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In Defense of Bumblng

JOSEPH W. ALBA

Throughout its history, consumer research has expressed nonuniform levels of affinity for alternative scientific styles. Fondness for the theory-oriented hypothetico-deductive approach has been understandably high, as there is much to recommend it. However, no approach is without shortcomings, and alternative approaches may offer unique avenues to knowledge development. The observations contained in this article are meant to illustrate some disadvantages of a monolithic view and, in so doing, foster tolerance of a research style that has been less favorably received.

Over the years, several of the disciplines that serve as inspiration for consumer research, particularly the many branches of psychology and decision science, have engaged in a considerable amount of cross-pollination. This pleasing convergence, however, masks some important differences that existed at the time consumer research was gaining its balance. For example, 30 years ago a narrowly trained cognitive psychologist would have reacted with curiosity to a discussion of “manipulation checks,” because early-day cognitive research comprised such now-forgotten manipulations as the stimulus-response interval, the number and type of stimuli that preceded or followed a target stimulus, and the nature of the memory measure—none of which was ambiguous or in need of verification. These idiosyncrasies of method, however, were symptoms of more fundamental differences in substance. The same cognitive psychologist would have been likewise mystified by the consumer researcher’s investigation of “constructs.” As reflected in the verbal learning paradigm, its associationistic accounts, and its functionalist leanings,

much early work on memory was conducted under the umbrella of behaviorism. Memory was taken to be a function of the learning environment and the individual’s history (Watkins 1990), neither of which taxed the descriptive powers of the investigator.

It is no secret that consumer research has rarely aligned itself with the functionalist perspective. However, affection for functionalism has not been completely drained from our science, and those whose sympathies lie elsewhere should understand that functionalism is not a cult and that its current lack of popularity is more properly understood as a matter of taste rather than scientific legitimacy.

It is also no secret that consumer research has blossomed over these years, often with the assistance of constructs, in part because consumer researchers are broad-minded about substance. However, one might argue that the discipline has also engaged in some counterproductive behavior. The problem revolves around the question of the appropriate role of theory in the pursuit of scientific progress.

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EDITORS’ NOTE.—This article was invited. It is adapted from the author’s 2010 ACR Fellows talk. While it is not customary for *JCR* to publish Fellow addresses, this article was invited because it contributes constructively to the reappraisal of *JCR*’s reviewing and publishing criteria that were first articulated in the editorial that appeared in the April 2010 issue of this journal. We believe this article contributes similarly to the discussion of the multiple routes to a contribution appearing in the June 2011 editorial.—Mary Frances Luce, Ann L. McGill, and Laura Peracchio

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WHAT IS THEORY?

An assessment of the virtues of theory requires agreement about its meaning. Experience suggests that theory is used in at least three ways by consumer researchers. The first is as a formalization, a view that conforms most closely to an uncontroversial textbook definition, as in “a statement of relations among concepts within a set of boundary assumptions and constraints” (Bacharach 1989, 498). A second use of the term is as a synonym for process, such that a request for theory is in reality a request for a description of the cognitive or affective processes that underlie some reported effect—a quite sensible desire rooted in the intellectual imperative to understand a phenomenon to its fullest. Finally, theory may correspond, as a research style, to the hypothetico-deductive approach. The wide popularity of this approach, which too suggests a lack of controversy, is plainly

evident in the literature in the form of explicit and extensively reasoned *a priori* hypotheses.

One can acknowledge the validity of each of these views while also questioning the need to conform to any of them. Contributions can be made in the absence of a complex conceptual scheme, process evidence, and one-tailed hypotheses, albeit at the personal cost of being labeled a “bumbler” (Wegner 1992). Indeed, anecdotal evidence that progress can be made beyond the boundaries of theory orthodoxy is abundant in the natural sciences, where some of the most profound achievements were realized through induction and observation (see, e.g., Barwise 1995; Ehrenberg 1995; Peter 1991; Rozin 2001; Watkins 1990). Closer to home, it is Greenwald (2004, 275) who has “long questioned the wisdom of a principle of proper method in psychology that is now widely advocated—the belief that empirical research is valuable only to the extent that it advances theory. . . . Among the important works that have managed to achieve publication without advancing theory are some of the major works by Asch (conformity), Sherif (norm formation), Milgram (obedience), and Zajonc (mere exposure).”

WHY THE OBSESSION?

In trying to understand the emphasis on theory in consumer research, at least three explanations come to mind. Each has its merits, but it is unclear that alone or together they justify devotion.

Good Intentions

It is widely acknowledged that research in the fields of business, including marketing, was far less sophisticated 40 years ago than it is today. The stinging criticisms leveled by prestigious organizations prompted colleges of business to shift from a vocational to a scientific mode, which, from a research perspective, translated into a shift from a descriptive to a theoretical orientation (see MacInnis and Folkes [2010] for a discussion).

Given the then state of the art, it is difficult to find fault with this shift, but there is much irony in the zeal—and narrowness—with which the theoretical approach has been promoted in our discipline. As Rozin (2001, 2006) has argued, theory testing is relatively rare in the so-called hard sciences that consumer research wishes to emulate. Emphasis on experimentation vies with an emphasis on description, and research is often motivated by a simple desire to probe the existence of a relationship between two variables. Graphs, which exhibit a strong relationship to the perceived hardness of a science, tend to be used toward descriptive rather than theoretical ends (Smith et al. 2002). These natural sciences recognize the evolutionary nature of science, embrace its multiple objectives, and are unembarrassed that a scientific article can serve simply to report a set of empirical observations (Thomson 1994).

How is it that we have deviated from the practices of science while believing that we were conforming to them?

Although our original intentions were laudable, we may have committed two errors of execution. First, we may have overinterpreted the call for theory. Business research in previous generations was at its worst when its descriptions lacked generalizability and broad insight. However, neither generalizability nor profundity is isomorphic with theory. Phenomena, as noted later, are generalizable by their nature, and important phenomena spark the imagination.

Second, the consistent emphasis on theory in the ensuing years converted a notion into a rule, and rule-based behavior incurs the risk of mindless application (cf. Baron 1994; Langer 1989). Reflexive obedience to theory can be self-defeating and, in some contexts, immoral. We would question the values of a discipline that withheld publication of a welfare-enhancing discovery. We should care little about whether a discovery stems from the predictions of a formal theory, the meeting of a serendipitous outcome and a prepared mind, or simple luck. A cure is a cure, even when the mechanisms of the cure are not fully understood—and there has been no shortage of discoveries in the natural sciences that owe their existence to good fortune (Myers 2008).

Mature sciences exploit the multiple avenues to discovery. Consider Mukherjee’s (2010, 45) characterization of cancer drug identification: “Traditionally, three strategies have produced anticancer drugs. The first relies on serendipity: someone hears of a chemical that works on some cell, it is tested on cancer and—lo!—it is found to kill cancer cells while sparing normal stem cells. The second approach involves discovering a protein present or especially active in cancer cells—and relatively inactive in normal cells—and targeting that protein with a drug. . . . The final strategy involves identifying some behavior of a cancer cell that renders it uniquely sensitive to a particular chemical.”

These “strategies” map roughly onto luck, deduction, and induction, respectively. The flexibility demonstrated in other fields begs the question of whether consumer research is more rigid simply because its discoveries are less consequential.

Confidence

Inexplicable empirical effects engender skepticism, and therefore requests for a process explanation are not unreasonable. Confidence in the reliability and validity of a relationship between two variables can be enhanced by a plausible causal explanation (Alba and Hutchinson 2000). Again, however, faith in the ability of theory to achieve its objective may be misplaced. In the social sciences we are fond of noting that no theory is likely to be correct and that a theory endures until it is displaced by a superior theory. Hence, just as the production of reasons for an outcome can lead to overconfidence in its likelihood, we should acknowledge that theory can provide a false sense of security—if not mortification. As Steven Weinberg, Nobel prize winner, ruefully noted after devising a theory to explain the reported discovery of (nonexistent) trimuons, “I’ve always been embarrassed that we managed to come up with a theory” (Overbye 2007).

We might dismiss embarrassment as infrequent, but the same is not true of frustration, which can arise when theories are abundant. Watkins (1990) identified a period in the history of memory research that he dubbed “an era of cheap theories,” an era that accompanied the ebb of functionalism and a growth in the popularity of memory constructs. Each of these many theories was sophisticated, but clearly not all (or any) were correct. Students who were compelled to learn these theories are particularly sensitive to the havoc that can be wreaked by intelligent people armed with a large toolbox of constructs.

A final irony of the confidence-driven call for theory involves research from within our own field regarding the time at which a process explanation is offered. When process is articulated a priori, it conforms to our hypothetico-deductive preferences. When offered after the fact, the temptation is to view it as “mere” speculation. Others (Brinberg, Lynch, and Sawyer 1992; Sternthal, Tybout, and Calder 1987) have spoken eloquently about this misconception but with an apparent lack of influence. The implications are as significant pragmatically as they are conceptually. Researchers who speculate are frequently asked to go to great lengths to confirm their speculations within the same research report—a demand that, as noted below, can be suffocating. Further, anticipating that the review process will demand a test of speculation, authors are understandably tempted to reframe their speculation as expectation—a weak form of fraud that is easy to rationalize.

In short, confidence has its costs. These costs are especially regrettable because they are avoidable. Those who earnestly wish to be confident about the reliability of a reported result should explain the motivation behind their demand for process. If confidence is the goal, it can be achieved in the absence of theory via tests of robustness.

Direction

A third source of affection for theory is very pragmatic and is captured famously by Kurt Lewin’s assertion that “there is nothing so practical as a good theory” (Lewin 1951, 169). When compared to random experimentation, theory does indeed convey advantage. However, for two obvious reasons, theory fails as a compelling directive if its benefit derives from practicality. First, pragmatics cannot be dictated. An important but inefficiently obtained discovery is a discovery nonetheless. With the possible exception of consumer researchers, it would be a rare patient who refused a remedy based on how it was discovered.

Second, it is not the case that scientists are faced with a binary choice between theory-based inspiration and random exploration. Bumpers in particular are familiar with the process of abduction—or “informed curiosity” (Rozin 2001). There is no reason to believe that research inspired by a hunch and then rigorously pursued is any less likely to shed light on consumer behavior than is research derived from a structured theory. In fact, as argued below, a reverse argument can plausibly be made.

Third, for those who rely on theory to confer scientific

legitimacy on their efforts, abduction should not be viewed as having less serious philosophical underpinnings than the hypothetico-deductive approach or as being any more detached from our ultimate scientific objectives (Haig 2005; *Stanford Encyclopedia of Philosophy* 2010).

NO FREE LUNCH

The theory imperative is more than a matter of philosophy. By uncritically accepting the advantages of theory, we ignore at least four consequential trade-offs.

Why versus What

It has been argued that “the primary goal of a theory is to answer questions of *how*, *when*, and *why*, unlike the goal of description, which is to answer the question of *what*” (Bacharach 1989, 498). True enough, but it is bumpers who have a special appreciation for the fact that *what* precedes all other questions, and it amazes bumpers and functionalists that such an assertion could be controversial.

It is notable that our colleagues on the nonbehavioral side of marketing have debated the merits of an extreme version of *what* in the form of the Empirical-then-Theoretical (EtT) approach (Ehrenberg 1993), wherein one first establishes an empirical generalization and then develops a model to explain it. Although behavioral bumpers can relate to the spirit of this recommendation, they are less ambitious and less contentious. They believe neither that *what* needs to rise to the level of an empirical generalization nor that a theory needs to be preceded by a well-established empirical generalization. They do agree with the general sentiment that data serve as the building blocks for theory and that some respect should be accorded an inductive (or abductive) approach to theory construction. To reprise an earlier irony, the hard sciences readily acknowledge this reality, such as when epidemiology provides a starting point for the investigation of an ultimately microbiological phenomenon.

If pursuit of *why* versus *what* reflected nothing more than idiosyncratic preferences, the issue would not preoccupy us. However, some have argued that theory can inhibit discovery—if for no other reason than that resources are finite and effort devoted to hypothesis testing might come at the expense of hypothesis generation (McGuire 1989). A more extreme position contends that, in some instances, the theory orientation may actively stifle discovery: “Theory is likely to obstruct research progress when the researcher’s primary goal is to test the theory. In testing a theory, the theory can dominate research in a way that blinds the researcher to potentially informative observations” (Greenwald et al. 1986, 217; see also Rozin 2001).

Mortensen and Cialdini (2010) make this point in a folkier manner by referring to the story of the drunk who searches for his car keys under the lamppost, not because the keys were lost under the lamppost but because the lamppost provides superior illumination. As with EtT, it is curious that Mortensen and Cialdini would feel a need to promote

a cyclical approach involving observation, theory, and experimentation. Given the rigors of the journal review process, it is less surprising that Cronbach (1986) felt the need to remind us that hypothesis testing follows a great deal of preparatory work and to urge tolerance of—and much patience with—the exploratory phase of the cycle, else good ideas be discarded prematurely. Indeed, Cronbach attributed dissatisfaction in the social sciences in part to the “idolization of formal theory.”

Finally, it is important not to misconstrue abduction as a lowering of standards or a trading off against rigor. When an effect lacks a causal explanation, there is all the more reason to demonstrate its robustness and probe its boundaries. There is simply less of a need to provide an empirically grounded process explanation in the short term.

Incremental versus Original

With regard to the broader field of marketing, Rust (2006) has wondered about outright hostility to original theory. Hostility, per se, does not characterize consumer research, but one may question the extent to which consumer research generates much original theory (MacInnis and Folkes 2010). Regardless of how one answers this question, it is undeniable that we have been unabashed borrowers of ideas developed elsewhere. The value of this strategy is a matter of opinion, but insofar as abduction is absent from our research, the risk of incrementalism lurks. Incrementalism has unfortunate negative connotations because, in truth, science is often a slow and deliberate process. Insofar as the foundation for consumer research is provided by external disciplines, the larger question concerns the magnitude of our contribution. No metric exists, but consumer research frequently runs the risk of examining near-tautologies (Wallach and Wallach 1994), wherein the predicted outcome is a necessary consequence of the established premises. Near-tautologies appear in the literature, in part because they can add nuance to an outcome, including a demonstration of practical significance—and what could be more practical than consumption?

There are two additional dangers that accompany incrementalism. The first speaks to the legitimacy of our enterprise. Popper notwithstanding, it has been argued that psychological research is more apt to engage in confirmation than falsification of theory, and that such mis-testing of theories can forestall true understanding of a phenomenon (Greenwald et al. 1986). Whatever the extent of this problem in the basic sciences, it is exacerbated in consumer research because we borrow theories for the express purpose of illustrating their relevance to consumer behavior. We have little scientific or personal incentive to borrow a theory in order to demonstrate its lack of relevance (for what journal would publish an article about the irrelevance of a theory from another field). Worse yet, intolerance of messiness in the testing of a borrowed theory leads to sanitized confirmation of the theory (McGuire 1989).

The second danger is that dogged pursuit of any theory runs the risk of producing evanescent minutiae. Research findings are produced at a prodigious rate. When the findings

are produced in service to a theory, their practical contribution may evaporate with the nearly inevitable disconfirmation of the theory. In the present context, this problem may be understood via the distinction between data and phenomena. Haig (2005, 374) argues: “Unlike phenomena, data are idiosyncratic to particular investigative contexts. Because data result from the interaction of a large number of causal factors, they are not as stable and general as phenomena, which are produced by a relatively small number of causal factors. Data are ephemeral and pliable, whereas phenomena are robust and stubborn.”

Similarly, in criticizing the proliferation of memory theories, Watkins (1990, 332) argued: “Research designed to address some person’s individual theory is unlikely to be of any use once that person allows the theory to wither and die. . . . Were we to shed mediationism, theorizing would in all likelihood become less labyrinthian, and research questions more straightforward. . . . The essential findings would be simpler. And simple findings, like simple items of furniture, can be used and reused. They would form a cumulative body of knowledge, and so would free future generations of researchers from the need to start anew.”

As a practical matter, we should also recognize the problem that esoterica poses for our community. As research questions become narrower, a field can become so balkanized that its members find topics outside their own area of interest to be arcane or incomprehensible (Reis and Stiller 1992). As a matter of science it is awkward to argue against deep understanding, but as a matter of influence it is less so. If we wish to understand consumers for the sake of understanding consumers, the deeper we will plunge. If we wish to speak to potential users of our findings, higher-order interactions will be a deterrent. Large main effects, whether accompanied by theory or not, are more likely to serve as an impetus for action—and appropriately so. Our counterparts in marketing science know this well, perhaps because they have a salient external constituency. Their ability to produce empirical generalizations about fundamental marketing phenomena has advanced understanding and practice, irrespective of underlying theory (Hanssens 2009; Lynch 2011). Consumer researchers, on the other hand, have historically shied away from managerial prescriptions, but policy makers constitute an appealing constituency that could benefit from any fundamental and enduring truths we might unearth (e.g., Johnson and Goldstein 2003; Thaler and Sunstein 2008).

Impossible versus Possible

Intellectual curiosity is admirable, but it is appropriate to ask whether we place an undue burden on scientists by raising the bar for an acceptable contribution to an unreasonable height (Sutton and Staw 1995). Consider the following observation that captures very well the philosophy of *JCR*: “This focus on the underlying process is critical and something that tends to distinguish BDT [behavioral decision theory] articles that appear in *JCR* from BDT articles that appear in other journals. BDT researchers who submit to *JCR* are encouraged not only to illustrate the phenomenon but also to develop theory that

specifies why phenomena occur, how they occur, and under what conditions” (Kahn, Luce, and Nowlis 2006, 131).

Such a policy is disturbing on multiple levels. First, some phenomena are not easily understood, and the importance of a phenomenon is not diminished by its mystery. For some widely appreciated phenomena, the record shows that a satisfactory understanding might await decades of effort rather than a manuscript’s worth. Indeed, by one measure the most influential article ever published in *JCR* (Huber, Payne, and Puto 1982) continues to this day to inspire a search for understanding (Tsetsos, Usher, and Chater 2010). Second, some phenomena are not reducible, at least within the confines of our current state of knowledge and the collective intelligence of the community. Looking forward, some fundamental decision phenomena (e.g., loss aversion) and psychophysical phenomena (e.g., visual dominance) may ultimately find process explanations, but the work that uncovers the processes will be Nobel worthy. Looking back, it is clear that a bar set at this level would have disqualified luminaries of decision research from publishing in consumer journals. We do not know how many consumer phenomena have gone undocumented as a consequence of our restrictive policy, but we can be grateful that the policy has not been monolithic. One might argue that Huber et al. were able to evade the requirement for theory by demonstrating a violation of classic choice theory. If so, theirs is an exception that proves the rule. If an intriguing but mysterious phenomenon merits publication only if it addresses existing theory, we need to reassess.

Finally, we should not minimize the dispiriting effect our policy has on our research efforts more generally. It is all the more lamentable when considered in light of the tolerance other scientific fields exhibit with regard to the question of the possible:

One reason researchers believe that heart disease and many cancers can be prevented is because of observational evidence that the incidence of these diseases differ greatly in different populations and in the same population over time. Breast cancer is not the scourge among Japanese women that it is among American women, but it takes only two generations in the United States before Japanese-Americans have the same breast cancer rates as any other ethnic group. This tells us that something about the American lifestyle or diet is a cause of breast cancer. Over the last 20 years, some two dozen large studies . . . have so far failed to identify what that factor is. They may be inherently incapable of doing so. Nonetheless, we know that such a carcinogenic factor of diet or lifestyle exists, waiting to be found. (Taubes 2007)

Yours versus Mine

The preceding passage illustrates yet another disturbing aspect of an uncompromising stance on theory—one that is especially bothersome to researchers who take a cognitive perspective. Causality can be described at a variety of levels. The epidemiological finding regarding breast cancer is causal but devoid of process. The cause that is tractable at

this juncture exists at the level of nature versus nurture. To identify the cause as one or the other is nontrivial, and this level of explanation may suffice depending on one’s taste and objectives. Cognitive researchers, on the other hand, are held to cognitive explanations. The wisdom of this requirement grows more dubious by the day as research increasingly reveals how physical cause competes with psychological cause to explain behavior (e.g., Iacoboni 2008). We would do well to abandon the conceit that we can identify true cause in our present state of knowledge.

A more catholic view of theory is not novel within the philosophy of science. Cook and Campbell’s (1979) primer on theory reminds us that different scientists may legitimately use theory to different ends. Not everyone is—or should be required to be—an essentialist. Positivists who appeal to theory as a convenience rather than a reflection of truth use a theory only insofar as it aids prediction. In other words, one’s view of theory is a matter of taste and, at the risk of being repetitious, taste should not be dictated.

Finally, a recognition that different scholars might view theory differently would be a triumph of pluralism over orthodoxy, which in turn would eliminate the contradictions we conveniently ignore. As noted, our peers in marketing science enjoy greater freedom to approach market behavior from an engineering perspective (Moorthy 1993). Within consumer research, classic ethnography may be viewed as dustbowl empiricism in the best sense of the term, that is, as an exercise in uncontaminated induction. Insofar as cognitively oriented consumer researchers must account for process, our field should be held accountable for its double standard.

CONCLUSION

Theory has many virtues (Sternthal 2011), but we should not regard theory as a defining feature of scholarship. Likewise, a request for pluralism should not be taken as radical (Simonson et al. 2001). The hypothetico-deductive approach is an esteemed mode of scientific practice, cause-effect relationships are desirable, and process explanations enrich our understanding. However, it is not illegitimate to engage in abduction, to pursue the truth via an effect-cause sequence (with a significant delay between effect and cause), or simply to acknowledge that an if-then statement can be valuable even if the intervening causal link has not or cannot be identified (Park 2009; Rozin 2009). When serendipity strikes, we should celebrate it rather than banish the discoverer to a hellish investigative journey. We should routinely strive for interestingness, robustness, and generalizability—but we can do so both within and beyond the confines of theory.

With the benefit of 40 years of hindsight, we can also ask whether theory has served its intended purpose (Hubbard and Lindsay 2002). It is not uncommon to encounter musings from leading consumer researchers that are inconsistent with the existence of a self-confident discipline that can point with pride to its many important and novel discoveries. One is struck instead by the frequency with which we question the parameters and contribution of our field. It is dif-

difficult to prove that our obsession with theory is responsible for this state of affairs, but it seems that discovery might be more frequent if we relaxed the constraints a bit. To reference one other Nobel Prize winner, the words of E. O. Wilson (1998, 6) are sobering: "Original discovery is everything. . . . The true and final test of a scientific career is how well the following declarative sentence can be completed: He (or she) discovered that . . ."

It seems unlikely that Wilson would be satisfied with a career in which the scientist "discovered" how a finding in a different discipline could be applied to one's own. Wilson's stringent criterion for a successful career is accompanied, however, by a less stringent road to success: "Advice to the novice scientist: There is no fixed way to make and establish a scientific discovery. Throw everything you can at the subject, so long as the procedures can be duplicated by others" (Wilson 1998, 7).

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