

# The Vicarious Construal Effect: Seeing and Experiencing the World Through Different Eyes

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That 2 individuals can look at the same stimulus and experience it differently speaks to the power of construal. People's construals are shaped by their idiosyncratic attitudes, belief systems, and personal histories. Eleven studies provide support for and explain the origin of a *vicarious construal effect*: Considering perspectives one once had but seemingly lost, one ordinarily would have only with more experience, or one would not have had spontaneously, all exerted an assimilative pull on one's ongoing experiences. This means habituation can be slowed or stalled by considering another's fresh perspective (Studies 1–6), desensitization can be preemptively achieved by considering another's stale perspective (Study 5), and the experience of a performance can change by considering how fans or nonfans would see it (Study 7). Blind to the power of construal in defining their experiences, participants believed they were learning about a stimulus's properties or their own underlying preferences, not simply the experience-distorting effects of the perspective manipulations (Studies 6–7). These effects emerged in examinations of positive emotions, negative emotions, interest, and perceptions of humor. The final 2 pairs of studies used causal chain designs to elucidate an underlying mechanism. Trying to understand another's perspective encouraged participants to approach a stimulus by posing different questions or directional hypotheses to themselves (Studies 8a and 9a), which caused participants' own experiences of the stimulus to shift (Studies 8b and 9b). The implications of this account for when considering another's perspective should change one's own experience are detailed.

**Keywords:** perspective taking, construal, habituation, naïve realism, attitudes

**Supplemental materials:** <http://dx.doi.org/10.1037/pspa0000179.supp>

Having lived in the same city for 10 years, New York City photographer Dolly Faibyshev had become uninspired by and even jaded about her surroundings. Intrigued by the excitement and zeal that each planeload of tourists brought to her environs, Dolly set out to become one of them. Like many a real tourist, Dolly packed up her camera and headed to well-trodden ground. She stood among a sea of amateur photographers at the foot of the Empire

State Building, outside a popular Broadway show, and on the top deck of a ferry as it approached the Statue of Liberty. Instead of viewing tourists as sidewalk obstacles to navigate around, Dolly embraced them as novices off of whose energy she could feed and from whose untarnished perspectives she could learn. Her resulting photography project "I Love New York" sought to capture what she resaw (Terranova, 2014).

Several psychology literatures on social influence can help to explain Dolly's rejuvenating experience. First, Dolly may have experienced emotional contagion—the "tendency to automatically mimic . . . expressions, vocalizations, postures, and movements with those of another person's and, consequently, to converge emotionally" (Hatfield, Cacioppo, & Rapson, 1993, p. 96). Surrounded by the real tourists' oohs and aahs and their delighted faces, Dolly may have caught this rush of excitement—what Hatfield et al. (1993) called primitive empathy and what Durkheim (2016) identified as a crowd's state of collective effervescence. Those who frequent cinema, concert halls, and other live performance venues know how emotional outbursts from a few can help to charge the room, intensifying others' experience as well (an intensification that may extend to watching live performances on TV; Cui, 2018). Second, Dolly may have benefited from vicarious learning (Bandura, Ross, & Ross, 1963; Ledoux, Robinson, Baranowski, & O'Connor, 2018)—directly learning (or relearning) from the tourists that she could directly observe and listen to as

This article was published Online First December 2, 2019.

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The research reported in this article was supported in part by U.S. National Science Foundation award 1749608 to Clayton R. Critcher. This research is also supported by the Center for Global Economy and Business Faculty Grant, Leonard N. Stern School of Business, New York University, to Minah H. Jung. We thank the following research assistants for their assistance with collecting data: Gloria Cheng, Eric Dahlen, Sophia Dawn, Randy Fan, Shradha Ganapathy, Henry Ho, Sagar Jajoo, Laura Kim, Joon Ko, Dui Lee, Nora Mohamed, Ishita Rustagi, Julie Song, Sarena Su, Josh Woznica, Michelle Yang, Sunny Young, Irene Yu, Rowena Zhang, David Zhao.

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they described their experiences (Richert & Harris, 2008; Koenig, Clément, & Harris, 2004). “I think the building touches the clouds!” one tourist might proclaim, causing Dolly to visually explore an angle she had seemingly lost the ability to notice. For both of these reasons, Dolly may have experienced renewed joy from essentially coexperiencing these attractions with others. Indeed, sharing experiences with others intensifies them—the positive and the negative, alike (Boothby, Clark, & Bargh, 2014).

Dolly’s approach, and the two possible mechanisms just described, suggest a social route to recapturing Dolly’s initial perspective. Consider, counterfactually, if Dolly had returned to these sites at a time when no tourists—or even anyone else—was there. There would be no squeals of delight to lift her spirits; no direct observations by which she could vicariously (re)learn to be a newcomer once again. Could Dolly benefit from merely trying to consider how a hypothetical tourist might experience these sites? And if so, how?

On first thought, such an enterprise might seem to be about as effective as a non-Spanish-speaker trying to consider how a Spaniard would speak to have a conversation in Spanish; without access to the relevant expertise (i.e., knowledge of Spanish), such a person will not be able to *will* a fluent conversation. However, as we will argue below, even though people tend not to spontaneously appreciate the power of construal—that people’s view of reality is subjectively filtered—in guiding their own experiences, people can be led to see the world through a lens that they had but seemingly lost, or maybe even one they never had (or would have had) at all. By trying to understand how someone else (e.g., a first-time visitor) sees and experiences a stimulus, people seek to test directional hypotheses with confirmatory questions (e.g., “What would this person find exciting?”) that affects what one sees and experiences. In what follows, we develop this *vicarious construal effect* (VCE), consider how it is both plausible and surprising in light of past research, and describe our empirical strategy for investigating the conditions under which it emerges and just how subjectively veridical (vs. distorted) of an altered experience it induces.

### The Vicarious Construal Effect

People’s understanding of and engagement with the world is only partly guided by the modularly processed low-level input to their perceptual systems. Instead, they lean on perceivers’ expectations and context (e.g., Chaigneau, Barsalou, & Zamani, 2009). Psychologists have long appreciated the role that construal plays in guiding understanding and experience of reality (Griffin & Ross, 1991; Hastorf & Cantril, 1954; Ross & Nisbett, 1980, 1991). A symphony goer’s auditory system is what detects a series of acoustic disturbances and shuttles that input to higher-level central systems (Fodor, 1984). Guided by this input, one may construe the sounds as a moving *avant-garde* masterpiece or an aversively cacophonous embarrassment. Whereas sensory perception reflects the direct detection of environmental input, construal involves a subjective interpretation that changes both one’s own experience and (given a typical blindness to the power of construal) one’s beliefs about what a stimulus actually is. At least for one of the authors’ father, modern pop music does not just sound bad to him; it *is* an earsore. Because people carry with them different experiences (Campbell, O’Brien, Van Boven, Schwarz, & Ubel, 2014), beliefs (Critcher & Dunning, 2009; Lord, Ross, & Lepper, 1979;

Wilson, Newins, & White, 2018), group identities (Vallone, Ross, & Lepper, 1985), and other motivations (Caruso, Mead, & Balcells, 2009), different people construe and experience the consequences of the same objective stimulus in subjectively different ways. In what has become a classic case study for social psychologists, Princeton and Dartmouth football fans who watched the same physical, intensely contested matchup between their respective teams appeared to have seen different games (Hastorf & Cantril, 1954).

The power of construal is important not merely in understanding why different people may respond to the same event differently, but also in understanding why the same person may show a change in response to the same stimulus. Repeated exposure to or consumption of a particular stimulus typically leads to habituation, a decline in response to that stimulus (Groves & Thompson, 1970; Thompson & Spencer, 1966). Of course, it is natural to ask whether such diminished responses reflect changing construals (shifted perspectives on a stimulus that leave one unable to see what one once saw) or instead a more basic physiological limit to how much one can show or experience certain responses (becoming “laughed out” or “cried out”). In general, habituation characterizes a response to a specific stimulus (a particular Jerry Seinfeld clip), not a specific style of emitted response (laughter), suggesting that a shifting construal or perspective on the stimulus may be a key contributor to habituation (Epstein, Caggiola, Rodefer, Wisniewski, & Mitchell, 1993; Rolls, Rolls, Rowe, & Sweeney, 1981; Rolls, Van Duijvenvoorde, & Rolls, 1984).

Several lines of research give examples of how the subjective construal of a stimulus influences how people respond to it across time. For example, when people consume jelly beans, they habituate to them more quickly by thinking of what they are consuming as members of one superordinate category (jelly beans) as opposed to the numerous subcategories (e.g., cherry, banana, or watermelon jelly beans) from which the flavors come (Redden, 2008). Of course, in this example, people are not exposed to a literally identical stimulus repeatedly. Calling attention to each stimulus’s novelty or glossing over those distinctions shapes people’s responses by framing their exposure as less or more repetitive, respectively. Although consuming a literal variety of stimuli decreases habituation (Brondel et al., 2009; Epstein et al., 2009; Galak, Kruger, & Loewenstein, 2011; Havermans & Brondel, 2013; Inman, 2001; Sørensen, Møller, Flint, Martens, & Raben, 2003; Temple, Chappel, Shalik, Volcy, & Epstein, 2008), focusing on that variety in particular reduces habituation as well. For example, even when people have the same habituating experience of listening to a favorite song repeatedly, merely thinking about other songs (but not other TV shows) they had recently heard (or seen) decreased habituation to the beloved tune (Galak, Redden, & Kruger, 2009).

Whether people retain or can be nudged to reaccess these seemingly misplaced construals is ultimately an empirical question, but we first consider how this might be done and why people tend not to do it spontaneously. A fundamental tenet of *naïve realism* is that people experience the world as if they see an undistorted reality (Ross & Ward, 1995). People are blind to the importance of their own idiosyncratic lenses that shape their own subjective reality. From this perspective, it may not be that people do not have access to alternative lenses through which they can see and experience the world. Instead, the subjective immediacy of their constructed reality blinds them from considering the alternatives they could attain.

If people are blind to the power of construal, how might they be nudged to adopt a different one? This might be achieved as a byproduct of giving people a goal, which then changes the way a stimulus is processed (Hamilton, Katz, & Leirer, 1980). For example, people who were asked to read a passage with the goal of studying engaged in more coherence-building (e.g., mental paraphrasing) than did those reading the same passage with the goal of being entertained (Bohn-Gettler & Kendeou, 2014). This leaves them with a different representation of or construal of the passage. Furthermore, although the majority of our studies focus on the context of habituation (and whether seemingly lost construals can be recovered), we also test more general implications of our logic to see whether people can be similarly nudged to try on and experience the consequences of perspectives they had yet to, or would not otherwise, have.

In the present research, we give participants an active goal when they experience certain stimuli. We tell participants that after a certain experience, they will be asked to indicate how someone—someone characterized by certain preferences or a specified history of experience—would respond to it. This should cause people to approach the experience trying to answer the question, “What would someone else—someone likely to approach this with a different perspective—see?” Often, such questions are accompanied by an expectation—for example, that a first-time viewer will be particularly moved by what is novel or stimulating or that one who detests a movie genre will be quick to find a film’s shortcomings. Such expectations may tip participants off for what to look for (“So what will she find annoying about this?”), encouraging people to engage in a confirmatory hypothesis search that tilts the scale in favor of construing a target in the way this other might (Critcher & Dunning, 2009; Darley & Gross, 1983; Duncan, 1976; Langer & Abelson, 1974; Lee et al., 2006; Snyder & Swann, 1978). That people’s construals shift should be reflected not only in their self-reported experience but in their judgments that suggest they have a different take on the stimulus itself.

Although our central idea and focus is that actively considering another’s perspective—even when that person likely has a construal that the self once had and seemingly lost—will shape one’s own experience, we also highlight how the above logic makes bolder predictions. That is, the very property that makes people unlikely to use this technique spontaneously—their blindness to the power and influence of construal (Ross & Ward, 1995)—may contribute to its power in creating what feels like a veridical experience. Consider the experience of new parents. With a vulnerable newborn in their care, they tend to become sensitized to potential threats to their safety. However, blind to the power of construal, they explain the additional crime they now observe not to a change in their own perspective on the world but instead to a true increase in danger in the world (Eibach, Libby, & Gilovich, 2003). In the case of the present research—in which considering a new construal is achieved more intentionally (following explicit instructions), it is an open question whether people will experience the consequences as reality (i.e., indicative of the true properties of the stimulus or the true preferences of the self) or merely the distorting influence of the manipulation. By analogy, if someone gets engaged at a restaurant, he may or may not appreciate that the meal he has may be less life-changing for someone else.

In identifying this effect as the vicarious construal effect, we use the term “vicarious” to recognize one is considering the perspec-

tive of someone else, not the unadulterated perspective of one’s present self. By “someone else,” we mean another person or even one’s own person at a different point in time (a recent past or future self). In most of our studies, we ask people to consider the experience of someone else as a way to encourage them to adopt another construal. However, note that for our purposes, this is mostly just an experimental tool: It is more natural for people to understand that different people have different perspectives than it is to consider intertemporal shifts in one’s own construal. In both cases, this *other* is simply a device that allows participants to pose the directional hypothesis that should kickstart the relevant psychological processes that, we predict, change one’s construal and experience. In this way, our focus differs from the typical one in the perspective taking or empathy literatures, in which the other person plays a more central role. For example, establishing an empathic connection with another can reduce prejudice toward them (Galinsky & Moskowitz, 2000), enhance cooperation (Batson & Ahmad, 2001; Batson & Moran, 1999), and even elevate altruistic behaviors toward them (Batson, 1991, 1998). Our efforts are quite different. In the present research, the other person is intentionally underspecified and depersonalized. The other is not a humanized soul for whom one acquires concern, but merely an experimental device about whom a directional question (e.g., “What would he or she find novel and stimulating?”) can be posed.

Our approach both draws on and contrasts with that taken in the emotional regulation literature. Gross, Sheppes, and Urry (2011) argue that a key feature that distinguishes emotion regulation (from emotion generation) is it involves the explicit goal to change one’s emotion. For example, those who engage in frequent emotional regulation agree with items like, “When I want to feel *less* negative emotion . . . I *change what I am thinking about*” (Gross & John, 2003). That said, one particularly successful regulatory tool reinforces the power of construal: cognitive reappraisal, changing whether or how one thinks about a stimulus in order to change one’s present state (Gross & John, 2003; Lazarus & Alfert, 1964). We argue that emotional experience may shift as an unintended byproduct—not of trying to change one’s own response, but of trying to understand another’s. This feature also bolsters our confidence that the vicarious construal effect may produce responses that feel veridical. After all, one engages in emotion regulation in response to a scary movie *because it is scary*. The vicarious construal effect may produce reactions that feel more informative about (and not simply a useful masking of the true effects of) a stimulus.

Although the hypothesized vicarious construal effect would not be a classic perspective taking phenomenon, one lesson from the perspective taking literature did make us pessimistic about perspective taking’s prospects for shaping experience. Relating to people’s blindness to the power of construal, people are also largely egocentric in their perspectives (Epley, Keysar, Van Boven, & Gilovich, 2004; Van Boven & Loewenstein, 2003; cf. Epley, Caruso, & Bazerman, 2006). This is apparent even when another’s perspective is objectively identifiable—such as the fact that what is on one’s left is on a facing person’s right (Epley, Morewedge, & Keysar, 2004; Galinsky, Magee, Inesi, & Gruenfeld, 2006; Keysar, Barr, Balin, & Brauner, 2000). Given such egocentrism, why would we have confidence that people can escape the experiential constraints of their own construals by considering another’s? In our experiments we push people to adopt an alternative lens *before* (and allow this intervention to operate

during) the actual experience. Much as people egocentrically have trouble considering a nonegocentric reality after they understand their own present world, we would not predict (and empirically confirm) that trying to consider another's construal would lead one to reconstruct one's *previous* experiences.

Finally, consider how the vicarious construal effect would contrast with a phenomenon that does share part of its name: vicarious reinforcement. Often discussed as a component of vicarious learning, the phenomenon is observed when the self modifies its own behavior to match that of someone else who is receiving rewards for their actions (Bandura et al., 1963). It is also used to explain altruism: One may repeat a behavior simply because it leads to rewards for someone else, even if not the self. Such vicarious reinforcement has been observed even in nonhuman primates (Chang, Barter, Ebitz, Watson, & Platt, 2012). The vicarious construal effect would differ in at least three key ways. First, it does not depend on observational learning. Second, it identifies an information processing mechanism that changes one's own experience and understanding of a stimulus and, thus, does not describe a conditioned response to provide someone else rewards that the self forgoes. Third, we use the term "vicarious" more broadly, showing that the present self can borrow from past selves, not simply other people.

The existing empirical evidence that provides evidence most similar to our proposal comes from a clever single-study paper by Gilead et al. (2016). They showed participants images that were emotionally neutral or negative. Participants' task was to predict the emotional responses of another participant who was said to be particularly tough or particularly sensitive. When making predictions about the negative stimuli, participants showed strong neural activation in regions associated with "picture-induced negative affect" when considering the response of the sensitive (instead of the tough) person. The authors took this as evidence that people understand others' mental states by simulating and reading them out in the self (Mitchell, Banaji, & Macrae, 2005), a strategy that is useful in part because of the difficulties of observing others' mental states directly (Zhou, Majka, & Epley, 2017).

Gilead and colleagues' work certainly lends plausibility to our hypotheses. We see our own focus as differing in at least three key ways. First, we explore whether considering another's perspective affects self-reported experience. Whether Gilead and colleagues' participants subjectively felt more or less negative affect—or simply showed a pattern of neural activity observed when looking at negative stimuli—is unknown. Second, we aim to understand whether the vicarious construal effect is experienced as veridical. That is, simulating another's experience may be just that—a simulation that does not color one's own basic understanding of a stimulus and the self's disposition toward it. Third, we explore a higher-level cognitive process that traces considering another's perspective to the warping of one's own experience. It is certainly possible that Gilead and colleagues have detected the lower-level neural correlates of the process we delineate and directly test.

## Overview of Studies

Across 11 studies, we test for evidence of the vicarious construal effect and how it operates. We ask whether trying to understand an alternative perspective of a stimulus colors one's own experience of the stimulus. Most studies focus on whether the VCE can slow

or even fully stall habituation (Studies 1–6 and 9b). This offers a particularly conservative context for testing our ideas, because it raises the intriguing possibility that construals one has lost can still be appealed to, to shape one's present experience. But also, we test whether the VCE can lead to a shift in people's one-time experiences—either by prompting them to preemptively habituate (Study 5) or see and feel what someone who is predisposed to like or dislike a target would see and feel (Study 7). In an effort to explore the generality of the VCE, we test how it operates with regard to a number of experiences: happiness (Study 1), disgust (online Supplemental Materials Study A), sadness (online Supplemental Materials Study B), engagement (Study 2), awe (Studies 3 and 4), humor (Studies 5–6, 9a–b, and online Supplemental Materials Study F), and enjoyment (Studies 7–8b and online Supplemental Materials Studies D1–D2).

We attempt to localize the vicarious construal effect to the influence of considering and adopting an alternative construal as opposed to alternative mechanisms like self-distancing (online Supplement Materials Studies A and B), social contagion (Study 3), or anchoring and adjustment (Study 4). We test a strong form of the VCE, that people accept the influence of adopting alternative construals as veridical—leading people to attribute the influence of the manipulations to the power of the stimulus or their own personal preferences instead of to the distorting influence of the manipulation itself (Studies 6 and 7). Finally, we use a pair of causal chain designs (Spencer, Zanna, & Fong, 2005) to test whether the goal to predict another's reaction to a stimulus changes the questions one reports asking oneself in approaching that stimulus (Studies 8a and 9a), questions that then affect one's own experience of the stimulus (Studies 8b and 9b).

We were particularly interested in taking multiple steps to achieve sufficient statistical power. One difficulty—faced by most research—is we did not know our effect sizes a priori. Recognizing this limitation, Simmons, Nelson, and Simonsohn (2013) recommended studies include at least 50 participants per cell. We decided to go well beyond Simmons et al.'s recommendations by aiming for at least 100 participants per conditions that were defined by factors of interest (as opposed to counterbalancing factors). That said, we recognized that our studies tested different effects—ones that come from various stimuli, measures, and even experimental conditions. Given this, it meant that even our early studies offered limited information about what effect sizes we could expect in later studies. This led us to focus on *ex ante* stopping rules that had the potential to offer even larger sample sizes than those we set as our minimum threshold. For undergraduate subject pools, we collected data until the end of the semester. For Amazon Mechanical Turk (MTurk), we collected as many subjects as the funding lab's budget permitted during a month. This approach led us to recruit on average more than 200 participants per condition.

## Study 1: Happiness

Study 1 offered an initial test of whether considering another's fresh perspective of a stimulus slows one's own habituation. Participants were exposed to a happiness-inducing video clip three times. Before watching it for the third time, some participants were told they had been paired with another participant who was watching the clip for the first time. Those *fresh perspective* participants



were asked to consider the other person's first-time perspective on the clip as the participants themselves watched the clip for the third time. By our vicarious construal account, considering the stimulus through fresh eyes should slow one's rate of emotional habituation, leading one to report experiencing the clip with relatively sustained happiness. However, by an alternative line of reasoning, imagining someone seeing the video for the first time may simply highlight the lack of novelty of the stimulus for the self. This could make one's own experience feel even more tired, stale, and emotionally blunted.

For every study, we designed an attention check that asked participants—at the study's end—to remember a feature of the study. Such attention checks are more difficult than those that merely assess whether one is paying attention and not responding randomly *in the moment* (e.g., “What is 2 + 2?” or “Choose 9 on the response scale below”). Although the present approach produces higher exclusion rates, such checks better guarantee that participants who remained in our final analyses read the study materials and displayed sustained attention through the procedure. That said, we report analyses including all participants—even those who failed these checks—in the [online Supplemental Materials](#). Unsurprisingly, including these participants often produced somewhat weaker results. However, assuaging concerns that the attention checks may have helped to provide artifactual support for our hypotheses, the statistical significance of results—in neither this nor any study—*depended* on such exclusions.

Study 1 also had a secondary methodological goal. We varied whether we tested participants' understanding of the key instructions (e.g., that the yoked participant was watching the clip for the first time) in an effort to identify participants for whom we would reinforce the manipulation. In plainer terms, we identified some participants who needed to read the key instructions a second (and sometimes a third) time. In so doing, we expected to reduce the number of these participants who ultimately failed our attention check, which later quizzed all participants about this key detail. However, whether such an approach would simply undermine the attention checks' screening function—to distinguish those with genuine versus flippant engagement—is a question we explore with secondary interest.

## Method

**Participants and design.** There were 1,244 Americans who were recruited from Amazon Mechanical Turk (MTurk) and paid a nominal amount for their participation. Participants were randomly assigned to one of four conditions in a 2 (perspective: fresh, control)  $\times$  2 (manipulation reinforcement: present, absent) full-factorial design.

**Procedure.** All participants began by watching a 1 min, 45 s video clip that was created from footage that originally appeared on the Canadian TV channel Love Nature. The clip documented a baby polar bear learning to walk. We selected the delightful content because we expected it would elicit happiness. After watching the clip, participants were asked to indicate “the extent to which you felt each of the following while watching the video clip” as a measure of their *Time 1* emotions. Participants responded on a slider scale anchored at 0 (*not at all*) and 100 (*most I have ever felt*). Crucial on this list of five emotions was “happy.” Confirming the prime applicability of happiness (over proud,

amused, angry, and guilty), participants reported experiencing happiness at Time 1 more than the other emotions,  $t_s > 12.62$ ,  $p_s < .001$ .

At that point, participants were told, “On the next screen, you will watch and listen to the same video clip for a second time.” Although we did not provide an explanation for why participants were being asked to rewatch the clip, we called attention to the repetition so participants would not think the experiment had malfunctioned. Participants then completed the *Time 2* emotion measures—“happy” and the filler measures. Participants then learned they would watch the video clip for a third time.

For those in the fresh perspective condition, we wanted them to consider the video through fresh eyes, even though their habituation to the video may have prompted them to lose their initial, untainted perspective. To accomplish this, we led participants to believe they were being paired with another participant who was about to watch the video for the first time. Participants were informed that after watching the video (now, for the third time), they would try to guess how the other participant responded to the video while watching it for the first time. We provide the verbatim manipulation instructions here because future studies will use modifications of this basic template as later described:

But this time, we'd like to see how well you can predict another participant's responses to the video. More specifically, you will be paired with someone (if possible) who is just starting the study and will be watching the video clip at the same time as you, but for the first time. On the next screen, you will wait for up to 1 minute for another participant to arrive at the video for the first time. (Many workers complete this study, so this will likely happen.) If someone does, you will be alerted and then both of you will watch the video at the same time. You will then estimate how this new participant—who was watching the video for the first time—was responding to the video.

To enhance the believability of the cover story, participants were told they would have to wait up to 60 s for another participant to arrive; after 15 s, the screen advanced, ostensibly because another participant was about to watch the clip for the first time.

Those in the control perspective condition did not receive information about being paired with another participant. However, to equate for the expected and actual time delay, they were told that it would take up to a minute for the next page to load. The screen actually advanced in 15 s, just as in the fresh perspective condition. Following the video, all participants reported on their own reaction to the third viewing. To minimize deception, we then had all participants estimate how another participant would react to the video while watching it for the first time (just as some participants had been warned they would). Interested readers can find information on these forecasts from this and all relevant remaining studies in the [online Supplemental Materials](#).

For participants assigned to the manipulation reinforcement condition, they were presented with a comprehension question just after learning that they had been paired with another participant. (Note that the timing of this question varied for those in the fresh perspective and control conditions.) They had to indicate whether the yoked participants were to watch the video for the first, second, third, or fourth time while the participants watched it for the third time. Participants who failed to correctly answer “first” had to read the key instructions

once more before seeing the comprehension check again. If they missed the question a second time, they were shown the instructions once more. After participants characterized their own experience watching the clip for the third time, all participants—regardless of whether they were exposed to this comprehension question and potential manipulation reinforcement or not—had to answer this question as an attention check.

## Results

**Attention check.** There were 149 participants who failed the final attention check. Those for whom we potentially reinforced the manipulation showed lower failure rates (6.19%) than those not subject to such screening and potential manipulation reinforcement (18.23%),  $\chi^2(1, N = 1,244) = 39.11, p < .001$ . This difference did not further depend on whether participants were in the fresh perspective or control condition,  $\chi^2(1, N = 1,240) = 1.56, p = .211$ . Our primary results include only those 1,095 participants who passed the attention check. We then turn to exploratory results that examine all participants and the influence of the manipulation reinforcement.

**Participants who passed the attention check.** These participants reported greater happiness at Time 1 ( $M = 79.89, SD = 22.57$ ) than Time 2 ( $M = 75.42, SD = 26.07$ ), demonstrating that participants became habituated to the clip with repeated exposure,  $F(1, 1090) = 87.11, p < .001, \eta_p^2 = .07$ . Did the fresh perspective manipulation slow the rate of subsequent habituation? We conducted a repeated-measures analysis of variance (ANOVA) to test whether the additional decline in happiness from Time 2 to Time 3 was stunted by the fresh perspective manipulation. The predicted Perspective  $\times$  Time interaction emerged,  $F(1, 1089) = 21.61, p < .001, \eta_p^2 = .02$ . As depicted in Figure 1, although participants in the control condition showed a clear decline in happiness ( $M_{T2-T3} = 8.41, SD = 19.26$ ; paired  $t(555) = 10.29, p < .001, d = .44$ ), those in the fresh perspective condition showed an attenuated decline ( $M_{T2-T3} = 2.97, SD = 19.37$ ; paired  $t(534) = 3.55, p < .001, d = .15$ ). Stated differently, the fresh perspective intervention slowed the rate of habituation by 65%. Furthermore, the crucial Perspective  $\times$  Time interaction was not further strengthened by the manipulation reinforcement manipulation,  $F < 1$ .

**All participants.** We reconducted the same set of analyses, but this time we included all participants. We still observed evi-

dence of habituation when comparing Time 1 to Time 2 happiness,  $F(1, 1239) = 74.14, p < .001, \eta_p^2 = .06$ . Furthermore, the crucial Perspective  $\times$  Time interaction remained significant,  $F(1, 1238) = 22.82, p < .001, \eta_p^2 = .02$ . Although (unsurprisingly) each effect size is directionally smaller, the robustness of our key results is comforting.

Our reinforcement manipulation did reduce exclusion rates, but might it also have changed the utility of the attention check in identifying participants who were versus were not sufficiently engaged with the experiment? There was one hint that that was the case. More specifically, we found that the degree of habituation from Time 1 to Time 2 was qualified by a Reinforcement Manipulation  $\times$  Attention Check Failure interaction,  $F(1, 1236) = 8.29, p = .007, \eta_p^2 = .01$ . This is notable in part because the reinforcement manipulation always occurred *after* both Time 1 and Time 2 happiness measures. The interaction reflected that when the reinforcement manipulation was reinforced, then the rate of habituation from Time 1 to Time 2 did not depend on whether participants passed the attention check,  $F < 1$ . However, when the manipulation was *not* reinforced, then those who passed the attention check showed clear evidence of habituation,  $M_{T1-T2} = 5.00, t(487) = 6.81, p < .001, d = .31$ . Those who failed the attention check did not,  $M_{T1-T2} = -1.99, t(108) = -1.26, p = .211, d = -.12$ . This difference was significant,  $F(1, 595) = 16.42, p < .001, \eta_p^2 = .03$ . To the extent that habituation should be expected, then these results imply that steps taken to lower exclusion rates can also reduce attention checks' screening function. After all, some percentage of participants simply does not pay attention, engage, or respond to questions asked of them.

Our remaining studies do not include such manipulation reinforcements. Study 1 suggests under such conditions, attention check measures may have more validity as screening tools to identify participants who are actually engaged with studies. Furthermore, by not drawing extra attention to the manipulation, we do less to telegraph to participants our focal interest. (Later studies will address in various ways more general concerns about demand.) However, to provide assurance that our results do not depend on such strict exclusion criteria, results reported in the [online Supplemental Materials](#) provide assurance that for all studies, the statistical significance of our results of interest does not depend on these exclusion criteria.

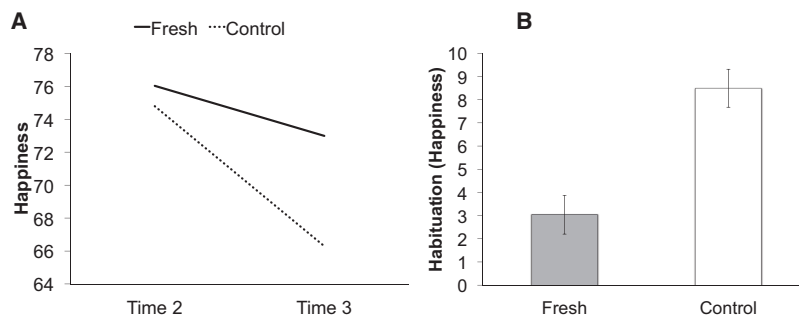


Figure 1. Study 1: (A) Self-reported happiness on the second and third viewings while watching the video in the fresh and control perspective conditions. (B) Mean indication of happiness at Time 2 minus indication of happiness at Time 3 in the fresh and control perspective conditions. Error bars reflect  $\pm 1$  standard error from the mean.

## Discussion

By our interpretation, people were able to slow their habituation to a stimulus by trying to consider how someone would see it through fresh eyes. This portrays habituation not as a passive process to which people succumb. Instead, considering alternative perspectives allows people to actively resist habituation. This should offer pleasant news for those who wish to prolong their enjoyment of stimuli that bring them happiness.

What the findings do not address is whether considering another's perspective slowed habituation, or whether the perspective taking itself was merely a pleasant task. We replicated the key elements of this design with disgusting and sad stimuli (online Supplemental Materials Studies A and B, respectively). The fresh perspective manipulation slowed habituation, even though this meant prolonging negative experiences. Although this suggests that considering another's fresh perspective slows habituation instead of merely boosting positive affect, neither Study 1 nor online Supplemental Materials Study A demonstrated that it is considering another's *fresh* perspective in particular that has this effect. Online Supplemental Materials Study B showed this more definitively. Whereas considering another's fresh perspective completely stalled habituation to a sadness-inducing clip, considering another's stale perspective (i.e., the yoked participant was also about to watch the stimulus for the third time) did not reduce habituation.

### Study 2: TED Talk

Study 2 built on our previous results in two ways. First, whereas our initial studies examined how vicarious construals slowed emotional habituation, Study 2 moved to a different sort of stimulus evaluation. Participants in Study 2 watched the second-most-watched TED Talk: Amy Cuddy's "Your Body Language Shapes Who You Are." However, there is a reason academics do not deliver the (exact) same conference talk every year: Engagement and fascination thrive on novelty (Bradley, Lang, & Cuthbert, 1993). We asked whether the fresh perspective manipulation could sustain interest in the repeatedly presented talk.

Second, although online Supplemental Materials Study B established the importance of whose vicarious perspective one considers, our previous studies have confounded what the imagined other is experiencing (i.e., a first look at a stimulus) with when this other's experience is occurring. That is, our previous participants were led to believe they had been matched with a participant who was being exposed to the stimulus *while* the participant was watching it himself or herself. We included this staged simultaneity of experience not because it is crucial to our logic, but because we thought it would make the experimental manipulation more vivid. However, by an alternative interpretation, it is this simultaneity of the other's fresh response—not the fresh construal that it encourages—that is blunting habituation. We added a *future fresh perspective* condition in Study 2 to disentangle these possibilities. More specifically, these participants were told that they would predict the response of a future participant who would watch the video for the first time the next day. We expected that this future fresh perspective manipulation would sustain interest at the same rate as the (concurrently experienced) fresh perspective one.

## Method

**Participants and design.** We recruited a total of 1,064 participants simultaneously from MTurk as well as undergraduate subject pools at the University of California, Berkeley, and New York University. MTurk participants were paid a nominal amount for their participation, whereas the university students received course credit. Participants were randomly assigned to one of three conditions: (concurrent) fresh perspective, future fresh perspective, or (concurrent) stale perspective. There were 240 participants who failed to answer the attention check correctly, leaving a total of 824 participants for all analyses reported below.

**Procedure.** All participants began by watching an edited version of psychologist Amy Cuddy's TED Talk "Your Body Language Shapes Who You Are." Although the entire talk lasted approximately 21 min, we spliced together a 2 min, 51 s clip that features Cuddy discussing her own research on power poses. These edits removed some of Cuddy's more personal anecdotes; thereby, it focused on the research results that many find incredibly fascinating. After the first exposure, participants were given the prompt: "Consider your experience watching the video just now. While watching the video, I found it to be . . ." They responded to interesting, engaging, and fascinating on a slider scale anchored at 0 (not at all) and 100 (most I have ever felt). We averaged responses to form a Time 1 *interest* composite ( $\alpha = .93$ ).

Next, participants watched the clip for a second time and then again responded to the interest measures. This Time 2 interest composite also had high internal reliability ( $\alpha = .96$ ). Before exposing participants to the clip for the third time, participants in the two concurrent perspective conditions—both fresh and stale—received interventions much like we used before. However, participants in the new future fresh perspective condition were not led to believe they were being paired with another participant in the moment, but that they would have to predict the experience of a future participant who would watch the clip for the first time. These future fresh perspective participants experienced the same 15-s "loading screen" delay that we had used with those in previous control conditions; this controlled for the time it took for participants in the other two conditions to (supposedly) be paired with a live other. Following the third video exposure, participants completed the Time 3 interest composite ( $\alpha = .97$ ).

## Results and Discussion

Showing that repeated exposure produces not only emotional habituation but also a decline in engagement, participants' interest dropped from Time 1 ( $M = 70.13$ ,  $SD = 22.96$ ) to Time 2 ( $M = 54.88$ ,  $SD = 28.29$ ),  $F(1, 816) = 512.78$ ,  $p < .001$ ,  $\eta_p^2 = .39$ . To determine whether our perspective manipulation sustained interest in a fascinating stimulus, we submitted the interest composite to a 2(time: Time 2, Time 3)  $\times$  3(perspective: fresh, stale, future fresh) mixed-model ANOVA, with only the first factor measured within-subjects. A significant Time  $\times$  Perspective interaction showed that the manipulation did change participants' rate of further habituation,  $F(2, 819) = 22.72$ ,  $p < .001$ ,  $\eta_p^2 = .053$ . To understand the nature of this interaction, we conducted a series of 2(time)  $\times$  2(perspective) ANOVAs.

Mirroring a pattern we have observed before, participants who watched Cuddy's video for the third time knowing their concurrently paired participant was watching it for the first time showed

a smaller additional decline in interest ( $M_{T2-T3} = 10.82$ ,  $SD = 18.70$ ; paired  $t(311) = 10.22$ ,  $p < .001$ ,  $d = .58$ ) than those told their concurrently paired participant was watching it for the third time ( $M_{T2-T3} = 16.83$ ,  $SD = 16.67$ ; paired  $t(220) = 15.01$ ,  $p < .001$ ,  $d = 1.01$ ),  $F(1, 531) = 14.60$ ,  $p < .001$ ,  $\eta_p^2 = .03$ . To determine whether the simultaneity of experience contributes to our effects, we examined whether participants showed sustained interest in the clip even when considering the first viewing of a future participant. Showing participants were responding to the other participant's perspective, not the timing of when such a perspective would occur, future fresh perspective participants showed a relatively small decline in interest ( $M_{T2-T3} = 5.65$ ,  $SD = 20.62$ ; paired  $t(279) = 4.58$ ,  $p < .001$ ,  $d = .27$ ). This decline was smaller than those considering another's concurrent stale perspective,  $F(1, 499) = 42.90$ ,  $p < .001$ ,  $\eta_p^2 = .08$  (see Figure 2). Unexpectedly, this rate of additional habituation was even smaller (thus, displaying a larger vicarious construal effect) than those considering a concurrent fresh perspective,  $F(1, 590) = 10.26$ ,  $p = .001$ ,  $\eta_p^2 = .02$ . Although this final effect was not predicted (nor do we have a compelling post hoc explanation for it), it makes it clear that the fresh perspective stems from the nature, not the timing, of the other's experience.

### Study 3: Awe, Through One's Own (Previous) Eyes

In our previous studies, we asked participants to consider the experience of someone *else* who was experiencing a stimulus for the first time. However, if people can reexperience a stimulus by trying to consider how it would be seen through fresh eyes, is it truly necessary that these eyes be another person's, instead of a past version of their own? In our earlier studies, we made this methodological decision for three reasons. First, we wanted to use a manipulation that would encourage participants to try to adopt another perspective, but wanted to disguise our true interest in the study. That is, although we framed our interest as being in participants' ability to predict the experience of another, we were ultimately interested in a different DV: participants' own experience. Second, we worried that participants might have a hard time reassessing their own initial construal of a stimulus (Campbell et

al., 2014). We thought that attempting to see the stimulus through another's eyes might make it simpler to retrieve an already-lost construal. Third, we worried that if participants were told they would have to report on their own initial experience with the stimulus, they might not pose the sort of directional questions we believe to underlie our effect ("What did I find novel and particularly engaging?"), but instead know that they could merely report from memory the responses they had made minutes earlier.

In Study 3, we added a new *self's fresh* perspective condition, in which participants were told before watching a video for the third time that afterward they would need to report how they themselves responded to the video when watching it for the first time. We explained that this meant they should try to consider that initial perspective while watching the video. If participants had truly lost the ability to retrieve their own initial construal, or if our earlier effects stemmed from people's delight or dismay at someone else's novel exposure to a stimulus, then this intervention should not curb further habituation. However, if participants can try on new perspectives and experience a stimulus through (fresher) eyes, then even this *self's fresh* perspective attempt should curb habituation. Whether one's own fresh perspective curbs habituation to the same extent as our typical (other's) fresh perspective condition will depend on whether our concerns with using this approach dampen some of the vicarious construal effect—regardless of whether they are sufficient to account for it. Finally, we aimed to replicate our findings using a stimulus that elicited a new emotional response: awe.

### Method

**Participants and design.** There were 466 Americans from MTurk who completed the study for nominal payment. Participants were randomly assigned to one of three perspective conditions: *other's fresh* perspective, *self's fresh* perspective, or a no-intervention *control* condition. There were 133 participants who responded incorrectly to an attention check. This left 333 participants in our final sample on which all analyses were conducted.

**Procedure.** All participants began by watching an awe-inducing clip from the BBC's *Planet Earth* miniseries. This 2 min,

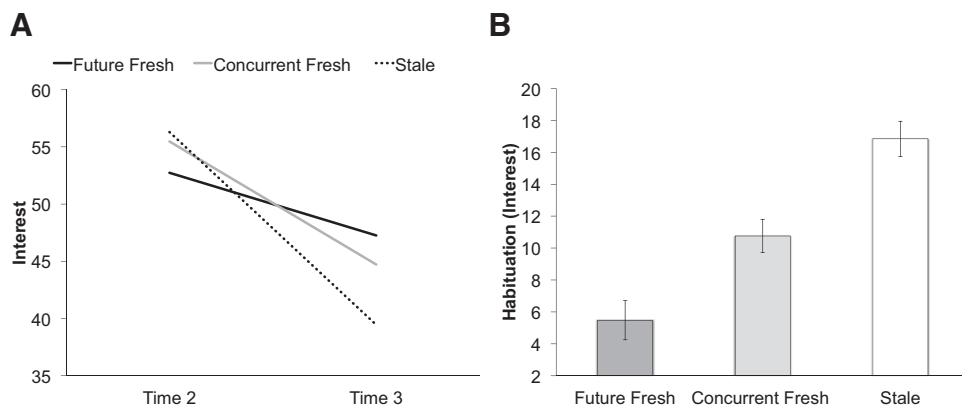


Figure 2. Study 2: (A) Self-reported interest in the video in the future fresh, concurrent fresh, and stale perspective conditions on the second and third viewings. (B) Mean indication of interest at Time 2 minus indication of interest at Time 3 in the future fresh, concurrent fresh, and stale perspective conditions. Error bars reflect  $\pm 1$  standard error from the mean.



36 s compilation of scenes from the miniseries has been used in previous research to elicit awe (Cricher & Lee, 2018; Piff, Dietze, Feinberg, Stancato, & Keltner, 2015; Valdesolo & Graham, 2014). The video features bird's-eye views of some of the world's most magnificent landscapes, accompanied by inspiring background music. After the first exposure to the video clip, participants were given the prompt: "Consider your experience watching the video just now. While watching the video, I found it to be . . ." They responded to awe-inspiring and inspirational. We averaged responses to form a Time 1 awe composite ( $r = .66$ ).

Participants then watched the clip again and rated their experience using the same awe measures. The Time 2 composite also had high reliability ( $r = .74$ ). Before watching the clip a third time, participants in the two fresh perspective conditions—both self's and other's—received special instructions. Those in the other's fresh perspective condition received instructions very much like those used in the previous study's future fresh perspective condition. Namely, participants were told that after their third time watching the video, they would have to judge how someone in the future watching the video for the first time would respond to the video. To encourage participants to adopt this perspective, we asked them to be mindful of this while watching the video. Those in the new self's fresh perspective condition were warned they would be asked to report on how they themselves had responded to the clip the first time it had been viewed. Those in the control condition did not receive special instructions before the third viewing. Immediately following the third video exposure (but before reporting on another's or their own former responses), participants completed the Time 3 awe composite ( $r = .84$ ).

## Results and Discussion

Showing that repeated exposure to the clip reduced participants' reported experience of awe, we observed a significant decline from Time 1 ( $M = 82.05$ ,  $SD = 18.58$ ) to Time 2 ( $M = 77.88$ ,  $SD = 21.27$ ),  $F(1, 332) = 41.88$ ,  $p < .001$ ,  $\eta_p^2 = .11$ . To assess whether the perspective manipulation had an effect on how much participants sustained their awe response on the third viewing, we submitted the awe composite to a 2(time: Time 2, Time 3)  $\times$  3(perspective: other's fresh, self's fresh, control) mixed-model ANOVA, with only the first factor measured within-subjects. A significant Time  $\times$  Perspective interaction showed that the ma-

nipulation did change participants' rate of further habituation,  $F(2, 330) = 8.75$ ,  $p < .001$ ,  $\eta_p^2 = .05$ . To unpack the interaction, we conducted a series of 2(time)  $\times$  2(perspective) ANOVAs (see Figure 3).

As we observed in previous studies, participants who watched the *Planet Earth* video for the third time knowing their paired participant was watching it for the first time showed a smaller additional decline in awe ( $M_{T2-T3} = 1.28$ ,  $SD = 17.75$ ; paired  $t(93) = .70$ ,  $p = .49$ ,  $d = .07$ ) compared with those in the control condition ( $M_{T2-T3} = 10.59$ ,  $SD = 19.68$ ; paired  $t(124) = 6.02$ ,  $p < .001$ ,  $d = .54$ ),  $F(1, 217) = 13.06$ ,  $p < .001$ ,  $\eta_p^2 = .06$ . We also found that participants considering their own fresh perspective showed a smaller decline in awe ( $M_{T2-T3} = 4.13$ ,  $SD = 12.98$ ;  $t(113) = 3.40$ ,  $p = .001$ ,  $d = .32$ ) than those in the control condition,  $F(1, 237) = 8.82$ ,  $p = .003$ ,  $\eta_p^2 = .04$ . The rate of habituation was similar in the self's and other's fresh perspective conditions,  $F(1, 206) = 1.78$ ,  $p = .18$ ,  $\eta_p^2 = .01$ , suggesting that the path to curbing habituation is not limited to or enhanced by considering another person's fresh perspective.

### Study 4: Anchoring and Adjusting From Estimated Others' Responses

We have argued that adopting a vicarious construal—even one had but seemingly lost—changes how one then experiences a stimulus. However, two alternative possibilities remain. First, perhaps people report responding to a stimulus more strongly simply because they were led to consider a novel response to the stimulus before reporting their own response. Second, and relatedly, by an anchoring and adjustment account, participants in the fresh perspective condition may have been focusing on another person's strong (initial) response to the stimulus, from which they then adjusted (insufficiently) when stating their own response. These alternatives speak to related alternative mechanisms—that the perspective manipulation does not modify subsequent construal and experience, but changes people's interpretation or reporting of their experience.

To address both concerns, we varied whether participants were asked to consider the novel experience of another before (and while) being exposed to the stimulus for a third time, or after this final exposure (but before rating their own final experience). If the previously documented vicarious construal effect is merely driven

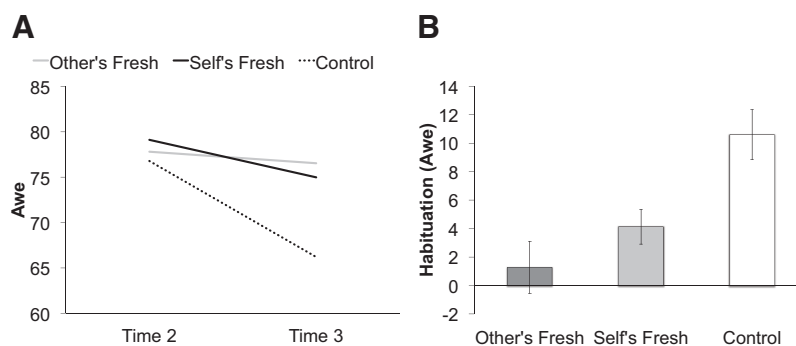


Figure 3. Study 3: (A) Self-reported awe in the other's fresh perspective, self's fresh perspective, and control conditions on the second and third viewings. (B) Mean feeling of awe at Time 2 minus the feeling of awe at Time 3 in the other's fresh perspective, self's fresh perspective, and control conditions. Error bars reflect  $\pm 1$  standard error from the mean.

by anchoring and adjustment, or a distortion to one's own representation of one's recent experience that comes from considering a novel response to the stimulus, then both manipulations should curb habituation to a similar extent. If instead considering a live experience through different eyes is what is responsible for changing one's own online reaction, then we should observe more of a stunting in habituation in our classic fresh perspective condition than in our new post hoc (fresh) perspective condition.

## Method

**Participants and design.** We recruited 413 Americans from MTurk in exchange for nominal payment. Participants were randomly assigned to our typical *preexposure* fresh perspective or the new post hoc fresh perspective condition. There were 103 participants who were unable to pass the attention check and were excluded from all analyses. The final sample included 310 participants.

**Procedure.** All participants began by watching the same awe-inducing clip from the BBC's *Planet Earth* miniseries used in Study 3. After watching the clip, they indicated how awe-inspiring and inspirational they found the clip to be on a 0 (*not at all*) to 100 (*extremely so*) sliding bar scale. We created a Time 1 awe composite by averaging responses ( $r = .76$ ).

Participants then watched the clip for a second time and rated their experience on the same awe measures. The Time 2 composite also had high reliability ( $r = .80$ ). Before watching the clip for a third time, participants in the fresh perspective condition were informed they would have to make an estimate after they watched the clip: how the next participant would respond when viewing the clip for the first time. Those in the post hoc fresh perspective condition received no instructions before the third viewing. Immediately after their third viewing of the clip, all participants first reported how they thought the next participant would report their experience when watching the video for the first time. They made these estimates using the same two awe measures ( $r = .76$ ). Finally, with all participants having just been anchored on their estimate of another's fresh response, they then reported their own experience watching the clip for the third time on the same two-item awe measure ( $r = .86$ ).

## Results and Discussion

Demonstrating that habituation occurred upon repeated exposure, the intensity of participants' awe responses declined from Time 1 ( $M = 81.07$ ,  $SD = 20.77$ ) to Time 2 ( $M = 76.84$ ,  $SD = 23.64$ ),  $F(1, 309) = 34.67$ ,  $p < .001$ ,  $\eta_p^2 = .10$ . However, did our preexposure fresh perspective manipulation (i.e., our intervention used across most of our studies) curb habituation compared with the new manipulation that anchored participants on a fresh perspective postexperience? We submitted the awe composite to a 2(time: Time 2, Time 3)  $\times$  2(fresh perspective: preexposure, post hoc) mixed-model ANOVA, with only the first factor measured within-subjects. A significant Time  $\times$  Fresh Perspective interaction confirmed our hypotheses,  $F(1, 308) = 5.94$ ,  $p = .015$ ,  $\eta_p^2 = .02$  (see Figure 4).

Participants who underwent our new post hoc fresh perspective manipulation showed a significant decline in awe from Time 2 to Time 3 ( $M_{T2-T3} = 9.12$ ,  $SD = 17.19$ ; paired  $t(141) = 6.32$ ,  $p < .001$ ,  $d = .53$ ). However, participants in our typical preexposure fresh perspective condition did show reduced, though still significant, habituation ( $M_{T2-T3} = 4.65$ ,  $SD = 15.07$ ; paired  $t(167) = 4.00$ ,  $p < .001$ ,  $d = .30$ ). That is, even though participants in the post hoc condition had been anchored on the perceived experience of the other participant just before stating their own Time 3 experience, they showed a stronger diminishment in their self-reported awe response. In other words, the vicarious construal effect seems to stem from the influence of the manipulation on people's subsequent interpretation and, thus, experience of the stimulus, not merely on their being prompted to consider a novel response as they reflect back on their experience.

### Study 5: Preemptive Habituation

In the previous studies, participants' self-reported experience suggested they were able to partially recapture a construal they once had, but had since lost. That perspectives can be rejuvenated may seem especially impressive, because it suggests they can be rediscovered (at least in part). On the other hand, this might suggest a limit to the vicarious construal effect: Might people be able to volitionally adopt a construal only because they themselves had once possessed it? Or instead, does considering another's experience one has not yet had permit one to experience it in a way

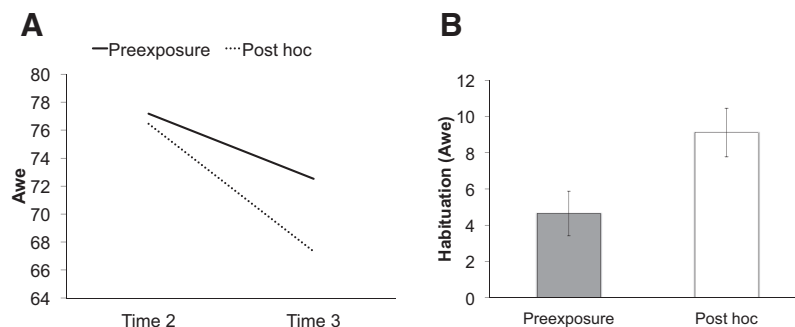


Figure 4. Study 4: (A) Self-reported awe in the preexposure fresh and post hoc fresh perspective conditions on the second and third viewings. (B) Mean feeling of awe at Time 2 minus the feeling of awe at Time 3 in the preexposure fresh perspective and post hoc fresh perspective conditions. Error bars reflect  $\pm 1$  standard error from the mean.

that one never has before? As long as people have a theory of how another's perspective would be different (a proposition that later studies test directly), our reasoning suggests that the vicarious construal effect should emerge even in these contexts.

In Study 5, participants were encouraged to consider the perspective of someone experiencing a stimulus for the first time or the third time. Although some participants received these instructions before being exposed to a stimulus for the third time, others received this manipulation before their first exposure. If vicarious construal can help participants only to recover a previous construal, we should observe an interaction: The perspective manipulation should have an effect when it precedes the third viewing (given the fresh perspective manipulation encourages participants to access a previously experienced construal) but not when it precedes the first viewing (given the stale perspective manipulation suggests a construal that has yet to be experienced). If instead people can volitionally see stimuli through novel eyes and not merely their former ones, we should observe two main effects: one of time (reflecting habituation) and one of perspective (with the fresh perspective encouraging a stronger response than the stale perspective manipulation). Furthermore, we expand on our previous studies by testing our hypotheses in the context of a new experiential dimension: humor.

## Method

**Participants and design.** There were 624 Americans from MTurk who participated for a nominal amount. Participants were randomly assigned to one of four conditions in a 2 (perspective: fresh, stale)  $\times$  2 (intervention: first exposure, third exposure) full-factorial design. Because of a programming error, half of participants did not see the attention check item. For this reason, no participants were excluded except one participant who indicated the video did not play on his or her computer.

**Procedure.** To begin, participants were asked to clear their minds. Participants were given 30 s to do this. We hoped this would put participants in a similar, calm state. For the focal part of the experiment, participants were exposed to a 2 min, 21 s clip from the improv comedy show *Whose Line Is it Anyway?* Those in the first exposure intervention condition saw the clip only once; the fresh or stale perspective manipulation was delivered in advance of this single exposure. That is, they knew they would have to predict how another participant would respond to the clip upon their first (fresh) or third (stale) exposure. Those in the third exposure intervention condition saw the clip three times. As before, one of the two perspective manipulations was delivered in advance of the third and final exposure. Following each exposure to the video, participants indicated how *amusing*, *entertaining*, and *humorous* they found the clip to be. Responses were made on a 101-point slider scale anchored at 0 (*not at all*) and 100 (*extremely so*). This humor composite was reliable at Time 1 ( $\alpha = .95$ ), Time 2 ( $\alpha = .97$ ), and Time 3 ( $\alpha = .99$ ).

To test our main hypotheses, we would not be able to test whether the rate of habituation changed (given participants in the first exposure condition would not have already seen the stimulus). As such, all participants—after clearing their mind but before watching the focal clip—saw a different baseline clip from *Whose Line Is it Anyway?* After watching this 2 min, 30 s clip, participants completed the same humor composite used for the focal

target clip ( $\alpha = .95$ ). Given these ratings offered one baseline indication of how funny participants found the show to be, we used these scores as a covariate in our main analyses.

## Results and Discussion

To understand whether the fresh and stale perspective manipulations influenced differently those watching the humorous clip for the first or third times, we performed a 2(perspective: fresh, stale)  $\times$  2(intervention: first exposure, third exposure) ANCOVA on the relevant humor composites. For those who received the intervention before the first or third exposure, the relevant composite was Time 1 or Time 3, respectively. And indeed, humor ratings of the baseline clip predicted significant variance in the subsequent humor response,  $F(1, 617) = 307.24, p < .001, \eta_p^2 = .33$ .

Demonstrating between-subjects that participants habituate with additional exposure, those in the Time 3 intervention condition ( $M = 56.13, SE = 1.35$ ), found the clip less humorous than those in the Time 1 condition ( $M = 67.09, SE = 1.32$ ),  $F(1, 617) = 33.61, p < .001, \eta_p^2 = .05$ . But also, as shown in Figure 5, we found a main effect of perspective demonstrating that fresh perspective participants—those paired with another who was watching the clip for the first time—reported a stronger humor response toward the stimulus ( $M = 64.09, SE = 1.33$ ) than those paired with someone who had already been exposed to the clip multiple times ( $M = 59.13, SE = 1.35$ ),  $F(1, 617) = 6.88, p = .009, \eta_p^2 = .01$ . Demonstrating that the vicarious construal effect reflects the influence of trying to understand another's novel construal, even if one has not previously experienced that construal, the Perspective  $\times$  Intervention interaction was not significant,  $F < 1$ .<sup>1</sup> That is, the perspective manipulation had a similarly sized effect regardless of whether it was administered before the third exposure (when the fresh perspective was a return to the past) or the first exposure (when the stale perspective was yet to be genuinely experienced).

### Study 6: Does the VCE Change One's Construal of the Stimulus?

In testing the VCE, we have relied on participants' self-reported experience. Is it possible that participants in our fresh perspective conditions reported having a more intense response despite not actually experiencing one? Even if the fresh perspective manipulation does help people to become resensitized to a stimulus, do participants (accurately) attribute this resensitization to the manipulation? By our reasoning, the vicarious construal effect produces

<sup>1</sup> Although we did not observe a significant interaction, we also analyzed only those who received the fresh or stale perspective manipulation at Time 1, given that was the new edition to this study. And indeed, Time 1 participants considering the perspective of someone watching the first time found the clip to be more humorous ( $M = 69.89, SE = 1.61$ ) than those taking the perspective of someone watching the clip for the third time ( $M = 63.69, SE = 1.57$ ),  $F(1, 317) = 7.65, p < .001, \eta_p^2 = .02$ . In other words, preemptive habituation emerged significantly on its own. Though curiously, Time 3 participants who considered a fresh perspective ( $M = 58.28, SE = 2.12$ ) did not find the clip to be more humorous than those who considered a stale perspective ( $M = 54.60, SE = 2.23$ ),  $F(1, 303) = 1.43, p = .232, \eta_p^2 = .005$ . Given the number of times we have observed (and in later studies will observe) the influence of the fresh perspective manipulation after multiple stimulus exposures, we see this effect as anomalous and potentially the result of reduced power that comes from subdividing the sample.

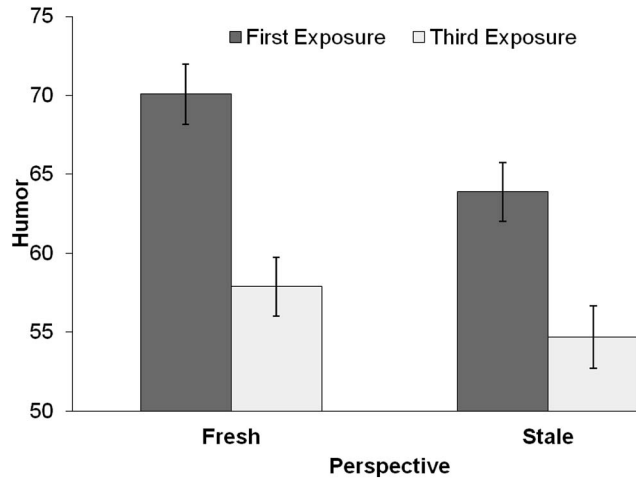


Figure 5. Study 5: Model estimated means (controlling for humor ratings of the initial clip) on the humor composite by fresh and stale perspective conditions for participants who received this intervention on their first or third exposure to the stimulus. Error bars reflect  $\pm 1$  standard error from the mean.

real changes in people's experience, but people's blindness to the power of construal in defining experience leads people to interpret such changes as veridical—properties of the stimulus or one's own preferences—not the interference of the manipulation itself.

As a first test of this possibility, we partially replicated the design of [online Supplemental Materials Study B](#) ( $N = 972$ ; [online Supplemental Materials Study C](#))—showing participants a sad stimulus three times. Before the third viewing, only some participants received the fresh perspective manipulation. Following the third exposure, participants did not report their own experience, but instead estimated how sad another participant—one who merely saw the video three times with no other instructions or intervention—would find the clip on the third viewing. In other words, we asked participants to report their construal of the stimulus itself, their belief about its properties absent the influence or interference of the perspective manipulation. Suggesting the manipulation changed participants' construal of the stimulus itself, fresh perspective participants thought an untreated other would find the stimulus to be sadder. This provides preliminary evidence that the fresh perspective manipulation leads people to construe the stimulus as sadder.

Study 6 goes further in its effort to directly connect changes in participants' self-reported experience to changes in their construal. Participants watched a series of short standup comedy clips from which all audience laughter—when it occurred—had been edited out. Participants reported not only their own experience, but also made inferences about which comedians were sufficiently funny that they induced audience laughter. If fresh perspective participants truly construe the comedians as more humorous, attributing their own amusement to the stimuli instead of the manipulation, then such participants should infer the audience will have been more amused as well. Furthermore, to connect this measure of construal to changes in participants' self-reported experience; we test whether the new construal measure relates to how much the manipulation slowed habituation. If instead fresh perspective participants did not feel more amused, or they recognized they felt

amused only because the fresh perspective manipulation had distorted their experience, then they should not estimate that the untreated audience laughed more.

## Method

**Participants and design.** There were 1,012 Americans who were recruited from MTurk and paid a nominal amount for their participation. They were randomly assigned to a fresh or stale perspective condition. Of these, 148 participants failed to answer the attention check correctly. This left 864 participants in all analyses reported below.

**Procedure.** All participants were told that they would watch a series of short standup comedy clips from which all audience laughter—when it occurred—had been edited out. The video (2 min, 40 s) comprised snippets of standup performances from 15 amateur comics (each identified onscreen by name) who were performing in front of a live audience. Following the first viewing, participants reported whether they found the video to be *amusing*, *entertaining*, and *humorous*. Responses were made on a 101-point slider scale anchored at 0 (*not at all*) and 100 (*extremely so*). We averaged responses to form a Time 1 *amusement* composite ( $\alpha = .96$ ).

Before watching the clip a second time, participants received one of our two perspective manipulations: fresh or stale. Those in the fresh perspective condition received instructions very much like those used in the future fresh condition in Study 2—that they would have to predict how a future participant would find the video when watching it for the first time. Those in the stale perspective condition would instead predict the person's response on a third viewing. Note that because this was participants' own second viewing, both manipulations required participants to consider a different experience than the one they were having at present.

Following the second exposure to the video, participants completed two measures in a counterbalanced order. One measure was the Time 2 *amusement* composite ( $\alpha = .98$ ), which paralleled the measure participants completed after the first exposure. The second was the *inferred laughter* measure, which allowed us to assess participants' construal of the performances' hilarity. Reminded that any audience laughter—when it occurred—had been edited out of the video, participants saw the names and faces of the 15 comics in the order they had appeared. For each, participants responded to, “Do you think the joke was funny enough that the audience produced audible laughter?” They indicated *yes* or *no*. They also could leave an item blank, but this occurred rarely (on 0.71% of trials; 92 of 12,960). We calculated on what percentage of trials to which participants responded they inferred laughter was present.

## Results and Discussion

We began by testing whether the perspective manipulations influenced how much habituation participants experienced. We submitted participants' reported amusement to a 2(time: Time 1, Time 2)  $\times$  2(perspective: fresh, stale) mixed-model ANOVA, with only the first factor measured within-subjects. The predicted Perspective  $\times$  Time interaction was significant,  $F(1, 862) = 24.25$ ,  $p < .001$ ,  $\eta_p^2 = .03$ . As depicted in [Figure 6](#), although participants



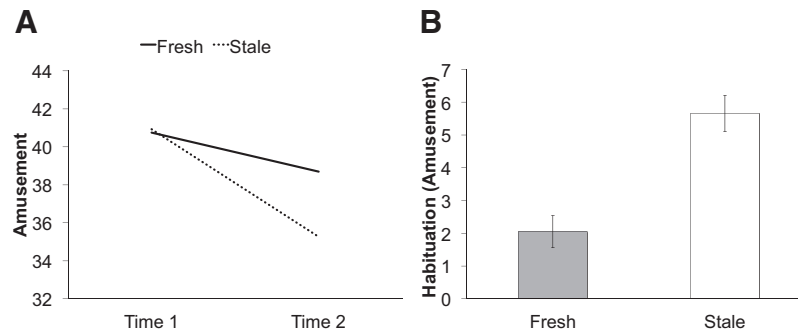


Figure 6. Study 6: (A) Self-reported amusement in the fresh and stale perspective conditions on the first and second viewings. (B) Mean feeling of amusement at Time 1 minus the feeling of amusement at Time 2 in the fresh and stale perspective conditions. Error bars reflect  $\pm 1$  standard error from the mean.

in the stale condition showed a clear decline in amusement ( $M_{T1-T2} = 5.66$ ,  $SD = 11.37$ ; paired  $t(431) = 10.35$ ,  $p < .001$ ,  $d = .50$ ), those in the fresh perspective condition showed an attenuated decline ( $M_{T1-T2} = 2.05$ ,  $SD = 10.15$ ; paired  $t(431) = 4.20$ ,  $p < .001$ ,  $d = .20$ ).

Stated differently, the fresh perspective intervention slowed the rate of habituation by 64%. Next, we examined which participants construed the comics as funnier—that is, as more likely to have prompted laughter. Of course, participants who (before any manipulation) found the comics to be more amusing would also presumably project that onto the audience. For this reason, we controlled for Time 1 amusement when comparing the inferred laughter composite between conditions. Controlling for such pre-existing preferences, participants in the fresh perspective condition did indeed think that more comics evoked laughter ( $M = 46.71\%$ ,  $SE = 1.04\%$ ) than did those in the stale perspective condition ( $M = 43.32\%$ ,  $SE = 1.03\%$ ),  $F(1, 859) = 5.35$ ,  $p = .021$ ,  $\eta_p^2 = .006$ .

By our reasoning, the effects of stimulus construal and experience should be related. That is, the more that participants' habituation slowed, the more they should have construed the comics as laughter-evoking. As a straightforward test, we computed a partial correlation (controlling for Time 1 amusement) between the degree of habituation (Time 1 amusement—Time 2 amusement) and the inferred laughter measure. It was negative and significant,  $pr(859) = -.20$ ,  $p < .001$ . For our purposes, we are not committed to a single temporal sequencing of these variables. Instead, we cite these dual correlated effects (habituation and inferred laughter) to support our contention that the VCE reflects a shift not only in one's own experience but an associated perspective on the stimulus.

Finally, Study 6 shows that it is focusing on another's fresh perspective—not merely new content—that slows habituation. That is, fresh and stale perspective participants considered the perspective of someone who was watching the clip for the first and third times, respectively, never for the second time like the self. This was of course already foreshadowed by Study 5, in which first-time viewers showed signs of preemptive habituation by considering a stale perspective. Furthermore, this is consistent with Galak et al. (2009), who found that calling to mind a variety of songs one had recently listened to (but not calling to mind a variety of TV shows one had recently watched) reduced habituation to a

song. In other words, it is not merely variation in or novelty of one's own thoughts at the time of encoding a stimulus that slows habituation.

### Study 7: Shifting Construal and Preferences

To this point, we have focused on the vicarious construal effect in the context of habituation. Study 7 moves into qualitatively new territory by asking whether participants would be influenced by considering another's perspective that they would never have had spontaneously. All participants watched a clip of Jerry Seinfeld performing standup on *The Late Show with Stephen Colbert*. However, participants were led to believe they had been paired with a participant who had a particular affinity for or aversion to the comedian. In this way, we ask whether someone who tries to see what a Seinfeld aficionado might love, or what a nonfan might detest, may emerge from the experience with their own enjoyment colored.

Study 6 (as well as online Supplemental Materials Study C) offered the first evidence that the fresh perspective manipulation shifted participants' take on the stimulus itself: how much laughter (Study 6) or sadness (online Supplemental Materials Study C) they assumed it would typically evoke. Study 7 extended on these findings in two ways. We moved beyond measuring inferences about others' experiences to see whether the perspective manipulations: (a) changed one's perceptual judgments that would reflect a shift in stimulus construal and (b) affected one's interest in continuing with similar experiences. In combination, this tests whether participants experience the vicarious construal effect as veridical—a reflection of the reality of the stimulus and their own preferences—instead of as an experiential boost that merely reflects the understood distortion of the manipulation itself. Next, we detail our empirical approach for these two goals.

First, instead of merely examining whether the perspective manipulations affected inferences about others' experiences, we examined whether it affected participants' own perceptual judgments. Unlike in Study 6, participants in Study 7 could hear how the audience was responding to the comedian. We asked participants to judge the extensiveness and intensity of that audience response, with the idea that participants who construed the clip as funnier may have actually interpreted the audience response as more raucous. To be clear, we are not challenging the modularity

of audition (Fodor, 1984). Instead, construing the clip as more or less enjoyable should color how one characterizes the crowd's response, much as Princeton and Dartmouth football fans seemed to see two different games even as the modularity of their visual systems need not have been compromised (Hastorf & Cantril, 1954). Second, we asked participants about their own interest in hearing more of the comedian's work. If participants considering the perspective of a fan (vs. nonfan) thought their experiences in part reflected their own undistorted construal (instead of the distorting influence of the manipulation), then they should show more or less interest, respectively, in hearing more from the comedian.

## Method

**Participants and design.** There were 789 Americans who were recruited from MTurk and paid a nominal amount for their participation. Participants were randomly assigned to one of two perspective conditions: *fan* or *nonfan*. Of these, 261 participants failed an attention check. This left 528 participants for all analyses reported below.

**Procedure.** To begin, participants saw the names of 10 well-known comedians. Participants were asked to indicate how much "you like (or think you would like) watching a standup performance by each of the following standup comics." Participants responded on 10-point scales anchored at 1 (*do not like at all*) and 10 (*like very much*). Crucial on this list was "Jerry Seinfeld." This item was included: (a) to support our cover story (given participants would receive this response from a supposedly yoked participant), and (b) because (like the baseline preference measure in Study 5) it should capture preexisting differences in liking for the target.

All participants were told that they would watch a video clip of a Jerry Seinfeld standup performance on *The Late Show with Stephen Colbert*. Before the film began, participants learned they would have to estimate the experience of a participant who had not seen this clip before. Participants were given one piece of information about this person, their response on the Jerry Seinfeld scale item that participants themselves had answered before. Those in the fan perspective condition saw that the other participant indicated a 9, whereas those in the nonfan perspective condition saw that the other participant indicated a 2. At that point, participants watched the 7 min, 1 s clip.

Next, participants characterized their own experience of watching the clip on three items: "I enjoyed it," "I liked it," and "I found it entertaining." Responses were made on 101-point slider scales anchored at 0 (*not at all*) and 100 (*extremely so*). These items were averaged to form an *enjoyment* composite ( $\alpha = .99$ ). To be consistent with the cover story, participants also estimated the future participant's experience by responding to the same three prompts, but concerning the yoked participant's enjoyment ( $\alpha = .99$ ).

At that point, we measured participants' broader *preference for Seinfeld* with two items. One item stated, "How much do you think you would enjoy the rest of the 90-minute stand up special by Jerry Seinfeld?" A second item read, "How much would you be interested in going to a live standup show by Jerry Seinfeld in the future?" Responses were made on 10-point scales anchored at 1 (*not at all*) and 10 (*very much*). The two items were correlated

( $r = .85$ ) and, thus, averaged to establish a broader *preference for Seinfeld* composite.

Finally, we asked participants to characterize the audience's response to Seinfeld's performance. These items all assessed the perceived extensiveness and intensity of the audience laughter: "What percentage of the time were audience members laughing during the 7-minute performance of Jerry Seinfeld that they just watched?", "What percentage of the audience do you estimate were laughing?", and "How loud or hearty was the laughter?" Participants responded to each question on a 0 to 100 scale. The first two items merited percentage responses. The final item's response scale included labels at 0 (*not loud or hearty at all*) and 100 (*extremely loud or hearty*). We averaged participants' responses to create a *judged laughter* composite ( $\alpha = .77$ ).

## Results and Discussion

We began by testing whether the perspective manipulation changed participants' self-reported enjoyment of the clip. We submitted the enjoyment composite to a one-way ANCOVA with baseline liking for Seinfeld as a covariate. Unsurprisingly, the covariate accounted for significant variance in participants' self-reported liking for the clip,  $F(1, 525) = 229.30, p < .001, \eta_p^2 = .31$ . However, consistent with our central hypothesis, we observed a significant main effect of the perspective manipulation,  $F(1, 525) = 44.52, p < .001, \eta_p^2 = .08$ . Participants preparing to predict the experience of a Seinfeld fan found the clip to be more enjoyable themselves ( $M = 66.82, SE = 1.57$ ) compared with those considering the perspective of a nonfan ( $M = 51.65, SE = 1.64$ ). This shows the vicarious construal effect does not merely allow participants to have experiences that they previously or would soon (with repeated exposures) have. Instead, it allows people to experience stimuli through a lens they would not ordinarily possess.

But did experiencing the stimulus through another's eyes lead to a more or less enjoyable experience that was attributed to the manipulation, or to one's own true or stable resonance with such stimuli? To answer this additional question, we submitted the preference for Seinfeld composite to the same one-way analysis of covariance (ANCOVA). The same perspective main effect emerged,  $F(1, 525) = 39.36, p < .001, \eta_p^2 = .07$ . That is, participants yoked to a Seinfeld fan expressed more interest in watching Seinfeld perform in the future ( $M = 6.33, SE = 0.14$ ) than did those yoked to a nonfan ( $M = 5.03, SE = 0.15$ ).

We explain this pattern by arguing that the perspective manipulation changed participants' own experience, which they attributed to their own stable preferences, not to the construal-warping power of the manipulation. But by an alternative explanation, the two effects we saw may be unrelated. That is, perhaps participants *did* attribute their enjoyment of the clip (or lack thereof) merely to the manipulation, but those who failed to see what the yoked participant saw were curious to learn more. By this alternative reasoning, perhaps it was those who *disliked* the clip, but who were yoked to a Seinfeld fan, who responded by thinking, "This seems terrible, but maybe if I watched the whole thing, I'd see what this whole Seinfeld craze is about!" Although we did not think this possibility likely, it was at least made possible by our inclusion of baseline liking for Seinfeld as a covariate (given individual differ-

ences in preference for Seinfeld are already partialled out in our main analyses).

To address this alternative, we tested whether participants' own enjoyment of the clip positively mediated the influence of our intervention on participants' preference for more Seinfeld content. When we added enjoyment of the clip to the last model, we found that enjoyment was a significant positive predictor of preference for Seinfeld,  $F(1, 524) = 949.72, p < .001, \eta_p^2 = .64$ . The effect of the perspective manipulation was no longer significant,  $F(1, 524) = 2.18, p = .141, \eta_p^2 = .004$ . We tested for the significance of the indirect effect (Hayes, 2013). With the baseline preference for Seinfeld controlled, we observed a significant indirect effect of preference for Seinfeld through enjoyment of the clip, 95% confidence interval (CI) [0.7568, 1.4585].

Finally, we tested whether the perspective manipulation changed participants' subjective characterization of the audience response. We submitted the perceived laughter composite to an ANCOVA, again controlling for baseline interest. An effect of perspective emerged,  $F(1, 525) = 29.82, p < .001, \eta_p^2 = .05$ . That is, participants considering the perspective of a fan judged the audience to be responding more strongly to the performance ( $M = 68.75, SE = 1.01$ ) than did those yoked to a nonfan ( $M = 60.80, SE = 1.05$ ). Finally, bolstering our interpretation that the vicarious construal effect as reflected in participants' self-reported experience is connected to this fundamental shift in stimulus construal, the enjoyment composite correlated with the perceived laughter composite,  $r = .55, p < .001$  (again controlling for baseline preferences).

### Study 8a: Perspective Prompts a Directional Hypothesis Test

Having demonstrated that considering another's perspective not only changes one's subjective experience but one's characterization of a stimulus, we proceeded in our final two pairs of studies to more directly document the mechanism by which the VCE emerges. We have argued that in trying to understand another's perspective, one is essentially looking for evidence that is consistent with a directional hypothesis. When yoked to someone who has an appreciation for or an aversion to Seinfeld, people may be asking themselves what that person would *like* versus what they would *dislike* about the clip, respectively. And because people tend to engage in confirmatory hypothesis searches, participants' stimulus construal is likely to be biased in the direction of the hypothesis (Critcher & Dunning, 2009; Darley & Gross, 1983; Lee et al., 2006).

Studies 8a and 8b test this process using a causal chain design (Spencer et al., 2005). In Study 8a, we exposed participants to the perspective manipulations used in Study 7. Instead of having them report on their subjective experience of the stimulus, we asked them what questions occurred to them while watching it. We expected that participants were essentially testing directionally opposing hypotheses—asking themselves what a fan would like about the video and what a nonfan would dislike about the video. Study 8b then replaced our perspective manipulations with instructions to consider these different questions—what someone might like or dislike about the video—before asking how much participants themselves liked it.

One purpose behind using this causal chain—instead of a mediational design testing whether self-reported reliance on directional questions mediates a change in self-reported experience—was to be mindful of limits of both mediational designs and introspective reports. Evidence consistent with mediation would still be fundamentally correlational, raising a concern that such directional questions do not produce the VCE but instead merely correlate with participants' own experience. Granted, the first half of our causal chain does have the limitation of relying on participants' self-reporting their own approach to the stimulus, a feature that is difficult to entirely avoid. However, the experimental manipulation used in the second half does have the potential to establish that the directional questions—which lack other elements of the perspective manipulation (e.g., knowledge of the other person's preferences, an expectation that one will have to guess the other person's reported enjoyment of the clip)—can *cause* shifts that mirror those observed with our originally observed VCE.

### Method

**Participants and design.** There were 610 Americans who were recruited from MTurk for nominal payment. Participants were randomly assigned to one of two perspective conditions: fan or nonfan. Of these, 170 participants failed to answer the attention check correctly. This left a final sample size of 440 in all analyses reported below.

**Procedure.** The procedure was similar to that of Study 7. Participants completed baseline measures of liking for different comedians. They then received the fan or nonfan perspective manipulation before watching the Jerry Seinfeld clip. However, instead of reporting on their own experience of the clip, participants reported on their approach to it. More specifically, participants were told, "While watching the clip, one might have been thinking 'What would the future participant like about it?' or instead, 'What would the future participant dislike about it?'" Participants indicated the extent to which they were asking themselves each question on 7-point Likert-type scales anchored at 1 (*not at all*) and 7 (*completely*).

### Results and Discussion

To determine whether participants reported testing different hypotheses while considering how a fan versus a nonfan would respond, we submitted the two hypothesis measures to a 2 (perspective: fan, nonfan)  $\times$  2 (search questions: like, dislike) mixed model. Only the second factor was measured within-subjects. We entered participants' baseline liking for Seinfeld as a covariate. As expected, the Perspective  $\times$  Search Questions interaction was significant,  $F(1, 437) = 139.32, p < .001, \eta_p^2 = .24$ . The pattern is depicted in Figure 7A.

Participants considering the perspective of a fan were more likely to ask themselves what that future participant would like about the clip ( $M = 6.04, SD = 1.84$ ) than those considering the perspective of a nonfan ( $M = 3.97, SD = 2.16$ ),  $t(407.78) = 10.78, p < .001, d = 1.03$ . In contrast, those considering the perspective of a nonfan were more likely to ask themselves what that future participant would dislike ( $M = 4.96, SD = 2.18$ ) than those considering the perspective of a fan ( $M = 3.12,$

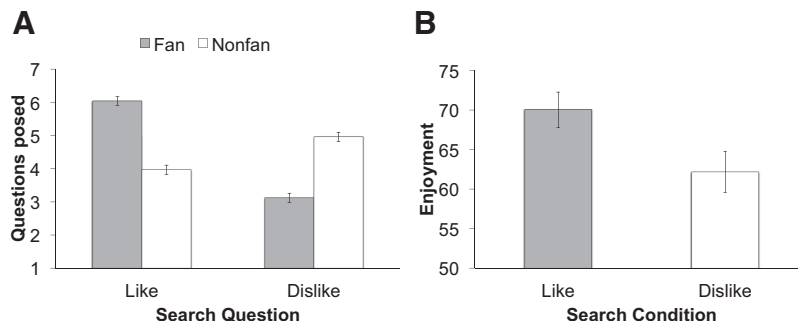


Figure 7. (A) Study 8a: The extent to which participants reported considering what a future participant would either like or dislike about the clip (B) Study 8b: Self-reported enjoyment in the like and dislike search task conditions. Error bars reflect  $\pm 1$  standard error from the mean.

$SD = 2.09$ ),  $t(438) = -9.02$ ,  $p < .001$ ,  $d = .86$ . Decomposed differently, fan perspective participants asked themselves more what the future participant would like instead of dislike, paired  $t(232) = 14.21$ ,  $p < .001$ ,  $d = .93$ . Instead, nonfan perspective participants reported focusing more on what the future participant would dislike instead of like, paired  $t(206) = -3.77$ ,  $p < .001$ ,  $d = .26$ . With evidence that the perspective manipulation encourages people to approach the stimulus by asking themselves different questions, we proceeded to try to complete the causal chain.

### Study 8b: Does Directional Hypothesis Testing Reproduce the VCE?

Study 8b completes the experimental causal chain by testing whether directly prompting participants to approach the stimulus by considering one question or the other (i.e., the dependent variables in Study 8a) would change their experience. We did not yoke participants to a Seinfeld fan or nonfan; we told participants only that they would be answering a question about the experience of a future participant. Participants knew that following the video they would need to answer one of the questions that participants in Study 8a reported asking themselves—“What would the future participant like about it?” or “. . . dislike about it?” We expected those who considered what someone would *like* about the video would end up reporting enjoying the video more than those considering what someone would *dislike* about it.

### Method

**Participants and design.** There were 300 Americans who were recruited from MTurk for nominal payment. Participants were randomly assigned to one of two *search* conditions: like or dislike. Thirty participants failed to answer an attention check correctly. This left a final sample size of 270 in all analyses reported below.

**Procedure.** This design was similar to that of Study 8a. After providing baseline preferences for 10 comedians (including “Jerry Seinfeld”), participants learned they would watch a Seinfeld standup performance. Instead of learning they would subsequently predict how much a fan or a nonfan liked the comic, they were told they would have to guess what a randomly selected future participant will “like about the video” (like condition) or “dislike about

the video” (dislike condition). At that point, participants watched the 7 min, 1 s clip. They then completed the three-item enjoyment composite also used in Study 7 ( $\alpha = .99$ ). To be consistent with the cover story, participants then did write down what they thought the future participants would like or dislike about the Seinfeld clip, depending on their condition.

### Results and Discussion

To examine whether considering what someone would like or dislike about the video changed one’s own reported enjoyment of the clip, we submitted participants’ own enjoyment composites to a one-way ANCOVA with participants’ baseline liking for Seinfeld as a covariate. As predicted, the baseline preferences accounted for significant variance in participants’ self-reported enjoyment of the clip,  $F(1, 267) = 86.12$ ,  $p < .001$ ,  $\eta_p^2 = .24$ . However, consistent with our central hypothesis, we observed a significant main effect of the search manipulation,  $F(1, 267) = 5.21$ ,  $p = .023$ ,  $\eta_p^2 = .02$  (Figure 7B). Participants preparing to answer what a future participant would like about the clip found it to be more enjoyable themselves ( $M = 70.03$ ,  $SE = 2.25$ ) compared with those preparing to explain what someone would dislike about the clip ( $M = 62.17$ ,  $SE = 2.57$ ). In combination, Studies 8a and 8b test the complete causal chain: In preparing to predict how a fan (nonfan) would respond to a stimulus, people focused on what one would like (dislike) about the experience, which colored their own experience of it.

### Study 9a: Extending Directional Hypothesis Testing to the Habituation Paradigm

Studies 8a and 8b illustrated that in considering another’s perspective, people are essentially testing different hypotheses in a confirmatory manner—posing to themselves directionally different questions that then shape their own experience. The final two studies consider how the same mechanism applies to the habituation context used for most of our studies. Merely knowing someone is experiencing a stimulus for the first time does not tell one that they have a predisposition to like or dislike it, but it still gives insight into how they will see it. It gives the perspective taker cues of what to look for.

We hypothesize that people who are trying to understand what a nonhabituated (fresh perspective) person would see ask them-



selves what that person would find unexpected, engrossing, and original about this novel stimulus. In contrast, those who are trying to understand what a habituated (stale) perspective would see should instead ask themselves what would become stale, predictable, or boring about the experience. Study 9a tests whether participants ask themselves these different questions when considering a fresh or stale perspective. Study 9b examines whether only asking participants to approach a stimulus while posing to themselves questions of one variety or the other exerts an assimilative pull on participants' own experience.

## Method

**Participants and design.** There were 229 Americans who were recruited from MTurk for nominal payment. Participants were randomly assigned to a *fresh* or *stale* perspective condition. Forty-five participants failed to answer an attention check correctly. This left a final sample size of 184 in all analyses reported below.

**Procedure.** Participants viewed "Googly Eyes," a 1 min, 29 s sketch from NBC's sketch comedy show *Saturday Night Live*. Actor Christopher Walken plays a gardener who has a phobia of plants. He advises (and demonstrates) placing plastic googly eyes on the plants to assuage such fears. Participants watched the clip twice in a row. After each viewing, participants completed three items asking how amusing, entertaining, and humorous they found the clip to be. These items were included only so that Study 9a's procedure matches Study 9b's (to make it a clean use of a causal chain), even though they have no relevance to the current hypotheses and will not be discussed further.

Before participants' third exposure to the clip, we delivered the perspective manipulation. All participants were told they would be paired with a future participant and guess how that person would respond to the clip. Fresh perspective participants would be making estimates about that person's first viewing of the clip; stale perspective participants, that person's third viewing.

Immediately following the third video exposure, participants were told that while watching the clip, they might have been asking themselves, "What would the future participant find \_\_\_\_\_ about it?" They were asked, "To what extent were you asking yourself these questions while watching the video?" Participants responded on a 1 (*not at all*) to 7 (*completely*) Likert-type scale about each of six stem completions, presented in a random order. We averaged the responses to the engrossing, unexpected, and original questions to form an *unexpected questions composite* ( $\alpha = .71$ ). We averaged the responses to stale, predictable, and boring questions to form a *predictable questions composite* ( $\alpha = .76$ ).

## Results and Discussion

Did our perspective manipulation prompt participants to report testing different hypotheses as they once again watched the clip? We submitted the two hypothesis composites to a 2(perspective: fresh, stale)  $\times$  2(search questions: unexpected, predictable) mixed model. Only the latter factor was measured within-subjects. As predicted, the Perspective  $\times$  Search Questions interaction was significant,  $F(1, 182) = 57.79, p < .001, \eta_p^2 = .24$  (Figure 8A).

Participants considering the perspective of someone watching the clip for the first time were more likely to report asking

themselves what would be unexpected, original, and engrossing about the sketch ( $M = 4.44, SD = 1.48$ ) than those considering another's third viewing ( $M = 3.35, SD = 1.46$ ),  $t(182) = 5.03, p < .001, d = .74$ . Those preparing to predict the experience of another's third viewing instead were asking themselves what would seem stale and predictable about the clip ( $M = 5.12, SD = 1.61$ ) more than those considering a first-timer's experience ( $M = 3.57, SD = 1.59$ ),  $t(182) = -6.52, p < .001, d = .96$ . Decomposed differently, fresh perspective participants asked themselves the predictable more than the unexpected directional questions, paired  $t(86) = 3.36, p = .005, d = .36$ . In contrast, stale perspective participants were focusing on what the other would find predictable more than what they would find unexpected, paired  $t(96) = -7.59, p < .001, d = .77$ .

## Study 9b: Does Directional Hypothesis Testing Influence Habituation?

Having shown in Study 9a that considering another's fresh or stale perspective leads participants to ask themselves different questions, we proceeded in Study 9b to complete the causal chain. We told participants who had been habituated to a stimulus that they would need to predict what another participant would find "engrossing, unexpected, and original" about the video or "stale, predictable, and boring" about the same clip. We predicted that the former set of questions—those naturally posed by people trying to understand another's fresh perspective—would slow habituation.

## Method

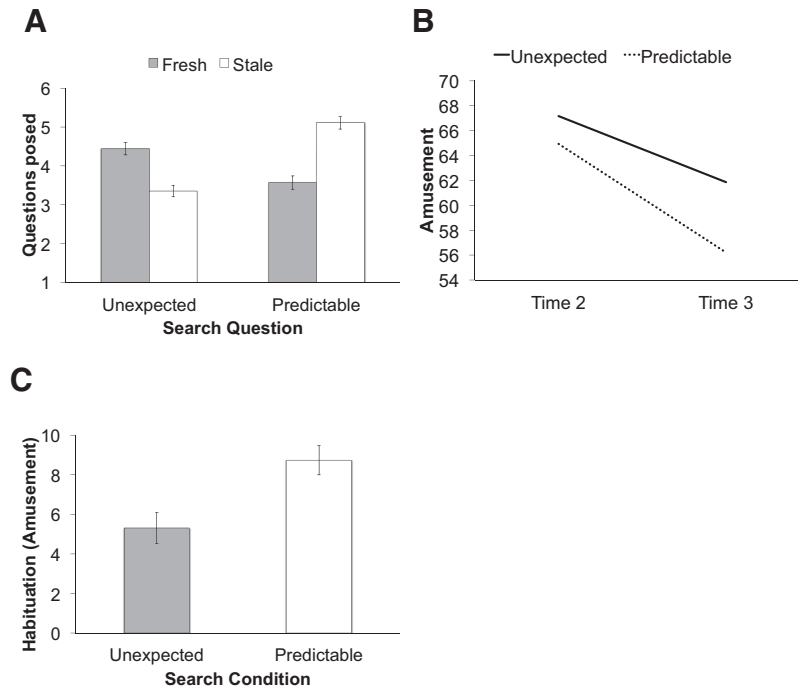
**Participants and design.** There were 735 Americans who were recruited from MTurk for nominal payment. Participants were randomly assigned to one of two search conditions: *predictable* or *unexpected*. Seventy-nine participants failed to answer an attention check correctly. This left a final sample size of 656 in all analyses reported below.

**Procedure.** Participants watched the same video as did those in Study 9a—"Googly Eyes"—three consecutive times. Following each viewing, participants indicated the extent to which they found the video amusing, entertaining, and humorous. These three items were each responded to on slider scales anchored at 0 (*not at all*) and 100 (*most I have ever felt*). We averaged these items to create an amusement composite (average  $\alpha = .98$ ).

Before the third viewing, all participants were told about a prediction they would have to make following that third viewing. Those in the stale and fresh perspective conditions saw: "More specifically, we would like to see if you can predict what a future participant will find engrossing, unexpected, and original about the video." We clarified that given participants would need to identify these things following the video, they should be sure to look for them while watching for the third time—that is, do what our fresh and stale perspective participants in Study 9a reported doing. To be consistent with the cover story, participants did end up reporting these thoughts at the study's ultimate conclusion (i.e., after their third completion of the amusement composite).

## Results and Discussion

Demonstrating habituation, participants found the clip to be more amusing the first time they watched it ( $M = 71.89, SD =$



*Figure 8.* (A) Study 9a: The extent to which participants were considering what they would find unexpected or predictable about the clip in the fresh and stale perspective conditions. (B) Study 9b: Self-reported amusement in the unexpected and predictable search conditions on the second and third viewings. Error bars reflect  $\pm 1$  standard error from the mean. (C) Study 9b: Mean feeling of amusement at Time 2 minus the feeling of amusement at Time 3 in the unexpected and predictable search conditions.

27.70) than they did the second time ( $M = 65.94$ ,  $SD = 29.30$ ),  $F(1, 655) = 151.67$ ,  $p < .001$ ,  $\eta_p^2 = .19$ . Did encouraging participants to search for what someone would find unexpected or predictable change their habituation on the third viewing? We submitted the amusement composites to a  $2(\text{time: Time 2, Time 3}) \times 2(\text{search: unexpected, predictable})$  mixed-model ANOVA, with only the first factor measured within-subjects. The predicted Time  $\times$  Search interaction was significant,  $F(1, 654) = 9.92$ ,  $p = .002$ ,  $\eta_p^2 = .02$  (Figure 8B).

When participants were asked to consider what a future participant would find stale, predictable or boring about the video, they showed clear additional habituation to the clip ( $M_{T2-T3} = 8.73$ ,  $SD = 14.14$ ), paired  $t(360) = 11.73$ ,  $p < .001$ ,  $d = .63$ . In contrast, when participants considered what a future participant would find engrossing, unexpected, and original about the video, their rate of habituation slowed by 39% ( $M_{T2-T3} = 5.31$ ,  $SD = 13.42$ ), paired  $t(294) = 6.80$ ,  $p < .001$ ,  $d = .40$  (Figure 8C). These findings complete the causal chain. Considering another's fresh or stale perspective prompts a focus on what would be unexpected or predictable about an experience (Study 9a). Focusing on what one might find unexpected about an experience slows habituation compared with focusing on what would be predictable (Study 9b). In short, considering another's perspective can change people's construal and experience because it encourages them to approach stimuli by asking themselves directionally different questions that change what they see and, thus, what they feel.

## General Discussion

As people sense and experience the world, they may feel as if they are learning directly about its contents. Some movies are sad; some people are endearing; some shows are funny; some ideas are inspiring. However, to offer a twist on a classic philosophical question about trees falling and sound: If an idea were shared but there was no one there to hear it, would it still be inspiring? Such descriptors characterize people's interpretation or construal of stimuli, not invariant readouts of the properties themselves.

Much as tastes vary, construals vary. Tastes vary in part because construals do. Two people can see the same stimulus differently at a single point in time; a single person will interpret a stimulus differently at two (even relatively close) points in time. But even though people typically underappreciate the power of such construals (a point that various aspects of our results emphasize), 11 studies highlight people's relatively easy ability to try on and experience the consequences of alternative perspectives—including those they seemingly had but lost (Studies 1–6, 9b, and online Supplemental Material Studies A, B, and F), as well as those they would not have had spontaneously (Studies 5, 7, 8b, and online Supplemental Materials Studies D1–D2). By trying to consider how a stimulus is seen through another's eyes, people's experiences changed in line with those alternate construals. This vicarious construal effect influenced a wide range of experiences and emotions: happiness (Study 1), disgust (online Supplemental Materials Study A), sadness (online Supplemental Materials Study B),

engagement (Study 2), awe (Studies 3 and 4), humor (Studies 5 and 6, 9a and 9b, and [online Supplemental Materials Study F](#)), and enjoyment (Studies 7–8b and [online Supplemental Materials Studies D1–D2](#)).

We isolated these effects to consequences of trying to understand alternative perspectives. That is, the VCE emerges not merely because of the contagiousness of someone else's simultaneous (inferred) experience (Studies 2, 3, 6, and 7), but because of the different perspective one is trying to understand. As a result, people were able to slow habituation by considering another's fresh perspective (Studies 1–6), preemptively habituate by considering another's stale perspective (Study 5), and find varying degrees of enjoyment in a clip by considering how those positively or negatively disposed to the content would react (Study 7 and [online Supplemental Materials Studies D1–D2](#)).

Although people are typically blind to the power of construal in shaping their experience, it might be natural to think that our heavy-handed interventions are putting the importance of construal in sharper focus for participants. To the contrary, we find that even when people are actively and intentionally considering the perspective of someone in a different situation, they fail to realize that it is this intervention—not merely the objective reality of the stimulus or their own predisposition toward it—that is driving their experience. Four findings support this conclusion. First, those whose rate of habituation was stalled by the VCE thought that untreated others would find the clip more emotionally intense ([online Supplemental Materials Study C](#)). Second, those whose experience of stand-up comedy had been altered by the VCE thought the comics were actually more or less funny—that is, would produce more or less audience laughter (Study 6). Third, those whose enjoyment of a Seinfeld routine was influenced by the VCE displayed matching shifts in their subjective characterizations of the positivity of an audible audience response (Study 7). Fourth, these same participants reported more or less interest in watching more of Seinfeld's performances in the future (Study 7). In other words, even when people are explicitly encouraged to consider different perspectives and, thus, achieve different construals, such people are at least partially blind to its distorting power.

When trying to understand what another would see, people asked themselves different questions that matched theories about how another's construal might be different (Studies 8a and 9a). Knowing they would have to predict how a performer's fan or a newcomer to a clip would respond, participants asked themselves what one would like, or find novel and engaging, about the stimulus, respectively. Posing these questions while having the experience was sufficient to reproduce the vicarious construal effect (Studies 8b and 9b). We will return to how this process evidence suggests possible boundaries on the VCE for future research to explore.

### Considering Threats to the Validity of Self-Reported Experience

To study people's subjective characterization of their own experience, we asked people to report on such subjective characterizations. But in relying on such self-report measures, we have a special responsibility to consider threats to measurement validity. We address such threats in four ways.

First, we showed that the VCE is not merely a measurement artifact of anchoring and adjustment. That is, participants who were anchored on someone else's unhabituated experience—*after* their own experience but before reporting on it—did not show the same vicarious construal effect (Study 4). This also supports that trying to understand another's perspective colors experience, not merely recollections. Second, our interventions also changed people's beliefs about how untreated others would respond ([online Supplemental Materials Study C](#)) or had responded (Studies 6 and 7), or how interested they themselves were in similar future experiences (Study 7). Third, we addressed a concern that those in our intervention conditions—those who had been warned that they would have to provide an estimate of someone else's experience—may have missed that we first asked about their own experience, not their estimate of someone else's. We conducted two studies ([online Supplemental Materials Studies D1 and D2](#)) in which we counterbalanced the order in which participants saw the key dependent measure (self-reported experience) and the measure they had been notified they would complete (estimating another person's experience). Participants should have the opportunity to be confused about the self-reported experience measure's actual focus when it came before, not after, the measure they were expecting to complete. No moderation by order was observed.

Fourth, we also conducted an additional study ([online Supplemental Materials Study E](#)) in which we probed the plausibility of a demand effect. That is, perhaps participants were merely “playing along” and reported an experience that they thought the experimenter expected of them, not one they actually had. However, if people are simply intuiting (and then stating) what the experimenter wants to hear, it requires that participants intuit what the experimenter wants to hear. We examined this possibility in a study (described fully as [online Supplemental Materials Study E](#)). Participants saw the full experimental materials from a habituation paradigm previously used to test the VCE, but were not asked to be a full participant. They read manipulation instructions, but did not actually carry them out. Not surprisingly, participants accurately anticipated the habituation typically observed in our VCE paradigm, because taking another's fresh or stale perspective should require some understanding of how first-time or third-time viewers may differ in their approach to a stimulus (see Study 9a). However, did participants anticipate the vicarious construal effect? Although participants showed a directional appreciation that the fresh perspective instructions, compared with the stale perspective instructions, were predicted to slow habituation ( $d = .16$ ), this intuition did not reach statistical significance (despite 529 *df*). More important, this difference was notably smaller than the one actually observed using the same paradigm ( $d = .56$ ). This—in combination with the three points initially reviewed—bolsters our interpretation that participants were reporting a shift in their actual experience, not their belief about what the experimenter expected of them.

### Remaining Questions Concerning Boundaries of the Vicarious Construal Effect

Our mechanistic evidence detailing the role of directional hypothesis testing (Studies 8a–9b) suggests several conditions that may need to be met for the vicarious construal effect to emerge. In enumerating six such possibilities, we both consider more deeply

the theoretical implications of our account and highlight questions that future research should address. First, someone must have a hypothesis about how another's construal would differ from the self's. These hunches formed the bases of the different questions that participants in Studies 8a and 9a reported asking themselves, which prompted shifts in construal and experience.

Second, the person attempting to consider an alternative perspective may need sufficient expertise to answer these directional hypotheses in a way that changes construal and, thus, experience. Although we have focused on stimuli to which one habituates, other stimuli are acquired tastes; they improve with experience. For many Americans, British humor falls in this bucket. It takes time to understand its subtlety. Whether Americans could consider a Brit's perspective and find *Mr. Bean* hilarious is an empirical question. More generally, although none of the psychology believed to underlie the VCE itself should be culturally bound, people may not be able to adopt other construals that lean on culturally specific knowledge of which they are not aware. That said, [online Supplemental Studies D1 and D2](#), which showed that American participants could find a Japanese anime clip to be more or less enjoyable depending on whether they considered the perspective of an anime fan or nonfan, may suggest that preexisting expertise or cultural boundaries are not particularly limiting factors. In that case, the VCE may stem from being attentive to and open to what is good or bad about a stimulus, even when one does not have the a priori expertise to know exactly what those good or bad features are.

Third, people's difficulty with adopting an alternative construal may not be limited to the ability to do so. It may also be restricted by their *motivation* to engage fully with the directional hypothesis testing that adopting a vicarious construal requires. For example, although an environmental activist is likely to have good guesses about a climate skeptic's orientation toward climate science, the activist may not be willing to fully partake in extremely skeptical hypothesis testing while watching Al Gore's *An Inconvenient Truth*. Thoroughly engaging with this counterfactual perspective may itself feel heretical (Tetlock, Kristel, Elson, Green, & Lerner, 2000) and a threat to one's values (Catapano, Tormala, & Rucker, 2019).

Fourth, there must be latitude in the subjective interpretability of a stimulus that permits directional hypothesis tests to alter one's construal. For rich and complex stimuli, this may be easily found, but in other cases, it may not exist. That is, some experiences may be primarily defined by low-level sensory or physiological feedback cues (Mook, 1996) instead of by higher-order construals. And in fact, we conducted one study with stimuli of this variety: potato chips. We found that although we could induce participants to habituate to an unusual flavor of potato chips (cheesy garlic bread or bacon mac and cheese), asking people to consider someone eating the chips for the first time did not slow their rate of habituation,  $F(1, 191) = 1.33, p = .25, \eta_p^2 = .007$ . Although it is necessarily difficult to extrapolate from null effects, this offers a hint that not all experience can be reintensified even if people know how another's experience is likely to differ.

Fifth, in what would seem to be a question about our existing data, but that also relates to an open question, one can ask how practically meaningful our effect sizes are. Consider again our studies showing that habituation can be slowed. Although we reported measures of effect size (partial eta squared), we sought a

more intuitive and simpler statistic to capture the magnitude of our intervention's influence. We returned to all of our studies examining habituation and calculated by what percentage considering a fresh perspective reduced habituation compared to what the control conditions suggested otherwise would have been observed. Averaging across the relevant comparisons (Studies 1–4 and 6), we find it is 61%. Is an intervention that slows habituation by 61% practically meaningful? We concede it is hard to say. But here it would be useful to determine whether our intervention has an additive effect. That is, a San Franciscan who prepares to take his fourth visitor of the year to Alcatraz—but who approaches *every* visit by considering what a first-timer would see—may have retained much more of his initial enjoyment than another whose interest eroded unimpeded on every visit. Examining how the vicarious construal effect holds up with repeated use may be an important topic for considering the magnitude of its practical significance.

Finally, it is unclear to what extent people would experience the VCE if they attempted considering a stimulus through another's eyes in a strategic attempt to change their own experience. In our studies, participants were blind to the power of the intervention in shaping their own experience. Once people are aware of (and even intentionally attempt to exploit) its effects, will their experience be similarly shaped? On the one hand, it would seem that such a strategic orientation might undermine its ability to produce seemingly veridical, undistorted experiences. On the other hand, despite such intent, the intervention may still encourage a confirmatory search in making sense of the stimulus, one that may push people to attend to different components and discover something *real* in the experience that they would not have spontaneously appreciated. Answering questions of this sort will help not merely to expand on the theoretical contributions begun here, but to offer insights into how the vicarious construal effect can help people to experience the delight that they would have missed, to see the sheen that still resides under the dust.

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Received December 31, 2017

Revision received September 16, 2019

Accepted September 26, 2019 ■