



Editorial

# The seven sins of consumer psychology<sup>☆</sup>

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## Abstract

Consumer psychology faces serious issues of internal and external relevance. Most of these issues originate in seven fundamental problems with the way consumer psychologists plan and conduct their research—problems that could be called “the seven sins of consumer psychology.” These seven “sins” are (1) a narrow conception of the scope of consumer behavior research; (2) adoption of a narrow set of theoretical lenses; (3) adherence to a narrow epistemology of consumer research; (4) an almost exclusive emphasis on psychological *processes* as opposed to psychological *content*; (5) a strong tendency to overgeneralize from finite empirical results, both as authors and as reviewers; (6) a predisposition to design studies based on methodological convenience rather than on substantive considerations; and (7) a pervasive confusion between “theories of studies” and studies of theories. Addressing these problems (“atoning for these sins”) would greatly enhance the relevance of the field. However, this may require a substantial rebalancing of the field’s incentives to reward actual research impact rather than sheer number of publications in major journals.

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## Introduction

In a number of respects, the field of consumer psychology is doing very well. It is growing very rapidly, as is reflected by (a) the size of our main professional organizations, the Association for Consumer Research (ACR) and the Society for Consumer Psychology (SCP), (b) the number of manuscripts submitted to our main journals, (c) the high attendance at our major conferences, and (d) the number of conferences and outlets now open to consumer researchers. In addition, in some respects, our research has become more rigorous and sophisticated over the years, both theoretically and methodologically. Whereas single studies and simple ANOVAs used to be the norm in our top journals, nowadays typical articles contain three or more studies, painstakingly rule out most alternative explanations, and report

increasingly complex analyses. Finally, the field has become more inclusive. Whereas publications in the most prestigious journals used to be confined to a fairly limited set of scholars from a limited number of academic institutions, today articles in our top journals are authored by a much larger community of researchers from a much broader range of institutions, including many outside North America.

Still, there is one major aspect in which consumer psychology is consistently falling short: Our research findings lack relevance and impact for both our external constituents (i.e., businesses, policy makers, and consumers) and our internal constituents (other consumer researchers and social scientists). In this article, I suggest that most of the field’s relevance issues emanate from seven fundamental problems in the way consumer psychologists plan and conduct their research—problems that could be called “the seven sins of consumer psychology.” Recognizing and correcting each of these seven problems creates a collective roadmap for improving the overall relevance and impact of the field.

## The relevance of consumer psychology (or lack thereof)

Over the years, a number of prominent figures in the field—including several past presidents of ACR and SCP—have repeatedly argued that the research that we collectively produce is

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not as relevant as it should be for our key external constituents. Calls for greater relevance in consumer research were made as early as the early-1980s (Sheth, 1982) and early 1990s (Lutz, 1991), if not earlier. Yet it does not appear that the field has made much progress in this respect. More than 20 years after Sheth's (1982) early call for greater relevance, David Mick (2006) observed as ACR president that consumer research was not as "transformational" as it should be, urging the field to tackle major issues of consumer welfare such as obesity, tobacco consumption, and television violence. Even more recently, in his 2012 presidential address to ACR, Jeff Inman (2012) took up the call, urging us as consumer researchers to be more "useful" to our external constituents.<sup>1</sup>

Note that concerns about the relevance of consumer research have mostly focused on the field's *external* constituents, which are primarily members of the business community and, to a lesser extent, the public policy community and consumers at large. I realize that a number of thought leaders in the field—including some whom I greatly respect intellectually—believe that consumer research should be a stand-alone academic discipline that is not subservient to the world of business and marketing (see, e.g., Holbrook, 1985). According to their view, research findings about consumer behavior do not need be managerially relevant to be scientifically worthwhile. As long as these findings contribute to our *theoretical* understanding of the consumer, this is sufficient. And if certain findings were to have substantive implications, such implications do not have to be for business only: they may instead be relevant for policy making and for consumers at large. I have to disagree.

First, the vast majority of academic consumer psychologists work in business schools rather than in social science departments. To the extent that it is ultimately the world of business that ostensibly motivates and supports our academic enterprise, it seems somewhat disingenuous to argue that the study of consumer behavior does not have to be, at least partially, accountable to the knowledge needs of the business community. Second, it is a little too easy to claim that a particular research finding has implications for public policy or consumer welfare. All too often, claims that a piece of research "is relevant for public policy or for consumer welfare" actually mask a fundamental lack of substantive relevance. Finally, even if it were sufficient that our findings have theoretical implications only—which would be a more *internal* conception of relevance—it is not clear that most of our research meets this criterion either.

How relevant is our research *internally*? How much intellectual influence and scientific impact does it really have on other consumer researchers and social scientists? Not that much, either. Fig. 1 shows the relative performance of *Journal of Consumer Research* (JCR) articles published from 2004 to 2008 in terms of

citations on the Social Science Citation Index. (More recent articles were not considered in order to give the articles a fair chance to be cited in the literature.) The chart is based on average number of citations *per year* rather than on total number of citations over the years in order to mitigate the sheer effect of age of the paper on number of citations. The articles are rank-ordered by average number of citations per year. As can be seen, some articles—but very few—are very well-cited, receiving 10 or more citations per year. The vast majority of articles, however—70% or so—hardly get cited at all, receiving four citations or less in a given year. Therefore, a small number of articles account for a disproportionate share of all citations, and a very "long tail" of articles garners very few citations overall. The top 10% of the articles published between 2004 and 2008 account for 35% of all citations, whereas the bottom 50% account for less than 19% of the citations. This is not a recent phenomenon. A similar analysis of JCR articles published in the 10-year period preceding the one captured in Fig. 1 (1994–2003) reveals an identical pattern of results. Again, very few articles—less than 10%—get very well cited, and the vast majority—roughly 70%—hardly ever get cited. In other words, the vast majority of the research that gets published, even in our top journals—perhaps 70% of it—hardly has any measurable scholarly impact in terms of citations. To put it bluntly: the bulk of our research isn't even interesting to ourselves and to other social scientists!<sup>2</sup>

In summary, the field of consumer psychology faces serious issues of relevance. These issues are not only external, as long decried by several thought leaders in the field; they are also internal, as evidenced by the large proportion of our top journal articles that do not have any appreciable scientific impact. If the bulk of our research is not relevant to our external constituents, nor to ourselves, our entire scholarly enterprise is at risk. Fortunately, however, it is possible to substantially increase the relevance of our research, both with respect to our external constituents and with respect to our internal constituents. This would require addressing what I see as the roots of our relevance shortcomings. Although these root causes are not independent of one another, they can be organized into discrete categories that each deserves to be discussed separately. I focus on the top seven—"the seven sins of consumer psychology"—in obvious reference to the eponymous biblical sins. Before I discuss these seven sins, I should point out that I have committed

<sup>2</sup> Of course, any journal is bound to have a long tail of relatively low-cited articles. However, compared to other major journals, JCR's tail is particularly long and flat. Whereas 56% of 2004–2008 JCR articles had an average of 3 citations or less per year as of August 2013, only 33% of *Journal of Experimental Psychology-General* articles, 27% of *Journal of Personality and Social Psychology* articles, and 27% of *Journal of Marketing* articles of the same period had 3 citations or less per year. (The *Journal of Marketing Research* has a citation profile very similar to JCR's.) The lack of interest elicited by the vast majority of articles in our top journals also transpires from the results of a recent survey conducted by the *Journal of Consumer Research* Policy Board among subscribers to JCR. According to this survey, on average, JCR subscribers reported having read only 15% of the articles published between 2007 and 2009 (John Lynch, personal communication, February 7, 2013)—a number that if anything was likely inflated (John Deighton, personal communication, February 7, 2013). Therefore, 85%, if not more, of the articles published in JCR are not even read by other consumer researchers.

<sup>1</sup> The closely related field of marketing faces similar issues. In 2011, seventy thought leaders in marketing, consumer researchers, marketing strategy researchers, and marketing modelers—a virtual "who is who" of marketing academia—attended the inaugural "Theory + Practice in Marketing" (TPM) conference at Columbia University. The overwhelming sentiment of attendance was that academic marketing research, including consumer research, has become much too technical or theoretical and lacks genuine managerial relevance.

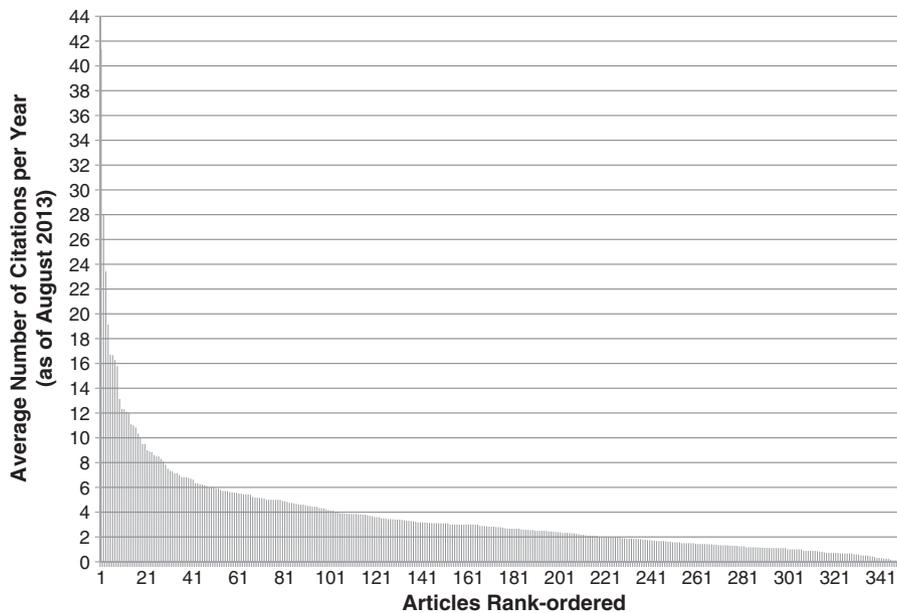


Fig. 1. Average number of citations per year of journal of consumer research articles, 2004–2008.

them all, and that I am therefore as guilty as anybody else in the field. This personal guilt is partly what allows me to discuss these sins knowingly and openly.

### What is wrong with consumer psychology?

#### *Sin #1—Narrow scope*

One of the most crippling aspects of our research comes from the limited scope of what we choose to study as “consumer behavior.” Most of us would likely agree with the following definition of consumer behavior: “How consumers come to learn about, desire, acquire, use, and dispose of goods, services, and activities available in the marketplace to satisfy their needs.” Pictorially, the scope of what we call “consumer behavior” could be represented as in Fig. 2: A series of stages progressing from the activation of a desire for some marketplace offering that can potentially fulfill a consumer need, followed by processes linked to the acquisition of this offering, followed by the actual use and consumption of the acquired product or service, and ending with the eventual disposal or divestment of the product or service.

The bulk of consumer psychology research focuses on one particular stage of the overall consumption process: the acquisition stage. Specifically, most research on consumer psychology focuses on predictors and proxies of purchasing behavior (e.g., attitudes and persuasion, search and consideration sets, decision making and choice, mental accounting and willingness to pay, etc.). However, as Fig. 2 illustrates, purchasing behavior is only a small subset of all consumption-related activities. Large swaths of consumer behavior remain mostly unaddressed (see also Sheth, 1982 and Wells, 1993).

I suspect that the main reason why the field has historically focused on purchasing behavior is a widely held assumption that it is of most relevance from a managerial standpoint. Having spent

a fair amount of time teaching and advising marketing and business professionals over the years, I think that this assumption is somewhat misguided. Businesses are not interested only in purchasing behavior (the acquisition stage), they are also interested what consumers need and want (the desire stage) and how products and services are actually used and consumed in the marketplace (the use and consumption stage). Similarly, policy makers are typically less interested in the act of purchase than in the act of consumption itself (e.g., overeating, smoking, reckless driving, digital-technology addiction), the factors that motivate the consumption in the first place, and the divestment or disengagement of the consumption (e.g., overcoming drug or alcohol addiction, or properly recycling). Therefore, many opportunities to make meaningful contributions to the world of business and the world of consumer policy lie outside the study of purchasing behavior and its proxies. Below are some examples.

*Needs and Wants.* Most research on consumer judgment and decision making assumes that consumer needs and wants are given and exogenous (e.g., “Imagine that you need to buy a camera, which one would you choose?”). In reality, however, consumer needs and wants are not a given and are in fact of great substantive interest to marketers. Most businesses want to understand what makes consumers want (or not want) their products and services in the first place. Why do consumers want to renovate their kitchen? When do consumers want to replace their car? Similarly, policy makers concerned with consumption-related behaviors (e.g., anorexia) need to understand the motivational underpinnings of these behaviors (e.g., why do teenagers binge-eat?). Therefore, understanding the needs, wants, and desires of consumers is important in its own right. Once we recognize this, fascinating questions emerge. For example, one of the great marketing successes of all time is De Beers’ positioning and selling of diamonds as a necessary component of the engagement ritual and the symbol

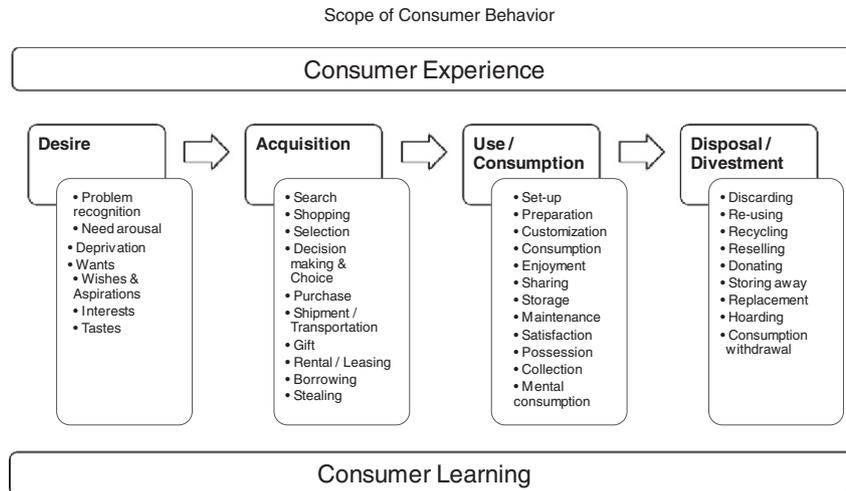


Fig. 2. Scope of consumer behavior.

of love in the US and many other parts of the world. How can consumption needs, such as the need for diamonds that otherwise would seem fictitious, be engineered “out of thin air” by marketers? Alternatively, how can certain perceived needs, such as the need to be digitally connected at all time, be suppressed? How can under-recognized needs be activated, such as the need for safe-sex practices and hygienic food preparation? What is the psychology of consumption need deprivation (due, for instance, to poverty, self-imposed restriction, or external prohibition)? These are questions that are both theoretically rich and substantively relevant, yet still await adequate study within the field.

*Usage and consumption.* Understanding how products and services are used in the marketplace is of great interest to businesses and of fundamental importance to policy makers as well (e.g., drug compliance, healthy exercise, gambling, appropriate use of limited natural resources). For businesses, some of the most useful insights for product innovation, improvement, and marketing arise from understanding consumers’ usage and consumption behavior. This is not surprising, given that it is at the point of usage and consumption—not at the point of purchase—that products and services typically provide value. The substantive importance of usage and consumption to businesses is also evident from the industry’s recent interest in understanding and shaping customer experiences—a trend pioneered by a fellow consumer psychologist (Schmitt, 1999). Other topics of great substantive interest regarding usage and consumption include pre-consumption activities (e.g., preparation, self-customization), shared consumption, consumption rituals, and product possession behavior.

*Other modes of acquisition.* We should not forget that purchasing is only one of several modes of product/service acquisition. Other forms of acquisition include borrowing, sharing, renting, gift reception, bartering, and even stealing. These other modes of acquisition have major impact on business and on the economy as a whole. For example, entire industries

are based on sharing as an alternative to acquisition and ownership (e.g., Zipcar, timeshares, fractional jets, cloud computing). Theft, another mode of acquisition, costs US retailers dozens of billions of dollars every year. Yet, except for some research on gift-giving, very little attention has been devoted to these alternative forms of product/service acquisition.

*Disposal and divestment.* There is also important knowledge to be gained from the disposal and divestment stage of consumer behavior. Disposal behavior, such as discarding and recycling, has an enormous impact on the environment. Overcoming addiction to various forms of consumption (e.g., alcohol, cigarettes, drugs, gambling) is a major public-policy priority. Donations and reselling greatly affect the economy and social welfare (as illustrated by the success of companies such as eBay and Craigslist). Product divestment is a major determinant of the replacement of consumer durables. Compulsive hoarding is a serious consumer welfare issue, especially among the elderly. Therefore, even if on the surface disposal and divestment behavior do not appear as alluring as purchasing behavior, it is nonetheless rich in potential for substantive contributions.

#### *Sin #2—Narrow lenses*

Not only are the consumer topics that we choose to examine overly limited, but the lenses that we put on to examine these topics are overly narrow. In the past 40 years, most of our research has been dominated by three theoretical paradigms: (1) cognitive psychology, (2) social psychology (with a strong emphasis on social cognition), and (3) behavioral decision theory (BDT). Constructs that pervade our theorizing include attention, perception, categorization, memory, information search, inference-making, attitudes, heuristics and biases, mental accounting, etc. These particular theoretical lenses have produced a rather narrow and mechanistic view of the consumer: “If we do X to consumers, process P will be triggered, and then outcome Y will take place.” This mechanistic view of the consumer fails to capture the true richness of how consumers actually operate. It is also at odds

with the way business and marketing professionals think about consumers.<sup>3</sup>

*A concentric perspective on consumer behavior theory.* As illustrated in Fig. 3, one can think of consumer behavior theory as a series of concentric circles, each circle representing a different type of lens on consumer behavior. At the center is a *mechanical core*: the information processing and judgment machinery that the field has studied extensively (e.g., attention, memory, inference-making, heuristics, and biases). Immediately outside this mechanical core is the *affective layer*: the feelings, moods, emotions, and affective preferences that were made salient and significant to psychological theory by pioneers like Zajonc (1980) and Plutchik (1980), and by Holbrook and others (e.g., Holbrook & Hirschman, 1982) in relation to consumer research, and to which I have dedicated most of my research (e.g., Pham, 1998, 2004, 2007). One can think of the forces within this affective layer as shaping what happens within the mechanical core from the outside in: feelings influence judgment, mood influences memory, emotions influence time discounting, etc. Right outside the affective layer is the *motivational ground*: this is where consumers' goals, motives, needs, and values reside. Again, one can think of the forces within this motivational ground as shaping what happens within the more internal affective layer, and thereby what happens within the mechanical core. For example, the goals and needs of consumers largely dictate the feelings and emotions that we experience, which in turn affects how we process information and make judgments.

Beyond the motivational ground, one reaches the boundaries of the self. The self is embedded within a *socio-relational context*, where social influences, family membership, and social roles come into play. Finally, consumption behavior takes place within a broader *cultural background* that is shaped by language, norms, history, economic system, etc. Evolutionary forces can be seen as contributing to this cultural background as well. As depicted in the figure, forces from the outside layers shape the inner layers, and conversely, operations within the inner layers can affect the outside layers.

This concentric view of consumer theory makes it clear that our theoretical perspectives are overly narrow, putting too much emphasis on mechanistic explanations of consumer behavior. As consumer psychologists, we should be more willing to explore additional theoretical lenses, especially those that tap into the outer layers of the figure, for example, emotion theory and affect regulation research, basic motivation theory (and not just self-regulation theory), psycho-dynamic theory, role theory, personality psychology, group and family psychology, cultural psychology (and not just cross-cultural), and evolutionary psychology (see, e.g., Griskevicius & Kenrick, 2013; Saad, 2013; see also Cohen & Bernard, 2013; Downes, 2013, for counterpoints).

*Obsession with unique explanations.* This concentric view of consumer theory additionally underscores the fact that theories of

consumer behavior are not necessarily mutually exclusive. An insidious form of the “sin of narrow lenses” is our field's obsession with *unique* theoretical explanations. Nowadays, it is almost impossible to get an article published in a major journal unless we are able to demonstrate in a convincing fashion that a particular account provides the *single* best explanation for the phenomenon of interest. The pressure to do so is often compounded by some kind of “theoretical tyranny,” whereby reviewers insist that authors differentiate their account from popular theories such as Prospect Theory, Construal Level Theory, or Regulatory Focus Theory, or instead re-express their findings in light of these theories. As a result, a considerable proportion of our research effort is devoted to the isolation (or manufacture) of some unique theoretical explanation.

While there is undeniable value in theoretical precision and clarity, our collective obsession with unique theoretical explanations may be counterproductive in several respects. First, many important and interesting consumption phenomena are clearly multiply determined—think, for example, of the attraction effect, self-control failures, or differences between Chinese and North Americans in terms of food preferences. In fact, phenomena that are truly uniquely determined stand a good chance of not being that important to begin with (a point related to the sin of confusion between “theories of studies” and “studies of theories” discussed later). In addition, many theories should be seen as complementary rather than competing because they capture different levels of explanation. For example, a given mental accounting bias may be driven by differential attention to gains and losses, which would be a cognitive explanation. However, the fact that attention mediates this bias does not preclude the possibility that the differential attention is itself driven by some basic motivational processes, which would be a complementary motivational explanation. Finally, let us not forget that theories are just theories. According to the Oxford dictionary, theories are “suppositions or systems of ideas intended to explain something” (New Oxford American Dictionary, 2010). In other words, theories are not meant to be statements of categorical truth; they are only meant to provide conceptual coherence to the phenomena that we observe. Theories are lenses that we use to internalize and generalize empirical observations about the external world. Therefore, we should be open to the co-existence of multiple theories rather than feel a constant urge to identify a single “best” theory. A good illustration is Chaiken and Trope's (1999) edited volume titled “Dual-Process Theories in Social Psychology,” which catalogues 20 or so dual-process theories of attitudes, person perception, stereotyping, self-regulation, etc. These theories are somewhat correlated in that they are not conceptually independent and do not yield perfectly separable predictions. Yet, they are allowed to coexist, because they each provide a useful lens on the phenomenon that they were designed to explain.

### *Sin #3—Narrow epistemology*

Our field is also too narrow in the way it defines what consumer knowledge is and how it should be advanced. Most of the knowledge that we produce in consumer psychology is

<sup>3</sup> To see this point, perform the following experiment. Approach some seasoned business professionals, and try to discuss what you know about consumer behavior using only principles of information processing, social cognition, and BDT. I predict that they will lose interest within a few minutes.

## A Concentric Circles Perspective of Consumer Behavior Theory

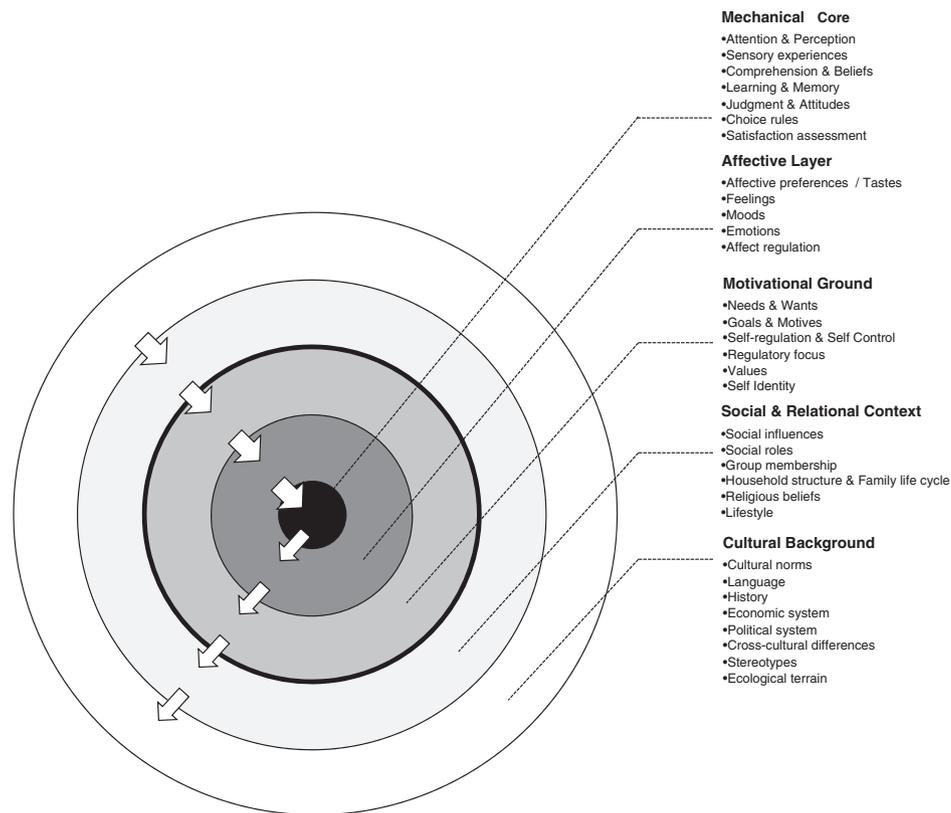


Fig. 3. A concentric perspective on consumer behavior theory.

of a relational type, whereby we connect theoretical constructs to one another (e.g., expertise and depth of search, expectancy-disconfirmation, and loss aversion) and use the resulting theoretical relations to explain some substantive phenomenon of interest (e.g., online price sensitivity, dissatisfaction from product failure). Two primary scientific paths have dominated our epistemology so far: (a) a hypothetico-deductive path, and (b) an inductive path. The field would strongly benefit from considering two additional paths: (c) a descriptive path, and (d) an “external theory validation” path.

*The hypothetico-deductive, “theory-driven” path.* By far, the dominant scientific path in our field has been the hypothetico-deductive path (Lynch, Alba, Krishna, Morwitz, & Gürhan-Canli, 2012). In this path, first constructs are related to one another to generate some theoretical hypotheses (e.g., a promotion focus increases the reliance on affect), then the hypotheses are tested with empirical data that are meant to capture a phenomenon of interest. As summarized by Lynch et al. (2012), one of the major drawbacks of this approach to consumer research is that given that the primary emphasis is on testing construct-to-construct relations, the connection between the theoretical relations being tested and genuine consumer behavior phenomena is often tenuous. The resulting research is often criticized for being “pure psychology” rather than studies about actual consumer behavior. Still, many papers of this

type continue to appear in our journals because they tend to be evaluated on the tightness of the conceptualization and the rigor of the empirical test rather than on the interest or importance of the phenomenon being explained.

*The inductive, “phenomenon-driven” path.* A smaller but growing body of work in our field follows a more inductive path. Interesting empirical phenomena (or effects) are first identified, and then they are gradually conceptualized through a process of induction based on systematic empirical testing. Typical examples would be the large body of work on the attraction effect (Huber, Payne, & Puto, 1982), research on the mere measurement effect (Morwitz, Johnson, & Schmittlein, 1993), and my own work on the “emotional oracle effect” (Pham, Lee, & Stephen, 2012). This alternative path is also relational in that it involves the development of theoretical connections. However, the process starts with empirical observations rather than with theoretical predictions. One of the main benefits is that this approach promotes a grounding of the research into substantive phenomena of genuine relevance to consumer behavior (the Achilles’ heel of hypothetico-deductive research). A number of thought leaders have since called for more phenomenon-driven research (Deighton, MacInnis, McGill, & Shiv, 2010; Janiszewski, 2009; Lynch, 2011; Park, 2012). I support this call provided that two important conditions are met. First, the phenomenon needs to be demonstrably robust and generalizable. Second, the phenomenon needs to be meaningfully related to consumer behavior.

*The descriptive, “empirical generalization” path.* Whether it follows the more traditional hypothetico-deductive path or the more recent phenomenon-driven path, most of our research is still theoretico-relational in that it aims to document particular relations among theoretical constructs or between certain empirical phenomena and certain theoretical constructs. Research that reports empirical findings without advancing clear theoretical relations is typically not well received in our field, often disparaged as “mere description” (Alba, 2012). Yet, some of the most useful findings about consumer behavior have come from studies that were essentially descriptive. Classic examples include Hoyer’s (1984) observation that consumers actually do very little search when in the aisles of a supermarket, Dickson and Sawyer’s (1990) finding that grocery shoppers have very poor knowledge of the prices of the products they just bought, and Hauser’s finding that consideration sets have a typical size and account for most of the variance in product choice (Hauser, 1978; Hauser & Wernerfelt, 1990) (see Lynch, 2011; Lynch et al., 2012). Important descriptive observations have also been uncovered by applied consumer researchers such as Underhill (2008), who introduced the notion of a “decompression zone” in retail environments. The decompression zone refers to an area, in space and time, where retailer information is effectively tuned out by shoppers who just entered a store. Apparently, consumers need some time and space to “decompress” and mentally switch from the outside world to being inside the store shopping. While I am not aware of any scientific validation of this descriptive observation,<sup>4</sup> it is enormously relevant to retailers, regardless of the actual theoretical explanation. Yet, such an important observation would typically have very low status within our field because it is mostly descriptive and furthermore it comes from the industry rather than from academia.

As Alba (2012) and others (e.g., Lynch et al., 2012) recently pointed out, some of the most important advances in the natural sciences emerged from findings that were essentially descriptive. We therefore need to be more supportive of consumer research that is primarily descriptive. Such research should be encouraged if it meets the following three criteria. First, the phenomenon needs to be demonstrably robust and general, that is, it should qualify as an *empirical generalization*. Second, it should tell us something that we did not already know about consumer behavior. Third, it should be useful from a substantive standpoint. Brian Wansink’s body of work on the contextual determinants of food consumption provides excellent examples (Wansink, 1996; Wansink, Painter, & North, 2005).

*The field-theory validation path.* I would like to suggest a variant of the hypothetico-deductive path as a fourth scientific path. Consumer researchers who follow the traditional theory-driven path usually test hypotheses that they generated themselves based on prior academic literature and substantive observations and intuitions. Most consumer psychologists are pretty good at generating hypotheses that are logically sound and theoretically well-founded. They are also pretty good at designing rigorous

tests of these hypotheses. On the other hand, they are not necessarily good at generating hypotheses that are genuinely important from a substantive standpoint. Interestingly, another community of consumer analysts has the opposite problem: Its members are pretty good at generating hypotheses that are substantively important, but are not as good at testing them empirically. The people that I am referring to are industry consultants. The marketing industry is full of consultants who generate their own “theories” about consumer behavior—theories that are specifically designed to appeal to and be seen as relevant by corporate executives or policy makers. However, most of these so-called “theories” are not real scientific theories in the sense of being supported by rigorous empirical evidence and prior scholarly literature. Rather, they are mostly speculations about consumer behavior that businesses or policy makers happen to find interesting and believable. These speculations are a treasure trove of interesting and substantively relevant hypotheses that we, as a field, should be willing to test. Rigorous testing is something that we are particularly good at—something for which we have a strong competitive advantage compared to the industry. Therefore, the new scientific path that I propose is one in which the generation of business- or policy-relevant “theories” is effectively “outsourced” to industry experts and consultants, while we, academics, assume the responsibility of validating (or invalidating) the “theories” by submitting them to rigorous empirical tests. Even though we might not be the originators of the hypotheses, we would perform a critical mission as consumer scientists: that of ascertaining the scientific merit of widely held industry “theories.” An example of research that followed this path is Martin’s (2012) recent test (and eventual validation) of the “butt-brush” phenomenon first identified and publicized by Underhill (2008), a prominent retail consultant. More field theory validation of this type should be encouraged by our major journals.

#### *Sin #4—Disregard for content*

David McClelland, the noted motivation psychologist, once wrote, “Psychologists used to be interested in what went on in people’s heads” (McClelland, 1955, p. 297).<sup>5</sup> In this *Psychological Review* article, McClelland bemoaned psychology’s radical move away from the study of mental *content* to the study of mental *processes*—a move that started in the first half of the 20th century, triggered by the behaviorist revolution, followed by the cognitive revolution, and a strong pressure to establish psychology as a scientific discipline (see also Rozin, 2006). This move has dramatically affected many branches of psychology, consumer psychology included.

One of the most debilitating aspects of our field is our almost exclusive emphasis on analyzing psychological processes, as opposed to understanding the mental contents on which these processes operate. Our research, for instance, would examine how the structure of consumers’ goals helps or inhibits consumers’ goal pursuit. The content of the goals,

<sup>4</sup> Field observations by myself and by my own students suggest that the phenomenon is real.

<sup>5</sup> I thank Joel B. Cohen for bringing this reference to my attention.

however, would be considered immaterial. Similarly, we might study how the perceived diagnosticity of feelings moderates the reliance on feelings (Pham, 2004), but the content of the feelings would be of no particular interest. Over and over again, what matters to us in our conceptualization of consumer behavior—whether through information processing, BDT, affect, or motivation—is psychological processes. The actual content of consumers' thoughts, beliefs, feelings, and motives does not really seem to matter. (It is quite revealing that we use the terms “explanation” and “process explanation” almost interchangeably.) A major contributing factor to our infatuation with psychological processes is a desire to identify principles of consumer behavior that are universal rather than particular. Whereas the mechanics of thinking, feeling, or judging might be somewhat universal, the contents of consumers' thoughts, feelings, and judgments are clearly much more variable.

Yet, for our field to grow in relevance, it is essential that we pay more attention to matters of content. Consumption behavior is a deeply substantive phenomenon. Content matters in consumer behavior. Attempting to explain consumption behavior without any reference to the content of consumers' motives, feelings, actions, beliefs, and thoughts is extremely impoverishing. When companies or policy makers want to understand consumers, they really want to know *what* consumers do, *what* consumers think, *what* consumers want, and *what* consumers feel—that is, they want to know about the *contents* of consumers' minds. They don't really want to know about the internal mechanics of consumers' minds. We would therefore greatly increase the relevance of our work to our external constituents by studying the psychological contents of consumption behavior.

Doing so would not only enhance our external relevance, it is likely to increase our internal relevance—the scientific impact that we have on other consumer researchers and social scientists. While many other branches of psychology focus similarly on psychological processes, one should not forget that some of psychology's most important contributions were about primarily content rather than about processes. Think of Maslow's (1954) pyramid of human needs, Freud's (1899/2010) psychoanalytic theory, Rokeach's (1973) typology of values (Rokeach, 1973), Milgram's (1963) experiments on obedience, and Hofstede's (1980) dimensions of cross-cultural differences to name a few. Consumer psychology findings that are content-rich have greater chance of being influential. For example, a very well-cited paper in the *Journal of Marketing* discusses the pervasive intuition that the healthiness of food is inversely related to its tastiness (Raghunathan, Naylor, & Hoyer, 2006). A large part of this paper's impact comes not from showing how this particular intuition operates, but rather from showing that (a) it exists, (b) it is pervasive in the marketplace, and (c) it affects consumption substantially. Another influential paper reveals, through clever field experiments, that requests to reuse hotel towels that are framed in terms of social norms (e.g., “The majority of guests reuse their towels”) are significantly more effective than other forms of requests typically used by hotels (Goldstein, Cialdini, & Griskevicius, 2008). Again, what is driving the impact of this paper is not something spectacular about the psychological

process at work, but rather the uncovering of a type of appeal (i.e., content) that works better in this particular marketplace.<sup>6</sup>

A fascinating read about consumer psychology is the *Handbook of Consumer Motivations* by Ernest Dichter (1964), the “father of motivation research.” Dichter's approach to consumer motivation was totally different from how motivation is studied today: it was all about content. Dichter was a Freudian. He believed that behind every consumption object, whether asparagus, wooden floors, or toothpaste, lies a symbolic meaning that is deeply rooted in our unconscious motives. He suggested, for instance, that large kitchen appliances, such as the fridge and the stove, are really surrogates for men to express their deeply rooted desire to be seen as providers for their family. This could explain why, still to this day, consumers seem to have a strong preference for fridges and stoves that are masculine-looking (large, square-shouldered, and stainless steel). Dichter offered similar analyses for hundreds of product categories, many of which I found rather insightful, despite the fact that they were conducted some 50 years ago. Although one may quibble with some of these analyses, a major lesson from Dichter's work is that if one is to generate genuine insights about consumer behavior, one needs to pay close attention to matters of content.

I therefore urge the field to pay more attention to the content of consumers' thoughts, feelings, motives, and actions. This is what our external constituents really need and want, and this would likely increase our scholarly impact. Doing so would require that we suspend our search for psychological universals, and be more willing to ground our theorizing in particular consumption contexts (see Rozin, 2006, for similar points). This is what Raghunathan et al. (2006) did in the food domain, and what Goldstein et al. (2008) did in the environment-friendly-hotel domain. We may not necessarily need to be as granular and specific as Dichter was, but we cannot afford to be as generic as we have historically been.

#### *Sin #5—Overgeneralization*

Another major problem in our field is a pronounced tendency to overgeneralize from available evidence. This is a sin that we commit both as researchers and as readers and reviewers of the literature.

*Overgeneralization as researchers.* Getting a study to “work” takes a lot of effort. One has to think very carefully about the consumption context to be used, the product category, the exact stimuli, the precise procedure, the measures that are most likely to pick up the effect, etc. Pretests need to be conducted. And more often than not, one needs to try different versions of the

<sup>6</sup> I also had the opportunity to witness the differential impact of content-versus process-oriented contributions first-hand. My most-cited article is a paper about the effects of anxiety versus sadness on decision making (Raghunathan & Pham, 1999). Although I did not foresee it at the time, I believe that the main reason why this paper had some impact is because its primary contribution—demonstrating the motivational difference between anxiety and sadness—was content-rich. On the other hand, another paper of mine on the reliance on feelings in the ultimatum game (Stephen & Pham, 2008) had hardly any impact. I am now convinced that this is largely due to the paper's positioning, which was mainly process-oriented.

study to eventually get it to “work.” This is normal and a natural part of the process of science. However, once a study eventually “works” as we intended, we quickly develop a supreme confidence in our results and interpretation, forgetting how much effort it took us to obtain the effect in the first place. We tend to perceive our own findings as much more general and robust than they really are. The phenomenon is akin to the classic “fundamental attribution error” in social psychology: We quickly attribute some trait-like, theoretical quality to data patterns that are statistically significant, forgetting the myriad of situational factors—chance included—that could have contributed to the observed data. As a consequence, we do not make sufficient attempts to replicate our own results, tend to overstate the replicability and generalizability of our findings, and are much less programmatic in our research compared to our peers in other psychological disciplines. This tendency to overgeneralize our own result, compounded by a frequent lack of transparency in scientific reporting, has led some to argue that our field, along with other branches of psychology, is replete with “false-positive” results that cannot be replicated (Simmons, Nelson, & Simonsohn, 2011)—an issue that SCP and JCP are actively addressing.

*Overgeneralization as readers and reviewers.* We exhibit a similar tendency when reading the literature and reviewing papers submitted for publication. Once a result has been published—especially if it is by a famous author in a prestigious journal—we tend to treat it almost as “gospel,” again forgetting that the findings may be more context-specific than a cursory reading of the study might indicate. We may even generalize the results beyond the researchers’ original interpretation. We tend to walk around with oversimplified theories that we use and promulgate indiscriminately. Again, such overgeneralizations hinder our scientific progress. All too often, research ideas are prematurely abandoned and findings hastily dismissed because of an unwarranted feeling that “we already know that.” Similarly, new findings and propositions are unfairly disputed because of an unwarranted impression that the literature supports the opposite (“This can’t be true because we know from X that...”).

A good example of a vastly overgeneralized finding is [Iyengar and Lepper’s \(2000\)](#) result on the demotivating effects of having too much choice (the famous “jam-in-supermarket” study). This is an important result. However, a careful reading of the original study reveals that the findings were obtained under very specific conditions (that the authors disclosed conscientiously). For example, all the jams were from a single brand; very popular flavors were removed from the choice sets; and the small choice set was pretested to exhibit substantial variability in taste preferences. Yet, once the finding became well known, the fine print of the study was quickly forgotten, the results were overinterpreted, and the field began to take it for granted that “consumers do not like it when they have too much choice” (which is not exactly what the original study shows). It turns out that the “too-much-choice” effect is in fact quite fickle. In a meta-analysis of 63 studies by [Scheibehenne, Greifeneder, and Todd \(2010\)](#), the basic effect was found in only 16 studies; the reverse effect was found in seven studies; and the large

majority of studies showed no significant effect. Therefore, despite a pervasive sentiment that [Iyengar and Lepper \(2000\)](#) “already showed” that too much choice demotivates consumers, the phenomenon is actually not that well established.

*Promoting knowledge calibration.* Our overgeneralization problem has clear solutions: We need more replications and more nuance and precision, both as authors and as readers and reviewers. As researchers, we need replications of our own results—replications across different samples of respondents, across different stimuli, across different operationalizations of the manipulations, etc. These replications ideally should be conducted by altering only one variable at a time. Altering too many variables at the same time would defeat the primary purpose of replication, which is to test the robustness of a finding to differences in methodology that are theoretically insignificant. We should also be more willing to increase our sample sizes in order to reduce the chance of false-positive results. In addition, we need to be more careful and nuanced in our writing and discussion of empirical results. Finally, we need greater transparency regarding the details of our methodology in order to increase the interpretability and replicability of our findings.

As readers and reviewers of the literature, we need to be more mindful of what past studies actually show and how they were actually conducted. We should avoid dismissing new findings based on mere impressions that “we already know that,” and instead develop greater appreciation for nontrivial distinctions that may exist between a new study and the ones that it reminds us of. If a new result appears to conflict with a prior result, we should not necessarily assume that the new result must be invalid: There may be genuine differences between the new study and the prior study; there may be important boundary conditions to the phenomenon; or the prior result may not be as robust as originally thought. Finally, we should be much more supportive of direct replications within papers, and conceptual replications across papers. How many times have authors been asked to drop replication studies from their submitted manuscripts on the ground that “your other study already shows that”? These are not a waste of journal space. These are a necessary part of building a suitably calibrated body of knowledge.

#### *Sin #6—Research by convenience*

In a well-known editorial, the famed [Robert Ferber \(1977\)](#) once decried the field’s over-reliance on student samples for research that is supposedly about consumers in general. Ferber questioned, for example, whether students were really the right respondents for classic consumer behavior topics such as family purchases, automobile purchases, and financial decisions. He also questioned whether—topic aside—results from college students could be generalized to the broader population of consumers that the samples are meant to represent (see [Sears, 1986](#), for a parallel discussion in social psychology). Ferber astutely titled his editorial “Research by Convenience,” thereby naming the problem while describing much of its underlying psychology. Back in 1977, most consumer research studies were conducted among college students

simply because students were more convenient. Unfortunately, not much has changed in the 36 years since Ferber's editorial.

If anything, the research-by-convenience problem is probably even more far-reaching than Ferber originally stated. The convenience sampling of our typical research population has resulted in a corpus of knowledge that is not only mostly college-student-based but heavily North American-centric. We know a lot more about North American consumers, especially North American *student* consumers, than we do about consumers from other parts of the world, especially nonstudents (Gorn, 1997). Raising a similar issue about psychology in general, Arnett (2008) questioned whether psychology, as recorded in the major journals, could plausibly claim to describe the mental functioning of the human species when its corpus is mostly based on studies of 5% of the world's population (Americans), leaving the remaining 95% essentially unobserved. He provides several examples of areas across different branches of psychology where the accepted findings, mostly derived from American samples, would likely not generalize to other parts of the world, such as gender stereotypes in social psychology, marital relations in clinical psychology, child rearing in family psychology, and teaching methods in educational psychology. Our "convenience" population sampling has also resulted in most of our theorizing tapping into the upper end of the knowledge/expertise continuum (Alba, 2000). Even within the US, large segments of the consumer population are bound to be less educated, knowledgeable, and "intelligent" than the typical student population. Many years ago, studies based on broader, nonstudent samples showed that substantial portions of the US population display poor comprehension of everyday television and advertising communication (Jacoby & Hoyer, 1982) and are unable to compute simple "best-buy" calculations (Capon & Kuhn, 1982). Such issues support Henrich, Heine, and Norenzayan's (2010) recent argument that the bulk of psychological findings in the world's major journals is based on WEIRD people—an acronym for Western, Educated, Industrialized, Rich, and Democratic—who are quite typical of the broader human population.

On the surface, it would appear that new sources of inexpensive experimental respondents such as Mechanical Turks, that have recently become popular in our field, should attenuate the research-by-convenience problem. In terms of demographics, MTurk participants ("MTurks") indeed appear to be a little more like "real consumers" than the typical college undergraduate (Berinsky, Huber, & Lenz, 2012). However, several concerns must be raised about using MTurks as surrogates for "real-world" consumers. First, it is not clear that this particular section of individuals, however large, who self-selected to participate in Amazon's crowdsourcing marketplace—individuals who are willing to perform often mindless, computer-mediated tasks for less than half the US minimum wage—are that much more representative of "real-world" consumers than are typical college undergraduates. Second, there is growing evidence of increased MTurk sophistication in seeing through and "gaming" social science studies, raising issues about the validity of the data provided (Chandler, Mueller, & Paolacci, 2013). Finally, and perhaps most seriously, there is a real danger of the low data collection costs associated with MTurks gradually shifting

our research agendas toward studies that *can* be done using MTurks—that is, short, online, vignette or survey-type studies—as opposed to studies that *should* be conducted to advance our field. This subtle shift in research agendas provides another meaning to the phrase "Research by Convenience"—one that Ferber did not originally discuss but is perhaps even more worrisome.

Finally, it should be noted that the research-by-convenience problem is not restricted to the sample of respondents that we study—it extends to the convenience of the instruments that we use. Much of our research is based on vignette-like studies in which respondents are asked to imagine certain consumption situations and report how they would respond in such situations. One may legitimately wonder whether the observed responses are good indicators of the actual responses that one would observe with actual consumption behavior. Colleagues in economics would probably say "no" because of a lack of proper response incentives ("This is just cheap talk"). I don't think that this is the main problem. The more substantial problem is that scenario-based studies tend to make focal aspects of the treatment prominent (e.g., "imagine buying insurance two years from now" vs. "next month"), thereby exaggerating the strength of the observed effects. Moreover, participants who are asked to project themselves into certain consumption situations are likely to adopt overly analytical mindsets that are not representative of how consumers actually respond to such situations in real life (see, e.g., Dunn & Ashton-James, 2008; Snell, Gibbs, & Varey, 1995, for relevant findings). Finally, scenarios are poorly suited to the study of "hot" variables such as emotional responses and motivational states, whose influence is difficult to imagine without a genuine experience (Pham, 2004).

In summary, too much of our research is dictated by sheer methodological convenience. We rely excessively on college students, MTurk participants, North American samples, North American and other Westernized contexts, and vignette studies. Our field clearly needs to encourage and support field studies that involve real consumers and real behavior. Studies of consumers from a broader range of marketplaces and socio-economic conditions should be encouraged, as well.

#### *Sin #7—Confusing theories of studies with studies of theories*

A final major impediment to the relevance of our field is a pervasive confusion between what I call "theories of studies" and "studies of theories." What passes as empirical studies testing novel theoretical propositions about consumer behavior ("studies of theories") is often no more than the conceptualization of very narrow phenomena that are created by the studies themselves ("theories of studies"). Too many of these "theories of studies" now appear in our major journals, contributing substantially to the 70% of articles with very low citation rates. Typical "theories of studies" go something like this:

Past research has shown that when consumers are hungry, they tend to purchase more when they go to the supermarket. However, suppose that we prime them either with the concept

of indulgence or with the concept of self-control. We predict that hungry consumers primed with the concept of indulgence will purchase more than consumers who are not hungry. However, hungry consumers primed with the concept of self-control will not purchase more than consumers who are not hungry. This is because ...

The preceding study might be followed by another study going like this:

Now suppose that half the participants are asked to remember a 7-digit number and the other half are asked to remember a 2-digit number. We predict a 3-way interaction showing that among participants asked to remember a 2-digit number, the results of the previous study would be replicated; in contrast, among participants asked to remember a 7-digit number, hungry participants would purchase more, regardless of whether they are primed with indulgence or self-control.

Although this is a hypothetical example, one will recognize that this research has some of the key qualities required to get published in our major journals today: (1) the research leverages previous findings that are well established; (2) it combines these previous findings in a way that has not been done before; (3) the research makes predictions that are plausible theoretically; (4) the studies use established methodological paradigms and, if the authors are reasonably competent, the studies have a good chance of being free of major confounds; and (5) reviewers would likely have a difficult time coming up with a better theoretical explanation than the one proposed by the authors.

This is the type of paper that is not easy to reject on methodological grounds. The paradigms are well established; the studies will likely be conducted in a competent manner; the analyses will likely be adequate; the results are likely to be statistically significant; and the data will not be easily amenable to obvious alternative interpretations. This paper would not be easy to reject on theoretical grounds either, because the conceptualization builds nicely on existing literature, and the predictions are novel and logically sound. The only way that such a paper might get rejected is if some of the reviewers are blunt enough to state that “the research is not interesting,” which is something that most reviewers would be reluctant to do, at least openly. And even if a reviewer does question the overall relevance of the research, the editor may not necessarily follow this reviewer’s advice.

Thus, overall, this paper has a very good chance of eventually getting published in one of our major journals. Many papers like that do. Yet, with careful consideration, it should be apparent that these hypothetical studies do not actually teach us anything meaningful about consumer behavior and are therefore unlikely to have any significant impact. This is because the whole research revolves around the logically consistent conceptualization of a very narrow phenomenon that most likely only occurs under the artificial conditions that the researchers seek to create in the lab. What real-world consumer behavior phenomenon are these studies modeling? How pervasive is this phenomenon outside the lab? Any conceptual claim that emerges from this research would amount to little more than the authors’ mini-theory (with a lowercase *t*) of the authors’ contrived studies. Just as our quantitative marketing colleagues should be skeptical of

analytical models that are based on totally implausible assumptions, consumer psychologists should be more skeptical of “theories of studies” passing as “studies of theories.” “Theories of studies” are bound to have negligible impact, and are therefore a waste of journal space.

### Conclusion—A roadmap for greater relevance

In summary, consumer psychology faces serious issues of relevance. Not only is our research not as relevant as it should be with respect to our external constituents—businesses, policy makers, and consumers at large—but the bulk of it, perhaps 70%, is not particularly relevant to our internal constituents either (other consumer researchers and social scientists). To increase the relevance of our work, both internal and external, the field needs to address seven fundamental problems in the way consumer psychologists plan and conduct their research—the seven sins of consumer psychology. A concerted effort to correct these sins provides a clear roadmap for how consumer psychology needs to evolve.

- 1 We need to extend the scope of our research beyond purchase behavior and its proxies. We need more research on the activation or suppression of consumer needs and wants, on nonpurchase modes of acquisition (e.g., sharing, borrowing, stealing), on various aspects of actual usage and consumption (e.g., preconsumption, customization, consumption experience), and on product disposal and consumption divestment (see Fig. 2).
- 2 We need to embrace a broader set of theoretical perspectives on consumer psychology beyond information processing, social cognition, and BDT. Alternative perspectives include emotion theory and affect regulation, basic motivation theory (beyond self-regulation), psycho-dynamic theory, role theory, personality psychology, group and family psychology, cultural psychology, and evolutionary psychology (see Fig. 3). In addition, we should put less emphasis on the isolation of unique, micro-level explanations, and be more open to the co-existence of multiple explanations, possibly at different levels.
- 3 We should expand our epistemology beyond the traditional hypothetico-deductive (theory-driven) path. Other scientific paths that should be sanctioned in our field include: (a) inductive, phenomenon-based research (provided that the phenomenon is robust and genuinely grounded in consumer behavior), (b) descriptive, empirical generalizations research, and (c) field-theory validations.
- 4 We should pay much more attention to the psychological contents of consumer behavior, as opposed to its psychological processes. The contents of consumers’ thoughts, beliefs, feelings, motives, values, desires, and actions are critical for our understanding of consumer behavior. Doing so would require a suspension of our quest for psychological universals in favor of a greater grounding of our theorizing in particular consumption contexts.
- 5 Instead of obsessing over unique theoretical explanations of data patterns, we should put greater emphasis on the robustness

and replicability of these data patterns. Direct replications within papers and conceptual replications across papers should be encouraged rather than discouraged. We should be more nuanced and precise, both in the reporting of our own results and in our reading and interpretation of other researchers' findings. Greater transparency in our methodology would increase the reproducibility of our findings.

- 6 We should conduct and encourage more field studies with real consumers and real behavior. Such studies deserve greater leniency and patience in the review process. We should also sample a broader range of consumers, from a variety of socio-economic conditions, and a broader range of marketplaces. Vignette and scenario studies should be used more judiciously, especially when dealing with hot processes of consumer behavior and sensitive consumer topics.
- 7 Finally, we should develop lower tolerance for mere “theories of studies,” both as reviewers and editors and as researchers. Studies should provide simplified models of the consumption world, not conceptual models of nothing.

Practically, the above recommendations would require a significant revamping of our doctoral training. Our doctoral curriculum should reflect a broader range of theoretical perspectives and promote a deeper grounding of our teachings in actual consumption behavior and its substantive implications. It would be useful, for instance, to encourage doctoral students to take more applied MBA-level classes in marketing and consumer behavior, and to serve as teaching assistants for these classes. Rather than confining themselves to their offices, libraries, and laboratories, consumer psychologists should try to increase their physical exposure to businesses, policy agencies, and actual consumers in the marketplace. This can be done through consulting, executive teaching, and field visits.

Finally, the field probably needs to rebalance its incentive structure. Too much of our current incentives—whether for promotions, raises, awards, or simple recognition—rewards the sheer number of publications in top journals (the “number of A’s”) rather than the actual impact and lasting contribution of these publications. Accordingly, researchers in our field dutifully list all their publications on their CVs, but only few additionally mention the citation impact of their papers. This is not an accepted practice. Our current incentive structure has clearly contributed to the ubiquity of the seven sins that I describe. As long as researchers are rewarded mostly for maximizing the number of articles that they publish in A-level journals rather than for the lasting impact of their articles, regardless of the journals where they were published, then narrow scope, narrow lenses, narrow epistemology, disregard for content, overgeneralization, research by convenience, and “theories of studies” will remain prevalent in our field, and the relevance of consumer psychology will remain a struggle.

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