A Tribute, an Old Challenge Revisited, and an Amplification

Michael R. Hyman, Distinguished Achievement Professor, New Mexico State University

I still prefer to consider myself a young academic turk. Nonetheless, the graying hair and beard that confront me in the mirror each morning—both easily discernible without eyeglasses—suggest otherwise. Rather than bemoan my journey to decrepitude, I choose to embrace my self-declared elder-statesman status by exploiting the one accommodation typically extended to senior academicians: to comment on their discipline from a blatantly personal perspective. Thus, I am grateful for the opportunity to participate in this text honoring Shelby.

My Tribute

Although I learned much about marketing while earning a Ph.D. at Purdue University, I learned little about the foundations of marketing theory or the philosophy of science. At the time, a traditional marketing theory course was not among the offered seminars. Absent this course, I recall only two basic philosophical debates as a doctoral student:

(1) Was marketing science best served by Bass' predictive-testing approach or Ehrenberg's empirical-regularity approach?

(2) Should modelers of consumer behavior assume that people act deterministically or stochastically?

Ehrenberg, in reflecting on his career, said "I was probably always aiming at findings that in themselves were both simple and generalizable. Simple, so that I and others could see the patterns in the initially often complex-looking data. Generalizable (within stateable limits), to provide validated benchmarks, possibly lawlike in due course" (Ehrenberg 2004, p.36). In contrast, Bass argued that the best process for advancing marketing science is (1) develop a theory, (2) specify an empirical model consistent with that theory, (3) identify the theoretically constrained limits to that model's parameters, and (4) test whether or not parameter estimates fall within those limits (e.g., Bass 1969). Thus, the question was 'Should data inform theory or should theory inform empirically testable models?' Needless to say, my answer to any exam-related question was obvious.

The correct behavioral assumption was questioner dependent; it was 'stochastic' for Bass and 'deterministic' for Pessemier. A fellow doctoral student and I often debated why these giants of marketing scholarship affiliated with their respective camps. Ultimately, we accepted this relativistic argument: Pessemier as our least predictable professor, must have viewed other people's behaviors as far more predictable than his own; and Bass, as our most predictable professor, must have viewed other people's behaviors as far less predictable that his own. Thus, each viewed consumers from the extremes of their own behavioral predispositions.

Although both debates are non-trivial, they represent a small fraction of the important theoretical and philosophical issues in marketing. I graduated oblivious to many basic theoretical debates, such as 'Is marketing a science?' It was not until I began to forge my research agenda that I delved into the marketing theory literature. Shelby's work largely inspired that agenda.

Based on my growing interesting in marketing theory and philosophy of science—as well as the lack of alternative instructors—a department head eventually assigned the marketing theory seminar to me. Given my doctoral training, I lacked a template for designing that seminar.

Fortunately, I secured copies of Shelby's theory seminar syllabus and marketing theory text. I organized my original and many subsequent iterations of that seminar around those materials. Thus, I will be forever in Shelby's debt, as I would have been lost otherwise. (Of course, my doctoral students may disagree, as any seminar inspired by Shelby's syllabus and text would be demanding.)

Perhaps best known among marketing academicians for his marketing theory text—now in its fourth incarnation as an impressive two-book series—the corpus of Shelby's scholarly work is awe-inspiring. Since my doctoral student years, my empirical preferences have drifted towards Ehrenberg's aforementioned empirical-regularity approach to knowledge creation. In this vein, I searched for patterns in Shelby's on-line CV (plus one addition from Business Source Premier). My efforts yielded the following:

- From 1970 to 2007, Shelby (co)authored 152 published works: 110 journal articles, 21 proceedings pieces, 13 non-refereed (mostly book chapter) pieces, and 8 books. His journal articles were reprinted 25 times. His productivity has been similar by decade; he (co)authored 28 works during the 1970s, 41 during the 1980s, 37 during the 1990s, and 43 during the 2000s.

- Shelby was listed as first author 120 times, as second author 26 times, and as third author 6 times. He has sole authored 44 works, jointly authored 87 works, and has two co-authors on 21 works. Seemingly, he has never published a work with more than two other co-authors, which speaks to his wisdom about 'too many cooks spoiling the pot'.

- Of his 110 (co)authored journal articles, 57 (51.8%) have appeared in six prestigious publications: *Journal of Marketing* (26), *Journal of Business Research* (8), *Journal of the Academy of Marketing Science* (8), *Journal of Macromarketing* (7), *European Journal of Marketing* (4), and *Journal of Marketing Research* (4).

- Colleagues who have co-authored with Shelby five or more times include Lawrence Chonko (14), Dennis Arnett (11), Robert Morgan (9), John Nevin (7), Scott Vitell (5), John Burnett (5), and Van Wood (5). Shelby must not bore of his doctoral students; he chaired the Ph.D. dissertations of frequent co-authors Arnett, Morgan, and Vitell.

- Of the 432 different words in the titles of his 152 published works, substantive words appearing ten or more times are marketing (71), theory (50), compet(itive)(ition)(ing) (23), ethic(s)(al)/moral (21), resource-advantage (17), strategy (11), success (11), research (11), and model (10). Surprisingly, philosoph(y)(ies)(ers)(ical) appeared only five times. Shelby must prefer titles with punctuation, as his titles collectively contained 86 colons and 22 question marks. They also included the numerous con-junctions—*and* (81), *for* (8), *or* (7)—noted by Brown (2005).

One-hit wonders are not limited to pop music. For example, Edmund Gettier is known for his single three-page paper "Is Justified True Belief Knowledge?" Although the sheer quantity of Shelby's published work is amazing, when combined with its impact on marketing scholarship, it could be argued that his influence on marketing thought is unsurpassed.

My professorial career began roughly 25 years ago. Although 'it's been a blast', academic research is often a mental and occasionally a physical challenge (especially when facing an impending submission deadline). Rigorous and well-crafted scholarly writing is hard work, as I tell my wife when searching for an acceptable excuse to avoid household chores. Given Shelby's prodigious scholarly output, he must have a superhuman mental and physical constitution.

In a 'you are what you claim to hate' argument, Brown (2005) characterizes Shelby as a

closeted postmodernist.

> Aside from their somewhat similar scholarly worldview—antipathy to excessive mathematization, rejection of base managerialism, fondness for qualitative research methods (such as historical analysis), interest in figurative language, metaphor, rhetoric and 'unpacking' the arguments of others—the look, tone and sheer cite-heavy literary character of [Shelby's] articles …are more akin to the preoccupations of postmodern marketers than…the marketing mainstream (Brown 2005, pp.94-95).

Brown (2005) argues that Shelby's works are characterized by five common features of postmodern writings: fragmentation, de-differentiation, retrospection, hyperreality, and pastiche. Although entertaining, Brown's efforts to dismiss Shelby as a philologist (a lover of words but not a lover of wisdom) rather than as a premier marketing philosopher-theoretician-scholar are weak attempts to trivialize Shelby's work. Brown's characterization notwithstanding, Shelby has earned the right to an occasional rhetorical flourish.

A colleague once claimed "Journal referees decide an empirical-based manuscript is either right or wrong and rate it accordingly. In contrast, those same referees can find 10,000 reasons for rejecting a theory-oriented manuscript." Although a tad hyperbolic, my experience suggests there is an element of truth to this assessment. Fortunately for marketing scholarship, Shelby often succeeded in surmounting this referee predisposition.

"I Came Here for a Good Argument!"

Early in my professorial career, I co-authored several articles with a philosopher (who was trained as a logician) and a historian (who earned a second doctorate in marketing, which speaks to his pain tolerance and/or sanity). The challenge in working with them, especially on the same project, was their dissimilar beliefs about evidence for a claim. For the logician, a single counterexample was sufficient to reject a claim. For the historian, in contrast, the preponderance of the evidence was sufficient to support a claim. If my tussles with journal referees are reflective, then most marketing scholars concur with the historian's perspective on evidence. Like my philosopher co-author, and occasionally to my publishing detriment, I prefer the meticulously constructed impregnable argument. In this regard, if I can judge by his scholarly work, Shelby and I seemingly are kindred spirits.

Publication of comments and rejoinders that oppose previously published articles should be an important mechanism for advancing marketing knowledge. Unfortunately, many of the resulting dialogues remind me of the classic 'argument' sketch from *Monty Python's Flying Circus* (with Michael Palin and John Cleese). Perhaps you recall this segment of the sketch:

> Man: I came here for a good argument!
> Other Man: AH, no you didn't, you came here for an argument!
> Man: An argument isn't just contradiction.
> Other Man: Well, it CAN be!
> Man: No it can't! An argument is a connected series of statements intended to establish a proposition.
> Other Man: No it isn't!
> Man: Yes it is! 'tisn't just contradiction.
> Other Man: Look, if I 'argue' with you, I must take up a contrary position!
> Man: Yes, but it isn't just saying 'no it isn't'.
> Other Man: Yes it is!
> Man: No it isn't!
> Other Man: Yes it is!
> Man: No it isn't!
> Other Man: Yes it is!
> Man: No it ISN'T! Argument is an intellectual process. Contradiction is just the automatic gainsaying of anything the other person says.
> Other Man: It is NOT!
> Man: It is!

If I want to engage in a good argument, there is no better opponent than Shelby; thus, I hope he will consider revisiting an old challenge. For readers familiar with the aforementioned comedy sketch, note that I hope my challenge will not result in a 'getting hit in the head' lesson.

An Old Challenge Revisited

In 1991, my plucky philosopher-historian-marketer triumvirate mounted a new challenge to the positive-normative dichotomy of Shelby's classic three-dichotomies model (Hunt 1976), which was (judging from the frequency it was reprinted) and seemingly remains (as roughly half the exposition in Chapter 1 of Hunt (2002) is dedicated to discussing it) a cornerstone of marketing theory. Hyman, Skipper, and Tansey (1991) challenged Shelby's argument for marketing's positive-normative dichotomy in his original model and subsequent rejoinders (e.g., Hunt 1978). (Note: I will now use Shelby's 'HST' convention in referring to this publication.)

To summarize, HST argued that Shelby's original semantic is/ought rule-of-thumb for identifying positive or normative sentences should be replaced by this context-sensitive rule-of-thumb: "*All normative sentences and only normative sentences offer a reason for action*"(p.420). (N.B. This rule-of-thumb is consistent with several articles on normative science, such as Janik (2007), Potter (1966, 1967), and Veatch (1945).) Armed with this improved criterion, HST then posited that "*Marketing language is so saturated with value-laden terms and marketing theories are so thoroughly imbued with normative claims that no translation into positive language is conceivable*" (p.420). Thus marketing, as studied by market-ing scholars, is a normative science because hidden normative terms (like 'ownership' and 'exchange') pervade it and positive science must be free of normative terms.

Although amusing, Shelby's counterargument to HST is unconvincing; for example Brown (2005) critiqued Shelby's response to it as "beard[ing] an antagonist with a hyperbolic tale about Hostess Twinkies" (p.108). Shelby claims that a hypothetical marketing researcher's statement "'Mary owns a box of Twinkies' must be conjoined with the values of the observer (it is right or wrong to steal) for the statement to be a reason for acting in a certain way (i.e., stealing or not stealing)" (Hunt 2002, p.39). However, the observer's values are irrelevant.

By writing 'Mary owns', the hypothetical researcher implicitly acknowledges the normative aspects of ownership. "*Ownership* is a normative term…. [because it] can exist only within a normative framework—a web of promises and obligations" (HST, p.420). The Twinkie statement offers reasons for many possible actions. For example, the societally agreed-upon definition of 'own' makes it inappropriate to take one of Mary's Twinkies without asking her permission. In contrast, the statement 'Mary sees a box of Twinkies' is a positive statement because it offers no reason for action. Of course, this latter statement has no marketing implications.

Shelby and HST concur that context is critical for judging a sentence as either positive or normative—regardless of the assessment rule—and that his original semantic rule-of-thumb fails because it ignores context. However, HST's reason-for-action rule differs from Shelby's semantic rule in a critical way: HST's rule relies on nouns (e.g., ownership, obligation, rights, values, and needs) primarily and intransitive verbs (e.g., buying, selling) occasionally, but Shelby's rule relies on 'be' and auxiliary verbs. As the marketing-related nouns in most sentences written/spoken by marketing scholars—and the accepted meanings of those words within the contexts they appear—should be sufficient to identify a normative sentence, the increased richness of the reason-for-action rule makes it superior to any semantic rule. (Note: The *should* in the previous sentence does not make that sentence normative.)

Even if HST's reason-for-action rule is as flawed as Shelby's semantic rule-of-thumb, HST's rule reveals a pivotal aspect of marketing language:

it contains many hidden normative terms. Both the discussion about coupling positive and normative statements in Hunt (2002) and the lengthy Brodbeck quotation in Hunt and Speck (1985) affirm that conjoining a multitude of positive sentences with even one normative sentence transforms the entirety into a normative assertion. *As marketing sentences tend not to exist in isolation, and many marketing sentences contain blatant or hidden normative terms, it is all but impossible to construct a positive theory in marketing using current marketing vocabulary.*

Why is this attention to language important? If marketing science is restricted to the positive half of the positive-normative dichotomy, as Hunt (1976) states, and this positive domain essentially is empty (HST, p.421), then it may seem HST argues that marketing is not a science. (Hunt (1985) mentions that the positive/normative dichotomy is one of four dichotomies central to logical empiricism. Thus, to reject this dichotomy seemingly is to reject logical empiricism as well. Clearly, Shelby would prefer otherwise.) However, HST does not argue that 'marketing is not a science'; instead, it posits that marketing is a *normative science* rather than a *positive science.* Although HST states that "the only way to make marketing positive is to start anew" (p.421), it does not advocate rebooting scholarship in marketing. In fact, creating a positive science of marketing would deprive marketing of its power as a social science.

> [A]ny science which would be descriptive of human nature must at the same time be normative, i.e., must consider those natural laws of human existence which prescribe how men ought to conduct themselves and must use these standards in order to pass judgment on how men actually do con-duct themselves (Veatch 1945, p.286).

Instead, HST was meant to alert marketing scholars that marketing is a meritorious *normative science* (with attendant structure and

goals); hence, the caveat that "If marketing is normative, neither its criteria nor its aspiration should be those of a *positive* science" (italics added) (HST, p.420).

A normative science seeks good ways to achieve recognized aims, ends, goals, objectives, or purposes. It

> is perfectly legitimate as science....[is] concerned with reality and with the existing natural order, and…[has] method[s] for investigating this natural order which leads to knowledge and not mere opinion....[A] normative science… must be quite as descriptive in its way as the so-called descriptive sciences are in theirs....[It] must be quite as much concerned with discovering and describing actual laws of nature as is any natural science (Veatch 1945, pp.284-285).

For C. S. Peirce, normative science is

> the study of what ought to be, or norms or rules which need not but ought to be followed....The 'ought' implies ideals, ends, purposes which attract and guide deliberate conduct....The statements of normative science...make a truth claim ....[N]ormative science enables one deliberately to approve or disapprove certain lines of conduct. Thus it is the science which...makes the dichotomy of good and bad....[It] is purely theoretical ...[and its] business is analysis or definition" (Potter 1966, pp.7-8).

The three normative sciences identified by Peirce are logic, ethics, and aesthetics.

> Logic, as the study of correct reasoning, is the science of the means of acting reasonably. Ethics aids and guides logic by analyzing the ends to which those means should be directed. Finally esthetics guides ethics by defining what is an end in itself, and so admirable and

desirable in any and all circumstances regardless of any other consideration whatsoever (Potter 1966, p.14).

If, as Peirce argued, "truth and goodness are intimately connected" (Potter 1966, p.18), then marketing would be in good company as a normative science. As HST's authors concurred with Peirce's favorable valuation of normative science (Rescher 1978), they did not believe the lack of a positive domain would discredit marketing as a science. (Please see the table for useful quotes about normative science.)

Insert Table here

In 1970, Don Robin offered an ethical argument for a normative science of marketing. He posited that "The proposed *goal* for a normative science in marketing is to *maximize total satisfaction for consumers as a group*. As in any normative science, the objective is a value judgment about 'what ought to be'" (Robin 1970, p.75). In a subsequent challenge to Shelby's three-dichotomies model, Don argued that "positive statements can be made *about* normative judgments, thereby allowing marketers to use positive explanatory models" (Robin 1978, p.6); thus, he argued that the positive-normative dichotomy is "unnecessary and confusing" (Robin, 1977, p.138).

The idiosyncrasy and scope of HST's view (Hunt 2002, p.38) does not invalidate its conclusion. Throughout the history of science, many initially idiosyncratic views ultimately proved correct; for example, Wegener's theory of continental drift, Marshall and Warren's theory that H. Pylori causes peptic ulcer disease, and Lemaître's big bang theory. Furthermore, Shelby is correct that HST's reasoning extends to all social sciences; as he notes, "according to HST's logic, all the social sciences are overwhelmingly normative" (Hunt 2002, p.37). Nonetheless, HST is but one source for this assessment. Ziliak (2008) argues that economists' studies of *preference* illustrate that "the failure to acknowledge the normative element in the allegedly positive social sciences has been the source of mischievous errors"

(p.535). The article closes with the following claim: "Normative and positive continue to figure prominently in social science discourse and education. But the distinction rests on the so-called fact/value dichotomy, long collapsed" (Ziliak 2008, p.536).

Finally, Shelby's critique of HST ignores the *principle of charity*. The text from an earlier draft of HST that alluded to physical detection was deleted from the final version of HST for good reason: it did not reflect the authors' ontology and was superfluous to the overall argument. For the authors of HST, normativity is unrelated to physicality and observability.

An Amplification: Is Trust > Truth?[1]

At least three of the works reprinted in this volume (Hunt 1990, 2005; Hunt and Edison 1995) briefly discuss trustworthiness and the scholarly enterprise. The sole authored articles focus on truth and dedicate only a closing page to trust. Clearly, truth was a cornerstone in Shelby's long-running debate with proponents of relativism, constructivism, and postmodernism—a debate I believe he won. My concern is that Shelby's focus on truth obscured the importance of trust in the social sciences.

In Hunt (1990, 2005), Shelby argues that the statements of relativists, constructivists, and postmodernists cannot be trusted because

> any research project guided by a philosophy maintaining that the research does not 'touch base' with a reality *external* to the researcher's own linguistically 'encapsulated' theory, or 'paradigm,' or 'research tradition' would provide no grounds for the client [i.e., student, government official, consumer, academician, general public] *trusting* the knowledge claims of the researchers. Thus, philosophies…that abandon truth are not only self-refuting for their philosophical advocates, but self-defeating for practicing researchers (Hunt 1990, p.12).

Then (in both articles), he briefly discusses the relationship between ethics and trust. Unfortunately, the issue of trust in social science research is far broader than accepting or rejecting the knowledge claims of certain philosophical advocates.

I agree with Shelby that "marketing academics have a responsibility to respect, uphold, and abide by the university's core mission of producing, warehousing, and retailing knowledge" (Hunt 2007, p.278). That said, I wonder about the extent to which marketing academicians are compelled, by circumstance and/or character, to ignore this responsibility. If that disregard is wanton, then the entire scholarly marketing exercise is suspect. Numerous factors encourage such inattention, such as structural factors (e.g., pressure to publish), organizational factors (e.g., goal to become a top academic department), individual factors (e.g., aberrant personality), situational factors (e.g., financial problems), and cultural factors (e.g., non-Western ethical perspective) (Davis 2003).

Most discussions about egregious research misconduct focus on fabrication, falsification, and plagiarism (National Academy of Science 1992). Although cases of scientific fraud are well documented (Broad and Wade 1982; LaFollette 1992), I have encountered fraud only once (in a data fabrication episode I will not recount). Streaming—the practice of authoring the 'smallest publishable unit'—and idioplagiarism merely inflate journal pages (Vincent and DeMoville 1993). The resulting article-proliferation problem is easily managed by using aggregator web-based services (like Business Source Premier) and standard search engines (like Google). Thus, I will now focus on questionable but recurrent research practices that compromise the integrity of published findings in marketing.

The Trust Daisy Chain

Shelby planted the seed for this amplification almost two decades ago. He once berated me about a sentence that appeared in a conference paper I co-authored. I told him my co-author insisted on that sentence. Shelby's response: I am responsible for every word of any paper that I co-author. Although he was correct, I trusted my co-author's claim and accepted it uncritically. Like my colleagues, I continue to accept the many knowledge claims of other co-authors and scholars. Is this behavior risky? A survey of prolific academic accounting authors indicates the risk is non-trivial (Bailey, Hasselback, and Karcher 2001). Specifically, authors' answers suggest that roughly 4% of articles in the most prestigious accounting journals are tainted; even worse, they believed that more than 20% of articles in these outlets are tainted!

Hardwig (1985) claims that

> one can have good reasons for believing a proposition if one has good reasons to believe that *others* have good reasons to believe it and that, consequently, there is a kind of good reason for believing which does not constitute evidence for the truth of the proposition….[Thus] appeals to epistemic authority are essential in much of our knowledge (p.336).

If experts depend on other experts to obtain and advance knowledge, then general knowledge resides with the community rather than each person. As a result, "the trustworthiness of members of epistemic communities is the ultimate foundation for much of our knowledge" (Hardwig 1991, p. 694). In essence, knowledge claims by trustworthy sources are distributive and "epistemic individualism (or epistemic independence) is not the right model…of how cumulative science proceeds" (Blais 1987, p.369).

Based on a Prisoner's Dilemma framework, Blais (1987) argues that

> Cooperation in science does not mean only working together; it means not defecting in the knowledge game…. Defection means succumbing to the

temptation of leaving the other players in the knowledge game with the sucker's payoff….The immediate gain for a scientific defector is the recognition that accrues to anyone who publishes in respectable journals. The temptation can be great; for, without publications, good jobs are scarce, academic tenure is impossible, and grants are not forthcoming. But, if an individual researcher cheats, the whole community stands to lose (pp.370-371).

Adler (1994) and Blais (1987) contend that replication and blind reviewing, which can rapidly detect research misconduct, are vital mechanisms for maintaining trustworthiness (although Blais (1987) acknowledged that "power and money can counter-balance a dishonest reputation to a certain extent" (p.371)). Supposedly, this threat of a diminished reputation is sufficient to deter most scholars from 'defecting'.

Unfortunately, these error-checking mechanisms perform poorly in the social sciences because researchers can offer many plausible reputation-saving excuses; for example, they can blame irreproducible results on temporal, regional, contextual, or sample idiosyncrasies. The cost, modest publication likelihood, and minimal recognition of the researcher discourage replication studies. Perhaps *social science* is a misnomer because epistemic independence is impossible and excessive defectors in the knowledge community's tit-for-tat game severely compromise the integrity of the knowledge base (Blais 1987).

Threats to Literature Integrity

Although numerous surveys have explored the problematic behaviors of business journal authors, few of the queried behaviors directly affect the quality of reported results. Typically, these studies focus on improper treatment of colleagues and their ideas, misuse of university resources, conflicts of interest, and so forth. Author-centric problems tend to focus on data-

related fraud, such as falsifying or fabricating data or information, omitting test results because they lack statistical significance, and refusing to share data upon request (Borkowski and Welsh 1998). Of 30 problematic behaviors examined in Meyer and McMahon (2004), only six pertained to trustworthiness of published reports; four to data issue (destroying original data, altering data to conform to a favored theory or inflate statistical significance, failing to report contrary data and/or results), one to replication (denying another researcher access to data), and one to suppressing work by a competing researcher. Similarly, of the roughly 60 ethically problematic behaviors studied in Mason, Bearden, and Richardson (1990), only four could compromise trustworthiness: failing to report contrary data in a manuscript, destroying the research instru-ment and data after study completion, denying other researchers' requests for data, and manipulating data to achieve acceptable results. Only two of 11 questions about research and publication practices asked by Laband and Piette (2000) related to trustworthiness: "failing to report contrary data/findings" and "manipulating the data so as to achieve acceptable results."

Here are some threats to literature integrity that I have encountered but have been ignored in previous faculty surveys.

'Code Blue' Collaborations

The number of co-authored articles and number of co-authors per article continues to increase (Hyman and Yang 2001; Wray 2002). Why? "[C]ollaboration plays a causal role in advancing scientists' epistemic goals, and...its growing popularity is a consequence of its effectiveness" (Wray 2002, p.151). In many cases, collabor-ation can increase research quality by combining expertise beyond the scope of any one researcher—thus making otherwise infeasible studies feasible—and boosts productivity by leveraging productive researchers' time and energy (Wray 2002). However, structural and organizational factors (Davis 2003) can compromise the integrity of

work produced through collaboration.

I recently was awarded a new professorship. The primary mandate for this professorship is mentoring my college's faculty and doctoral students (current and ex) on research. Although mentoring could include offering sage advice and brainstorming, I am certain that one metric attached to assessing my performance is the number of co-authored publications with these persons. Unfortunately, that metric is at best irrelevant and at worst counter to literature integrity.

Out of loyalty to colleagues and protégés with lagging research records, I have participated in several 'Code Blue' (manuscript resuscitating) collaborations (Hyman 2001). Although these efforts yielded several useful theory-oriented articles, revising the empirical manuscripts and tweaking them in response to referees' comments has entailed downplaying research flaws (for example, problematic pre-testing or data collection procedures), augmenting original literature reviews to provide additional support for already tested hypotheses, performing forensic data analyses without the data-collecting co-author's assistance, and forcing original data summaries (tables and graphs) to suffice because the original data was misplaced. To boost research trustworthiness, I could have insisted on 'new and improved' data collection and analysis. In response, my struggling co-authors would have relegated these manuscripts to a 'circular file' (or its electronic equivalent), which in turn would have jeopardized their job/tenure prospects. I suspect many academicians routinely face this collaboration dilemma.

Questionable Data Analysis Practices

Overt lapses in proper data analysis and reporting includes "dichotomomizing continuous data (i.e., median splitting), which drastically reduces variability and can create significant results" (Sterba 2006, p.307), cross-validating exploratory data with confirmatory analyses, and ignoring alternative models that fit as well but imply non-preferred theoretical underpinnings.

Covert lapses include omitting outliers to boost significance levels, misrepresenting data-driven exploratory analysis as theory-confirming analysis, and selective reporting model-fit indices and the like (Sterba 2006). Additional practices that compromise research integrity are as follows.

Failing to Admit Subsequently Discovered Errors. After extensive self-application of a social lubricant, a colleague confessed this post-publication discovery: similar analyses run for a subsequent study revealed a methodological error in the original study that invalidated all published findings. The study, which was reported decades ago in a prestigious marketing journal, remains unchallenged. Neither the review process nor any subsequent (partial or full) replication spotted the problem. Entrenched author disincentives—to reputation, future pay raises, co-author and resource access, and so on—guarantee my colleague will not retract the article. Again, the research environment discourages the weeding out of unreliable reports.

Ignoring Idiosyncratic Empirical Results. As a precursor to improving a popular multidimensional attitude scale, I reviewed all the published empirical studies based on questionnaires that included the original version (Hyman 1996). Of the then 18 studies that included the scale or a superset of scale items, the reported factor structure correlated highly with authorship. Specifically, the scale developers and their colleagues reported a consistent factor structure, which they offered as evidence for a highly reliable and valid measure. In contrast, other researchers almost always reported a different factor structure, which implied a theoretically problematic measure. I did not then, and do not now, accuse the scale developers of fraud. Frankly, I suspected this improbable pattern of findings—ceteris paribus—was attributable to local research artifacts that repeatedly yet unknowingly biased their studies systematically. Nonetheless, I expected my article would receive a chilly reception from the scale developers and discourage future use of the scale. Instead, the reputation of these scale developers was unaffected and survey

researchers continued to use the scale.

Questionable Editorial Practices

Although the problematic behaviors of business journal editors and reviewers are well-known (Carland, Carland, and Aby 1992) and frustrating to authors, few of these behaviors affect the trustworthiness of reported results. Of the 19 problematic editor/reviewer behaviors studied in Borkowski and Welsh (1998), only five could affect the quality of results directly: for editors, steering a paper to reviewers who are likely to accept/reject it; for reviewers, rejecting a manuscript that challenges one's published work, evaluating a manuscript despite lack of qualification, and pretending manuscript assessment was 'blind'. Two ignored yet problematic editor behaviors are as follows.

Stacking the Review Process. Circumstantial evidence (which I prefer not to review for obvious reasons) suggests that journal editors are more likely to publish manuscripts submitted by friends, ex-students, and local colleagues. For special journal issues, I have benefited and been hindered by predisposed editors. On the benefit side, a professional acquaintance who was short a few acceptances volunteered to 'fast track' any manuscript I submitted. On the hinder side, more than one special issue editor seems to have 'allocated' many of the article slots to loyal protégés and colleagues; as a result, the call for papers was not truly open.

Self-Publishing as Special Issue Editor. Although many journals prohibit special issue editors from including their own articles—other than an introductory essay—other journals enforce no such restrictions. As a result, some special issues include non-introductory articles co-authored by an editor who may have solicited lax reviews or sidestepped the review process completely.

Other Questionable Practices

Wording Questionnaires Carelessly. The extensive survey research literature offers many good reasons for designing questionnaires meticulously. Regardless, several colleagues have told me—despite their awareness of my survey methods research interests and doctoral seminar—that precisely worded questions and properly pretested questionnaires are unimportant because 'previously used scales were adapted and respondents will interpret each question correctly'. Unfortunately, such seemingly trivial sloppiness can compromise study integrity meaningfully.

Knowingly Biasing Questionnaire Design. To avoid anchoring effects, questionnaire designers should shuffle the questions from their multi-item scales. In addition, the items used to measure a construct should jointly exhaust its domain. Nonetheless, researchers often place scale items consecutively and construct overly redundant and incomplete multi-item scales (seemingly with the aid of a thesaurus) to inflate scale reliabilities, which artificially enhances the publication prospects of their survey-related manuscripts.

Misrepresenting Previously Published Work. Given their relative skill sets, professors often assign the literature review to doctoral student co-authors. I used to insist that doctoral students provide a copy of all works referenced in our manuscripts. Then, as a dutiful co-author, I read them all to ensure they were represented accurately. However, as the years progressed and I worked concurrently with evermore doctoral students who had increasingly divergent research interests, I read a progressively smaller proportion of these articles. Thus, I can no longer guarantee that previously published work referenced in these co-authored manuscripts has been characterized properly. I suspect this problem is not unique to me.

Improperly Reporting Data Collection Methods. To avoid 'stacking the deck', content analysis coders should be naïve to the research hypotheses. Yet, I know of two advertising studies in which the researchers saved time (in coder training) and money (in coder remuneration) by coding the ads themselves.

Given the well-known unacceptability of this shortcut, their study-related manuscript implied—but did not claim—otherwise.

Wanted: A Solution

We must maximize the trustworthiness of marketing's scholarly literature. "Declining trust in the existence of long-term exchange relationships increases transaction costs, because people must engage in self-protective actions and be continually making provisions for the possibility of opportunistic behaviour by others" (Tyler and Kramer 1996, p.4). Thus, if we fail to defang the current threats to research integrity, then either the trust described by Hardwig (1985, 1991) will evaporate—as researchers perpetually defend themselves against untrustworthy reports—or marketing's knowledge base will increasingly resemble a house of cards.

Typically, I do not present a problem unless I can offer a viable solution. Unfortunately, the best I can provide here is a mandate. Academia's vaunted check-and-balance system, anchored by the double-blind reviews and replications that effectively—but not unerringly—safeguard the physical and biological sciences, fails excessively in the social sciences. Proposed remedies typical entail either enhancing ethics education or restructuring the academic reward system. Although education seems to deepen researchers' awareness of proper conduct, there is little evidence that it changes attitudes or behaviors (Plemmons, Brody, and Kalichman 2006). Thus, restructuring the reward system in yet-to-be-determined ways seems the best hope.

Postscript

Shortly after he accepted my manuscript that eventually appeared in the October 1987 issue of *Journal of Marketing*, I bumped into Shelby at an AMA conference. In response to my profuse thanks, Shelby said he had performed one small but critical favor for me: he sent my manuscript to reviewers who would read it carefully.

Although initially flattered by this seemingly special treatment, in retrospect I believe—given the high quality of *Journal of Marketing* under his editorship—that Shelby extended this 'favor' to many authors. Through my subsequent refereed articles and special journal issue gigs, I learned that the ability to properly match manuscripts with reviewers is rare editorial talent. Thus, we should honor Shelby for his contributions as a premier author and editor.

Footnote

1. This amplification will parallel my previous positive-normative-dichotomy challenge in that it is generalizable to all social science. Although its scope makes it inconvenient, it does not invalidate the amplification.

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Table
Useful Quotes about Normative Science

Topic	Quote
Logic depends of ethics	"Logic deals with thinking, and thinking is a kind of deliberate activity. It therefore has an end. But if ethics is the science which defines the end of any deliberate activity, it also defines the end of thinking. Logic is a study of the means of attaining that end, that is, the study of sound and valid reasoning. The dependence of logic on ethics, therefore, is apparent" (Potter 1966, pp.12-13). "[N]ormative ethics is concerned with behavior in relation to the Ends of action; it does not ask simply about good or bad, but *what makes good good and bad bad*. Ethics determines rules and aims for rational action, therefore its bond with logic is obvious since logic is the study of means, the study of solid and proper reasoning. Peirce claimed in this regard: 'It is, therefore, impossible to be thoroughly and rationally logical except upon an ethical basis'" (Janik 2007, p.136) "[G]ood reasoning and good morals are closely allied....[T]o reason well, except in a mere mathematic way, it is absolutely necessary to possess, not merely such virtues as intellectual honesty and sincerity and a real love of truth, but the higher moral conceptions" (Burks 1943, p.192) [quoting Peirce]. "The distinctions which are of interest in normative science are those of kind, not of degree....[Thus], the important questions for normative science is not how good something is, but whether it is good at all....Peirce would not deny that one might be able to set up a quantitative scale of measurement to indicate the degree in which a certain set of concrete subjects share in goodness, but he would insist that such a scale is to some extent, at least, arbitrary and can never claim exactitude" (Potter 1966, p.10).
Aesthetics	[E]sthetics, as a theoretical discipline, does not judge this or that to be beautiful or ugly, but tries to decide what makes the beautiful beautiful and the ugly ugly. It has to do with norms and ideal in terms of which we can define and ultimately apply to these categories ....Esthetics, then, deals with ends...in themselves. It studies the admirable *per se*, regardless of any other consideration....As such it needs no justification, it is what it is and gives meaning to the rest (Potter 1966, p.13)
Goodness versus badness	"[T]he normative sciences...all have to do with goodness and badness, with approval and disapproval. Thus the essence of logic is to criticize arguments, that is, to pronounce then acceptable or not, good or bad" (Potter 1966, p.20) "[T]he true is an aspect of the good and that therefore logic can be satisfactorily studied only once it has taken into consideration its purpose and end" (Potter 1966, pp.16-17). "To make a normative judgment is to criticize; to criticize is to attempt to correct; to attempt to correct supposes a measure of control over what is criticized in the first place. Any other kind of criticism, any other conception of goodness and badness is idle" (Potter 1966, p.20). "[A] good aim is one that can be consistently pursued, a bad aim is one that cannot. It follows…that a bad aim cannot be ultimate....The question, then, is to ascertain what end or ends are possible, that is, what end or ends can be consistently pursued under all possible circumstances" (Potter 1966, p.26).
Ought	"'[O]ught' presumes a *surplus*, a *something more*, in determining a future action. There is no room for *ought* where *must be* prevails or else where there is *no choice at all*….[It] involves freewill, auto-determination, a voluntary act, and therefore 'implies ideals, ends, purposes which attract and guide deliberate conduct'" (Zanik 2007, p.134).