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METHODS DIALOGUE

Commentaries on "An intervention-based abductive approach to generating testable theory"

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Abstract

This paper assembles five comments on Janiszewski and van Osselaer's (this issue) article that promotes abductive research as a way to generate new psychological theory. The review process began by asking those making comments to be part of collaborative communication between themselves and Janiszewski and van Osselaer. The five comments arising from that process provide well-honed insights into the strengths and weaknesses of the abductive research. The first commentary, by Frank Kardes, offers convincing evidence showing that the techniques of abductive thinking are similar to other explorative techniques currently being successfully used in deductive research. Eileen Fischer sees abductive thinking as integral to inductive and qualitative thinking as it facilitates the generation of new constructs and remaps established ones. Stephen Spiller explores the implication of starting from interesting and paradoxical data rather than from established theory. The research challenge then requires a focus on strategic sampling of methods, responses, and critical constructs that confirm or limit a provisional theory. Aparna Labroo articulates the benefits of abductive thinking to help resolve complex practical problems, but warns against the proliferation of multiple findings that may be difficult to validate. Finally, Bublitz and Peracchio celebrate the value of abductive research to help resolve social issues and enable the fruitful merger of publishable research with personal social action.

KEYWORDS
Abduction, Commentaries, Consumer research, Method, Psychology, Theory construction

THEORY CONSTRUCTION IN CONSUMER PSYCHOLOGY BY FRANK R. KARDES

Janiszewski and van Osselaer (2021, this issue) provide an intriguing discussion of the role of abductive research methods in consumer psychology. I agree that the field needs a much greater emphasis on theory construction (and on theory, more generally, rather than on effects), and that iterative data collection is important. However, they characterize experimental methods too narrowly in terms of pure deduction, and they underestimate the utility of programmatic experimentation for theory construction. Programmatic experimentation in social psychology led to the development of cognitive dissonance theory, accessibility theory, construal level theory,

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attribution theories, social information processing theory, balance theory, lay epistemic theory, the elaboration likelihood model, the heuristic-systematic model, the stereotype content model, feelings-as-information theory, the linguistic category model, action identification theory, terror management theory, self-determination theory, the theory of planned behavior, social comparison theory, regulatory focus theory, self-control theory, self-verification theory, attachment theory, equity theory, system justification theory, social identity theory, and many others (Van Lange et al., 2012).

The concern that experimental psychological research focuses too heavily on theory testing and not enough on theory construction is quite old. To address this imbalance, McGuire (1997) outlined 49 heuristics for creative hypothesis generation, which he reduced to five major categories (e.g., of the use of these heuristics in consumer psychology, see Kardes & Herr, 2019). Many of these heuristics require induction and deduction, and many require iterative data collection.

For example, the simplest category calls for the careful observation of natural occurrences. Many marketplace phenomena are inherently interesting and inductively generating multiple possible explanations for a given phenomenon can lead to many insights. In my own work, a student asked me why pioneering brands exhibit an advantage over later entrants, and this led to the generation of many possible explanations and some experiments demonstrating a knowledge asymmetry on the part of consumers (Kardes & Gurumurthy, 1992). Consumers often learn more about novel pioneering brands, relative to less interesting copycat brands. Of course, this phenomenon and most robust phenomena are multiply mediated and moderated.

Introspection and retrospection can also be used to generate hypotheses based on observation. For example, why did I buy a product that I never use, and how do researchers in other industries or fields (e.g., biology, medicine, and engineering) solve similar problems? McGuire's inoculation theory is a good example of borrowing an idea from medicine and applying it to psychology: Asking people to counterargue weak counterattitudinal arguments increases attitudinal resistance to counterpersuasion. McGuire (1997) lamented that people tend to focus too much on surface details, and not enough on underlying abstractions, and this impedes reasoning by analogy.

McGuire's (1997) second general category requires direct inference, such as taking an obvious hypothesis and thinking about when the opposite could occur. For example, economics teaches us that demand increases as price decreases, but, in some circumstances, demand increases with price due to price–quality inferences. Furthermore, these inferences are consequential as they have been shown to influence non-hypothetical choices (Cronley et al., 2005, experiment 5).

Conceptually reversing the direction of causality is another possibility. For example, common sense tells us that attitudes influence behavior, but reversing the direction of causality led to decades of productive research on cognitive dissonance showing that behavior can also influence attitudes. The causal direction of nearly any independent variable and dependent variable can be reversed conceptually, and this can lead to surprising insights.

Pushing a hypothesis to an implausible extreme helps researchers to define the boundaries of a hypothesis, as recommended by Janiszewski and van Osselaer (2021, this issue). In my research program on omission neglect, or insensitivity to missing information, my students and I continually attempt to increase the importance of a product attribute that is not mentioned in a product description (Kardes et al., 2021). In some of our recent studies, we discovered that consumers frequently neglect hidden fees, such as roaming fees, when evaluating cell phone plans. This occurs even though most consumers later acknowledge that these fees are very important for evaluating cell phone plans. Consumers also neglect hidden baggage fees for discount airlines, and interest rates for credit cards, and many other types of missing information.

Conceptually reducing an independent variable to zero is another possibility. For example, what happens when price is zero (Shampanier et al., 2007)? Similarly, imagining that attitudes are zero or nonexistent led psychological researchers to recognize the importance of social norms and habits as drivers of behavior. Finally, McGuire (1997) recommends generating multiple possible moderator variables for a phenomenon, and this has become a common practice in consumer psychology. Moreover, this approach has led to the development of several theories unique to consumer psychology (Wyer & Adaval, 2008), consistent with one of Janiszewski and van Osselaer's (2021, this issue) key goals.

The third category uses mediated inference. This involves inductively generating many alternative hypotheses for a phenomenon and alternating inductive reasoning and deductive reasoning processes, consistent with Janiszewski and van Osselaer's (2021, this issue) recommendations. Many consumer psychologists routinely run multiple exploratory studies that are not included in a published article. These studies typically inform and refine the studies that eventually get published. If all studies were reported, it would be very clear that consumer psychologists rely on iteration quite heavily. Many of these studies focus primarily on confirmatory (rather than disconfirmatory) hypothesis-testing strategies, which is appropriate for testing non-absolute hypotheses (Sanbonmatsu et al., 2015).

The fourth category involves reinterpreting past studies and meta-analysis. The classic example is Bem's reinterpretation of the results of cognitive dissonance studies. Cognitive dissonance theory suggests that people are motivated to attain cognitive consistency, but Bem demonstrated that non-motivational self-perceptual inference processes can produce the same

attitudinal effects. Reinterpretation of past studies is also typically involved in demonstrations of moderated mediation, in which two (or more) different processes are shown to account for the same phenomenon under different conditions.

The last category involves exploratory qualitative and quantitative data collection and analysis, consistent with Janiszewski and van Osselaer's (2021, this issue) abductive approach. Using open-ended measures, actively participating in data collection (rather than relying exclusively on research assistants), pitting confounded variables against each other, programmatic research, multivariate fishing expeditions, subtracting out the effect of a known mediator, simulation, and modeling were some of the recommendations offered by McGuire (1997). Template matching (Bem & Lord, 1979) and the narrative approach (Adaval et al., 2019) can also be used to integrate qualitative and quantitative data.

To summarize, Janiszewski and van Osselaer (2021, this issue) should be applauded for encouraging a much greater emphasis on theory construction. Some of the procedures they advocate are currently used by consumer psychologists, and some seem inconsistent with current editorial practices. For example, many editors and reviewers encourage authors to exaggerate the novelty of their findings, discourage researchers from conducting conceptual replications and extensions (i.e., programmatic research), and encourage researchers to artificially inflate effect sizes through the use of demand-inducing procedures. Intrusive procedures such as collecting manipulation check and mediator measures prior to dependent measures can produce substantial demand effects. The remedy is the use of indirect measures and holdout samples in which participants are not exposed to potentially biasing manipulation check and mediator variable measures (Kardes et al., 2019). It is also possible to measure mediators after measuring the dependent variable, or better yet, to conduct a chain of experiments to test for mediation, rather than relying on correlational methods (Kardes & Herr, 2019). The use of intrusive demand-inducing procedures in some studies, but not others, and the war against programmatic experimentation on the part of editors and reviewers contribute to the replication crisis, impedes theory construction, and reduces scientific progress. Programmatic experimentation is essential to ensure that phenomena are replicable, to promote theory construction, and to advance the scientific study of consumer psychology.

ADVANCING ABDUCTIVE THEORY CONSTRUCTION BY ATTENDING TO CONSTRUCT MAPPING BY EILEEN FISCHER

Chris Janiszewski and Stijn van Osselaer's "Abductive Theory Construction" deserves the attention of every reader of this journal. Especially when read in conjunction with their recent treatise on the benefits of engaging in, and candidly reporting on the use of exploratory experimental research (Janiszewski & van Osselaer, 2021), the authors can be seen as inviting their fellow experimentalists to try something that could be regarded as revolutionary—deviating from purely deductivist research practices and openly embracing abductivisim in order to participate more fully in the creation of novel theory, and speaking of novelty, I believe that Janiszewski and van Osselaer's "intervention-based abductive theory construction" approach is something truly new. As someone very familiar with what they refer to as the "qualitative inductive-abductive theory construction" approach, I think their intervention-based approach is an exciting new—and potentially fruitful—direction for consumer researchers to explore. I want to see it succeed.

In that spirit, I offer a few remarks on one aspect of abductive research that received relatively limited direct attention in Janiszewski and van Osselaer's articulation of their intervention-based approach, but that is often a part of impactful theory creation: the mapping of constructs. I believe those who take up the challenge of following the intervention-based approach will benefit from thinking systematically about whether they need to map new constructs or remap existing ones to advance theory.

What do I mean by "mapping constructs"? This is a term Cele Otnes, and I (Fischer & Otnes, 2006) used to describe both the introductory conceptualization of new constructs, and the reconceptualization of existing ones. As Janiszewski and van Osselaer acknowledge, researchers who engage in qualitative inductive-abductive theory building frequently do so in part by introducing new constructs; I would add that they even more frequently "remap" existing ones by elaborating on their dimensions or properties. For an example of a new construct, consider the "brand community" concept introduced by Muniz and O'Guinn (2001); for an example of a reconceptualized construct, consider Fournier's (1998) contribution to refining the notion of "brand relationships." I choose these as illustrations not only because they are likely to be familiar to readers of this journal, but also because both these articles illustrate how impactful construct mapping kinds of theoretical contributions can be. Both these papers received "Best Article" Awards for having "made the greatest contribution to knowledge about consumer behavior" (https://consumerresearcher.com/awards/bestarticleaward), and both have in excess of 7500 citations according to Google Scholar as of August 2021. Given that Janiszewski and van Osselaer are urging consumer researchers trained in experimental methods to engage in abductive theory construction in part to increase the impact of their work, it makes sense to consider construct mapping as part of this undertaking.

In Janiszewski and van Osselaer's article, it is unclear whether they envision construct mapping as something that can be part of the approach they propose. On one hand, they appear to associate the introduction of concepts which have not previously been identified with what they label "the inductive research paradigm" which, given the distinctions they draw between induction and abduction, would suggest construct mapping could not be part of their intervention-based abductive theory construction. On the other hand, in their table on "Research Paradigms," they state that the approach they advocate uses "data irregularities to inform conjectures about potential explanations" and it is difficult to imagine that those potential explanations would never involve new constructs or revised versions of existing ones.

I believe one reason for the ambiguity regarding whether construct mapping is explicitly part of the approach that Janiszewski and van Osselaer are calling for lies in the fact that they seek to draw categorical distinctions between work conducted in inductive and abductive approaches, as is reflected in the discussion summarized in table on research paradigms. In their exposition, the authors assert that those scholars who they label "inductive researchers" do not seek to develop a formal theory" but instead "strive to document regularities in a substantive domain." This could be regarded as something of an unrealistic caricature of the research approach that is actually embraced by those scholars whose work Janiszewski and van Osselaer included in this category. This is because those who are associated with the interpretive turn in consumer research do not stick within the narrow confines of induction as portrayed here. Instead, they seamlessly mix induction with abduction to come up with new theory, which includes but is not limited to, introducing new constructs or remapping existing ones. To be fair, Janiszewski and van Osselaer do acknowledge the abductive, theory-generating, and nature of much research aligned with the interpretive tradition. What leads to some confusion is the section of the paper that equates work that "provides insights using depth interviews, ethnography, or netnography" with purely inductive research.

Leaving aside concerns that the work of those associated with the interpretive turn in consumer research has been somewhat mis-represented, my main point here is that experimental researchers to who seek to develop novel contributions via intervention-based abductive theory construction will need to be open to discerning new constructs or refining existing ones in the course of their work. Moreover, in order to do so, they may need to mix some inductive observation of situated behaviors with abductive inferencing to be successful in generating or refining constructs. It may not be sufficient to use "data irregularities" to inform conjectures about potential explanations for candidate phenomena. At a minimum, without an explicit recognition of the role of construct mapping in theory building, I believe the potential for this new approach to generate novel theory that addresses disciplinary dissatisfaction with "what is learned" will not be fully realized.

While on the topic of relevance, I will make one additional suggestion to researchers regarding a construct that I think is ripe for remapping. While there may be many constructs that deserve reconsideration, I believe that if we wish to ensure that both "what is studied" and "what is learned" do indeed generate consequential insights that address real-world problems, it may be helpful to rethink the very construct of "consumer." As I have mentioned elsewhere (Fischer, 2020), we need to recognize that people we regard as engaging in consumer behaviors are "enterprising entities." Often, what we label as consumption is not as an end itself but a means to some end, such as generating opportunities or income, think of aspiring influencers, or politicians or artists. Are not they just as much "consumers" as the people pushing their carts through the aisles of Whole Foods or the online shoppers putting Amazon through its paces? Yet, how often do we turn our gazes toward them and understand them through a consumer lens? The answer is "rarely" because our understanding of the construct of consumer requires remapping. Thinking about consumers more broadly will bring into view that people (a.k.a. consumers) are not just reacting to marketing stimuli; they are agentic actors who are combining resources to create outcomes of value to themselves. Sometimes, these actions have value for others, and sometimes, they destroy it. These are things we need to better understand.

To be clear, I am not calling for a reconceptualization of the construct of value co-creation through engagement with marketers (though this construct may be ripe for disruption too). What I am calling for is greater attention to the agentic, enterprising elements bundled together with the experiential and utilitarian elements that routinely attract attention when we study people as consumers. Of course, human agency is constrained, but until we open our eyes to the enterprise that is entangled with what we bracket as consumer behavior, we miss opportunities to develop insights that can help address the problems we as human beings are facing and the problems we are contributing to creating or accelerating. Turning our scholarly attention to such possibilities may well lead to research opportunities that are well suited to intervention-based abductive theory construction.

I look forward to seeing this new approach gain traction!

THEORY GENERATION AS MODEL BUILDING BY STEPHEN A. SPILLER

Decomposing variation in data into variation accounted for by a model and variation not accounted for by the

model (i.e., the error) is a guiding principle in data analysis, succinctly captured by "data = fit + residual" in Tukey (1977) or "DATA = MODEL + ERROR" in Judd et al. (2017). This simple-yet-powerful mental model of how to conceptualize the role of a model in data analysis provides a useful analogy by which we can consider Janiszewski and van Osselaer's (2021, this issue) proposal to generate theories of consumer behavior through abduction.

In a typical deductive research paper, a researcher starts with a theory or a model of the world. This theory generates hypotheses and, using data gathered through an experiment or otherwise, the researcher attempts to assess support for the hypothesis, often through the use of a null hypothesis significance test (with sometimes a bit of handwaving). Under a simplified version of JvO's abductive model, one starts with data, generates a theory to account for those data, and chooses what subsequent data to collect and how to collect it based on the thus far knowns and unknowns of the relevant theoretical space. A key question that faces the theoretician in JvO is analogous to the question that faces the analyst in Judd et al. (2017): given the variability in the observed data, what is to be allocated to the model and what is to be left out and allocated to error?

Whereas the data for a statistical model typically represent individual datapoints, the data for JvO's theoretician comprise not individual observations but rather particular *findings* or, as Abelson (1995) referred to them, *ticks*: "the detailed statements of distinct research results," reflecting "the use of tick marks for each separate point in a summary" (p. 104). For example, using JvO's example of intertemporal choice, we might say that value varies non-monotonically with time for hedonic activities, but monotonically with time for utilitarian activities. These two findings or ticks are then to be explained by the theoretician or else relegated to the error bin (capturing all variation not captured by the focal theory). One proceeds acting as though such findings can be accepted as local *stylized facts* within reasonable bounds (Kaldor, 1961).

A key difficulty in engaging in such an endeavor is establishing a sufficiently firm foundation on which a theory may be built without imposing the requirement that it necessarily must be tested. This raises important questions for both the researcher and the gatekeepers charged with evaluating the resulting theory: (i) Are the findings on which the theory is based sufficiently reliable? (ii) Which findings should a researcher pursue? (iii) Does the resulting theory provide a satisfactory explanation?

JvO lay out four criteria by which abductive theory construction ought to be evaluated:

1. the sampling breadth of the concepts investigated in the studies,
2. the congruence of the procedure with the domain being studied,
3. the degree to which the interventions are sufficiently diagnostic, and
4. the plausibility and insight provided by the original theory.

Identifying whether findings are sufficiently reliable is related to the interventions' diagnosticity. Identifying which findings to pursue is related to both the sampling breadth and procedure congruence, and identifying whether the end result is a satisfying integration is an assessment of the theory's plausibility and insight. I briefly discuss each in turn using the analogy to the data analysts' role.

Reliable findings and theoretical overfitting

JvO contrast the goals of data collection for abductive purposes with those of data collection for deductive research purposes. When collecting data for deductive research purposes, the researcher typically aims to minimize error and collect a sufficiently large sample, often in pursuit of a highly powered test of a key null hypothesis. This explicit contrast suggests a reduced emphasis on those aims for abductive theory generation, focusing instead on identifying patterns in the error.

Identifying patterns in the error is a powerful idea and important approach. Yet to the extent that those findings are intended to inform subsequent theory generation, it also has the potential to lead authors astray by chasing noise. JvO appropriately emphasize that statistical inference is not the goal in abductive theory construction, but to build an explanation to account for the observed facts, we must be confident in what those observed facts are. People see patterns in noise and researchers are no exception (e.g., Abelson, 1995; Meehl, 1990). While the motivation to observe a particular finding in a particular direction may be reduced in this case, it is unlikely to be entirely absent as a theory begins to cohere, making some findings less convenient than others.

Consider again JvO's example of what results might show from the first intertemporal choice study: non-monotonic value for hedonic activities but monotonic value for utilitarian activities. A key question is what "results might show" means. It could mean the observed means. Yet even if all six values had the same expected value but were measured with some measurement error, one would expect to observe monotonicity for one condition and nonmonotonicity for the other nearly half of the time (and *some* pattern 100% of the time!).

We are unlikely to rely on a mere difference in means: neither the researcher nor the gatekeeper is likely to be keen about a novel theory to account for a difference readily attributable to sampling error. Instead, "results might show" ought to refer to a locally generalizable finding such that for these activities (perhaps not others), this population (perhaps not others), and these

time intervals (perhaps not others); utilitarian activities generally exhibit monotonic valuation and hedonic activities generally do not. These would be the findings, ticks, or local stylized facts that the new theory ought to accommodate.

As a result, one needs to know that this pattern truly represents a set of stylized facts on which it is worth building. These stylized facts need not be universal, but they need to be sufficiently reliable as to avoid building a theory on a shaky foundation. Forgoing formalized statistical inference does not avoid the risk of inadvertently exploiting researcher degrees of freedom. As a result, one potential danger with building from noisily assessed stylized facts is *overfitting*; exploiting researcher degrees of freedom is one form of "procedural overfitting" (Yarkoni & Westfall, 2017). The danger is thus in generating a theory to accommodate mere noise in the data which ought to instead be smoothed over by the model.

Even if the abductive theory generation process excludes formal statistical testing, we cannot ignore the importance of sufficiently precise core findings. Some such findings are central to the theory, and some are peripheral. Even for nascent theory generation, researchers and gatekeepers would do well to ensure that the central findings can be admitted as established stylized facts to which the theory may be secured.

Searching the right places and useful variation

Drawing from Abelson's (1995) approach, one can sometimes look for a data *signature*, a coherent collection of research findings or ticks, which ought to follow from theory. The theory makes a set of predictions indicating what to look for, and the signature either does or does not support it. In intervention-based abduction, the set of findings to search for is necessarily a moving target: The theory is generated in part to account for the signature, but until the end of the process, the signature is unknown and incomplete. As with any good mystery, there will be not only diagnostic clues, but also red herrings and dead ends. What is a researcher to do?

To assess how a dependent variable varies with an independent variable, the analyst needs useful variation in the independent variable. For a theoretical model, how do we generate useful variation in the sets of contexts, stimuli, and populations to assess how the findings vary? One defensible norm, on both theoretical and empirical grounds, is to collect information to maximize one's expected *change in belief* (e.g., Nelson, 2005): after all, testing a case in which the outcome is already known is not particularly useful. Broadly, this suggests targeting interventions that (a) are highly diagnostic about core questions in the candidate theory and (b) have highly uncertain outcomes.

This principle of sampling to maximize expected change in belief directly relates to JvO's exhortation to examine a diverse set of stimuli, populations, domains, and measures (e.g., Wells, 2001). All else equal, expected change in belief is greater for parts of the stimulus space that have not yet been explored. Deliberate sampling for heterogeneity may be particularly powerful to examine stability across various background factors. Changing multiple background factors at once can increase one's belief in the generalizability across a set of factors if none of them interact with the focal intervention; if one or more does interact, one may be left with a difficult attribution problem requiring additional examination (Lynch, 1982).

Assessing expected change in belief requires knowing where one's beliefs stood before the results were known. As a result, for the data collection endeavors to make sense to readers (including gatekeepers), the underlying logic that led to the decision to conduct those tests in those circumstances is important. This helps to put the findings in context: The dog that did not bark (or the intervention that did not cause) is a more meaningful clue in the context of what one believed in advance. Perhaps, even more so than in other research projects, decisions for intervention-based abduction are likely to be path dependent, so reporting sequence matters.

Satisfying integration and meaningful predictions

JvO note the theory generated through intervention-based abduction will likely be a "mid-range" theory and therefore ought to account for many, but not all, of the reported findings and findings from the literature. This leads to the final question: Does the novel theory lead to a satisfying integration?

There are two distinct forms of integration that ought to be addressed. First, how well does the new theory account for findings and make meaningful predictions? As JvO's rich example suggests, accounting for a full collection of findings, or signature in Abelson's (1995) terms, makes a more compelling case than a simple directional pattern. Meehl (1990) advocates for the importance of making *risky predictions*: "the working scientist is often more impressed when a theory predicts something within, or close to, a narrow interval than when it predicts something correctly within a wide one" (p128). While new consumer behavior theories are unlikely to generate truly risky predictions (e.g., a narrow range in which a parameter is expected to lie), some predictions are riskier than others. Such riskiness makes the predictions more meaningful and the integration more satisfying.

Of course, one can precisely account for any pattern given a sufficient number of free parameters and flexibility in relating them. An imprecise analogy to data analysis is useful here: to reduce overfitting, analysts sometimes penalize non-zero coefficients (e.g., Yarkoni

& Westfall, 2017). For nascent theories in particular, one may wish to penalize complexity until the theory's core stands on firmer ground.

Second, how does the new theory relate to existing theories? This is perhaps the most difficult criterion for a new theory to face. As JvO adroitly note, advanced research streams in consumer behavior often lead to theory proliferation rather than theory convergence. It is useful to evaluate how the proposed theory ought to be considered relative to the context into which it is introduced. Does it coexist, perhaps speaking to considerations not yet addressed? Or does it aim to reconcile or replace? In the long run, one might hope that taking an explicit stance on the relation of the proposed theory to the background into which it is introduced can reduce the extent of unnecessary proliferation without occasional trimming.

Conclusion

JvO set an admirable challenge for researchers and gatekeepers alike: to generate novel, productive consumer theory through intervention-based abduction. This requires researchers and gatekeepers alike to consider which facts can be accepted as known, which knowable facts ought to be known, and which known or knowable facts ought to be explained. While theory-testing and theory-generating papers are perhaps ideal points, in practice, researchers sometimes find themselves in between, seeking abductive explanations in theory-testing papers. A broad range of researchers would benefit from taking JvO's lessons to heart and considering the implication of the unmodeled errors across their findings, even when stopping short of new theory generation.

MUCH ADO ABOUT ABDUCTION? FROM PITFALLS TO POSSIBILITIES BY APARNA A. LABROO

Janiszewski and van Osselaer (this issue) make a case that the deductive method (i.e., top-down theorizing that prespecifies hypotheses and tests) that is popular among quantitative consumer researchers (those who run experiments, field studies, and surveys) orients researchers toward testing narrow applications of existing theory rather than toward generating novel theory. It also increases pressures on researchers to find evidence confirming their hypotheses, which increases their pressures to p-hack (i.e., run multiple tests but selectively report the handful that "work," Simmons et al., 2011) and publish false-positive results. To encourage novel theory generation and improve research replicability, Janiszewski and van Osselaer (this issue) propose abduction—an "iterative investigate–learn–update process that generates, but does not rigorously test, a candidate theory"—as a replacement to deduction.

In abduction, researchers conduct "informed exploratory studies" and employ a "breadth of domains, procedures, populations, and analyses" to create "broad, boundary-defining research." Abduction thus begins by proposing a question, but then, it investigates multiple datasets, populations, and methodologies. By progressively learning from whatever the data reveal from one variable to the next in each dataset, from one population to the next, and from one methodology to the next, researchers can then propose, but leave others to test more rigorously, their new theory.

This commentary first defines some difficulties with abduction. It next describes some strengths of deduction and closes proposing that abduction can be valuable when working with rather than against deduction.

Problems using abduction to generate novel replicable theory

Abduction may on its own be insufficient to generate novel theory. Informing this possibility, Janiszewski and van Osselaer (2021; p. 3, table 1) find that quantitative consumer researchers report already employing abduction about 74% of the time when conducting research. Pure deduction is used only 25% of the time. Thus, abduction, rather than deduction, is the dominant research method presently followed by quantitative consumer researchers. If novel replicable theory generation is absent in consumer research, then an absence rather than presence of pure deduction, and a presence rather than an absence of abduction, corresponds with it. Furthermore, the missing novel theory problem with deduction may also not be as serious as implied. The base rate of novel theory may be low because there are few generalizable psychological principles that illuminate fundamental aspects of human thought or behavior. Subsequent papers typically test these theories for variability across populations, methodologies, and situations, referred to as theory application. As there are many contextual variables, there are many such papers, whether researchers currently underindex in generating novel theory or over index on theory applications compared with how many are out there is unclear. Deduction therefore may not be the cause, and abduction not the panacea, to solving the missing novel theory problem in consumer research.

The usefulness and the type of possible theories that an investigation of contextual variance (broad theory building—i.e., abduction) as opposed to a search for generalizable principles (building broad theories—i.e., deduction) might deliver is also unclear. In deduction, researchers propose a causal relationship between two constructs, and they then operationalize these constructs in different ways across studies, varying only one

element at a time and holding all background factors constant. Holding background factors constant inflates the observed study effect sizes relative to the true population effect sizes, but across studies, through triangulation and converging evidence, deduction provides confidence that the proposed constructs are causally related (construct validity). Across papers, by documenting contextual variance, deduction demonstrates theory generalizability (external validity).

Instead, abduction encourages the exploration of variance generated simultaneously by a multitude of known and unknown elements associated with changes in time and population contexts across studies. Inferring with confidence why an observed association changes, what element most accounts for the change, or the extent each of several elements that covaried do so, becomes unclear. The more different the contexts, the more substantive the error exploration, the lower the confidence in what readers can infer or learn. Because time and population cannot be assigned to at random, any causality claims may also be tenuous. As a result, an intended psychological theory of generalizable behavior may instead turn out to be a theory of context.. For instance, in computer science, to investigate a theory that facial characteristics are predictive of criminality, Wu and Zhang (2016) compared diverse minute facial features of numerous known criminals against those of non-criminals. They found criminals have smaller angles between the nose and the corners of the mouth. Subsequent critiques pointed out convicted criminals seldom smile in mugshots, whereas non-criminals typically do smile in photographs, which can account for this result, revealing this stimuli-driven finding to be a theory of context.

Turning to the issue of abduction improving replicability, Janiszewski and van Osselaer (this issue) suggest that removing confirmatory testing biases through abduction will increase publication of replicable findings. Confirmatory testing is not the sole or primary cause of replication failures. The unconstrained testing of an infinite combinations of possible outcomes, variables, contextual factors, and populations, in a quest to publish the most provocative findings, can also encourage publication of false positives. A classic example is Bem (2011) that identified an ambitious problem to solve and presented exploratory studies in nine social-cognitive domains. The research followed an iterative investigate–learn–update process where each result justified new ways to explore a revolutionary effect. Unsurprisingly, the studies later failed to replicate (Ritchie et al., 2012). This example illuminates where unconstrained data exploration can lead science.

In confirmatory deductive testing, false positives typically are constrained by prespecifying the set of variables to be tested. In abduction instead, the identification of surprising contexts is an important measure of the value and breadth of the theory. However, this approach can encourage unconstrained testing and exploration of multiple associations. It is also possible that only the most interesting subset of results will be reported and that those reported are more likely to be false positives. Abduction could thus magnify the replicability crisis, especially when known theory and established relationships are not used in the test, adjust, retest cycle. Consequently, abduction may require even stronger deductive training if one desires to create new theory, which could also increase confirmatory testing biases. Notably, almost all the examples Janiszewski and van Osselaer (this issue) provide of abduction demonstrate a nuanced understanding of existing theories and data, a rare talent to integrate these insights into a metatheory, to then elucidate one's own novel theory, and then to suggest specific confirmatory and predictive tests of their theory, all *before* observing or testing any data.

Since the usefulness of abduction is enhanced by researchers who clearly relate theory to the differences, they expect across contexts, that suggests most investigations could be preregistered. While preregistration may slow down initial discovery, it could also improve the quality of discovery. Having to articulate predictions might increase researcher attention to the problem, their in-depth consideration and integration of known knowledge into a metatheory, and the choice of more appropriate variables, populations, and contexts. Furthermore, preregistrations need not be limited to exact predicted results (predictive testing), but instead can specify exploratory tests that broadly prespecify comparisons of interest so that Bonferroni and other corrections can be applied as necessary (planned exploration), as both these inspire more confidence than completely unplanned tests (data mining). Rather than to disallow researchers from publishing findings different from preregistered ones, the goal would be to increase transparency regarding the type of test conducted and the confidence readers can have in a result.

To increase confidence that an effect exists, any counterintuitive findings from *data mining* should be preregistered and replicated exactly with a similar population context when possible. Alternatively, robustness checks could be conducted by checking holdout samples that were created in advance, simulations that provide insight into likelihood of occurrence of the finding, or conceptual replications with other datasets that are preregistered. Preregistered *planned explorations* need not be replicated exactly if consistent results across multiple similar (but not dissimilar) variables are found, and appropriate post hoc multiple-comparison corrections are employed. Here, preregistered *exact predictive tests* of conceptual replications that additionally increase confidence in construct validity may be more useful. Notably, as *predicted exact tests* are planned, researchers should be allowed to report one-tailed results for these. In some data-rich, truly substantive, and complex domains, making a handful of initial predictions may not be possible, and in these situations, perhaps one final study

with the most important or provocative result could be preregistered.

A question that merits discussion then is what would count for acceptability and what should the stop rule for an abductive paper be? Without clear stop rules, some reviewers might push authors into endless cycles of revisions and ultimately reject papers after several rounds because they expect more or better domains, methods, or theories, and have insufficient confidence in the proposed explanations. The ideal abductive paper may be so complete that it may constrain rather than aid identification follow-up research. Other reviewers may accept purely exploratory, possibly unreliable, theories that then fail to replicate which could lead to diverging standards of publication and increase author frustration with review process.

In sum, abduction may be insufficient on its own to generate novel replicable theory. It appears to be a dominant methodology, it potentially might orient researchers even more toward theory application, and there are questions of interpretability of any comparisons it reports because of multiple differences across studies. Whether it would improve replicability is also unclear.

In defense of deduction: Can it be replaced by abduction?

Despite these pitfalls, abduction arguably could replace deduction if it is significantly better at novel replicable theory generation, but that also may not be the case. By essentially defining deductive research as narrow applications of existing theories, Janiszewski and van Osselaer (this issue) draw lines in the sand for what they consider to be, and not be, deductive consumer research. They exclude from their consideration exemplars of deductive research that employ their suggested approaches to generating novel theory—i.e., to *counter* existing theories, to *reconcile* multiple conflicting theories, or to agnostically test two *competing* theories—ascribing these solely to abduction.

But psychology is rich with examples of deductive research employing these three approaches, generating perhaps as many novel theories as prominent psychologists. Even among quantitative consumer researchers, there are ample examples of new theory generation of all three approaches. For instance, proposing novel theories *counter* to existing theories that depletion reduces overall disbursement of cognitive resources, Agrawal and Wan (2009) showed that depletion instead increases selective disbursement of cognitive resources. *Reconciling* multiple conflicting theories that a positive mood can increase or reduce self-control; Fishbach and Labroo (2007) theorized instead it increases flexible pursuit of any accessible goal by spotlighting opportunities. Testing *ambiguous theories*, Lee (2001) found that uncertainty reduction rather than mood misattribution underlies the mere exposure effect—the finding that preferences for stimuli increase after repeated exposures. Novel theories generated by quantitative consumer researchers have even been picked up in psychology. For instance, *counter* to prevailing views that the objective amount of willpower a person has impacts self-control, Mukhopadhyay and Johar (2005) showed that self-control relies on whether willpower is construed as a depletable resource or not. Job et al. (2010), Inzlicht and Schmeichel (2012), among others, later applied this theorizing to other contexts.

Deductive theory-generating research may also sometimes be misclassified as applied. For instance, Wood, McInnes, and Norton (2011) showed that traffic fatalities are higher after close games in the winner's hometown. This result could be mistaken as theory application; however, the authors specifically tested two *competing* psychological theories, but in an applied domain—does drinking more from *boredom* with drawn out games, or does *testosterone* from winning close games, increase fatalities? To confirm their preferred hypothesis that testosterone accounted for fatalities, they showed more of these occur only after close games and in the winner's hometown, regardless of whether it is a home game or not, when and where testosterone would run high. Some may disagree these examples constitute theory generation versus application, but then, one could also question Janiszewski and van Osselaer's (this issue) examples of what does constitute it.

Deduction thus does not lack instances of novel theory generation. To increase *novel deductive theory generation*, an alternative to replacement could be to improve the methodology. As a field, we could increase the awareness and training among researchers to themselves ask and appreciate from others theory-generating questions. Furthermore, the *replicability* problem with deduction could also be resolved more easily through improvements that encourage preregistration, conducting of higher-powered and better planned/executed studies, and endorsing of data sharing practices.

The possibilities: Enhancing science with abduction

The usefulness of abduction emerges when it complements rather than substitutes for deduction in generating novel theory and understanding its bounds. Deduction looks for depth, and depth is invaluable in providing confidence through triangulation and converging evidence, that a proposed generalizable relationship between two constructs exists, but assuming away heterogeneity through randomization and tight experimental controls might inflate study effect sizes relative to population effect sizes. Abduction instead makes differences—the investigation of "noise"—its

focus of investigation. Breadth is invaluable for robustness testing by documenting effect size differences across contexts and establishing the scope and bounds of theory application. In the trade-off between confidence and scope of applicability, deduction favors the former, whereas abduction favors the latter. Because abduction is designed to explore and learn, as it may provide better insights than deduction when it focuses on certain kinds of problems that Janiszewski and van Osselaer (this issue) identify.

First, abduction is most useful for largely *unstudied* domains/populations with problems *specific* to their population context, such as drug abuse, crime, homelessness, or poverty. Here, deduction may be impractical because enough knowledge does not exist, randomization is difficult, and generalizing to other domains/populations is not meaningful. Uncovering contextual variations in behaviors/judgments specific to that population/domain while employing multiple methodologies may instead provide invaluable insights.

Second, abduction may be more suited than deduction to understand substantive *complex* domains, such as charitable giving, financial decisions, or medical decisions. Those domains comprise rich ecosystems of interdependent actors, choices, influences, and outcomes where experimentally controlling variation provides insights of limited value. For instance, tightly controlled tests of how to push one motivational lever to get people to donate small amounts of money may offer limited practical value, particularly to nonprofits and organizations observing donors facing trade-offs between different types of causes, and uncertain about how and when to donate (Labroo & Goldsmith, 2021). By exploring substantive domains more *realistically*, abduction is likely to provide richer, more actionable insights.

Third, abduction may be especially useful for answering *longitudinal* questions, such as life experiences, habit formation as people react to different experiences over time (Labroo & Goldsmith, 2021). Because variation over time is the central question being asked, the other sources of variation in panels of populations and contexts can be prespecified, and interpretability of the data and confidence can be high. Indeed, Janiszewski and van Osselaer's best examples of abduction pertain to *unstudied* domains (augmented/ synergistic consumption), *complex* substantive domains (prosocial behavior), or *longitudinal* effects (intertemporal choice).

In sum, abduction can serve as a valuable option for researchers interested in addressing problems in domains where theory testing is nascent, too complex to test, or too variable to study. Working alongside rather than instead of deduction also can encourage the integration of findings into metatheories. Good research can take many forms: Theory generation, theory application, or solutions to substantive problems and research should be evaluated for how important the question of interest is and how credible the insight provided is, rather than why it does not provide a certain type of contribution (Mukhopadhyay et al., 2018).

ADVANCING RESEARCH WITH SOCIAL IMPACT THROUGH ABDUCTION BY MELISSA G. BUBLITZ AND LAURA A. PERACCHIO

> Change will not come if we wait for some other person or if we wait for some other time. We are the ones we've been waiting for. We are the change that we seek.
>
> U.S. President Barack Obama

Hunger. Poverty. Racial injustice. Gender inequity. Climate Change. All these societal ills compounded by the worldwide COVID-19 pandemic. How can we as individuals—as people, as community members, and as those who care about our collective future—act to address these challenges in our daily lives? What can we, as consumer psychologists and marketing academics, do to use our professional skills and knowledge to help find solutions to these pressing social problems? Today, more researchers in consumer psychology and marketing are seeking ways to use their talents, passions, and abilities to conduct research that is focused on ways to create societal good (Mick et. al., 2012; Nardini et. al., 2021; Ozanne et al., this issue). In their article, Janiszewski and van Osselaer (this issue) add their voices to this call for "research that yields consequential results that address real-world problems." With their introduction of Abductive Theory Construction to our field, Janiszewski and van Osselaer (this issue) offer us new possibilities to do research that will have positive social impact.

Conducting research with social impact

We, perhaps like you, considered the pursuit of social good to be something we thought about and acted on in small ways in our personal lives—for example, by volunteering in our communities, tutoring at local schools, or tending a community garden. We saw this personal social impact work as siloed, separated from the research pursuits in our professional life. In working together, we have found the intersection between these personal and professional silos, acted on our shared interests and concerns, and pursued opportunities to conduct research focused on social good (Bublitz et al., 2021, 2019a, 2019b; Nardini et al., 2021). We offer a visual depiction of this process in Figure 1.

Why and how did we take action to bring together the personal and professional to focus on research with social impact? Here is a brief look at our individual and collective personal and professional journeys.

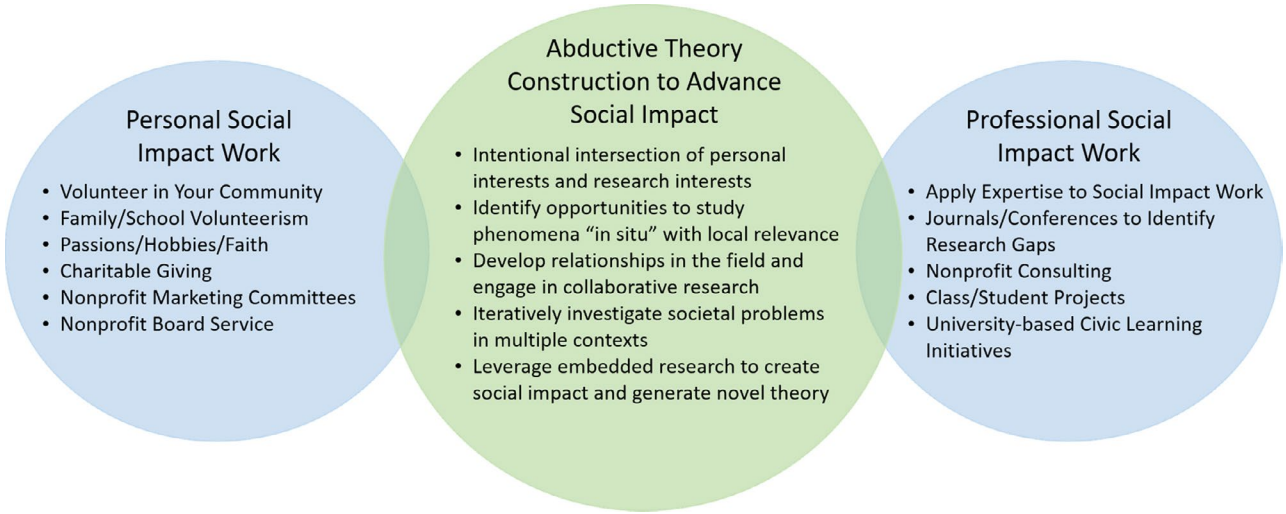

FIGURE 1 Merging personal and professional silos to advance social impact

Laura: A family story I was told as a child influences my life and research. When she was 16 years old, my great-grandmother Elvera and her 18-year-old sister emigrated together from Italy, traveling by themselves from their home in the hills of Compania to New York City. Years later, when asked by my uncle upon his return from a holiday in Italy, "Nona, how could you leave such a beautiful place?" Elvera responded, "You say that on a full stomach." Hunger and poverty propelled Elvera and her teenage sister to take risk by leaving their home for the possibility of a better life. In my life, family, food, and giving back are central. For many years, my family and I have volunteered with a local food bank, Hunger Task Force, collecting jars of peanut butter, sorting cans of food, and packing holiday food boxes.

Melissa: My mom is an avid gardener and an excellent cook. Growing up, we did not have much, but I never experienced hunger. As a child, I saw working in our garden as a chore, one I dreaded and complained about. Yet years later, I realized that garden was a critical lifeline for my family, providing food during times when my parents struggled to make ends meet. My parents instilled in me a strong work ethic and a sense of responsibility to serve others, and I have shared those ideas with my own children. My family has volunteered with our church at meal-packing events and in local food pantries. I have even passed down the dreaded "chore" of weeding the garden. After Laura introduced me to Hunger Task Force when I was a doctoral student, I shifted my research focus on food decisions to food access and the global problem of hunger.

Collective: In our research focused on social impact, we found a joint intersection between the things we care about. By integrating the personal and professional, we merged these silos and identified the sweet spot at the intersection of the two. We built on the relationships we had fostered in our personal lives with nonprofits such as Hunger Task Force to forge research partnerships grounded in Ozanne et al. (this issue) call to create multi-project relational engagement relationships with nonprofits.

If you are thinking about doing research with social impact, we encourage you to consider weaving synergies between the personal and professional in your life. Fortunately, Janiszewski and van Osselaer (this issue) offer us new paths to conduct research with social impact. Specifically, abductive theory construction suggests a way to study phenomenon using a series of iterations to observe, investigate, explore, and develop novel theories while expanding the possibilities for research to create not only local, but also broader, societal impact.

Abductive theory's contribution to research with social impact

Janiszewski and van Osselaer's (this issue) work is motivated by the goal of broadening our thinking about how to design and conduct theoretical research with relevance. They make clear that we, as researchers, currently are using a limited set of tools in our professional journey to advance knowledge. In their article, the authors not only build a case for expanding our methodological tool kit, but also provide a stepwise process to pursue abductive research, describe exemplars that spotlight abductive research, offer vivid illustrations of how to leverage this approach in consumer psychology, and—importantly—detail a set of guidelines to help reviewers and journal editors make space for this new way to build a theoretical contribution. Beyond challenging our thinking, Janiszewski and van Osselaer provide a path for how we can take abductive research from idea

to action. In this, we see clear potential for abductive theory construction to further research with social impact.

Janiszewski and van Osselaer describe how "the abductive research paradigm encourages an exploration of potential construct relationships" without a vested interest in a particular explanation (p. 41). As a result, researchers using abductive methods are more likely to explore alternative explanations that may not fit neatly into an existing theoretical framework. For example, studies on complex societal problems such as hunger and poverty that have many causes and complex interactions may benefit from such a methodological approach. Current theories to explain these phenomena elude us and investigations into applied solutions that seem to make headway in one community or locale often do not work in another.

Complex social problems, problems that bleed into so many other related problems, are "grossly similar but discretely different" in each locale (Kolko, 2012). As a result, it is all but impossible to develop one single template to solve complex social problems. Here, abductive research can be particularly powerful given that its methods are by their nature "subjective, local, and sensitive to the populations that are investigated" (Janiszewski & van Osselaer, this issue). The authors acknowledge that others may consider this a limitation of abductive research. Instead, and particularly within the context of research with social impact, we see this as a superpower of abductive research: It is "locally actionable," thereby allowing researchers to better understand local communities and the unique challenges of people in a specific place. As David Brooks (2018) suggests, the solutions for social problems can be found on the ground in communities, "at the tip of the shovel, where the actual work is being done."

We see synergies between Janiszewski and van Osselaer's article on abductive theory construction and the Ozanne et al. (this issue) article on expanding pathways for conducting research using a relational engagement approach. Both works suggest ways to advance research with social impact. A relational engagement approach encourages academics to develop partnerships with "relevant stakeholders" ranging from policymakers and business leaders to nonprofit organizations to produce knowledge that is "scientifically robust and socially relevant" (Ozanne et al.,this issue). Working with stakeholders embedded within the field in a relational engagement partnership provides a ground-level view of social issues. It also forges relationships that have the power to put research insights into action to iteratively test and learn from ideas designed to solve those problems. In our relational engagement work with our research partner, Hunger Task Force, we gained a tip-of-the-shovel perspective on the intricacies and complexity of how emergency food access programs work in concert with a web of federal and state programs to provide food access (Bublitz et al., 2019a, 2019b). This iterative approach is at the heart of the abductive theory construction process Janiszewski and van Osselaer so comprehensively describe.

Janiszewski and van Osselaer begin their illustration of abductive research methods with a description of our research with 15- to 24-year-old social entrepreneurs working to eradicate pressing societal problems such as climate change, gun violence, and racial injustice. Each one of these youth changemakers founded an initiative that created measurable social impact, and organized and led a team to advance the initiative's work (Bublitz et al., 2021). As Janiszewski and van Osselaer (this issue) point out, this research follows the tenets of abductive theory construction and offers a theoretical contribution via the presentation of "a systematic reorganization of an entrepreneurial ecosystem" (p. 17.) In addition, this research offers guidance that can encourage parents, educators, and other influencers to support youth in their quest to be changemakers. As we document in Figure 1, in this research and other projects, we merge personal and professional interests to advance social impact.

Thanks, Chris and Stijn, for giving us another way to conduct research with impact via abductive theory construction. We call on our consumer psychology and marketing colleagues—we call on you—to seek out opportunities to conduct research with social impact at the intersection of your personal and professional interests. Give research with social impact and abductive theory construction a try. Yes, there are risks in trying something new. One of the youth changemakers we partnered with, Eli Nichols of Everytown for Gun Safety, explained that risk tolerance is vital to the success of young social entrepreneurs, noting that "Youth are not afraid to take bold action." Or, perhaps like us, youth sometimes are afraid, but nevertheless believe the potential for social good outweighs the risks and go on to act despite their fear. Finally, we leave you with the words of 21-year-old Katie Eder, founder of Future Coalition, our partner for our youth changemaker research. Katie advises, "You have ideas that can make an incredible impact on the world today, big or small. There will never be the perfect time. You will never have enough knowledge. If you have an idea—make it happen now. Take action. Create change."

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