, 799) = 0.13$, $p = .717$, $\eta_p^2 < .001$ ($d = 0.04$), 95% CI_{diff} [−0.17, 0.25]. Pairwise comparisons across Role are also informative: Replicating the basic effect, Predictors underestimated their enjoyment for repetition compared to Experiencers' actual enjoyment for repetition, $F(1, 799) = 9.54$, $p = .002$, $\eta_p^2 = .01$ ($d = 0.29$), 95% CI_{diff} [0.12, 0.54], whereas Predictors accurately estimated their enjoy-

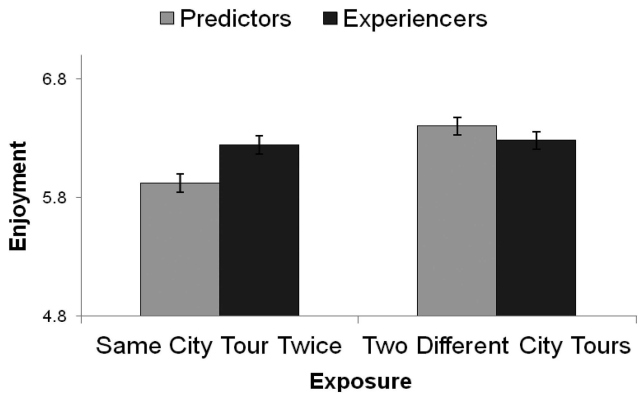

Figure 7. Enjoyment as a function of Role and Video (Study 6). $M \pm 1$ *SE*.

ment for novelty compared to Experiencers' actual enjoyment for novelty, $F(1, 799) = 1.20$, $p = .274$, $\eta_p^2 = .001$ ($d = 0.12$), 95% CI_{diff} [−0.33, 0.09].

Finally, in terms of other variables, nearly all participants in Phase I reported that all videos successfully played (95.52%, 767 of 803), all audio successfully played (99.38%, 798 of 803), all 360-degree technology was successfully used (99.25%, 797 of 803), and reported no other technical issues (94.27%, 757 of 803). Nearly all participants passed the attention checks for city names (96.76%, 777 of 803), Role (96.01%, 771 of 803), and Video (95.02%, 763 of 803). There were no differences between difficulty of imagination (Predictor conditions: $M_{Repeat} = 1.86$, $SD = 1.14$ vs. $M_{New} = 2.03$, $SD = 1.29$), $t(399) = 1.41$, $p = .158$, $d = 0.14$, $\eta^2 = .01$, nor any main effects nor interaction for study confusion (all conditions; $M = 1.37$, $SD = 1.02$), $Fs \leq 0.61$, $ps \geq .434$, $\eta s^2 \leq .001$.

Phase I shows that, if people's goal is to maximize their enjoyment on these dependent measures, they would enjoy themselves to a similar degree regardless of whether they experienced repetition or novelty in this context.

Phase II Method (Real Choosers)

Participants. Second, after completing and analyzing the study above, we requested a separate sample of 200 unique participants from the same population, yielding 201 participants ($M_{age} = 36.48$, $SD_{age} = 11.27$; 50.75% female; 76.12% Caucasian American/White, 8.96% African American/Black, 7.46% Asian American/Asian, 7.46% Mixed/Other Ethnicity) who served as Choosers for $2.00.

Procedure. Phase II was advertised and completed exactly like Phase I to match all expectations and experiences, up until the critical difference in Phase II. This occurred after Choosers finished watching their first video, at which point they were informed that they could select what to watch for Viewing Session #2: either to "rewatch [name of city from Viewing Session #1]" or "watch [name of 1 of the remaining 5 cities, selected at random]," using identical phrasings as in Phase I. The presentation order of these options was counterbalanced. We expected that most Choosers would avoid repetition and select a new video, conceptually extending the basic effect to actual choice.

As discussed, however, such a choice needs to be understood in context. At this stage, choosing novelty may seem reasonable because there are no other costs to acquiring it. Thus, on the next screen, we introduced a real (nonhypothetical) novelty premium. Choosers were asked to indicate their highest possible bid for being able to watch the new video. Specifically, they read:

> Recall that the pay rate for this HIT is $2.00. You now have the ability to play with this number if you so choose. What's the most you're willing to pay, in order to guarantee that you watch the new video? When you click ≫, the survey will calculate the highest bidders among all other MTurkers taking the survey during this current window of time, and you'll find out which video you get based on what you bid here. If you are among the highest bidders, we will lower your $2.00 payment based on your bid, and you'll watch the new video. If not, your $2.00 HIT payment is retained, and you'll rewatch the same video you just watched. Please read and consider carefully. Mark the most you're willing to pay, before the new movie would no longer be worth it to you if your bid was accepted.

Underneath, participants reported their actual bid via a 201-point forced-choice scale representing each of the 200 cents of their $2.00, plus an additional option for paying nothing. For clarity, each point was labeled with full text, beginning with "I'd pay as much as $2.00 for this (I'd be willing to have my HIT payment drop to as little as $0.00)"; "I'd pay as much as $1.99 for this (I'd be willing to have my HIT payment drop to as little as $0.01)" "I'd pay as much as $1.98 for this (I'd be willing to have my HIT payment drop to as little as $0.02)" . . . down to . . . "I'd pay as much as $0.02 for this (I'd be willing to have my HIT payment drop to as little as $1.98)"; "I'd pay as much as $0.01 for this (I'd be willing to have my HIT payment drop to as little as $1.99)"; "I'd pay as much as $0.00 for this (I'd be willing to have my HIT payment drop to as little as $2.00)."

In addition, we reminded participants that they must fully watch whatever video is shown at Viewing Session #2 as agreed upon when signing up for the study. They read:

> This is a real tradeoff! We can detect when you click out of the window, and we will also ask you attention questions unique to Viewing Session #2 that you'll need to get right to get paid at all. Thus, your tradeoff is between how you spend the next 4–5 min of your time, and how much you care: between watching the new movie versus rewatching the first movie.

We included this reminder to reduce incidental confounds from using Amazon Turk: Participants on this platform may be motivated to get paid rather than enjoy themselves, and may be able to access the other city tour videos (or novelty in general) by other means without sacrificing their HIT payment (e.g., doing other things while the repeat plays).

Finally, Phase II had two additional features to ensure a fair interpretation. First, it may be that Choosers infer from our design that the novel option must be exceptionally worthwhile beyond the fact that it is novel. This design has high external validity (in real everyday choices as in this study, novel options rather than old options are advertised at a cost), but nonetheless, to rule out this possibility, we required *all* Choosers (regardless of their choice) to complete the same bid question about paying for the novel option. If the design leads people to infer that the novel option must be exceptionally worthwhile beyond the fact that it is novel, then even those Choosers who initially chose repetition should end up bidding. Second, it may be that Choosers buy novelty for other reasons beyond enjoyment per se (e.g., they may know full well that the repeat option would be just as enjoyable in real time, but want to buy novelty for sake of curiosity). To rule out this possibility, we asked all Choosers after they made their choice and after the bid item to indicate why they made their choice. They were shown the exact scale items from the main study and indicated whether they made their choice to maximize their study experience via these items, or to maximize something else (forced-choice). Among Choosers who would prefer to buy novelty for other reasons, our study cannot speak to whether their choice is mistaken.

To finish Phase II, Choosers were debriefed and were informed that they would not actually watch a video for Viewing Session #2 in the interest of their time. They rated how confusing they found the overall study from 1 (*not at all*) to 7 (*extremely*), reported demographic information, completed an attention check regarding which city or cities they encountered (forced-choice to check as

many as eligible from a list of the 6 cities), and completed the same technology checks that participants completed in Phase I.

Phase II Results and Discussion

Do participants appreciate the null effect between repeat videos and new videos? The Predictor data in Phase I suggest not, and thus the Real Choosers in Phase II may mistakenly pay to avoid repetition and watch the new video.

First, we sought to clearly identify the sample of interest. Among the full sample of 201 Choosers, the majority (88.06%, 177 of 201) indicated that they made their choice precisely to maximize their study experience as measured on the enjoyment scale. Thus, we examine only these 177 Choosers. Our study cannot speak to whether choosing for other reasons warrants an error in this context (our data file retains all participants).

A chi-square testing against chance revealed a strong novelty preference: The significant majority of Choosers (92.09%, 163 of 177) chose to watch a new city, $\chi^2(1, N = 177) = 125.43$, $p < .001$. In turn, these remaining 163 Choosers count as the final qualifying subset for our main measure: The correct choice is to pay nothing for novelty, because they can achieve their goal for free simply by rewatching their original video.

Yet, on average these 163 Choosers bid \$0.25 ($SD = 0.52$)—a 12.50% novelty premium. This was significantly more than \$0.00: one-sample t test, $t(162) = 6.14$, $p < .001$, $d = 0.48$, 95% CI_{diff} [0.17, 0.33]. Moreover, this was not simply due to a few outliers: 75 of these 163 Choosers (46.01%) bid more than \$0.00, with an average bid of \$0.55 ($SD = 0.66$)—a 27.50% novelty premium (min, \$0.01, max, \$2.00; mode, \$0.05).

Providing additional insight, the 14 Choosers who initially chose repetition were not willing to pay a novelty premium: 13 of those 14 Choosers bid \$0.00 (the remaining participant bid the full \$2.00 and thus might also be viewed as making an error).

As in Phase I, nearly all Choosers reported that all videos successfully played (98.51%, 198 of 201), all audio successfully played (99.50%, 200 of 201), all 360-degree technology was successfully used (100.00%, 201 of 201), and reported no other technical issues (96.52%, 194 of 201). Nearly all participants passed the attention checks for city names (98.51%, 198 of 201). Likewise, ratings of study confusion were low ($M = 1.41$, $SD = 0.87$), indistinguishable from Phase I participants.

Phase II documents a case when the basic effect objectively led people astray. Most Choosers chose novelty, and did so precisely to maximize their study experience on the enjoyment scale; yet many of these participants even bid real money to watch the new video, when they could have kept all of their money and enjoyed the study experience just as much via rewatching. All told, 37.31% of all participants in the study (75 of 201) ended up making this error. Among all those who qualified for error (the 177 participants who sought to maximize enjoyment and thus should have bid nothing), the average bid was \$0.24 ($SD = 0.52$), significantly more than \$0.00: $t(176) = 6.18$, $p < .001$, $d = 0.46$, 95% CI_{diff} = [0.16, 0.32]—that is, on average participants were willing to pay a needless novelty premium of 12.00% all told.

Study 7: (Mistakenly) Avoiding Repetition

Finally, Study 7 examined another possible case when the basic effect might objectively lead people astray, one that may be especially common in daily life: the allure of novelty provided by our phones. At any moment, cell phone users can take a peek at endlessly novel information: reading news, checking new e-mails, accessing new videos, and so on. Yet, what one finds is not always rewarding: When trying to find something fun, we might get distracted, get stuck searching, or choose something from this enormous range of offerings that ends up being less enjoyable than advertised. Repeating content that we know we have already enjoyed once before might sometimes be the safer bet to maximizing our hedonic experience during that time spent.

In Study 7, all participants first played the art video game used in previous studies. Based on the results of previous studies, repeatedly playing this game should remain highly enjoyable—contrary to participants' intuitions. Then, some participants were assigned to spend the remainder of the study playing the game repeatedly, whereas others were assigned to spend the remainder of the study finding something new to enjoy on their phones. We hypothesized that, in this context, repetition may make for a more enjoyable overall study experience. More critical, participants in a third condition were given the choice of how to spend the remainder of the study. We hypothesized that they may choose poorly: Choosers with the goal of maximizing experienced enjoyment may wrongly prefer the novel, but inferior, option (i.e., to play on their phones instead).

Method

Participants. We recruited 300 subject pool participants to complete the study for \$6.00 ($M_{age} = 29.53$, $SD_{age} = 13.47$; 42.67% female; 33.33% Caucasian American/White, 32.67% African American/Black, 18.00% Asian American/Asian, 15.99% Mixed/Other Ethnicity).

Procedure. Participants were recruited in-person where an experimenter guided them through all study procedures via private individual sessions. The study was advertised as containing 15 min of study tasks. The experimenter ensured compliance in all conditions (e.g., the experimenter ensured that participants who repeatedly played the game did not use their phones while playing, by collecting their effects beforehand). First, all participants played the game in a playing session—in this study, lasting a period of 5 min. Having now experienced the stimulus once in full, at this point participants were randomly assigned to one of three between-subjects conditions.

Participants in the Repetition condition ($n = 96$) were informed that, to finish out the remaining 10 min, they would play the same game repeatedly, divided into two consecutive Playing Sessions with the game starting from scratch (thus, their overall study experience involved three 5-min Playing Sessions of the same game). They completed these tasks as instructed, then rated their overall study experience via three items: "enjoyable," "pleasurable," and "liked it," each from 1 (*not at all*) to 7 (*extremely*).

Participants in the Novelty condition ($n = 103$) were informed that, to finish out the remaining 10 min, they would find something new to enjoy on their phones, entirely up to them, and that whatever they did would be unmonitored and unchecked by the experimenter (so to reduce concerns or expectations about what to do). A countdown timer then appeared on the computer where the game had been played, and they took out their phones and the playing period began (thus, their overall study experience involved

one 5-min playing session of the game, plus freely playing on their phones with new content for the remaining 10 min). When the timer expired, the page automatically continued and participants rated their overall study experience via the same three items.

Participants in the Choose condition ($n = 101$) were informed that, to finish out the remaining 10 min, they could choose what to do: to be in the Repetition condition or to be in the Novelty condition (these explicit labels were not used). They read:

> What would you like to do for the next 10 min? We will enforce whatever you actually choose. There is truly no right or wrong answer. Please do whatever you personally feel like doing at the moment! The choices below are presented in randomized order, to further emphasize this point (we are not intending for you to think that choosing one "looks better" than choosing the other; please just choose whatever you currently feel like doing!).

Choosers then read identical descriptions of each task as used in each of the other conditions, presented in counterbalanced order, and made their choice. After making their choice, they were asked to indicate why they made their choice. As in Study 6, they were shown the exact scale items from the main study and indicated whether they made their choice to maximize their study experience via these items, or to maximize something else (forced-choice). Among Choosers who choose novelty for other reasons, our study cannot speak to whether their choice is mistaken. Then, expanding upon Study 6 (which did not require Choosers to actually experience their choice), all Choosers in this study completed whatever task they chose in the same way as in each of the other conditions, followed by rating their overall study experience via the same three items.

Finally, all participants rated how confusing they found the overall study from 1 (*not at all*) to 7 (*extremely*), reported demographic information, and reported any technical difficulties (forced-choice: *yes*; *no*). We also included an exploratory item about choice regret: All Choosers were asked, "Are you happy with the choice you made earlier in this study?" (forced-choice: *Yes [I'm glad I chose to do what I did for the remaining 10 min]*; *No [Looking back, I wish I had chosen the other option for the remaining 10 min]*). We preregistered this item as exploratory because we suspected most Choosers would say "yes," as they lacked the counterfactual experience (phone users presumably still held the intuition documented in previous studies, believing that repetition would have been even worse even if their phone experience proved to be just okay). In any case, we found it interesting, and potentially informative, to test explicit awareness among Choosers.

Results and Discussion

The three dependent measures were collapsed into an enjoyment scale ($\alpha = .95$). As in Study 6, this design assesses a single dependent rating (i.e., no repeated measures), and so the appropriate analyses are named below.

First, we tested for the basic effect in terms of actual experience: whether repeatedly playing the game indeed resulted in a more enjoyable overall study experience than playing the game once, then finishing the study on one's phone. An independent-samples t test confirmed that repeat-gamers enjoyed the study more ($M = 5.72$, $SD = 1.27$) than novel phone-users ($M = 5.24$, $SD = 1.66$), $t(197) = 2.29$, $p = .023$, $d = 0.32$, 95% CI_{diff} [0.07, 0.89]. The correct choice for how to finish the study is to choose repetition, if one's goal is to maximize enjoyment during this time on these measures.

Next, we sought to clearly identify the sample of interest. Among the full sample of 101 Choosers, the majority (80.20%, 81 of 101) indicated that they made their choice precisely to maximize their study experience as measured on the enjoyment scale. Thus, we examine only these 81 Choosers. Our study cannot speak to whether choosing for other reasons warrants an error in this context (our data file retains all participants).

Going against the correct choice, a chi-square testing against chance revealed a mistaken novelty preference: The significant majority of these Choosers (75.31%, 61 of 81) chose to play around on their phones rather than repeat the same game, $\chi^2(1, N = 81) = 20.75$, $p < .001$. Putting this number into overall context, all told, 60.40% of all Choosers in the study (61 of 101) ended up making this error.

In this study (unlike in Study 6), we also had Choosers then actually experience their chosen option and report their actual ratings of the study overall on the dependent measures. Interestingly, even based on *their own* ratings, they chose poorly (if anything, the effect was stronger): The minority of Choosers who chose to be repeat-gamers ended up enjoying the study more ($M = 6.07$, $SD = 0.98$) than the majority of Choosers who chose to be novel phone-users ($M = 4.93$, $SD = 1.59$), $t(79) = 3.00$, $p = .004$, $d = 0.86$, 95% CI_{diff} [0.53, 1.73]. It cannot be that these novel phone-users chose this condition for nonhedonic reasons like catching up on work or e-mails (explaining why they did not particularly enjoy the study overall), because our analysis includes only those participants who made their choice with the explicit goal to maximize enjoyment of the study overall.

For thoroughness, we also analyzed these data in the same model: A one-way ANOVA confirmed an omnibus effect of condition, $F(2, 277) = 3.40$, $p = .035$, $\eta^2 = .02$ (see Figure 8). All told, participants were worse off when able to choose what to experience, due to the predominant intuition to (mistakenly) avoid a repeat choice.

Finally, in terms of other variables, nearly all participants reported that the game loaded with no technical issues (99.25%, 397 of 400), and there was no omnibus effect of condition for ratings of study confusion (all conditions; $M = 1.66$, $SD = 1.29$), $F(2, 297) = 0.69$, $p = .504$, $\eta^2 = .004$. Only a handful of Choosers

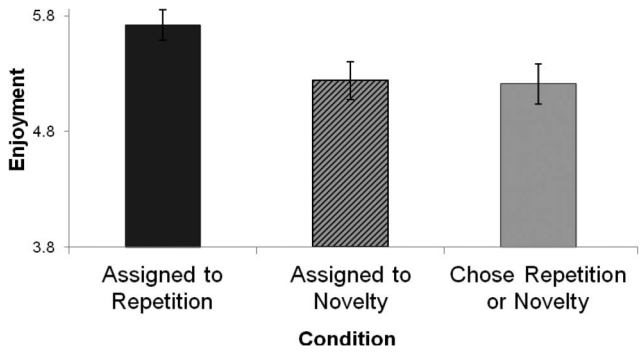

Figure 8. Enjoyment as a function of condition (Study 7). $M \pm 1$ *SE*.

reported at the end of the study that they wished they made the other choice (7.41%, 6 of 81)—which we suspected given that Choosers never experienced the counterfactual—but five of six of this handful were participants who chose novelty.

Study 7 documents a second case when the basic effect objectively led people astray. As in Study 6, most Choosers chose novelty, and did so precisely to maximize their study experience on the enjoyment scale; yet in this context, repetition rather than novelty would have provided the more enjoyable study experience.

General Discussion

How enjoyable would it be to revisit last month's museum, rewatch last week's movie, or replay last night's game? Seven studies suggest that many such activities may turn out to be less dull than people think. First, Studies 1–3 documented the basic effect: After experiencing these activities just once, Predictors underestimated their enjoyment for repeats as compared to how much Experiencers actually enjoyed themselves. Next, Studies 4 and 5 replicated this misprediction and documented the driving role of unforeseen novelty: Predictors underestimated the extent to which Experiencers would discover new information within a complex stimulus by virtue of repeating it, thus diluting uniform dullness. Finally, Studies 6 and 7 highlighted consequences: An underappreciation for repeat experiences led participants to incur costs to avoid a repeat and acquire novelty instead, so to maximize enjoyment—in cases when a repeat would have made the superior choice.

Insights and Implications

Two features of our study designs render these insights especially compelling, advancing research on exposure effects more broadly. First, our study designs utilized a novel "pre-experience" paradigm, such that even Predictors first experienced the stimulus once in full before predicting repeat exposures. Previous research on phenomena such as affective forecasting also documents errors between predictions and experiences, but by design this research examines contexts in which people making predictions lack the first-hand experience necessary for accuracy. For example, participants in this literature have mispredicted how happy they would be if they were rich, had tenure, or lived in sunny California, but some degree of error may be understandable given that they had never before been rich, been tenured, or lived in California and therefore must rely on imperfect general theories (for a review, see Wilson & Gilbert, 2005). This explanation is not possible for our studies: Our participants were first given the full bottom-up experience, yet proceeded to make imperfect forecasts about repeating that exact experience. Second, for a more conservative and precise test, our study designs utilized immediate repetition. Previous research has focused on the misperceptions that arise from the lengthy times in between exposures (e.g., failing to anticipate "resetting to baseline": Galak et al., 2011). In our studies, this window was closed yet the misprediction still emerged. Together, these two features demand a novel mechanism for forecasts of hedonic repetition. We propose the candidate possibility of an inflated tendency to feel like one has "seen all there is to see" after experiencing an activity just once—a core component of efficient information processing (e.g., Fiske & Taylor, 1991; Gigerenzer, 2008; Kahneman, 2011; Keil, 2003; Klein & O'Brien, 2018) but to date has not yet been applied to understanding how people think about repeat experiences. Studies 4 and 5 most directly documented this mechanism, beyond the added possibility that people underestimate the pleasures of fluency and mere exposure. In daily life, these effects may be even more pronounced given that people likely do not consider repeats until after some time has passed.

These features also shed light on the broader literature on hedonic adaptation. A prominent theme from this literature is that hedonic adaptation is inevitable, regrettable, and ought to be avoided by acquiring novel entities (e.g., "Hedonic adaptation can be resisted, but only with conscious, active efforts": Lyubomirsky, 2010, p. 219; for reviews, see Lyubomirsky et al., 2005; Sheldon et al., 2012). Other studies on people's beliefs about hedonic adaptation echo this theme, implying that people ought to be more wary of looming decline ("What we miss is one simple thing: Once we have owned the car for a few weeks, other things will be on our minds while driving and we would feel just as well driving a cheaper alternative": Schwarz & Xu, 2011, p. 144: for reviews, see Frederick & Loewenstein, 1999; Wilson & Gilbert, 2005). Our findings seem to suggest the exact opposite: Hedonic adaptation is not as universal as advertised, or as bad as imagined. Understanding this discrepancy provides a fruitful opportunity to advance the hedonic adaptation literature. On the one hand, our preexperience paradigm may be critical; perhaps people's uniform excitement at Time 0 (before any consumption) quickly shifts more pessimistic at Time 1 (after some consumption), reflecting our proposed mechanism ("I've now already seen it all"). It may also be that past participants would have showed our patterns if both Predictors and Experiencers reported on the same explicit repetition cycle, as in our studies. On the other hand, we suspect an overarching psychology can explain the discrepancy: In all cases, people inflate the value of whatever information is in front of them, at the expense of other pieces that will inevitably still grab attention and wield influence (e.g., Kahneman, 2011; Wilson et al., 2000). The critical, novel insight raised here is that these other pieces are also found *within the stimulus itself*—not just from external sources that disrupt attention to that stimulus. This possibility is lost in past designs and thus lost in the grim take-home message about hedonic adaptation. Our findings highlight the need for a clearer taxonomy of the kinds of activities that grow less enjoyable versus more enjoyable across exposure than people initially expect, and why (see also O'Brien & Kassirer, 2019).

This notion of active discovery being involved in repetition also invites a closer look at the literature on novelty preferences. Previous studies establish a widespread preference for the novel over the familiar when people make absolute choices (e.g., McAlister & Pessemier, 1982). The prevailing interpretation is that people are actively drawn toward the novel option (e.g., Fennis & Stroebe, 2016), and for good reason given the hedonic benefits of novelty (e.g., Lyubomirsky et al., 2005). Our studies qualify these ideas by assessing continuous ratings of repeated hedonic exposure, to real-world activities—disentangling the possibility that people may avoid repeats not just because they strongly desire novelty, but also because repetition seems overly dull (which, as the current studies reveal, may be exaggerated). Further, this intuitive aversion to the familiar option may be misguided even in cases when people do desire novelty: Novelty may be found not only in experiencing new things for the first time but also in

reexperiencing the things one already has. The literature to date may underemphasize opportunities for uncovering novelty-via-depth (i.e., via repetition), in addition to novelty-via-breadth (see also O'Brien & Smith, 2019; Zhang, Kim, Brooks, Gino, & Norton, 2014).

Future Directions

The current findings raise various directions for research beyond these broader considerations. First, future research should further unpack practical implications. The basic effect implies that people stop consuming familiar entities while enjoyable surplus is left on the table. This tendency is not an "error" per se if people choose novelty for nonhedonic reasons (e.g., to satisfy curiosity or grow one's "experiential CV": Keinan & Kivetz, 2011; March, 1991; Ratner et al., 1999), or to maximize enjoyment but their novel choice was at least as enjoyable as a repeat and no more costly to acquire; although perhaps lamentable that people may not tap the full hedonic potential of the things they acquire (e.g., thereby generating consumption waste), this must be referenced against how people spend their time instead. To be clear, many novel entities certainly *are* more enjoyable than repeats. However, Studies 6 and 7 are existence proofs that people also sometimes avoid repeat experiences at objective costs, paying needless premiums and choosing activities that proved less enjoyable when maximizing enjoyment was their goal. We suspect that the error is not uncommon in many contemporary consumer environments, which offer ever-newer (but not necessarily better) options (Grant & Schwartz, 2011). Study 7 partly captures these dynamics, with the majority of participants attempting to maximize enjoyment by choosing to be on their phones rather than replay the same game again and again—ultimately providing a less enjoyable experience. Indeed, a new entity may be enjoyable but does not guarantee enjoyment; choosing novelty entails risk that repeating a known-enjoyable activity may not entail, which also must enter into the cost-benefit equation.

Second, the notion that people underappreciate the emergence of novelty via repetition may inform outcomes beyond hedonics. In classroom settings, for example, students may intuitively complain about having "already heard" a lecture or "already learned" a concept and so skip the class, underestimating how continued exposure might promote continued learning. Our findings likely also extend to interpersonal outcomes; partners choosing a movie together may be too quick to assume an option is now spoiled if one has already seen it, leading to needless search time and suboptimal replacements. Our findings might also help to explain other effects related to wellbeing, such as the phenomenon that people are averse to being left alone with their own thoughts without any external stimulation (Wilson et al., 2014); put in our terms, it may be that people underappreciate the "replay" value of enjoyable memories and thus do not think to bring them to mind when trying to entertain themselves. Lastly here, testing stimuli that are perceived as explicitly negative would be further informative, in particular for a static simulation mechanism (e.g., people may *under*estimate the pain of the nightly train next door, with the actual experience of each night proving to be its own unique displeasure).

Third, future research should further establish boundary conditions. We assessed a variety of domains and experiences, all falling under what we called "complex" hedonic activities: those that offer a multitude of information. We suspect that many real-world hedonic activities have this quality, particularly in relation to their oversimplified mental representations. Note, however, that we still observed a significant misprediction for Study 4's simple stimulus, despite the more relevant finding that the size of the gap was greater for complex stimuli; even repeatedly looking at simple blue orbs is apparently somewhat enjoyable. This finding is suggestive of the unforeseen pleasures of fluency and mere exposure, and so the basic effect of underestimating enjoyment may be quite general across stimuli (but sustained by different mechanisms). Future research should further unpack these roles of fluency versus discovery. Even when enjoyment clearly declines for other reasons (e.g., satiation), it may not decline as much as people think because of these other diluting pleasures elicited within the act of repetition—and particularly so for complex activities that elicit the additional pleasure of unveiling new information.

At the same time, there are apparent exceptions that should be explored. For some domains, people likely prefer the old over the new (e.g., socializing with an old friend may be much preferred to dinner with a novel stranger). However, this does not preclude our basic effect (e.g., the thought of seven evenings in a row with the same person may elicit expectations of eventual decline that occurs even later in reality). Other domains that entail nostalgia, routines, or rituals may mark similar boundary conditions, although again these do not preclude our basic effect (e.g., *It's A Wonderful Life* may prove to be a perennial winner once playing, but may first require some internal convincing to load up yet again each and every holiday season). To observe a proper flip of the basic effect such that predicted repeats are better than actual repeats, our framework points to activities that turn out to be less "complex" than they first seem, closer to what we had assessed in Study 4. A choose-your-own-adventure book may seem thrilling at first—until one catches on to the same hackneyed pattern at each turn. As reviewed in the introduction, however, static simulation suggests that these cases are the exception rather than the rule. Finally, individual differences might also be informative. Our participants were diverse (drawn from student and community subject pools, national online panels, and naturalistic settings around a large American city), but consider one clear exception to the basic effect: the strong desire to repeat the same activity again and again among *young children* (e.g., to rewatch a movie or to rehear a story in immediate succession). One intriguing explanation may still be found in the current account: To the extent that capacities for cognitive efficiencies grow into adulthood (Gigerenzer, 2003), young children may have yet to develop the capacities designed to help adults navigate increasingly complex informational environments—yet allowing children to be superior at anticipating the newness left remaining in the familiar.

Until these possibilities are tested, the current studies document a robust underappreciation for repeat experiences across a number of real-world activities: After completing an activity once, participants underestimated the value of doing it again. Rather than suggesting a curb on acquiring novel entities, these findings instead remind us that the past may sometimes feel just as "new," and as enjoyable, as the future—at least, not as dull as it plays out in one's mind. Repetition too could add an unforeseen spice to life.

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