Keeping It Real in Experimental Research—Understanding When, Where, and How to Enhance Realism and Measure Consumer Behavior

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In this article, we consider why employing realistic experimental designs and measuring actual behavior is important and beneficial for consumer research. More specifically, we discuss when, where, and how researchers might go about doing this in order to increase the veracity and believability of their work. We analyze the choice of independent variables (IVs) along the experimental-realism dimension, ranging from artificial to realistic, and the choice of dependent variables (DVs) along the behavioral-measures dimension ranging from hypothetical intention to actual behavior. Importantly, we also map various goals of consumer research along these two dimensions to highlight when it is most appropriate to enhance the realism and behavioral measures of an experiment. Using a number of illustrative examples from research in the extant literature, we specifically highlight how consumer researchers can increase experimental realism and utilize actual-behavior measures in their experiments in order to improve both the fidelity of the research and the likelihood that the research provides insight into real consumer behavior.

Keywords: consumer research, research methods, experimental design, external validity, realism, behavior, field experiments, measurement

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As indicated in my comments to the authors, I like the proposition, and the main result is interesting. But is it true*?*

—Comments from an anonymous *JCR* reviewer

Consumer behavior research is a broad field bringing together diverse research methodologies. What binds these myriad methodologies together is the drive to really understand the behavior of individuals as they interact with one another and their environment in a consumption context. Somewhat ironically, understanding consumer behavior through experimental research does not always involve examining the actual behavior of consumers. More often, in fact, it involves manipulating aspects of a stylized, artificial scenario and measuring consumer responses to these scenarios, responses that typically reflect consumers' thoughts and theories about the scenarios, and not their actual behavior. This approach can contribute to concerns—such as that of the reviewer above—over whether the research is true, or at the very least, valid. In this work, we

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examine the importance and benefits of utilizing realistic manipulations and measuring actual behavior, and discuss when, where, and how researchers might go about doing this in order to increase the veracity and believability of their work. We analyze the choice of independent variables (IVs) along the experimental-realism dimension, ranging from artificial to realistic, and the choice of dependent variables (DVs) along the behavioral-measures dimension ranging from hypothetical intention to actual behavior. We also map various goals of consumer research along these two dimensions to shed light specifically on when it is most appropriate to enhance the realism and behavioral aspects of an experiment in order to improve both the fidelity of the research and the likelihood that the research provides insight into real consumer behavior.

To begin, we first need to explain what we mean by the terms *realism* and *behavior* that we use to classify IVs and DVs, respectively, along the two aforementioned dimensions. For the purpose of this tutorial, we take a very broad approach in the design of IVs. More specifically, we consider IVs as encompassing the experimental manipulation, as well as the choice of the experimental setup, context, environment, and participants. Collectively, these design factors determine the experimental realism of the study and can be placed on a continuum ranging from purely artificial to very realistic. The closer the IVs are to matching an actual consumption experience that individuals encounter in real life, the higher the experiment is on the realism dimension. For example, if a researcher is interested in the impact of temperature on grocery shopping behavior, having participants imagine what different temperatures feel like would be a less realistic IV than actually changing the temperature of the store.

We draw a similar distinction for the DVs of an experiment, which can range from hypothetical measures to actual behavior. At first glance, anything participants might do in an experiment could be considered a form of behavior. However, such a definition may be too inclusive to be helpful in furthering the aim of the current discussion. Since our goal is to increase the validity, generalizability, and applicability of experimental consumer research, we define behavior as actions, consciously intended or not, that participants engage in as part of the DV. Notably, these are actions resembling those consumers might take in a consumption context and that also carry some level of consequence. Such actions could range from choosing to purchase an item to clicking keys vigorously or speaking loudly. And because we include both intended and unintended actions in our definition of behavior, for the purpose of the current discussion we also consider even more subtle automatic or physiological responses, such as moving ones' eyes to a particular location on the screen or having an elevated heart rate, as examples of behavioral DVs. This definition of behavior differs from how the term is used in other fields. To provide more guidance, we will discuss the breadth and usefulness of available alternatives for enhancing the realism (IVs) and behavioral measures (DVs) of consumer research experiments in more detail in later sections, but first, we delve into the question of when it is most appropriate to do so.

WHEN TO USE REALISTIC MANIPULATIONS AND/OR MEASURE BEHAVIOR?

One of the primary propositions of this tutorial is that consumer research that is higher on experimental realism and that employs more behavioral measures is more effective in illuminating important insights about real consumer behavior. However, we acknowledge that providing insight about actual consumer behavior may often not be the main goal of a research project.

There are many different goals for which experimental consumer research projects may be conducted—all equally valid, but diametrically different in their intent and corresponding appropriate execution. Complicating things further, often authors themselves have not clearly identified the goal for a given research project before they begin collecting data, or alternatively, they may attempt to accomplish several (and often too many) goals in one manuscript. The latter is especially problematic because the appropriateness of experimental realism and/or behavioral measures largely depends on what the research is trying to accomplish. Thus, ideally the first step in the research process should be to decide what the goal of the research is.

Because the goals of experimental consumer research papers vary widely, the research design and corresponding levels of realism and behavioral measures that are best suited to meet each goal also differ. Since some research designs are better than others in terms of their external validity compared to their ability to measure mediators and moderators, the research design that provides the most compelling evidence to establish that an effect exists will not be the same as one that demonstrates the strongest support for a definitive, causal process. As a case in point, though field-study data is often quite persuasive in convincing readers that an effect occurs outside the confines of a controlled lab environment, it rarely can provide any insight into the psychological underpinnings of a phenomenon and may not in fact be helpful for every paper. Importantly, this suggests that there is not a stand-alone template for writing the perfect experimental consumer research paper. Rather, the recommendations for what is best for a given paper depend critically on its intended research goal.

Below we provide several theoretically oriented, as well as more practically oriented, goals that experimental consumer researchers may hold for their work, and highlight where enhancing realism through more naturalistic

manipulations and utilizing behavioral measures, as opposed to artificial manipulations and nonbehavioral measures, is most pertinent. Note, however, that this is not an exhaustive list and other research goals exist that are not discussed here.

The first goal that an experimental consumer research paper may have is to develop a new theory and/or test the legitimacy of a theory. For example, using a realistic experimental manipulation to deplete executive resources (such as attention regulation, automatic response override, or rule switching), as well as behavioral measures of choice, Pocheptsova et al. (2009) exposed boundary conditions to known contextual effects on choice, with the notion of limited executive resources. In doing so, they further shed light on the processes underlying those known effects and the theories explaining them. In another study, Chartrand et al. (2008; study 3) asked participants to make either a real or a hypothetical choice between a more expensive branded product and a less expensive product after exposing them to different goal primes (i.e., prestige vs. thrift). Subsequently, participants were asked to make a second product choice as part of their potential reward in a real lottery draw. By manipulating whether the first product choice was real or hypothetical as well as using a behavioral measure as the DV (i.e., the second product choice), the researchers were able to demonstrate convincingly that consumer goals can be nonconsciously activated while ruling out an important alternative account (i.e., mere cognitive activation).

In another example of a theory-focused study, Amir and Levav (2008) looked at the notion of whether one should interpret decision inconsistency as preference construction—the prevailing theory at the time. They set up a theoretical framework and provided experimental tests that rejected the overarching theory in favor of a more nuanced one—consumers often learn how to "solve" the specific decision problem, but that learning does not carry over to other contexts. In this example, it wasn't the realism of the design that allowed them to achieve the research goal. Rather, the design's fidelity to the revised theory tested yielded a convincing rejection of the predominant theory.

As highlighted by these examples, an important takeaway from this tutorial is that enhancing realism and utilizing behavioral measures isn't always necessary or even preferred. Instead, what matters most is aligning the research methodology to the research's overarching goals. For some papers, stimuli positioned clearly on the artificial end of the experimental-realism continuum and hypothetical DVs are perfectly suited to accomplishing the focal research goals. A case in point is Kahneman and Tversky's (1979; see also Tversky and Kahneman 1986) seminal work to develop prospect theory, which utilized stylized scenarios framed as losses or gains and measured respondents' hypothetical choices. The stimuli did not need to be realistic, nor the choices that respondents made consequential, in order for the experiments to demonstrate the principles that the researchers proposed. Since the goal of the research was to develop an alternative theory of decision making (that applied generally across domains and contexts)—or in the researchers' words, a new "theory of mind"—the researchers needed to show that the basic tenets of expected utility theory could not explain choice patterns under risk, and to cleanly isolate the differences across scenarios. There is no question that the use of more artificial stimuli and hypothetical choices in this case was legitimate and justifiable. Thus, when the research goal is to develop a theory of how consumers think or behave (that is independent of context), it may be less important to enhance experimental realism and employ measures of actual behavior.

A second goal that an experimental consumer research paper may have is to exhibit a phenomenon. Papers with this goal are commonly referred to as "effects papers" within the field because their primary purpose is to document the existence of a phenomenon that has not previously been reported in the literature or is not otherwise well understood. Because such papers aim simply to provide compelling evidence that a phenomenon exists, a definitive process explanation or demonstration of moderators of the phenomenon may not be needed, but if the phenomenon involves responses to a specific context or documenting behavior (as opposed to judgments or attitudes), demonstrating its existence through a more naturalistic experimental design with actual behavioral measures would help to meet this goal. For example, the goal of Moreau, Markman, and Lehman (2001) was to show that consumers' categorizations, expectations, and preferences for really new products depend on the first plausible category label they are given for the product, typically cued in advertisements. Thus, in order to adequately test the hypotheses, it was vital that the stimuli viewed by participants reflected reality and resembled actual ads consumers see in the marketplace. In the same vein, while classic demonstrations of the well-documented attraction effect (Huber, Payne, and Puto 1982; Simonson and Tversky 1992) involved predominantly stylized product stimuli such that product attributes were represented numerically, Frederick, Lee, and Baskin (2014) contended that the effect could be attenuated, eliminated, or even reversed when product attributes are represented perceptually, rather than numerically. Therefore, the researchers had to incorporate more realistic perceptual attributes into their product stimuli (e.g., depicting the image quality of a TV with a picture instead of a numeric rating) in order to achieve this goal.

Highlighting the importance of employing behavioral measures to establish a phenomenon in the literature, Goldsmith and Amir (2010) showed that uncertain promotions can be more efficient than certain ones by looking at both actual and hypothetical purchase decisions. Similarly, Lee, Amir, and Ariely (2009) found in a series of

experiments that greater reliance on emotional reactions during decision making results in greater preference consistency, inferring preferences from actual choices. In both of these papers, the behavior involved making a choice—to purchase in the former, and to indicate preference in the latter. Eliciting one's theory about the effects or one's intention to act would have provided less compelling evidence for a counterintuitive effect.

Understanding the causal process underlying a phenomenon and establishing moderators or boundary conditions of an existing phenomenon are two other goals for experimental consumer research. Lee and Tsai (2014), for instance, examined whether price promotions affect the consumption experience of a product. In a series of experiments that involved real product consumption (chocolates, orange juice, and music), they found that whether price discounts enhance or impair how much consumers enjoy consuming a product depends on when product consumption takes place following purchase. They further demonstrated two distinct processes—an affective path and a cognitive path—that underlie these effects. In this case, the experimental realism of actual product consumption helped the research to deliver on its goal.

Providing insights into the retail context, Kristofferson et al. (2017) examined the impact of promotions, but focused specifically on scarcity appeals, finding that consumers exposed to such ads behave more aggressively because they perceive other shoppers as competitive threats. Importantly, this work is the first to show not only that promotions can drive consumers to aggress by highlighting the limited supply of nonessential luxury items, but also that aggression can originate simply from exposure to such promotional materials and then generalize beyond the promotional context. Notably, this research utilized a naturalistic scarcity manipulation by giving participants a color printout that looked like a real advertisement, and the measures of aggression were actual behavior. Whether it was violently interacting with a vending machine that failed to dispense a food item or trying to hit targets while gaming, the authors investigated how scarcity appeals led to actual aggression, as opposed to theories of aggression, or imagined situations. Another "behavioral" measure that the authors used in their experiments falls into the category of physiological measures—level of testosterone—which allowed the authors to strengthen their process claims.

The research goal of a paper may be even narrower in its application, with the sole intent of measuring the impact of a specific marketing variable on another variable of interest (behavior, attitude, etc.). Lynch and Ariely (2000), for example, set up an elaborate experimental design aimed at measuring the effect of search costs for price and quality on consumer price sensitivity. Lee, Frederick, and Ariely (2006), on the other hand, examined whether informing consumers (i.e., the patrons of a pub) about the ingredients of a product (i.e., beer) prior to consumption would influence their preference for the product. In these studies, the experimental realism and behavioral nature of the measures allowed the researchers to convince the readers that a very surprising or counterintuitive result exists. Less realistic IVs and nonbehavioral measures would have weakened the impact of these findings.

In cases where the focal DV is not a specific behavior but is instead a category of responses and/or behaviors (higher risk taking, more creativity, greater price sensitivity, etc.), it may be helpful to vary not only measures along the behavior dimension but also the types of measures taken in order to triangulate and build stronger empirical support. For example, Coleman and Williams (2013) show that consumers desire consistency between their salient social identity and their emotions, leading them to upregulate identity-consistent emotions and down-regulate identity-inconsistent emotions. This in turn leads to a variety of psychological consequences demonstrated using behavioral measures: music listening choices, quantity of tea consumed, and more mouse clicks. Collectively, this assortment of measures provides more compelling evidence for the authors' assertions.

Be it to develop a theory, test a psychological process or establish boundary conditions, investigate a counterintuitive effect, or demonstrate the managerial or policy implications of a phenomenon, increasing the experimental realism of the IVs and using behavioral measures as the DVs has the potential to contribute substantially to the goal of the research. Though not always necessary, it is often the case that enhancing the realism and behavior dimensions can make for more compelling consumer research. In addition, conceptually replicating the focal effect using naturalistic experimental designs with an assortment of behavioral measures can lend much confidence to the robustness and generalizability of the research.

EXPERIMENTAL REALISM IN THE IVS

As discussed above, the experimental realism of a study can be placed on a continuum ranging from purely artificial to very realistic, depending on the choice of IVs. While there are proper justifications for many locations along this continuum, we propose that the more these variables involve realism and entail a more naturalistic setting in which to measure or observe responses, the easier the generalization, the larger the impact, and notably, the greater the likelihood of convincing the editorial team of the veracity and importance of the research.

Given the potential benefits of employing more realistic IVs, one might think there would be substantial costs in doing so. However, increasing the realism of experimental designs can often be as simple as bringing real products into the lab instead of showing participants pictures of

products on a computer screen. For example, Morales (2005) and Castro, Morales, and Nowlis (2013) both used real consumer packaged goods to create physical product displays inside the lab, just like those that would be found inside a grocery store. Using actual, physical products as stimuli rather than pictures of products can also have far-reaching implications on participants' responses, as prior work has demonstrated a clear difference in how participants respond to hypothetical versus real products in a lab setting. Specifically, hypothetical products do not generate the same level of affective response as real products (Shiv and Fedorikhin 1999). Similarly, hypothetical (vs. real) choices do not lead to satiation of activated goals (Chartrand et al. 2008). Note, however, that using real, physical product displays to enhance the experimental realism of a study is beneficial only when the researchers want to examine how consumers react to product displays in a brick-and-mortar grocery store, because the manipulation is closer to the actual consumer experience. If the researchers were instead interested in consumer responses to online grocery shopping, physical product displays would actually decrease the realism of the experiment by making the experience less akin to real online shopping, and product pictures on a computer screen would be preferred.

Sometimes, increasing the realism of a lab experiment extends well beyond using real versus hypothetical products. In an article examining how consumers respond to having to buy larger-than-expected clothing sizes, in order to create a more naturalistic experience, Hoegg et al. (2014; studies 1 and 2) set up a dressing room in the lab where participants took their own body measurements (bust, hip, neck, shoulder, waist, and lower waist) using a tape measure and a mirror in a private fitting room. Afterward, they brought their measurements to the lab administrator who ostensibly used these measurements to determine the size of a custom-fit suit they would be evaluating. The administrator cross-checked each measurement with a fictitious size chart, assigned a size, and then gave the participant a card with the clothing size, a picture of the suit, and some details about the suit. Half of the participants were assigned their actual (self-reported) size, while the other half received a numerical size two sizes larger. Participants then took the card to a computer and answered the main dependent measures about their evaluations of the suit. Thus, although the suit remained hypothetical and the DVs measured thoughts and intentions related to the suit rather than real behavior (e.g., purchase), the experience was designed to be as natural and true to an actual shopping experience as possible in order to increase the experimental realism of the study and bring responses closer to what they would be in a real shopping experience. The researchers could have asked participants only to "imagine they purchased a suit two sizes larger than their usual size" without having to engage in any additional actions, but by having participants go into the dressing room to take their measurements for the suit, the researchers were able to make the lab experience more realistic for participants. Consequently, we contend that participant evaluations of the (larger-than-usual sized) suit should be closer to what they would be in an actual consumption experience than if they had only imagined purchasing the suit.

Ideally, experimental realism is at its maximum when the research can be conducted outside of the lab, in the field. In other words, when the experiment can take place in the same setting as the actual consumption experience, the experimental realism dimension is maximized. For example, Morvinski, Amir, and Muller (forthcoming) hired research assistants (RAs) to dress up as salespeople wearing branded outfits, and use professional-looking marketing materials when they approached potential customers to offer them a trial of a new product. Similarly, Lee and Ariely (2006) had RAs hand out different types of discount coupons to shoppers at a convenience store in order to examine how these promotions influence consumer spending. To enhance the experimental realism of the manipulation, not only were the RAs disguised as store assistants (each wearing a store t-shirt, similar to real store assistants), the coupons also had the store logo imprinted on them. Again, in both of these studies, because the manipulations of the IVs were as high as they could be on experimental realism, the dependent measures were also more in line with how we would expect consumers to behave in real consumption situations.

Increasing the experimental realism of the IV is particularly important in cases where consumers may be unaware of the impact that a given factor has on their behavior, because it takes away the need for consumers to "predict" how they might react in a given consumption experience. Focusing specifically on food consumption, for example, McFerran et al. (2010) identified an important moderator of the impact that social influence has in this domain. Although consumers generally anchor on the quantity chosen by others such that they choose larger portions after viewing someone else doing likewise, the researchers found that the other person's body type (i.e., thin vs. obese) changes the degree to which consumers adjust both their choices and actual consumption quantity—a source of influence of which consumers may be unaware. Notably, the researchers did so by having participants observe a thin versus an obese confederate make actual food choices in the lab right before them, thereby increasing the realism of the consumption experience. To be clear, this article also included a hypothetical scenario study, where participants imagined the situation instead of experiencing it directly, and found the same pattern of results. However, as we will describe later, this is one of the recommended strategies for experimental papers to follow in order to make the empirical package more compelling. Including some studies that are higher on experimental realism than others, where appropriate, serves only to make the overall empirical evidence more convincing.

More generally, we contend that anything that makes the IVs of the experimental design more similar to the actual consumption experience will increase the experimental realism of the experiment and therefore also enhance the naturalism of the responses. As noted above, this could be as simple as using actual, physical products instead of pictures (in cases where the research focuses on brick-and-mortar retail experiences), but extends to every aspect of the experimental procedure. Some additional examples include using a voice recording of dialogue instead of having participants read about a conversation on paper, utilizing confederates to engage in direct social interactions with participants instead of imagined ones, making students literally wait a few minutes instead of thinking about having to wait, and so on.

Of particular interest is the plethora of web-based designs that exist in consumer research today. The remote and anonymous nature of the interaction between the researcher and the participants often stymies the willingness and ability of researchers to substitute the canonical hypothetical scenario with a more realistic one. This is somewhat paradoxical, as much of actual consumption currently occurs via the very same medium. It is therefore relatively easy to increase experimental realism online, have participants invest actual effort in searching or in acquiring information, have participants put products in an online shopping basket, and have participants make a post using their own Facebook or Instagram account. Examples of other welcome substitutions include having participants write actual recommendation messages to their friends instead of imagining doing so; read actual reviews of a product as opposed to imagining doing so; write an actual review of a distant destination for a travel website, as opposed to completing sentences that prime distance; and undertake a task that causes actual guilt because the alternative was more virtuous, instead of reading a paragraph priming guilt or imagining doing so.

BEHAVIORAL MEASURES AS DVS

When experiments focus on behavior, they tend to measure choice as the primary behavioral DV: the decision to buy (Chen, Lee, and Yap forthcoming; Simonson 1990), the decision to sell (Irmak, Wakslak, and Trope 2013; Peck and Shu 2009), the decision to trade (Brenner et al. 2007; Carmon and Ariely 2000), the decision to click through (Aaker and Lee 2001; Dhar and Nowlis 1999; Zwebner, Lee, and Goldenberg 2013), the decision to search (Häubl and Murray 2003; Mandel and Johnson 2002), and the decision to get rid of something (Haws et al. 2012; Okada 2001), to name but a few, are all choices. Other experiments focus on behavior that is not an overt choice—for example, how much aggression a person exhibits in attempting to shake a vending machine when a candy bar gets stuck (Kristofferson et al. 2017), how much food to consume under certain conditions (Garg, Wansink, and Inman 2007; McFerran et al. 2010; Scott et al. 2008), and how much time to wait before giving up on a task (Zhao, Lee, and Soman 2012). It is important to note that behavior is even broader than these and sometimes even more subtle. One may consider physical measures such as facial expressions (Teixeira, Wedel, and Pieters 2012; Ward and Broniarczyk 2011), voice intonations (Cavanaugh, Nunes, and Han 2017; Duke and Amir 2017; Lieberman, Duke, and Amir 2017), changes in hormone levels (Kristofferson et al. 2017), or eye movements/fixations (Chandon et al. 2009; Teixeira et al. 2012), if those behaviors are relevant to the research question.

The richness of potential behavioral DVs may lead the reader to conclude that everything is behavior. In fact, from a very broad perspective, anything that a participant does in an experiment may be considered behavior. It is therefore imperative to define more concretely what we mean when we refer to a dependent measure as behavioral. For example, asking moviegoers for their evaluation of the movie they have just watched in the theater (Russell et al. forthcoming) leads participants to think and provide a response, but we would not call this measure behavioral. From the perspective adopted here, a behavior carries some consequence that extends beyond indicating one's thoughts about a given matter. Responding to a scale, or even declaring behavioral intentions in a hypothetical manner, does not fit this criterion. This is because reporting one's theories about behavior, emotions, or intended actions does not directly translate to actual behavior (Nisbett and Wilson 1977).

As with IVs that are higher in experimental realism, we believe that behavioral DVs that carry some form of consequence (e.g. social, financial, effort, time, self-efficacy) are substantially more informative of real consumer behavior in the marketplace. Notably, by focusing on behavioral consequentiality, our definition of behavior does not imply that some actions are always behavioral measures and others are not. Rather, it means that, depending on the context, the exact same action (e.g., clicking a mouse) can be classified as either a nonbehavioral measure when clicking a mouse is utilized to indicate a self-reported rating of determination on a nine-point scale, or a behavioral measure when the number of times one continues clicking a mouse is used to assess how determined a participant is to persevere on a focal task (e.g., Coleman and Williams 2013 for mouse clicks, or Amir and Ariely 2008 for keyboard clicks). Moreover, even unintended actions (e.g., mimicry) or physiological responses (e.g., testosterone levels) represent behaviors (or facts) that can be consequential and fall within our definition of behavioral DVs.

Within the consumer research literature, there are countless examples of behavioral measures and here we provide just a few to highlight how such measures carry

consequences and therefore meet the criteria of being a behavior. For example, as discussed above, having participants expend effort to measure motivation or commitment (i.e., by clicking on a mouse or keyboard) is a behavior, whereas having participants rate their level of motivation is not. Similarly, the same classification exists for writing recommendations to friends as opposed to rating the likelihood they would do so on a scale. Employing readily available technology that allows the recording and analyzing of facial expressions or voice quality, as opposed to asking participants to rate how they feel on a scale, assesses behavioral expressions of emotion. Another possibility is to have participants engage in a behavior that is a proxy for the underlying construct, such as having them wait for a reward and recording how long they wait or having them seek out a reward and measuring how much effort they are willing to exert, instead of having them rate their valuation of the reward on a scale. Similarly, one can measure time spent reading about a new product as a proxy for interest, as opposed to merely asking participants to rate their interest; have participants sign up for another experimental session instead of indicating their interest in participating in a future session; or have participants sign a petition instead of rating their level of agreement/disagreement with a certain policy.

The researcher can also infuse consequence into the DV—for example, by actually making a charitable donation based on participants' decisions, as opposed to asking for their likelihood to donate, or allowing participants to place a bet with their own money for a chance to get more (or less), as opposed to asking about their degree of risk aversion. Obviously, having participants make actual as opposed to hypothetical donations, or selling actual products to participants as opposed to hypothetical ones (often as part of or instead of some of the payoff), changes the manner in which participants and readers treat the results. Essentially, we are looking for a behavior that we can objectively observe that would follow from the hypothesis, but not from the null. Put differently, we are looking for the most objective evidence that fits the proposed account, but does not fit an alternative one.

HOW TO ENHANCE REALISM AND MEASURE ACTUAL BEHAVIOR IN EXPERIMENTS

The manner in which the realism of an experiment can be enhanced, and the ways behavior can be measured most effectively, critically depends on the design of the experiment. There is a continuum of experimental designs ranging from purely hypothetical vignettes in the lab (low on experimental realism and low on behavioral measures) to experiments run in the field on real consumers (high on experimental realism with varying levels of behavioral measures). The majority of experimental work in consumer research falls into one of the following three bins: field experiments, experiments in the field, and lab experiments. Though field experiments, by definition, are naturally higher in realism for the IVs (experimental realism) and behavior for DVs (behavioral measures), it is possible to increase both dimensions in all three types of designs. To this end, we first define each of the designs in more detail and then highlight how experimental realism and behavioral measures can be increased in each design.

Field experiments in the strictest sense are experiments where participants do not know they are taking part in a research study (Charness, Gneezy, and Kuhn 2013); they are unaware that an experimental manipulation has occurred and are engaged in real consumption behavior, which is observed and/or measured unobtrusively (e.g., the highest on both experimental realism–IVs, and behavioral measures–DVs; Gneezy forthcoming). For example, Kristofferson, White, and Peloza (2014) intercepted passersby in the main student union building at the University of British Columbia, handing out free poppy pins for Remembrance Day, and then had another researcher down the hall ask them for a donation on behalf of Canada's war veterans. The students were unaware that they were part of a research study, unaware that some students received the pin in an envelope while others did not, and unaware that their decisions about whether or not to donate and how much they donated were being observed. Thus, all of the criteria for a field experiment were met; the IVs were high on realism, and the DVs involved actual behavior.

However, it is often the case that a study does not meet all of these criteria. This begs the question of whether or not it should still be considered a "field experiment," and more importantly for the current point, whether it, in fact, incorporates realism and actual behavior. The answer depends critically on what the study is lacking. For example, in study 1 of Russell et al. (forthcoming) consumers went to a movie theater to watch a movie, unaware that a study was being conducted. Depending on the condition, consumers watched just the movie, standard-industry movie previews and the movie, or both commercials as well as standard-industry movie previews and the movie. That is, the IVs were high on experimental realism. After watching the movie, consumers were invited to complete a brief survey about their movie-watching experience, reporting their enjoyment of the experience and attitudes toward the ads, previews, and product placements within the movie. Thus, in this study, at the time when they watched the movie, participants did not know they were part of a research study, they were unaware that an experimental manipulation had occurred, and they engaged in a real consumption experience (by watching the movie). The DVs, however, were not observed/measured unbeknownst to participants. Instead, as is sometimes the case in experimental research, the DVs were self-reported by participants in a survey.

Therefore, this study was high on experimental realism (IV = realistic), but low on behavioral measures (DV = not actual behavior). Such a study in which the DV is not a behavioral measure is subject to issues associated with all self-reported measures (Nisbett and Wilson 1977), and generalizing from these measures to strategy or policy implications still requires a bit of a leap of faith. However, we contend that this study should still be considered a field experiment because the only unmet criterion is that the DVs were not behavior. Such studies still greatly enhance the naturalism of the consumption experience, the hallmark of what we argue should constitute a field experiment.

More specifically, based on our definitions of realism and behavior, we propose that for consumer research, the most critical aspect that determines whether or not a study can be classified as a field experiment is that participants do not know they are part of a research study *when* the manipulation is occurring and *when* they are engaging in real consumption behavior. Once the consumption experience is over, however, participants may become aware that a study (or at least an inquiry about their experience) is being conducted without changing its field experiment classification, as is the case when participants are asked to complete a survey. For example, Morvinski et al. (forthcoming) had salespeople ask customers to try a new product, and only after the purchase decision was made (i.e., the main DV was real behavior), requested that customers complete a short survey. We contend that what is most essential is that the consumption experience being studied needs to be "clean," meaning that consumers need to be unaware that there is anything different about that particular consumption experience from those in which they typically engage. Thus, it is possible for a field experiment to take place anywhere, even inside a laboratory (though this is more challenging to implement in a manner that would indeed be considered a field experiment), provided that it meets the essential criteria for a clean, naturally occurring consumption experience (i.e., all field experiments should be high on the experimental-realism dimension). As a case in point, Williams, Lieberman, and Amir (2017) manipulated the instructions for signing up for a well-paying, fun experiment in the lab, and measured participants' choice of time slots. Since participants did not know there was a manipulation; made realistic, consequential decisions (i.e., signed up for the slot they most wanted); and were not aware they were part of a study or that any measurement of their behavior occurred, the setup constitutes a field experiment by our definition.

Another type of design that can involve realism in the IV and actual behavior in the DV is *realistic experiments in the field*. These are experiments conducted outside the lab in actual consumption environments, but consumers are aware that they are taking part in a research study. Thus, realistic in-the field experiments are lower on the experimental-realism dimension than field experiments (which, by definition, keep the consumption experience as natural as possible), but higher than lab experiments, and as high as a nonfield experiment possibly can be in terms of realism. In addition, although the experimental designs of such studies can be carefully orchestrated to hold as many factors constant as possible, a key drawback of this type of experiment is that the researcher ultimately has less control over the environment in which the study is conducted, precisely because of its naturalistic aspects. Key for the current discussion, it is usually easier in these types of studies to include realism in the design, as part of the IVs. As an example of this type of study, in Argo, Dahl, and Morales (2006, 2008), participants were told to visit the university bookstore and complete an assigned task (e.g., trying on a particular t-shirt) and then provide their thoughts about the experience. Although the task was identical within each condition, the actual experience of trying on the t-shirt in the bookstore differed from participant to participant (e.g., the volume, type, and proximity of other shoppers in the store differed), because it was a behavioral manipulation that occurred in an uncontrolled environment, during regular business hours.

Finally, *lab experiments* are conducted in controlled settings where participants are fully aware that they are part of a research study. The participant samples for such studies are often less than representative of the average consumer (commonly known as "convenience samples"), but what matters most is their random assignment, which we take to assume equal *a priori* distributions of key variables and which allows the identification of treatment effects across conditions. Lab experiment IVs range from artificial to more realistic, and their DVs can vary from hypothetical to real behavior. Importantly, just as collecting data in the field does not necessarily make an experiment a field study, collecting data in the lab does not mean it has to be low in experimental realism or behavioral measures. In many cases, it may be important that the DV be behavioral. Such DVs could be financial in nature, whereby participants might spend, gamble, or save their own (out-of-pocket) money, or money that the researcher gives them as part of the experiment that belongs to them from that point forward, or the DVs could be material, whereby participants make a product choice that they could keep, consume, sell, or trade. With respect to the IVs, this type of experiment can employ realistic methods and stimuli (e.g., have participants eat half a doughnut), or more abstract ones (e.g., have participants circle a picture of a doughnut on a piece of paper), but its key defining characteristic is that participants must experience some sort of consequence as a result of participating in it.

For example, in a study that examined the effects of environmental cues on task persistence (Zhao et al. 2012; study 4), participants, in groups of eight, were asked to form a line in front of a food-tasting station in the lab for a taste test. Unbeknownst to the participants, the researchers manipulated between sessions the length of a black runner

on the light-gray floor carpet under the station: in half the sessions, the researchers laid a short black runner on the floor that covered only the area under the station, whereas in the remaining sessions, they laid a long black runner that extended from under the station to the opposite end of the room. Participants were informed that the food samples were not yet available, and after waiting for a few minutes, were told that they could collect the full participation fee and leave if they wished, as the session would run overtime. Participants who were standing on the long black runner (and who presumably had already crossed a "virtual task boundary" and were thus more committed to the focal task) were more likely to stay than those who stood on the floor carpet. Although this experiment was conducted in the lab, participants were neither aware of the experimental manipulation (i.e., the length of the runner; IV high on experimental realism), nor, more central to the current point, aware that their decision of whether to stay or to leave the session was being recorded (behavioral DV).

Other examples observe and record participants' actions in the lab that carry actual monetary consequences. Among others, both Mazar, Amir, and Ariely (2008) and Winterich, Mittal, and Morales (2014) had participants complete a task (i.e., a general knowledge exam, a coin flip, or an unsolvable anagram task) and then report the outcome to the experimenter. Participants could cheat and report an outcome resulting in a more favorable payment amount, and both the monetary reward received and the psychological consequences of cheating were real. In these studies, and in the previous example, the laboratory settings did not detract from the ability to employ behavioral measures, despite the lower realism of the experimental design.

The orthogonal view we present between the dimensions of experimental realism and behavioral measures suggests that a researcher can design a study that is high on experimental realism, but that does not employ any behavioral DVs, just as it is possible to have a study that is low on experimental realism but uses actual behavior as the primary DV. However, because a consequential choice to an artificial, hypothetical scenario is by definition nonsensical, the researcher can measure other responses that fall under our definition of behavior: time spent reading a website as a measure of interest, commitment, engagement, or effort; the active signing of a petition as a measure of preference, commitment, attitude, or interest; and clicking on a link to receive more information as a measure of interest, preference, or attitude. The latter, as discussed previously, may serve as a substitute for the canonical scale of the intention to act.

Based on our broad definition of behavior, behavioral measures may also be unintentional, in the form of automatic actions, physical reactions, or even physiological responses. With advances in technology, these could also serve as convincing measures following any of the designs mentioned previously. Examples of such measures include eye movements pointing to attention and information exposure (Pieters and Wedel 2007; Teixeira et al. 2012), voice intonations (Cavanaugh et al. 2017; Duke and Amir 2017; Lieberman et al. 2017) and facial expressions (Teixeira et al. 2012; Ward and Broniarczyk 2011) to assess affective states or emotions, heart rate or GSR to measure emotional arousal (Mehta, Zhu, and Cheema 2012; Noseworthy, Di Muro, and Murray 2014), and saliva to detect hormone levels, such as testosterone for aggression (Kristofferson et al. 2017; Saad and Vongas 2009; Weiss and Johar 2013) and oxytocin for compassion (Kosfeld et al. 2005). Such measures can also include unintentional behavior, such as the physical distance one chooses to sit away from a fellow participant (Adams et al. 2015), the amount of time one spends in a restaurant (Milliman 1986), or nonconscious social mimicry (Tanner et al. 2008).

HOW TO REPORT "BEHAVIOR" AND DRAW APPROPRIATE CONCLUSIONS

The simplest rule of thumb for reporting experiments is to report precisely what was measured (DVs), in light of what was manipulated (IVs), conditional on the context in which the data was collected. However, the goals of experimental research often demand broader claims involving generalizations to other contexts, other people, and sometimes other measures. For clarity and scientific ethicality, we recommend separating the two phases; that is, first report exactly what the data shows, and only then, construct the argument for generalization. For example, if the researcher did not collect actual behavior, but rather collected action predictions (e.g., whether participants would go see the new *Star Wars* movie) in a hypothetical experiment, then the report should say "participants' *intentions* to go see the movie were different across conditions" as opposed to "the proportion of participants who chose to go see the new *Star Wars* movie were different across conditions." The researcher could then speculate and provide support for the correspondence between action intentions in this context and actual behavior. This will allow both the reader to see the truth of the reported empirical results and the researcher to remain true to the research objectives.

The above movie example underscores a key advantage in employing behavioral measures, as such measures do not require this extra step of speculation. If, instead of measuring intentions, the researcher had sold tickets to the movie or discount vouchers for the theatre, or allowed participants to actively search for more information or email a friend to schedule a joint outing to the movie, the results would have led to the desired conclusions much more directly and smoothly. The ability to utilize a behavioral DV instead of a hypothetical one is bound mostly by the researchers' creativity and willingness to invest more in the experimental design, but the returns have the potential to

be substantial and well worth the investment. The ability to generalize from the results is even stronger the more realistic the IVs and context are. In the aforementioned examples, if the context includes actual movie tickets, actual marketing collateral for the movie, or an email to genuinely invite a friend for an outing, the generalization step would be minimized.

In trying to decide how to incorporate realism and behavior into a research project, it is also important to remember that this is not a binary choice. Most papers include multiple studies, and the degree of experimental realism and behavioral measures may differ across the empirical package. For example, as discussed previously, in McFerran et al. (2010) the ability of the researchers to demonstrate the same effect of social influence on food consumption using both hypothetical studies and real-behavior studies renders the empirical evidence more compelling and increases the overall strength of the empirical package. Similarly, Goldsmith and Amir (2010) first used a series of hypothetical lab experiments to document a phenomenon, and then followed these with a field experiment to strengthen the external validity of the claims.

Together, what the empirical approach in the above articles suggests is that a viable research strategy for a paper is to utilize measures that vary along the experimental-realism dimension or the behavioral-measure dimension across studies. In other words, not every single study has to be high on realism or measure actual behavior. Instead, some of the studies can be higher on the realism and behavioral dimensions than others. Remember, readers evaluate the overall empirical package, so papers that increase the degree of realism on the IVs and measure actual behavior in the DVs across studies will make for a more compelling empirical package than ones that remain artificial and hypothetical only.

SUMMARY

In the current article, we have discussed the reasons why employing realistic experimental designs and measuring actual behavior are important and beneficial for experimental consumer research. Importantly, using a number of illustrative examples from research in the extant literature, we have highlighted when, where, and how consumer researchers could inject experimental realism and utilize more behavioral measures in their experimentation. We hope that we have convinced the reader why doing so has the potential to strengthen consumer research, and that we have provided some useful experimental guidance toward uncovering valid and reliable insights in consumer behavior. Along the same lines, in conclusion, we propose that using more artificial, stylized scenarios to measure participants' beliefs about what they might do or how they might feel often has merit in building theories of consumer behavior, but using more realistic experimental manipulations and measuring actual behavior can help test these theories and provide a greater understanding of real consumer behavior.

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