

Working Paper 19-087

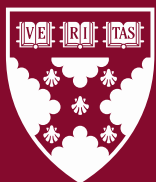
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Ximena Garcia-Rada

Leslie K. John

Ed O'Brien

Michael I. Norton



**Harvard
Business
School**

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Ximena Garcia-Rada
Texas A&M University

Leslie K. John
Harvard Business School

Ed O'Brien
University of Chicago

Michael I. Norton
Harvard Business School

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A Preference for Revision Absent Objective Improvement

XIMENA GARCIA-RADA

LESLIE K. JOHN

ED O'BRIEN

MICHAEL I. NORTON

Ximena Garcia-Rada (xgarciarada@mays.tamu.edu) is an Assistant Professor of Marketing at Mays Business School, Texas A&M University; Leslie K. John (ljohn@hbs.edu) is a Marvin Bower Professor of Business Administration at the Harvard Business School, Harvard University; Ed O'Brien (eob@chicagobooth.edu) is an Associate Professor of Behavioral Science at the University of Chicago Booth School of Business; Michael I. Norton (mmorton@hbs.edu) is the Harold M. Brierly Professor at the Harvard Business School, Harvard University. The authors thank Derek Koehler for helpful feedback and Holly Howe, Shannon Sciarappa, Anne Marie Green, Trevor Spelman, and Shun Wang for research assistance. To collect data for the field study with MBA resumes, the authors worked closely with the team in a professional development office at a business school and are grateful for their support. Please address correspondence to Ximena Garcia-Rada.

A Preference for Revision Absent Objective Improvement

From downloading never-ending updates to tracking ever-newer releases, consumers today are surrounded by revised offerings that purport to have improved upon what was previously available. Although revising things often makes them better, the current research reveals that merely being told that a product has been revised makes consumers *think* it is better, even absent objective improvement. Eleven studies document this effect and its psychological underpinnings. Study 1 establishes the basic effect: Consumers are swayed to choose an inferior product when they think it is a revised version. Studies 2A-2C replicate this effect across many products and marketing contexts. Studies 3-6 show that this “revised-is-better” heuristic lowers consumers’ efforts to scrutinize a revised product, thus explaining the effect (Study 3), and therefore are especially susceptible to the effect under conditions that recruit intuitive judgment (Study 4, facing time pressure; Study 5, lacking diagnostic product information; Study 6, lacking product expertise). Studies 7A-7C extend this effect to naturalistic settings among students working to improve something they revise regularly: their resumes. Together, these findings bridge and advance consumer research on expectancy effects and product change, revealing when, why, and how consumers risk being deceived by the “new and improved.”

Keywords: product change, versioning, expectancy effects, heuristics, intuitive processing

Things change. Things also *get* changed—often. For example, companies regularly release revised products, editions of books, director’s cuts of movies, and technological updates. Individuals regularly tweak recipes, edit resumes, and revise manuscripts, sometimes endlessly. Why? The obvious reason is that revising things often makes them better. In the current research, we document a less obvious reason: Revising things makes people *think* they are better, even absent objective improvement. In a public demonstration providing anecdotal support for this idea (see TIME 2017), city passersby were invited for a sneak-peak to play with the soon-to-be-released iPhone X (to be released later that month, in November 2017). Unsurprisingly, many raved about the phone’s sleek new feel and performance; in reality, all had been handed a well-worn iPhone 4 (released all the way back in June 2010). Even when people can go beyond a label and actually test out a “new” (yet unimproved) product themselves, they can exhibit a preference for revision.

To be clear, it is often reasonable to assume that revisions *should* reflect improvements; after all, companies working in good faith presumably decide to tweak and update their products to make them better, and they may succeed in doing so (at least to some degree). In many cases, however, changes can be merely cosmetic or superficial. If the goal of a revision is to draw renewed attention to a product, for example, a firm may simply change its coloring or packaging to improve its aesthetics rather than make any substantive changes (Townsend and Sood 2012). Moreover, a resulting difference could also go in the other direction: “Planned obsolescence” is the practice of introducing products with short useful lives (Bulow 1986), and “product versioning” is the strategy of deliberately degrading features in existing models (Gershoff, Kivetz, and Keinan 2012), both of which are designed to spur faster repurchase. And when

companies revise their technological products to include a larger number of features, the revised version can be difficult to use and lead to feature fatigue (Thompson, Hamilton, and Rust 2005).

In the current research, we show that even in such cases when no objective improvements are made—as was the case among our aforementioned iPhone testers, for example—consumers systematically exhibit a preference for revision: merely being told that something has been revised can lead consumers to prefer it—even at a cost—to its unrevised counterparts. We argue that this effect arises from a “revised-is-better” heuristic, the application of which can lower consumers’ product scrutiny—such as their efforts to find flaws in products—resulting in a preference for revision. Like other heuristic-based processes, although a preference for revision is likely often valid, we propose that it is unwittingly applied in cases when it does not reflect reality (Simon 1957; Tversky and Kahneman 1973, 1974).

THEORETICAL FRAMEWORK

We propose that consumers have a general preference for revisions, and that this preference arises from a heuristic judgment process. A wealth of consumer research attests to how consumers rely on cognitive shortcuts (i.e., heuristics) to make judgments and decisions (Payne, Bettman, and Johnson 1992; Slovic 1995; Tversky and Kahneman 1974). A hallmark of heuristic-based judgment processes is that they are susceptible to the top-down influence of “frames” (Tversky and Kahneman 1981)—in other words, the different ways in which an otherwise identical entity is merely described. For their part, marketers are strategic in what they emphasize versus downplay in a product’s offerings; such frames have a marked influence on brand perceptions and product adoption (e.g., Campbell 1995; Cotte et al. 2005) and explain why formally equivalent ways of describing a product can produce different judgments (Sher and

McKenzie 2011). For example, consumers view the same food item more favorably when it is labeled as “75% lean” rather than “25% fat” (Levin and Gaeth 1988) and view the same weight-loss ad more favorably when it shows step-by-step images of a customer’s progress rather than when it only shows the before-and-after images (Cian, Longoni, and Krishna 2020).

We propose that merely framing a product as “revised” (or something synonymous, such as an “updated” or “newer” version) will exert a top-down influence on consumers’ preferences, shaping their perception of and experience with the product. Put differently, given that it can often be difficult for consumers to assess product quality, we suggest that revision framing can serve as an easily evaluable, peripheral cue to aid their judgment (Petty and Cacioppo 1986). This possibility is consistent with marketing research documenting the effects of expectations on consumption experiences of identical stimuli (e.g., drinking a nasty “vinegar” beer can make it taste worse; drinking a “pricey” wine can make it taste better; eating a “fatty” dessert can make it taste more decadent: see Deliza and MacFie 1996; Hoch and Ha 1986; Lee, Frederick, and Ariely 2006). It is also consistent with confirmatory search (e.g., Lord, Ross, and Lepper 1979; Snyder and Swann 1978); for example, Deighton (1984) showed that advertisements operate by first arousing consumers’ expectations and then spurring them to confirm those expectations.

Thus, prior work in marketing and allied fields supports our proposal that consumers may be swayed by revision framing. However, up to this point in our theorizing, such a claim is agnostic about direction; more specific theorizing is needed to account for the hypothesis that revision framing *positively* influences product perceptions. One could imagine, for instance, that a product reformulation might spur *negative* perceptions, as when consumers revolted against tweaks to a favorite recipe or cherished franchise, like infamous “New Coke” (Klein 2015). Throughout our studies and in the General Discussion, we will expand upon some of these

boundary conditions as part of our proposed framework. In general, however, we propose that such boundaries are exceptions to a bigger rule: Consumers, on average, respond *positively* to revision information, operating on the assumption that a revised product must be *better* than whatever its predecessor offered (even when this is untrue).

Positive Associations with Product Change

Our proposition that revision framing induces *positive* product perceptions stems from prior work suggesting that consumers are prone to inferring *improvement* from mere change. O'Brien and Kardas (2016), for example, documented a semantic association between the concept of change and distinctly *positive* reactions: participants who were asked to reflect on how they had changed over time only brought to mind ways they had *improved*—despite easily bringing to mind decline when prompted. Those authors further argue that people are especially motivated to perceive change as positive when they have personal stakes in improving—which may also be the case in consumer contexts (e.g., indicating one's liking of a product is taken by consumers as a signal of their identity: Belk 1988). Relatedly, research on product change (Okada 2001, 2006) suggests that consumers sometimes go so far as to actively denigrate their possessions so as to increase the perceived improvement of a newer version (presumably, helping them to justify buying it; Bellezza, Ackerman, and Gino 2017; see also Wilson and Ross 2001). Thus, although product change can take many forms (including decline), we propose that revision framing generally prompts consumers to approach a revised stimulus with positive expectations in mind.

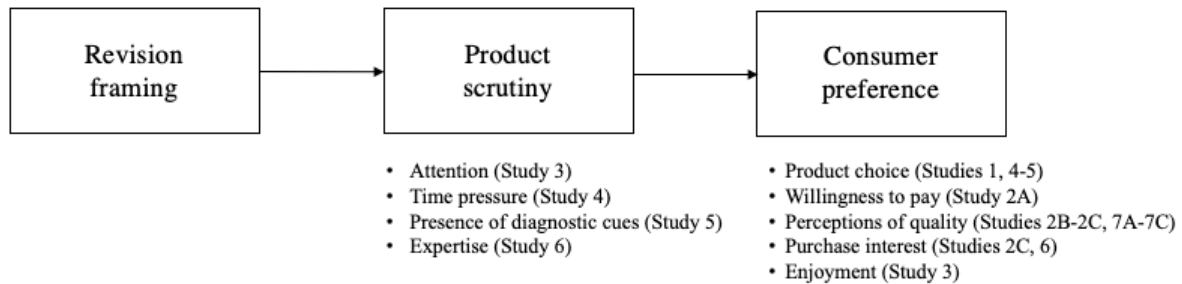
Product-Scrutiny-As-Process

In turn, our theorizing points to at least one novel, and more specific, attentional process underlying this hypothesized effect. We propose that consumers rely on a “revised-is-better” heuristic, making them less skeptical of products framed as revised, and resulting in a preference for revisions. Assessing whether or not a revision actually represents an improvement requires attention to detail in a more bottom-up manner. But if, as we posit, the preference for revision is a product of a top-down heuristic that revisions are always better, then it should be undergirded by an intuitive or System 1 thinking style (Kahneman and Frederick 2002; Kahneman 2011). In other words, we argue that the preference for revision is self-fulfilling, wielding a top-down influence that alters how observers experience the stimulus and how much attention they pay to scrutinize a given stimulus.

In daily life, factors that co-vary with revision status may contribute to this effect (e.g., revised products are often newer products, and consumers may prefer newer versions for purposes of variety or status signaling: Henard and Szymanski 2001; Ratner and Kahn 2002)—but such preferences also cannot account for our hypothesis, as we theorize (and test for) a positive effect of revision framing while holding chronological newness constant. As such, our work is related to, yet distinct from, recent work documenting how factors other than objective product improvement can lead consumers to adopt newer versions of a product (e.g., self-brand connection: Dagogo-Jack and Forehand 2018; number of add-on features: Bertini, Ofek, and Ariely 2009).

Our proposed process of product scrutiny allows us to make predictions about when and why consumers should be especially likely to exhibit a preference for revision (and likewise helps explain cases of the opposite occurring in daily life, such as when “New Coke” backfired), all predicted by the same theoretical framework (see Figure 1).

Figure 1. Conceptual model of how revision framing promotes positive consumer preference.



First, attention should mediate the effect, such that consumers lower their efforts to scrutinize a product labeled as revised. Second, and broadly speaking, a preference for revision should be especially strong in the presence of factors that prompt consumers to rely on heuristic-based processing styles. Time pressure for choosing a product, which causes consumers to adopt less effortful thinking strategies in favor of heuristics (Payne, Bettman, and Johnson 1988; Gigerenzer and Gaissmaier 2011) is one such factor. The presence (or absence) of diagnostic cues—cues that indicate whether a revision is truly an improvement—may also moderate the effect. Prior work has found that especially in the absence of diagnostic cues, consumers tend to rely on peripheral (or even irrelevant) cues when forming judgments (Petty and Cacioppo 1986; Langer, Blank, and Chanowitz 1978; Bastardi and Shafir 1998). Thus, a preference for revision should be especially large when consumers lack diagnostic cues to evaluate revision quality.

Finally, the extent to which consumers have sufficient experience with a product should also moderate their preference for revision. Consumer expertise moderates message learning and product evaluations (Maheswaran, Sternthal, and Gürhan 1996), and abilities to account for specific product details improves as category expertise increases (Alba and Hutchinson 1987). Thus, a preference for revision should also be especially large when consumers lack specific

product expertise and thus must rely on peripheral cues like a revision label; in contrast, consumers who know a lot about the product may be unmoved (or even actively angered) by purported change. Hence, a preference for revision should be weaker, or perhaps even reverse (such that consumers instead exhibit a preference for originals) when consumers are “experts” in the domain such that they do not need to rely on a heuristic to make decisions (e.g., *Coke lovers*, rather than those without an allegiance, largely led the charge against “New Coke;” Klein 2015).

Are Consumers Really “Wrong” in Trusting Revisions? A Changing Consumer Landscape

Our hypothesis that consumers rely on a “revised-is-better” heuristic is more notable to the extent that revised products are not always improvements over their unrevised counterparts; after all, in such cases consumers are *right* to be swayed by revisions (assuming no added costs). Our studies directly test cases in which this is decidedly untrue, and even those that come with added costs; as we will detail, consumers choose a revised offering even when it is objectively inferior to its un-revised counterpart. At the same time, it is worth considering the broader ecological validity of our research question: If most product changes in the marketplace indeed reflect genuine (and successful) attempts to improve one’s product, then a generalized preference for revision would seem to serve consumers well all told.

Unfortunately for consumers, however, cases of “revised = *unimproved*” may represent more than marginal exceptions to the rule. As noted earlier, companies often release annual updates to increase sales or renew public attention and not because change is warranted (“If it ain’t broke, fix it anyway”). The practice of planned obsolescence refers to when a manufacturer deliberately ensures that their product will become out of date or useless within a known period of time (Bulow 1986; Choi 1994; Waldman 1993). To take just one example, iPhone users have

reported being extremely frustrated when their devices wear off too fast (Adorno 2021; BBC 2018), with some scholars accusing an “Apple conspiracy theory” where the company deliberately slowed down past models upon new releases as to kindle customers’ desires to upgrade (Mullainathan 2014)—a claim that has since been supported following a lengthy legal dispute (Allyn 2020).

Furthermore, consumers often default to searching for additive transformations (Adams, Converse, Hales, and Klotz 2021) and fail to identify subtractive changes (Agostinelli, Sherman, Fazio, and Hearst 1986), and so designers often add features to a product without necessarily increasing capability, leading to “feature creep” (Surowiecki 2007; Thompson and Norton 2011). Likewise, companies also deliberately subtract functionality from a product resulting in revised offerings of lower quality (i.e., product versioning; Gershoff, Kivetz, and Keinan 2011). Such evidence suggests that the relationship between revisions and improvement is not as obvious as it may seem on the surface.

Indeed, today’s “information age” leaves consumers increasingly flooded with knowledge of ever-newer products that are not necessarily warranted, or even better than their predecessors (for one review, see Hoornweg and Bhada-Tat 2012): car manufacturers release new models each year even if their changes are merely cosmetic; book sellers release updated editions of their textbooks by tweaking the cover and skewing page numbers; and companies of consumer electronics and software are constantly updating their offerings, though not every update leads to a functionally superior product. And thanks to technological advances and social media, consumers are immediately informed about the existence of a newer version or upgrade they *could* acquire; for example, Amazon now adds a “*There is a newer version of this item*” tag to prior versions of products that point users to recently revised versions, which indeed drives

sales (Peterson 2021). Offline, too, marketers have long advertised yet-untested product upgrades as “must-have features” (Bayus 1988). Thus, relying on a “revised-is-better” heuristic might often lead consumers astray, especially in today’s marketplace of ever-newer (but not necessarily ever-better) offerings.

To test this claim, we asked real-world managers with “insider knowledge” to report on their experiences developing revised products on the job ($N = 126$ MBA students at a Midwestern university in the United States; 56.3% male; $M_{\text{age}} = 28.60$ years, $SD = 2.46$; 25% were current or former brand managers and marketers, with 2.69 years on average of significant professional marketing experience). They reported on their experiences of firms’ motivations for releasing revised versions of a product (see full methods and results of this study in web appendix). Nearly half of these managers (46.0%) indicated that they have worked for a company that launched a revised product that was *not* an improvement over a previous version (see Table 1 for some of their written examples). Nearly *all* of them (92.9%) could generate an example of a revised product they had seen in the market that was not a genuine improvement over its predecessor, estimating that 1 in 3 revised products on average were not truly improved. Most striking, when asked to independently rate various reasons for why a company would revise a product, the top-rated reason was *not* the obvious one—“to improve the product” ($M = 5.39$ on a 7-point scale from 1 = Not at all to 7 = Very much, $SD = 1.06$)—but instead, more plainly, was “to increase sales” ($M = 6.05$, $SD = 0.99$), $t(125) = -5.81$, $p < .001$); these reasons were followed by “to renew their brand image” ($M = 5.07$, $SD = 1.19$), “to grow their portfolio” ($M = 4.74$, $SD = 1.32$), and “to generate word-of-mouth” ($M = 4.49$, $SD = 1.42$).

Table 1. Sample quotes from industry insiders regarding why their company released a revision.

Product Category	Quote
Beauty	“I worked for a company that released a revised version but it was primarily repackaged and there wasn't a meaningful improvement or adjustment to the formula.”
Appliances	“The new version is somewhat similar to the previous version. However, it is branded as the best of the best to target new customers.”
Education	“It was not an improved version because of the lack of teachers to continue to offer courses offerings as in the previous way.”
Banking	“I used to work in a bank and one of the credit options was released with a new name but the same conditions.”
Automotive	“There weren't many major revisions to this model. The product did well in our portfolio and management didn't want to make many changes.”
Technology (software)	“We released a new version that didn't have significant changes, but we released it anyway with the hopes of showing the community that we were diligently working on our product and providing new features”.
Technology (hardware)	“The new generation was under-performing, and we had excess stock from previous gen so we released it again under new brandings.”
Industrial goods	“While at first we were determined to build a premium product that addressed the shortcomings in the market, due to time constraints and pressure, we ultimately created one that was basically the same as those of our competitors.”

In other words: Given that a non-trivial percentage of industry professionals, including marketing-managers themselves, are willing to admit to producing unimproved revisions, and to admit to various other motives for releasing such products beyond genuine improvement, these data provide especially compelling evidence for the possibility that everyday consumers, who lack such inside knowledge and therefore may blindly trust revised offerings—which, as we hypothesize, most of them will do on average—may be led astray in daily life.

OVERVIEW OF STUDIES

Eleven studies explore a preference for revision, as well as its psychological underpinnings, across a variety of study contexts and marketing-relevant outcome measures.

Study 1 documents the basic effect: Participants made a consequential choice between two products, and indeed became swayed in choosing an inferior option that was merely labeled as revised. Studies 2A-2C then replicate this basic effect using other marketing outcomes (willingness to pay, Study 2A; product taste and quality, Study 2B; purchase interest, Study 2C). Study 3 then examines one underlying mechanism, via a mediation-based approach: Consumers are less skeptical of products labeled as revised. Studies 4-6 extend these mediation insights via various moderation-based approaches, all derived from the same posited mechanism: Consumers are especially susceptible to exhibiting a blind preference for revision when under high (vs. low) time pressure (Study 4), when the revision cue is peripheral (vs. diagnostic: Study 5), and when consumers have low (vs. high) product expertise (Study 6). Finally, Studies 7A-7C extend this effect to naturalistic settings among students working to improve something they indeed revise regularly: their own resumes.

Unless otherwise reported, we pre-specified the sample sizes to at least 100 participants per condition and report all observations. We report all manipulations and measures and did not analyze data until collection was complete. Data and stimuli for all studies except Study 7A are available via Open Science Framework

(https://osf.io/4x76c/?view_only=b3e050d5cb774780ac2a576b84660eca); we did not post data or resumes from Study 7A because we did not receive permission to do so from the university with which we collaborated. We preregistered all experiments conducted since January 2021, which we ran after preregistration became our standard practice; for those studies, all analyses reported in the main text were preregistered unless otherwise indicated. Finally, all results reported in the main text hold when excluding participants who failed comprehension check questions (see robustness check analyses in web appendix).

STUDY 1: THE BASIC EFFECT

Study 1 tests our core proposition: consumers have a preference for revision absent objective improvement. Specifically, this study tests whether consumers will be more likely to choose an inferior product over an unrevised counterpart when the inferior alternative is merely labeled as revised. We used a “selfie stick” for the stimulus, which is a popular everyday product that also allows us to concretely define “inferior.” As the goal of a selfie-stick is to help users take pictures of themselves from a distance, their *length* is central to their quality: All else equal, longer selfie sticks are better than shorter ones.¹ In this study, some participants saw an inferior (i.e., shorter) selfie stick labeled as “NEWER version” while other participants saw the same product without this label—with both products for all participants having the exact same release date (thus holding chronological newness constant). We hypothesized that participants would be more likely to choose the objectively-worse selfie-stick when they thought it was revised—as assessed via a real, incentivized choice.

Methods

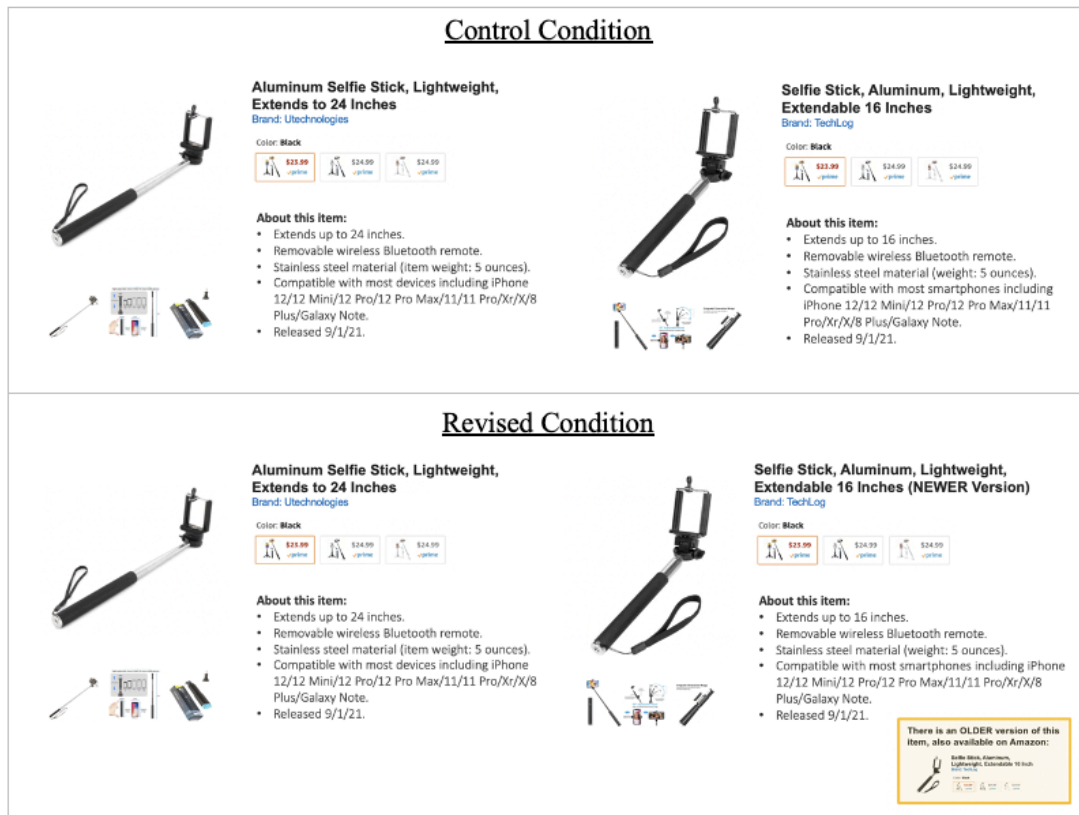
Participants. We recruited adults ($N = 602$; 51.0% male; $M_{\text{age}} = 42.01$ years, $SD = 12.23$) from Amazon’s Mechanical Turk (MTurk) for a fixed payment. This study was preregistered: https://aspredicted.org/8VF_HC9.

Procedure. Participants imagined that they wanted to buy a selfie stick, and had narrowed their search to two options, both available on Amazon.com. They were shown information on the two options, including a product photo, brand, price, dimensions, and features (see Figure 2).

¹ In case this point is not self-evident, we also conducted two pretest studies that confirmed that consumers view longer selfie-sticks as objectively better than shorter ones (see web appendix).

These products were sold by different manufacturers (Utechnologies and TechLog), but otherwise were identical in price, color, and other features; both were “Released 9/1/21.”

Figure 2. Study 1: Product descriptions between conditions.



The only substantive difference between the two selfie sticks was their extension length: one extended to 16 inches (the objectively inferior option) and the other extended to 24 inches (the objectively superior option). Between-subjects, we randomly assigned participants to one of two conditions: those in the *control* condition saw the two options without labels while those in the *revised* condition read that the inferior option (16-inch selfie-stick) had been revised. For ecological validity, we denoted revision following Amazon’s practices for communicating the existence of newer or older versions of the same item (Peterson 2021): participants in the *revised*

condition read the inferior selfie-stick was a “NEWER Version” and were informed about the existence of an older version, also available on Amazon. Participants were asked to choose one of the two selfie-stick options and their choice was made consequential using a lottery paradigm: prior to choosing, participants were informed that one participant would be selected at random and would receive a gift card to purchase their chosen selfie-stick. Finally, participants answered a comprehension check asking them to identify how the selfie-sticks were different. This and all subsequent studies concluded with basic demographic questions (e.g., gender, age, education level, income). As soon as the study was closed, we randomly selected a lottery winner and gave them an MTurk bonus of \$24 (i.e., the cost of the selfie stick).

Results

A greater proportion of participants chose the *worse* selfie stick over the objectively-better option merely when the worse one was labeled as revised (chosen by 30.9% of these participants) vs. when it had no such label (chosen by 14.5% of these participants), $\chi^2(1, N = 602) = 23.14, p < .001, \phi = 0.20$.

Discussion

Study 1 demonstrates the basic effect. Participants were led astray by revision framing: they became more likely to actually choose an inferior product merely if it happened to be labeled as revised. Next, we sought to replicate this basic effect across many products and marketing relevant outcomes.

STUDIES 2A-2C: A WIDESPREAD PREFERENCE FOR REVISION

Studies 2A-2C replicate and extend this basic effect. Study 2A assesses whether consumers are willing to pay more for revised products across many different kinds of products. Study 2B assesses whether the basic effect emerges even after participants actually sample the revised (but unimproved) product themselves, and also rules out the role of effort inferences in explaining this effect (perhaps consumers become more likely to prefer “revised” offerings not because of the change per se, but because they want to reward effort: Buell and Norton 2011; Morales 2005). Study 2B also pinpoints that the effect is driven specifically by revisions being appealing (as opposed to originals being unappealing). Study 2C further extends the effect to still another product, via perceptions of quality and purchase interest.

Study 2A

Methods

Participants. We recruited adults ($N = 199$; 49.7% male; $M_{\text{age}} = 32.10$ years, $SD = 10.90$) from Prolific Academic for a fixed payment. This study was preregistered:

https://aspredicted.org/GXY_PQT.

Procedure. This study employed a simplified design that allowed us to test the robustness of the effect across many product categories, beyond any one chosen stimulus. Participants reported their willingness to pay (WTP) for 40 different products on a scale from “1 = I’d pay as little as I could” to “7 = I’d pay as much as I could.” Each product was described along three dimensions (*product type*: material, experiential, utilitarian, hedonic, natural, synthetic, digital, physical, social, non-social; *version*: original, updated; *price*: low, high) not only for maximizing

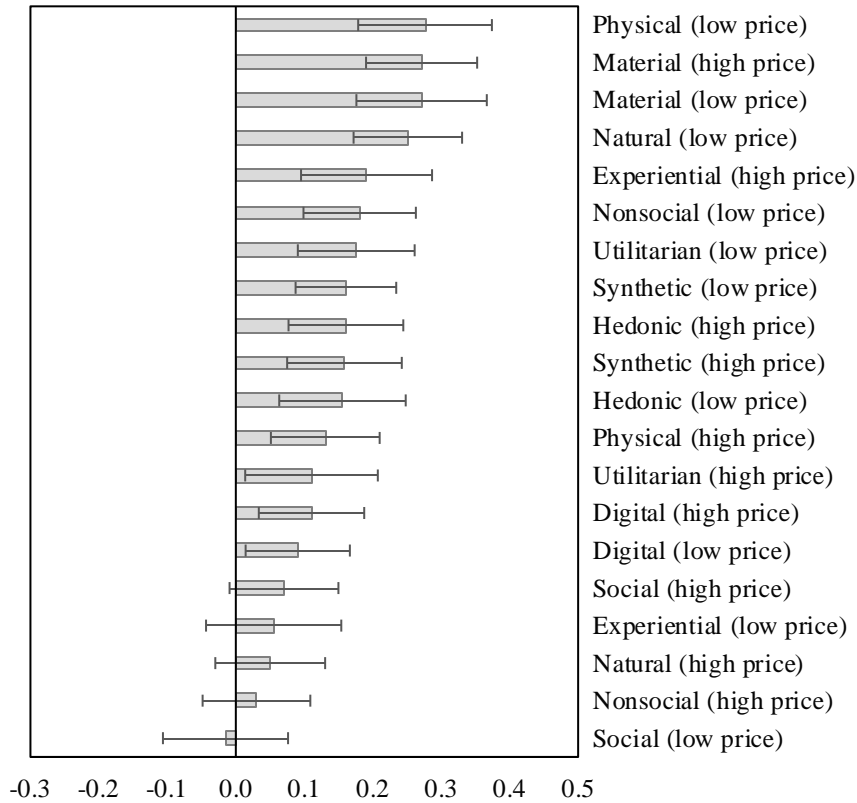
generalizability but also for disguising our hypothesis². The combination of 10 product types, two versions, and two price levels resulted in 40 products; each product was presented on a different screen and the order of products was randomized. After rating all 40 products, participants answered three comprehension check questions asking them to identify the three dimensions presented in this study (product, version, and cost).

Results

To test our hypothesis that consumers are willing to pay more for the same good when it is labeled as the updated (vs. original) version, we computed a generalized linear model entering participant id as a random-effect predictor, and version, price, and product type as fixed-effects predictors of WTP. Of key interest, this model revealed a significant effect of version ($b = 0.14$, $SE = 0.03$, $p < .001$): participants were willing to pay more for products that were labeled as updated ($M = 3.61$, $SE = 0.08$, 95% $CI = [3.47, 3.76]$) relative to those labeled as original ($M = 3.47$, $SE = 0.08$, 95% $CI = [3.32, 3.62]$). Figure 3 plots the difference between updated vs. original versions of each product type, showing a strikingly robust effect.

Figure 3. Study 2A: preference for revision, computed as the difference in WTP between the original and updated version for each product (positive values = pay more for revision). These values are sorted by difference in WTP (top to bottom, strongest to weakest); error bars indicate ± 1 SEM.

² A post-test study confirms that our hypothesis was not apparent: Participants were offered a financial incentive to correctly guess the study purpose and only 9.7% guessed correctly (significantly worse than the chance rate of 16.7%; see web appendix).



Study 2B

Methods

Participants. We recruited community members ($N = 239$; 49.8% male; $M_{\text{age}} = 23.19$ years, $SD = 4.50$) from a participant pool of a Northeastern university in the United States. Individuals received a fixed payment for participating in an in-person session including a series of unrelated studies.

Procedure. Each participant was seated at a desk with two cups, each containing one of two similar but distinct gummy candies (see web appendix for photographs of this laboratory setup). Participants followed instructions that appeared on successive computer screens. First, they were informed that they would sample two gummies made by different companies, and that the gummies cost about the same and also took about the same time to be made. Next,

participants ate one gummy, which served as the *control* gummy, and indicated their agreement with three statements: this candy is high quality, this candy is tasty, and the company put a lot of effort into making this candy, each from 1 (*strongly disagree*) to 7 (*strongly agree*). Participants then ate the other gummy, which served as the *revised* gummy, reading that: "...the recipe for this candy is the final, market-ready version of the recipe. Before the recipe was finalized, the company had tried several different recipes before finalizing the current formulation." After eating this second gummy, they completed the same three items as for the control gummy. Critically, all labels were arbitrary: between-subjects, we counterbalanced which of the two gummies was placed on the left versus right, and the order they were eaten and rated.

At the end of the study, participants indicated whether they had sampled both gummies (i.e., whether they had complied with the instructions) and answered three comprehension check questions asking them to identify if the gummies cost the same, took the same time to be made, and which one had been revised.

Results

The preference for revision again emerged: Relative to the control gummy, participants deemed the revised gummy to be of higher quality ($M_{\text{revised}} = 3.66$, $SD = 1.80$ vs. $M_{\text{control}} = 3.34$, $SD = 1.60$, $t(238) = 2.62$, $p = .009$, $d = 0.17$) and marginally tastier ($M_{\text{revised}} = 3.88$, $SD = 1.95$ vs. $M_{\text{control}} = 3.61$, $SD = 1.81$, $t(238) = 1.92$, $p = .056$, $d = 0.12$). A composite measure of the two primary outcome measures (quality and taste, $r_{\text{control}} = 0.71$, $r_{\text{revised}} = 0.78$) revealed that the revised gummy was deemed superior to the control gummy ($M_{\text{revised}} = 3.77$, $SD = 1.77$ vs. $M_{\text{control}} = 3.47$, $SD = 1.57$, $t(238) = 2.41$, $p = .017$, $d = 0.16$) even though the gummies were merely

framed this way. Finally, the effect of revision framing held when controlling for the difference in perceived effort in creating both gummies ($F(1, 237) = 4.10, p = .044, \eta_p^2 = 0.02$).

Study 2C

Methods

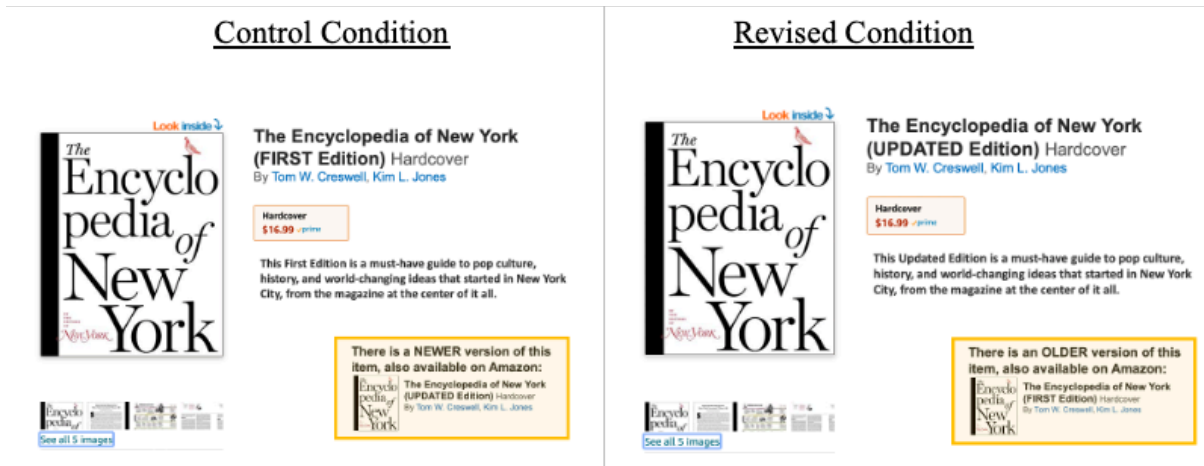
Participants. We recruited adults ($N = 400$; 56.5% male; $M_{\text{age}} = 40.44$ years, $SD = 11.89$) from MTurk for a fixed payment. This study was preregistered:

https://aspredicted.org/ZRL_PXQ.

Procedure. Participants imagined they wanted to buy an encyclopedia and were considering one option they found on Amazon. All participants were shown product information, which included an image of the encyclopedia cover, the title, authors, a short description, and the price (\$16.99). The only difference between conditions was that in the *control* condition, the encyclopedia was labeled as the “FIRST Edition” and participants read that there was a newer version also available on Amazon; in the *revised* condition, the encyclopedia was labeled as the “UPDATED Edition” and participants read that there was an older version also available on Amazon. Like in Study 1, we denoted revision following Amazon’s practice for communicating the existence of other versions of the same item (see Figure 4 for stimuli).

Next, all participants rated the quality of their focal version of the encyclopedia (1 = worst quality; 9 = best quality) and indicated their purchase interest (1 = not at all; 9 = very much); the order of these two questions was counterbalanced. Participants then answered two comprehension check questions asking them to identify the version they were asked to rate and if there were other editions of the same encyclopedia also available on Amazon.

Figure 4. Study 2C: Product information used in both conditions.



Results

We replicated the basic effect, here in a fourth context: Participants in the revised condition judged the encyclopedia to be of higher quality relative to control participants ($M_{\text{revised}} = 6.99, SD = 1.32$ vs. $M_{\text{control}} = 6.47, SD = 1.38$; $t(398) = 3.80, p < .001, d = 0.39$) and also reported stronger purchase interest ($M_{\text{revised}} = 5.74, SD = 2.24$ vs. $M_{\text{control}} = 4.59, SD = 2.38$; $t(398) = 5.00, p < .001, d = 0.49$). Next, we tested whether the effect of condition on purchase interest was mediated by perceived quality, using PROCESS Model 4 and 5,000 bootstrapped samples; indeed, we find a significant indirect effect of condition on purchase interest through perceived quality: $a \times b = 0.45, SE = 0.12, 95\% CI_{\text{Boot}} [0.22, 0.70]$.

In a follow-up study, we tested whether the result is robust to the specific wording used to denote “revision.” Participants ($N = 501$ MTurkers; 51.1% male; $M_{\text{age}} = 41.31$ years, $SD = 12.27$) were shown either the first edition of the encyclopedia, or one of three different revised versions of the encyclopedia, labeled either: the fourth edition, the updated edition, or the revised edition. All of the effects replicated regardless of the specific word used to denote revision; the

three revised conditions all differed from the “first edition” yet did not differ from each other (see methods and results in web appendix).

Studies 2A-2C: Discussion

Studies 2A-2C offer convergent tests—across varied paradigms and measures—of the preference for revision. Each study demonstrates this preference using a different method of conveying revision status across product labels (“newer version” in Study 2A; “updated edition” in Study 2C) and product descriptions (in Study 2B). The effect emerged over and above perceptions of increased effort, and even when giving participants the entire bottom-up experience (i.e., actually eating the gummies in Study 2B) before making their evaluations.

STUDY 3: PROCESS EVIDENCE (MEDIATION)

Study 3 has two aims. First, this study further extends the basic effect by testing whether the preference for revision manifests using yet another operationalization of revision status (via version numbering). Second, and most critical, in this study (and as we continue to do in studies 4-6), we go beyond documenting the basic effect to also testing our proposed heuristic-based process for why it occurs; we begin here by testing attention, such that revision framing may lead consumers to be less scrutinizing of a (flawed) product, thus explaining their preference for it.

Methods

Participants. We recruited adults ($N = 500$; 53.8% male; $M_{\text{age}} = 36.02$ years, $SD = 11.14$) from MTurk for a fixed payment. This study was preregistered:

https://aspredicted.org/VCS_CXT.

Procedure. To begin, all participants read that we—the requesters of the Mturk HIT—had been working on developing a game called *ART Time*, and were shown a brief description of the game. The game allows users to freely “paint” a blank canvas, with a selection of different tools that create different shapes and colors. We hired a developer to create the game for us, ensuring that all participants would objectively have the same novel experience. Moreover, we instructed the developer to design the game with subtly “buggy” features (e.g., the cursor would sometimes lag a split-second behind).

All participants read that, to date, we had released five updates to the game. Participants in the *revised* condition were informed that they had been randomized to play “Version 5 of 5 (i.e., our newest update)”, whereas those in the *control* condition were informed that they had been randomized to play “Version 2 of 5 (i.e., an older, pre-updated version).” In both conditions, we described the given version as having been developed in “early 2019” to hold participants’ knowledge of chronological recency constant.

Next, all participants played the game for two minutes, during which all other keyboard controls were disabled. When time expired, the page automatically continued to a survey screen where participants rated a block of enjoyment questions and a buggyness question (the order of the enjoyment block and the buggyness question was randomized, each presented on individual pages). In the enjoyment block, participants rated five items each on a 1 (*not at all*) to 7 (*extremely*) scale: how much they liked the game, how fun, enjoyable, and cool it was, and how happy they were playing it (hereafter referred to as the enjoyment scale; $\alpha = 0.97$). For buggyness, participants reported the number of “specific individual bugs” that they had experienced in the game, from 0 to 20 (with a 21st option, “More than 20 [please type your number]”). Afterwards, participants who reported noticing any number of bugs greater than zero

were asked to provide some examples, via an open-ended essay box. Finally, all participants reported any general confusion with the task (99.6% of participants reported no confusion) and answered a comprehension check in which they identified which version (of five) they had played.

Results

Three critical findings emerged. First, participants again exhibited a preference for revision: participants enjoyed their experience more when told they were playing Version 5 ($M_{\text{revised}} = 5.85, SD = 1.29$)—relative to Version 2 ($M_{\text{control}} = 5.26, SD = 1.50, t(498) = 4.75, p < .001, d = 0.42$)—even though all participants played the same game.

Second, participants noticed fewer bugs when told they were playing Version 5 ($M_{\text{revised}} = 1.75, SD = 4.09$) compared to Version 2 ($M_{\text{control}} = 2.85, SD = 4.13, t(498) = -2.98, p = .003, d = -0.27$). Common descriptions of these alleged bugs included perceived speed issues (examples of actual quotes: “It lagged a bit overlaying other stroke patterns”; “It did not move where I wanted it to”) and a perceived lack of features (examples of actual quotes: “unable to erase”; “no choice of color, limited control”). Interestingly, an exploratory (i.e., non-preregistered) analysis indicated that significantly more participants noticed zero bugs in the revised condition (65.1%) compared to those in the control condition (40.7%, $\chi^2(1, N = 500) = 29.76, p < .001, \phi = 0.24$).

Third, the effect of revision framing on enjoyment was mediated by number of bugs noticed while playing the game (PROCESS Model 4 with 5,000 bootstrapped samples and 95% confidence intervals). As predicted, we find a significant indirect effect via number of bugs noticed: $a \times b = 0.10, SE = 0.05, 95\% CI_{\text{Boot}} [0.03, 0.20]$.

Discussion

Study 3 further replicates the basic effect established in our prior studies, across another new stimulus, new measures—and, most critically, also shines light on *why* this basic effect occurs. Consistent with a “revised-is-better” heuristic, participants appeared to blindly trust the revision label such that they noticed fewer bugs in the game while playing, leading to a more enjoyable experience. Next, we further unpack this process via various moderators stemming from our conceptual account.

STUDIES 4-6: PROCESS EVIDENCE (MODERATION)

To the extent that the basic effect indeed reflects heuristic-based processing, driven by a lack of product scrutiny (as established in Study 3), then consumers should be especially susceptible to the effect under conditions that recruit heuristic-based processing styles. Next, we tested three such conditions: the preference for revision should be pronounced under time pressure (Study 4) and when consumers lack product expertise (Study 6); in contrast, it should be reduced in the presence of diagnostic cues pointing to the vacuousness of the revision (Study 5).

Study 4

Methods

Participants. We recruited adults ($N = 1,503$; 49.9% male; $M_{\text{age}} = 41.75$ years, $SD = 12.43$) from MTurk for a fixed payment. We predetermined this (large) sample size by using the results of an earlier, smaller-sample version of this study which suggested a small effect; thus, for Study 4, we assumed a small effect ($w = 0.08$), alpha of 0.05, and 85% power. This study was preregistered: https://aspredicted.org/M7H_VVG.

Procedure. Participants read that they found two selfie-stick options on eBay and were asked to make a choice between options: one extended up to 24 inches and the other one up to 16 inches (again: this shorter option thus represents the objectively worse choice). Similar to Study 1, participants read that the shorter 16-inch selfie stick was labeled as “NEWER Version” (see product descriptions used in Figure 5). All participants were informed that eBay had a promotion for 15% off their purchase. Between-subjects, we randomly assigned participants to one of two conditions: those in the *low time pressure* condition read “the promotion does NOT expire – take your time to choose!”; those in the *high time pressure* condition read “the promotion EXPIRES SOON – in 40 seconds! Act fast and choose now!” Participants in the high time pressure condition also saw a countdown clock above the products.

Participants then chose a selfie stick. As in Study 1, this choice was incentivized: participants were informed that one participant would be selected as the winner of a lottery and would receive a gift card to obtain the selfie-stick they selected. After making their choice, participants reported to what extent they felt they had a lot of time to make a decision (1 = not at all; 7 = very much) and answered two comprehension check questions asking them to (1) identify how the two selfie-stick options were different from each other and (2) confirm if one selfie-stick was labeled as a “NEWER version.” As soon as the study was closed, we randomly selected a lottery winner and gave them an MTurk bonus of \$24 (i.e., the cost of the selfie stick).

Figure 5. Study 4: Products descriptions.



**Selfie Stick, Lightweight,
Extends to 24 Inches**

Brand: UleTechnologies

Price: **\$23.99**

Buy It Now

Add to cart

Item specifics:

- Condition: brand-new, unused, unopened item
- Color: Black
- Extends up to 24 inches.
- Removable wireless Bluetooth remote.
- Stainless steel material (item weight: 5 ounces).
- Compatible with most devices including iPhone 12/12 Mini/12 Pro/12 Pro Max/11/11 Pro/Xr/X/8 Plus/Galaxy Note.



**Selfie Stick, Lightweight,
Extends to 16 Inches (NEWER Version)**

Brand: TechLog

Price: **\$23.99**

Buy It Now

Add to cart

Item specifics:

- Condition: brand-new, unused, unopened item
- Color: Black
- Extends up to 16 inches.
- Removable wireless Bluetooth remote.
- Stainless steel material (weight: 5 ounces).
- Compatible with most smartphones including iPhone 12/12 Mini/12 Pro/12 Pro Max/11/11 Pro/Xr/X/8 Plus/Galaxy Note.



Results

First, the manipulation worked as intended: participants in the high time pressure condition felt they had less time to decide ($M = 2.44$, $SD = 1.60$) than participants in the low time pressure condition ($M = 6.01$, $SD = 1.23$, $t(1,501) = 48.49$, $p < .001$, $d = 2.50$), and also spent less time choosing a selfie-stick ($M = 23.23$ seconds, $SD = 14.18$) than participants in the low time pressure condition ($M = 38.53$ seconds, $SD = 37.97$, $t(1,501) = 10.38$, $p < .001$, $d = 0.54$).

The basic effect indeed varied as a function of pressure, with *high* time pressure making participants *more* susceptible to being swayed by revision framing: More participants in the high time pressure condition chose the inferior revised selfie-stick (33.1%) compared to the low time pressure condition (27.0%, $\chi^2(1, N = 1,503) = 6.49$, $p = .011$, $\phi = 0.07$).

Study 5

Methods

Participants. We recruited adults ($N = 599$; 53.1% male; $M_{\text{age}} = 40.77$ years, $SD = 11.97$) from MTurk for a fixed payment. This study was preregistered:

https://aspredicted.org/HPC_H28.

Procedure. Participants made a choice between two sets of bedsheets. Both sets were made of the same material, were the same size, and cost the same. The only difference was that they were sold by different (fictitious) manufacturers: MellowSleep and Marie Bellesa. For all participants, one option (MellowSleep) did not have an additional label. Between-subjects, we manipulated the labeling of the other option (Marie Bellesa; see Figure 6). Some participants saw that this option also had no label (*control* condition), while others saw it was Marie Bellesa's "updated edition" (*revised* condition); here, we hypothesized to replicate the basic effect, such that more revised than control participants would choose this latter option. More critical for this study, a third group of participants also saw an "updated" option—but it was merely the *packaging* that was updated (*revised-packaging* condition). Here, we hypothesized that the preference for revision should be reduced, as participants can rely on other obvious cues to inform their choice (i.e., that revised packaging is not exactly a substantive improvement) rather than blindly relying on revision framing.

Participants then made their choice. Again, this was a real, incentivized choice via the same lottery paradigm from prior studies. Finally, participants answered a comprehension check asking them to identify how the sheets were different. As soon as the study was closed, we randomly selected a lottery winner and gave them an MTurk bonus of \$35 (i.e., the cost of the sheets).

Figure 6. Study 5: Product descriptions by condition.

Control Condition



Queen Size Bed Sheets, Cotton
Brand: MellowSleep

Price: **\$34.99**

Buy It Now

Add To Cart

Item Specifics:
• MATERIAL: Ultra-soft, light-weight cotton material
• SIZE: Queen
• CARE: Machine washable



Queen Size Cotton Sheets, UPDATED EDITION
Brand: Marie Bellesa

Price: **\$34.99**

Buy It Now

Add To Cart

Item Specifics:
• MATERIAL: Ultra-soft, light-weight cotton material
• SIZE: Queen
• CARE: Machine washable

Revised Condition



Queen Size Bed Sheets, Cotton
Brand: MellowSleep

Price: **\$34.99**

Buy It Now

Add To Cart

Item Specifics:
• MATERIAL: Ultra-soft, light-weight cotton material
• SIZE: Queen
• CARE: Machine washable



Queen Size Cotton Sheets, UPDATED EDITION
Brand: Marie Bellesa

Price: **\$34.99**

Buy It Now

Add To Cart

Item Specifics:
• MATERIAL: Ultra-soft, light-weight cotton material
• SIZE: Queen
• CARE: Machine washable

This is our **UPDATED EDITION** of cotton bed sheets.

Revised-Packaging Condition



Queen Size Bed Sheets, Cotton
Brand: MellowSleep

Price: **\$34.99**

Buy It Now

Add To Cart

Item Specifics:
• MATERIAL: Ultra-soft, light-weight cotton material
• SIZE: Queen
• CARE: Machine washable



Queen Size Cotton Sheets, UPDATED PACKAGING
Brand: Marie Bellesa

Price: **\$34.99**

Buy It Now

Add To Cart

Item Specifics:
• MATERIAL: Ultra-soft, light-weight cotton material
• SIZE: Queen
• CARE: Machine washable

This product now comes with **UPDATED PACKAGING**.

Results

First, we again replicated the basic effect: More participants chose the Marie Bellesa sheets (key option of interest; label varying by condition) over the MellowSleep sheets (always no label) when those sheets were claimed to be the updated edition; indeed, these latter sheets were chosen by 76.2% of participants in the revised condition, but only by 39.4% of control participants (logistic regression, $b = 1.60$, $SE = 0.22$, $Wald = 52.53$, $p < .001$).

Second, and critically: This effect was reduced among participants in the revised-packaging condition; 58.0% of these participants chose Marie Bellesa's "updated packaging"

offering, which was significantly less than the 76.2% of participants in the revised condition who chose Marie Bellesa's "updated edition" offering (logistic regression, $b = 0.84$, $SE = 0.22$, Wald = 14.98, $p < .001$). Interestingly, participants were *still* swayed by mere-revised packaging (58.0% revised-packing vs. 39.4% control: logistic regression, $b = 0.75$, $SE = 0.21$, Wald = 13.47, $p < .001$; chi-square including all conditions, $\chi^2(2, N = 599) = 55.61$, $p < .001$, $\phi = 0.31$).

Study 6

Methods

Participants. We recruited adults ($N = 600$; 50.8% male; $M_{\text{age}} = 43.29$ years, $SD = 13.13$) from MTurk for a fixed payment. This study was preregistered:

https://aspredicted.org/ZLN_YKD.

Procedure. This study was a 2x2 between-subjects design in which we manipulated product version (original vs. updated) and consumer expertise (low vs. high). Participants imagined they were at a store considering a specific purchase of an item that they rarely purchase and had little personal experience and expertise with (*low expertise* conditions) or an item they frequently purchase and had a lot of personal experience and expertise with (*high expertise* conditions). Then, participants were informed that there were two current versions of the item (original and updated) and today the store only had one version available: original or updated. Immediately below, participants indicated their purchase interest by responding to the question "Based on this information alone about this version available as you're looking at it in the store..." on a 7-point scale with end-points "I definitely WOULDN'T just buy it at this point; still need to find out more details (1)" and "I definitely WOULD just buy it at this point; no need to find out more details (7)." Finally, participants answered two comprehension checks asking

them to identify specific details about the scenario (which version was available and their prescribed expertise), and also indicated the specific item they brought to mind and the cost of that item (two open-ended questions).

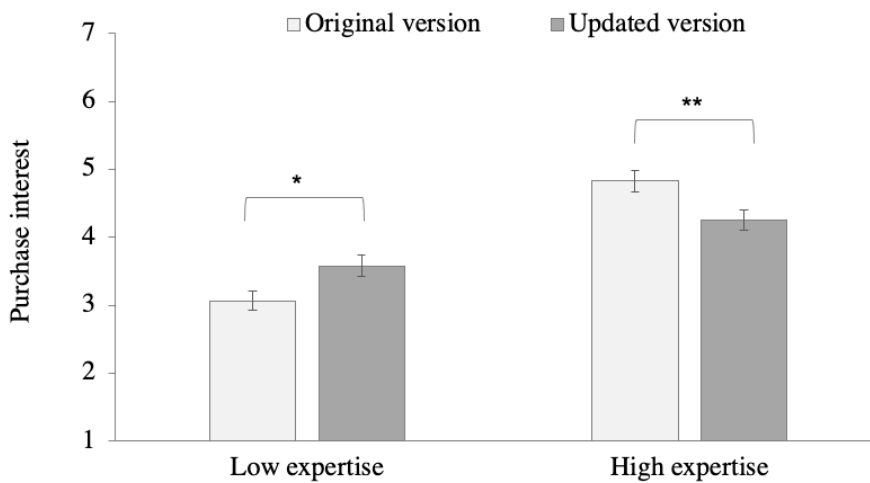
Results

Critically, there was a significant interaction between version and expertise ($F(1, 596) = 13.17, p < .001, \eta_p^2 = .02$). For low-expertise participants, as predicted, we replicate the preference for revision: These participants felt significantly more ready to purchase the product when it was framed as an updated version rather than as an original version ($M_{\text{revised}} = 3.58, SD = 1.89$ vs. $M_{\text{original}} = 3.07, SD = 1.73; F(1, 596) = 5.57, p = .019$). However, this basic effect in fact *reversed* among high-expertise participants, who felt significantly *less* ready to purchase revised versions versus original versions ($M_{\text{revised}} = 4.25, SD = 1.80$ vs. $M_{\text{original}} = 4.83, SD = 1.94; F(1, 596) = 7.70, p = .006$; see Figure 7).

Studies 4-6: Discussion

Together, Studies 4-6 offer convergent tests—across a diversity of paradigms and marketing outcomes—that again replicate the basic effect, while also highlighting why it occurs: Like for heuristics in general, participants were especially likely to exhibit a “revised-is-better” heuristic when under high time pressure, when lacking other diagnostic cues, and when lacking product expertise—all of which fall from a broader heuristic-based process of product scrutiny.

Figure 7. Study 6: mean ratings of purchase interest across conditions. Error bars indicate ± 1 SEM; * $p < .05$, ** $p < .01$, *** $p < .001$.



STUDIES 7A-7C: EXPLORING MORE NATURALISTIC SETTINGS

Finally, Studies 7A-7C comprise a systematic series of studies using field and lab data with the goal of providing evidence for how the basic effect may extend to more naturalistic settings, for a very personal product that is indeed regularly revised: resumes. We tested whether creators themselves, and those evaluating them, both exhibit a preference for revision.

First, in Study 7A, we assessed MBA students’ perceptions of their own resumes as a function of how much they had revised them. As part of a career and development program, students submitted their resume, received feedback from resume coaches, and submitted a revised version for inclusion in a “resume book” distributed to prospective employers. We hypothesized that the more extensive their revisions, the more positively creators would judge their own resume. Then, in Study 7B, we tested whether this perception is warranted. Observers judged both versions of a student’s resume; critically, however, the two versions were simply presented as two “different” versions (these observers were not told that one was the original version and one was the revised version). We thus tested whether revisions were objectively improved relative to the originals, without any revision labels to potentially distort this judgment.

Last, in Study 7C, we tested whether observers judged revised resumes more positively when they *were* labeled as such; critically, however, we *randomly assigned* these labels. Some observers simply judged the actual original and the actual revision, both accurately labeled; others, however, judged a version that was labeled “original” (that was really the revision) and a version that was labeled “revised” (that was really the original). We hypothesized that these participants would rate any “revised” versions to be better than any “original” versions—regardless of the true value of these labels.

Methods

Study 7A. This study took place during a process to help MBA students of a Northeastern business school in the United States to prepare their resumes. We collaborated with the school’s Career and Professional Development (CPD) team during their process to help students revise their resumes: students submit their initial resume to CPD; receive feedback over the course of two months from a resume coach, and then submit a revised resume that is distributed to prospective employers. This process granted us the opportunity to analyze a diverse set of genuinely revised stimuli.

In the fall of 2016, 302 students requested feedback. After students had submitted their final version, CPD asked whether they were willing to be contacted about the review process, and we obtained the email addresses of the 77.1% of students who agreed (233 out of 302). We sent an email to these 233 students, hereafter referred to as “creators,” containing a link to a short survey. A priori, we decided to send one follow-up email to non-responders and to close the survey once two full days had passed without any new survey responders, resulting in a response rate of 18.9% (44 out of 233).

In the survey, these 44 creators responded to four questions about the resume revision process. First, they rated, “What percentage of the final version of your resume is different from your original resume?” on a 0–100 scale with endpoints labeled 0 (*the final version is exactly the same as the original*) and 100 (*the final version is completely different from the original*). Second, they rated, “Relative to my original resume, my final resume is...” using the options: -3 (*dramatically worse*), -2 (*moderately worse*), -1 (*a bit worse*), 0 (*about the same*), 1 (*a bit better*), 2 (*moderately better*), and 3 (*dramatically better*). Third, they rated, “How satisfied are you with the final version of your resume?” on a scale from 1 (*not at all satisfied*) to 7 (*extremely satisfied*). Finally, they rated, “How many times did you obtain feedback from a resume coach?” via a numeric text entry box. This survey allowed us to assess whether creators indeed revised their resumes and viewed the final products as improved—which presumably is their goal in having formally worked on their resumes over the course of the semester. At the end of the survey, we asked creators whether we could use their (anonymized) resumes for future research. Of these 44 creators, data from eleven creators were excluded: five creators did not grant us permission to use their resumes and six creators did not answer all four survey questions. This process resulted in data from 33 creators (i.e., 33 pairs of resumes) to be used in Studies 7B-7C.

Study 7B. Next, an independent sample of participants evaluated one of the resume pairs generated in Study 7A. These observers ($N = 204$, 46.6% male; $M_{\text{age}} = 35.58$ years, $SD = 13.83$) were community members who came to the laboratory to participate in this and a series of unrelated studies. Observers rated both versions of one randomly selected resume pair from Study 7A (i.e., one student’s original resume and the same student’s revised resume; the name of each creator in all resumes was replaced with the generic name “Alex Newman” with all other identifiers removed). However, resumes did not have labels of original and revised. Instead,

participants only read: “On the next pages, you will see two resumes and you will be asked to rate the appeal of each resume.” Each resume was presented on a different page on a computer screen and participants were asked to rate the “overall appeal” on a scale from 1 (*very low*) to 7 (*very high*). Between-subjects, we manipulated the order in which the resumes were presented. Some of these observers rated the original (though it was not labeled as original), followed by its corresponding revision (again, the revision was not labeled as such); others were presented with the versions in the opposite order (i.e., the revision first, followed by the original version, though they were not labeled as such). Thus, we ended up with a pool of 66 resume pairs (i.e., we had a pool of 33 resumes pairs in Study 7A where we counterbalanced which version was judged first).

Study 7C. Finally, another independent sample of participants evaluated the same resume pairs. These observers ($N = 453$, 50.1% male; $M_{\text{age}} = 30.27$ years, $SD = 12.27$) were also community members who came to the laboratory to participate in this and a series of unrelated studies. As in Study 7B, observers rated both versions of one randomly selected resume pair (i.e., one student’s original resume and the same student’s revised resume); however, the resumes were indeed labeled. First, these observers were shown a version and were asked to “please rate the ORIGINAL DRAFT of the resume (shown above) with respect to the following dimension: Overall appeal” on a 1 (*very low*) to 7 (*very high*) scale. Next, they were shown the other version in the pair and were asked to “please rate the REVISED DRAFT of the resume (shown above) with respect to the following dimension: Overall appeal” on a 1 (*very low*) to 7 (*very high*) scale.

Between-subjects, we manipulated whether the resume that was labeled “original” was truly the original, and whether the resume that was labeled “revised” was truly the revision. That is, some of these observers simply rated the actual original, followed by its actual revision; we swapped these labels for other observers (unbeknownst to them). This setup produced 66 resume

pairs (i.e., a pool of 33 control pairs, in which actual originals were paired with their actual revisions; and a pool of 33 experimental pairs, in which actual revisions labeled as “original” were paired with their corresponding originals labeled as “revision”). Thus, each observer was randomized to rate both versions of a randomly selected resume pair from either the control pool or the experimental pool. Our results collapse across the 33 resumes within each pool.

Results

Study 7A. Do creators think their revisions are improvements? Creators deemed their revised resume to be significantly different from their original resume ($M = 36.34\%$, $SD = 26.33$; one sample t -test against 0%, $t(37) = 8.51$, $p < .001$), and deemed their revised resume to be significantly better than their original resume ($M = 1.55$, $SD = 0.90$; one sample t -test against the scale midpoint, $t(43) = 11.38$, $p < .001$). Most importantly, the more dissimilar their two versions were, the higher quality the creators perceived their revised resume ($r(38) = .61$, $p < .001$)³. Thus, these findings confirm that revised resumes differed from original resumes, and in the creators’ eyes, should be noticed by others as indeed improved. Finally, creators were on average highly satisfied with their final products ($M = 5.55$, $SD = 1.00$) and had obtained feedback 2.34 times ($SD = 0.96$) over the semester.

Study 7B. Are the revisions really any better than the originals? When the resumes were simply presented as “different” versions—i.e., without revision labels that may elicit our effect—observers judged both resumes in the pair as similarly appealing ($M_{\text{revised}} = 5.01$, $SD = 1.43$ vs. $M_{\text{original}} = 5.07$, $SD = 1.47$). A 2×2 mixed ANOVA revealed that resumes that had been revised

³ Six creators in Study 7A did not answer the following question: “What percentage of the final version of your resume is different from your original resume?” Thus, for analyses involving this measure, the valid sample is 38 participants.

by creators in Study 7A were judged as no better than the original resumes, $F(1, 202) = 0.99, p = .321, \eta_p^2 = 0.01$; similarly, resumes presented on the first screen were considered equally appealing as resumes presented on the second screen (no effect of presentation order: $F(1, 202) = 0.32, p = .572, \eta_p^2 = 0.00$; no significant interaction: $F(1, 202) = 1.72, p = .191, \eta_p^2 = .01$).

Study 7C. Are resumes evaluated more positively when labeled revised? Observers exhibited a preference for resumes labeled as “revisions,” independent from the veracity of this label. Specifically, observers perceived resumes labeled as revisions more positively than those labeled as originals ($F(1, 451) = 51.61, p < .001, \eta_p^2 = 0.10$); the main effect of accuracy and the interaction were not significant ($ps > .250$). When resumes were labeled accurately, participants perceive revised resumes to be of higher quality than original ones ($M_{\text{revised}} = 5.25, SD = 1.37$ vs. $M_{\text{control}} = 4.85, SD = 1.34, F(1, 451) = 33.86, p < .001, \eta_p^2 = 0.07$). However, when resumes were labeled inaccurately, participants still perceived resumes *merely* labeled as revised to be of higher quality than resumes labeled as original ($M_{\text{revised}} = 5.24, SD = 1.37$ vs. $M_{\text{control}} = 4.94, SD = 1.39, F(1, 451) = 18.83, p < .001, \eta_p^2 = 0.04$).

Discussion

Studies 7A-7C extend our findings into a more naturalistic setting, suggesting that creators confound change with improvement: the more they revised their own resumes, the more they thought their resumes had improved (Study 7A). Alas, naïve observers did not find the revisions to be any better than the originals (Study 7B); in line with our proposed framework and all prior studies, the only thing that made observers—probably most of them with low expertise in the domain of “evaluating resumes” perceive improvement was mere revision framing—irrespective of whether the resume had truly been revised (Study 7C).

GENERAL DISCUSSION

To “revise and resubmit” is far more than academic exercise. Companies often work to revise their products and services, just as individuals often revise their own creations and contributions. In principle, a world of constant ostensible improvement should lead to uniformly better outcomes for both creators and consumers; in practice, the current research counsels caution. Eleven studies document a robust preference for revised products across a wide variety of stimuli and contexts, with participants becoming more likely to choose and pay up for revised offerings even at added, and mistaken, costs (e.g., doing so for revised offerings that were *worse* than the alternative). Our studies also offer one mechanism explaining why this effect emerges. Supporting a heuristic account, we demonstrate that consumers lower their efforts to scrutinize a product labeled as revised; in turn, they are especially swayed by revisions under conditions in which heuristic-based processing styles are especially engaged, such as when facing high time pressure for choosing a product, when lacking other diagnostic product cues, and when lacking product expertise. Finally, even though consumers may also reward firms for their mere intent to improve (Buell and Norton 2011; Morales 2005), we find that the preference for revision emerges even when controlling for perceived effort.

As our studies demonstrate, this preference for revision was found in a wide range of marketing-relevant outcomes, including purchase interest, real product choices, and willingness to pay. The effect emerged even after giving participants complete first-hand knowledge of the entity in question (e.g., eating a candy, playing a video game). If consumers cannot easily trust their bottom-up experience to draw more informed conclusions, they could be influenced by companies that are motivated to make revisions for reasons beyond the desire to improve the quality of their products. As such, our findings reveal novel and important implications, for

consumers, companies, and policy makers. On the one hand, things that are objectively improved in the revision process may go underappreciated, if the fact that they have been revised is not made deliberately clear (e.g., a candy's recipe almost certainly underwent extensive editing and tweaking behind the scenes, even if it is not advertised that way). On the other hand, products that are objectively unchanged (or even made worse) in the revision process may nonetheless be adopted, so long as consumers *believe* they possess a "revised" version. This may happen innocently (e.g., consumers may be prone to upgrading to a revised version even if that version is not objectively better), but also intentionally (e.g., companies that release annual updates for the sake of releasing annual updates).

Thus, our findings raise timely consumer welfare implications given that consumers live in a world of constant revision. It is not simply that, all else equal, a revised product is preferred over an unrevised one. Rather, we show that participants choose revised-but-unimproved products even when they must incur some kind of cost to do so. Specifically, in some of our choice studies, we created a tradeoff between product quality and revision status – for example, a choice between a selfie stick that has been revised, versus a superior (i.e., longer) but unrevised option. We show that a significant proportion of our participants chose the revised but lower quality option when labeled as revised. Another way of creating such costly tradeoffs would be to manipulate price (i.e., to present participants with a choice between a revised option versus a less expensive but unrevised option). We opted against doing so because participants may have inferred that the revised offering was indeed truly better thanks to its higher price (Rao and Monroe 1989), thus undermining the ability to test our hypothesis. However, to the extent that newer (but not necessarily better) versions of products are also more expensive in everyday life, our findings suggest consumers outside the laboratory may literally pay added costs due to mere

revision framing. Indeed, Study 2A (which assesses willingness to pay) attests to this point: consumers are willing to pay a premium for revised products.

Another welfare implication is that consumers might also feel there is a *need* to purchase the revised product assuming that it has been improved, even when their current version is perfectly fine (Bellezza et al. 2017) contributing to the societal trend of overconsumption, a problem that has severe economic and environmental consequences. However, our work suggests strategies that consumers can adopt to protect themselves when evaluating a revised offering (e.g., taking more time to decide; looking for other cues to assess objective quality; obtaining advice from experts). Taken together, the preference for revision raises dual practical implications for revisions wielding influence when perhaps they should not, and failing to wield influence when perhaps they should, due merely to the label affixed to them.

Just as important, these findings also advance two important theoretical contributions. First, our research extends prior work on how heuristics drive consumer choice. The preference for revision contributes to understanding an emerging collection of related phenomena that are susceptible to framing effects. For example, building on the tendency for the first item in a given array to be preferred (Carney and Banaji 2012)—a sequencing effect—research has shown that “phantom firsts”—merely framing something as “first”—increases its appeal (LeBoeuf, Williams, and Brenner 2014). Interestingly, these phenomena suggest that in the absence of explicit revision, stimuli that are framed to have occurred earlier in a sequence are preferred (Smith, Newman, and Dhar 2015). At the same time, other research points to the notion that items that have been in existence for longer are preferred, as with the longevity bias (Eidelman, Pattershall, and Crandall 2010). While our results cannot be accounted for by the longevity bias—for example, in Study 1 both selfie-sticks had been released on the same date and in Study

3 the video game had existed for the same amount of time in both conditions—clearly more research is needed to understand when different temporal sequences are preferred. Expanding on this point, future research is needed to examine the preference for revision in the context of apparent exceptions, such as when people desire older, “pre-revised,” experiences—as when experiencing nostalgia (Wildschut, Sedikides, Arndt, and Routledge 2006), rediscovering the joy of past experiences (O’Brien 2019), or valuing original renditions of collectibles over later renditions (e.g., artwork; Newman and Bloom 2012).

More generally, an important theoretical implication of this work is that it bridges our understanding of when product revisions are successful (e.g., Amazon’s “newer version” tag can drive sales; Peterson 2021) and when they backfire (e.g., New Coke; Klein 2015). Thus, this work offers a unified framework integrating research on preferences for revised offerings (e.g., Bellezza et al. 2017; Dagogo-Jack and Forehand 2018; Okada 2001, 2006) versus original renditions (e.g., Newman and Bloom 2012; Smith, Newman, and Dhar 2015) via the same broader heuristic-based style processing account. For example, stemming from our theoretical account, we show that revision labels significantly boost preferences for a revised offering when consumers lack expertise, but also significantly *decrease* preferences when consumers have expertise. Finally, this work also offers opportunities for future research to examine other moderators such as product involvement, brand loyalty, decision complexity, decision ambiguity, and degree of revision, all of which similarly fall from our proposed heuristic-based framework.

Second, our findings enrich and advance current understandings of how marketing communications influence consumer perceptions (e.g., Campbell 1995; Cotte et al. 2005; Friestad and Wright 1994) by examining how consumers can be influenced by merely advertising a product as revised. Furthermore, it contributes to examining the link between

expectations and experience, addressing an issue raised by Lee et al. (2006) in their review of outstanding questions on such links: “A third question concerns how specific perceptual, attentional, and cognitive mechanisms mediate the effect of expectations on experience (or reported experience)” (p. 1057). Extending this research, studying the effects of “revision” framing explores a novel aspect of the idea that expectations affect experiences: even in the absence of specific information about *how* the experience will be changed (“tastier,” “fattier”), we suggest that mere expectation of change itself (“revised”) influences consumption experiences. Our studies provide finer-grained and novel evidence on one mechanism explaining how expectations more specifically change consumers’ experience: in our case, revision framing lead participants to lower their efforts to scrutinize a revised product. Providing direct evidence for this heuristic account—documenting specific behavioral changes—offers novel insight into the drivers of changes on outcome measures such as liking and preference. For example, one competing mechanism running against our heuristic account is that revision framing should elicit especially *close* inspection of the product, to the extent that yet-unknown novelty invites explanatory reasoning (as posited by basic theories of reinforcement learning: Behrens, Woolrich, Walton, and Rushworth 2007). In consumer contexts at least, we instead reveal the opposite. Consumers may often come to like a product not just from the desirable features they notice, but also from the potentially “buggy” features they overlook (thanks to revision framing).

In sum, the current research bridges and advances consumer research on expectancy effects and product change, revealing when and why consumers risk being deceived by the “new and improved.”

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