MARKETING SCIENCE
Vol. 23, No. 2, Spring 2004, pp. 173–179
ISSN 0732-2399 | EISSN 1526-548X | 04 | 2302 | 0173

informs

DOI 10.1287/mksc.1040.0078

Editorial

Consulting, Research, and Consulting Research

Steven M. Shugan
Warrington College of Business, University of Florida, Bryan Hall, Box 117155, Gainesville, Florida 32611-7150,
steven.shugan@cba.ufl.edu

Consulting and scholarly research often have very different objectives with respect to advancements in practice, theory, and observation (e.g., data collections). For example, consulting often emphasizes immediate benefits, specialized applications, and a focus on only the key variables. Scholarly research often emphasizes replicability, generalizability, and introducing previously uninvestigated variables. However, these activities complement each other, and each activity is important for the advancement of the other. Benefiting from that complementarity requires the literature to bridge knowledge gained from each activity. It is unnecessary for every researcher to try to bridge theory and practice by working on the interface between academics and practice. However, it is critical that some researchers do so. This issue of *Marketing Science* examines several excellent applications of *Marketing Science* that provide detailed microexaminations of the fundamental marketing practices that we seek to understand and improve.

Beyond demonstrating how to solve specific problems in practice, in my opinion, the articles and commentaries in this issue also illustrate at least the following ideas: Short-term tactics can produce significant short-term advantages. Existing models in the literature can be useful with proper implementation. State-of-the-art research is most useful for infrequent decisions. Finally, knowing the decision-making context is essential for determining which variables to include in the analysis.

Key words: marketing models; scholarly research; industry practice; academic consulting; theory

1. Consulting

Consulting by a faculty member of a university involves providing a professional or technical service to benefit a specific client (i.e., a third party), where a fee-for-service or equivalent relationship exists with the client. Consulting by academic faculty is an interesting and widespread activity that evokes vastly different reactions from different groups. Once, great universities frowned on academic consulting. Some university administrators considered academic consulting to be only a distracting activity that diverted faculty from the primary mission of the university (i.e., teaching and scholarly research) and to be used solely to enrich individual faculty members at the expense of the university (Sennetti 1981). Hence, these administrators imposed time limits and other restrictive conditions on outside consulting activities. Even today, some universities, as well as many faculty members, still question the distracting nature of consulting activities (Dacin 2003).

Over time, many universities gradually grew more sympathetic toward academic consulting as administrators realized the benefits from faculty consulting. Given limited funding to pay high faculty salaries, allowing outside consulting can keep key faculty members from leaving (Whitford 2000). Universities also capture overhead revenue from consulting when the outside consulting produces contract research (Flynn 2000). As business executive education has burgeoned, business schools, under increasing pressure to appease more experienced students, have found that faculty with consulting experience are often more effective in the classroom (Whitford 2000). Finally, despite controversies concerning confidentiality (Fine and Castagnera 2003) and conflicting incentive structures (Newberg and Dunn 2002, Geuna 2001), as well as dire warnings that it is dangerous for industry to drive research agendas (Crowther and Carter 2002), joint ventures between universities and industry are now common and continue to generate considerable revenue for most universities (Santoro and Betts 2002).

Of course, consulting performs a valuable function for the hiring clients (i.e., at least a function that has substantial monetary market value). That function probably goes far beyond providing political support, in the form of academic credibility, for the positions of the clients. Academic consultants have highly specialized information and skills.

Editorial pages are not part of the regular *Marketing Science* page budget. We thank the INFORMS Society of Marketing Science for paying for all editorial pages. We also thank the Society for granting every page supplement requested by the current editor.

Steven M. Shugan is the Russell Berrie Foundation Eminent Scholar in Marketing.

One might expect that academic consulting will remain common, and perhaps become more prevalent, as knowledge increases at an exponential rate, further heightening the need for specialization and increasing the corresponding demand for specialists. In addition, specialization leads to more outsourcing because firms face more nonrecurring decisions (Shugan 1994). A firm, for example, might only occasionally need to enter a new market, defend against a new entrant, face a new regulation, adapt to a shift in consumer preferences, encounter a new technology, or encounter a major exogenous shift in the market. Most firms are probably fairly proficient at routine reoccurring decisions (given extensive past experimentation) relative to new unexpected decisions.

One might also expect that academic consulting benefits both the clients and the consulting academics as well. Beyond the financial rewards and beyond providing anecdotes and credibility for more effective teaching, consulting might provide academics with many advantages for academic research. These benefits might include a greater awareness of contemporary management problems, business institutions, data availability, decision-making contexts, and issues for future research.

Unfortunately, despite the straightforward spillover benefits of consulting for teaching, the creation of spillovers for research are less apparent. Bost and Haddad (1996), for example, reports only minimal benefits from consulting for generating academic research ideas and gaining access to databases. Requirements for consulting, which include privacy concerns, little need for state-of-the-art techniques, and a priority for immediacy over rigor, each hinder subsequent open publication of findings and limit possible spillovers for scholarly research.

Consulting in the form of litigation support creates still greater obstacles for spillovers, given ironclad confidentiality agreements. Although it is insightful to view confidential memos from different competitors and to compare actual shipment data for every competitor with reported Nelson data, these insights usually fail to survive the peer-review process without some collaborating evidence.

Moreover, consulting puts considerable time demands on faculty as they compete with many new private consulting firms. Modern consulting requires substantial investments in relationship management (Weiss 2001) and protracted investments in implementation (Berry 1997). These investments often create insurmountable demands on faculty time that can frustrate active scholarly research programs. Argyris (1996), who discusses consulting activities in depth, quotes Cornelis J. Lammers (1981), who proclaims that "it is possible for scholars to be scholarly consultants" but it is "empirically rare . . . why should not those less gifted mortals, who apparently are endowed 'only' with scientific capacities and inclinations be permitted to stick to their métier?"

Finally, academic faculty, who are attempting to allocate their limited time, now have many more opportunities and obligations (Faria 2000, Rapert et al. 2002). These opportunities and obligations include traditional teaching, executive education, Internet teaching, serving as expert witnesses, activities with academic centers, university service, overload teaching, doctoral programs, contract/grant research, and recruiting. One cynical economist (McKenzie 1979) claims that unproductive faculty members (in their self-interest) also conspire to create time demands on productive faculty members. In sum, outside consulting activities can produce valuable input for academic research, but the cost can be high.

2. Scholarly Research

It seems fairly easy to contrast scholarly research with both consulting and most contract research that involves producing specific deliverables for a funding client. Unlike confidential consulting projects, the results of scholarly research are completely open, are public, and are subject to scrutiny. Nothing should be hidden, and every detail should either be revealed or revealed upon request in the peer-review process. Unlike consulting projects, the results of scholarly research should be replicable so that another researcher can reproduce the project from scratch. Unlike many consulting projects, the financial rewards to the researcher from scholarly research should be independent of the findings of the research. Hence, scholarly research should have independent credibility. Unlike many consulting projects, scholarly research should use the best available tools, methods, and data for the task despite the sometimes-protracted time requirements. Also, unlike consulting that must only produce new knowledge for the client, scholarly research should produce new knowledge (e.g., findings, methods, approaches, theories) that is new to the entire academic literature (i.e., advances the state of the art). Unlike consulting, where a less-than-perfect solution is better than none, research must provide compelling arguments for all claims and include appropriate caveats. Unlike consulting reports that stress actionable recommendations, scholarly research articles must meet the standards of peer review, including rigor, technical accuracy, and substantiation of the conclusions.

Although, as argued earlier, consulting and interacting with practitioners can benefit scholarly research, these activities are superfluous for solving most research problems. Although practitioners can offer valuable theories, they often are unable to provide the evidence required by academic standards.

For example, researchers can determine when retailers should set prices by haggling rather than fixing prices without funding from a specific retailer (Desai and Purohit 2004). Researchers can determine when paid licensing is superior to free licensing without advising a specific licensing firm (e.g., Sun et al. 2004). Researchers can demonstrate that deeper price discounts in the current period increase future purchases by first-time customers while reducing future purchases by established customers without the need-specific input from management (Anderson and Simester 2004). Also, without the advice or support of any specific organization, researchers can discover that when unsolicited advice contradicts a consumer's initial impression, the consumer will often ignore that advice (Fitzsimons and Lehmann 2004). To draw an analogy, cancer researchers can do important and even breakthrough cancer research without talking to cancer patients (or being one).

Given that immediate applications are not absolutely necessary for most scholarly research, we might wonder whether that scholarly research remains relevant to practitioners. One might expect that in all applied academic disciplines and, in particular, in professional areas such as marketing, there would still be a strong relationship between scholarly research and practice because, even without practitioner input, the academic literature itself would draw most researchers to relevant topics. Of course, that relationship has been and continues to be highly controversial.

Academic research is often criticized for a lack of relevancy for practitioners, the difficult writing style of academic articles, the inattention to problems in implementation (e.g., constraints facing practitioners), the narrow nature of the research (e.g., not considering the big picture), and for being data-driven rather than problem-driven (Shugan 2002). For example, Ankers and Brennan (2002) find that "experienced marketing practitioners . . . knew very little about the current state of academic research in marketing, and considered that academic researchers did not understand the realities of business life and could not communicate effectively with managers." Crowther and Carter (2002), referring to management education, argue that academics "are in fact legitimating their own irrelevance and marginalisation and that the discourse of teaching management subjects needs to include not just academics, and their needs and desires, but also those of their customers."

In stark contrast to these views, other researchers argue that academic research is invaluable to practitioners. Given that research involves new ideas, using a new product analogy, we would expect that most extremely innovative ideas will fail but that the development process will yield a few extraordinary ideas that will have considerable impact on practice. Hansotia (2003) argues that "there is a diffusion process before a method proposed by academics becomes mainstream or at least widely accepted by leading practitioners." Indeed, a recent publication by a major market research consulting firm (Orme 2000), referring to Hierarchical Bayes (HB) methods, states: "Until recently, we too . . . were doubtful that HB would soon achieve very widespread use in the marketing research community. But . . . knowledgeable academics such as Greg Allenby of Ohio State have taught tutorials, published algorithms on HB estimation, and have supported the efforts of individuals such our ourselves in creating off-the-shelf HB software."

Turning to general academic research, Mansfield (1991) found that 11% of the new products in seven manufacturing industries could not have been developed without academic research. Grossman et al. (2001) finds substantial contributions of academic research in five industries—aerospace, financial services, medical devices, network system/communications and logistics/transportation.

Hence, the value of scholarly research is hotly debated with extreme positions being taken on both sides. In fact, some authors argue the validity of both sides. Sutton (2004), for example, argues that many professors are "remarkably ignorant about what actually happens in organizations" and that they only "spend their time . . . analyzing archived statistical evidence on computers, doing contrived experiments with undergraduates or MBAs, or writing "theory" that is based in pure logic that is unsullied by actual evidence." Sutton (2004) also argues that "academics use fancy and complex jargon to disguise that they are studying simple things: power, money, and conflict." However, Sutton (2004) does conclude that "despite all the drawbacks, scholarly research has clear advantages in comparison to other information sources such as popular business magazines, books, gurus, and management consulting firms . . . Academic researchers are held to high standards for the quality of [their] logic and [their] evidence, and are expected to point out possible flaws in [their] own work." Finally, Sutton (2004) argues that scholarly research is valuable because academics have the "luxury of time" that allows a more "long-term, rigorous" approach.

3. Consulting Research

Sometimes, it is possible to bridge scholarly research with consulting to create consulting research. This research is often immediately relevant to practitioners while still being more rigorous, better documented, and less limited in scope than most traditional consulting projects. Consulting research, as with all forms

of research, will excel on some but not all attributes. One attribute or contribution of this research is the "bridging function." Consulting research can often bridge well-known academic research with the immediate problems faced by practitioners. That contribution is both an immediate gain for the client as well as new knowledge for the academic literature. However, that new knowledge might be more related to implementation issues, ideas for future research, information about priorities faced by practitioners, and the adequacy of current theories than to the development of new abstract theories or advances in the so-called start of the art.

In my opinion, the most important part of the bridging function is the identification of irrelevant variables. We have no easy academic techniques for uncovering irrelevant variables. It is often easy to show that a variable matters (i.e., explains some variance). With sensitive measurement and multiple measures, for example, one might be able to show that sunspots influence the price of cornflakes through their influence on the weather and, subsequently, wheat crops (Muir 2004). However, we have no easy method for testing whether one should consider sunspots when making decisions about promoting cereals.

It is difficult to find that a variable does not help in some way (i.e., explain some variance or influence a decision to some degree). After all, statistics is only designed to find significant variables. If a relationship is not found, we do not know whether the researcher did not look sufficiently hard or whether there was no relationship. A lack of significance might imply a more complex relationship (or a different one) than the research examined. It might imply a lack of precision in the data collection. It might imply that the wrong measure was used. The researcher might have used an inappropriate analysis, used inappropriate moderating variables, had an insufficiently large sample size, used an insufficiently powerful statistical test, neglected heterogeneity, or just had insufficient data. We are unable to prove that no relationship exists by simply saying we found none. A researcher who fails to uncover a relationship has no findings.

In sum, it is nearly impossible to produce scholarly research that shows that a variable is completely unimportant in an analysis, particularly when one can make a loose theoretical argument that the variable should have some influence. Moreover, the peer-review process for scholarly journals nearly prevents authors from publishing negative findings—as it should prevent it.

Consequently, it is difficult to determine which variables to include in a model. The inclusion decision, as most decisions do, goes far beyond statistical considerations. There are costs associated with additional measurement. There is a loss of focus on the key issues when additional factors are introduced. There is a loss of tractability and learning as the analysis begins to include only marginally important variables. Also, results can become less stable as the number of variables increases.

Consulting research allows us to determine (to some degree), from the actual decision maker in an actual context, what factors dominate the decision (at least, in that context). Moreover, our observations usually only consist of existing practice. If some decision makers change their actions because of the research, we observe the consequence of changing existing practice and the subsequent results. That information creates valuable variance in our analysis. Hence, consulting research has the great potential to determine how to simplify, which variables to include and, more importantly, which variables can be excluded, ignored, or made exogenous (e.g., Shugan 2004). The decision maker's experience and ability to interpret the results becomes an additional source of information.

Consulting research also provides an independent assessment of the research on a variety of criteria not usually found in peer review. For example, the decision maker might consider ease-of-implementing the recommendations, the ability of the decision maker to interact with the research (i.e., learn from the research, rather than be told what to do by the research), the ability to reproduce conclusions already known by the decision maker, and the ability to be involved in the direction of the research. For example, in developing a marketing decision-support model, Gensch (2001) states: "Modelers must recognize that decision-support systems are more than technical algorithms for integrating and analyzing relevant information. By explaining the logic of the modeling approach and giving guidance on the type of data input required, modelers can work with the managers to identify, obtain, and organize better information than that currently available."

We should admire the few researchers who do consulting research and who are willing to make the substantial effort to publish that research in scholarly flagship journals. Publishing this research in flagship journals is difficult because academic reviewers often ignore the previously discussed strengths of this research (Shugan 2003). Other channels (e.g., applied journals, books, etc.) are more sympathetic. As Parasuraman (2003) notes that "some scholars who have had sustained success in, say, publishing their work in the discipline's flagship journals may want to stay focused on that particular pathway to maximize their contributions to the discipline's core body of knowledge, a highly commendable pursuit."

Hence, there is no intent here to encourage all researchers to publish consulting research.

There is, however, a strong argument that the bridging contribution of this research is so significant that this research should be widely read by everyone doing scholarly research. Although the majority of researchers must devote nearly all of their effort to their narrow expertise, every researcher in an applied area such as marketing, regardless of their personal expertise, must sometimes examine the interface between scholarly research and marketing practice at a more than superficial level. For that reason, most researchers should conscientiously follow research done on the interface between the academic literature and practice. It is not necessarily the case that this research must always apply previously published methods and findings. Consulting research can be extraordinarily original and provide completely new perspectives. However, this type of research can have an impact on scholarly academic pursuits without having these additional contributions.

Another reason why we need consulting research is very fundamental. Researchers sometimes, if not often, choose research problems based on gaps in the current academic literature. Indeed, it is necessary to find a gap in order to ensure a contribution. Conversely, using product markets as an analogy, gaps in the market often exist because there is no demand for a product in that location. Indeed, new products should both fill gaps and satisfy real needs. It is only by having consulting research that we can ensure that the gaps in the literature have corresponding relevance. We are, after all, an applied discipline with several very obvious important constituencies that include practitioners.

Economists who seek to advise policy makers on antitrust concerns (e.g., inferring collusive behavior, understanding the implications for extant regulation) might sometimes glance at consulting activities on current policy formulation. Finance researchers who seek to determine whether minority shareholders require better protection might sometimes glance at consulting activities related to minority shareholder litigation. Accounting researchers who evaluate business unit performance might sometimes glance at consulting activities that implement tools such as the balanced scorecard. Examining actual practice, in a more than a superficial manner, provides a reality check for research.

Researchers in marketing who seek a better understanding of information in UPC scanning data, the impact of slotting allowances, retailer pricing, and other marketing topics should also examine (in some detail) contemporary consulting activities and industry practice related to the corresponding topics (e.g., see Bucklin and Gupta 1999, Rao and Mahi 2003, and Shankar and Bolton 2004 for excellent analyses of actual industry practice).

4. Scholarly Consulting Research

Beyond consulting research, there is research that excels on relevance as well as rigor and innovativeness. This research applies both theories and concepts in the extant literature while developing both new theory and solving a problem in the context of a real application (i.e., an immediate need). This scholarly consulting research is difficult to execute and might only be done by senior researchers with both credibility and tenure. However, when properly executed, this research provides important directions and lessons for other researchers, regardless of whether they ever intend to do applied work.

Professor Gary Lilien has initiated, implemented, and nurtured the INFORMS Society for Marketing Science Practice Prize for the outstanding implementation of marketing science concepts and methods. The prize, at minimum, encourages researchers to publish articles that develop new theory and models in the context of an application. The prize also encourages authors to meet both the scholarly standards of *Marketing Science*, such as rigor, accuracy, and originality, and the standards in practice such as having the ability to implement, having the required flexibility, and having an impact (please see Lilien 2004 for more details).

Marketing Science is very pleased to publish the 2003 winner of the Practice Prize, "Optimizing Rhenania's Direct Marketing Business Through Dynamic Multi-Level Modeling (DMLM) in a Multicatalog-Brand Environment" (Elsner et al. 2004). This implementation produced original scholarly research that provided spectacular returns for the implementing firm (Lilien 2004), as well as advancing the state of the art in academic research in an area receiving increasing attention (e.g., Anderson and Simester 2004).

This issue of *Marketing Science* includes commentaries by the other two finalists for the 2003 Practice Prize, i.e., "Predicting Sales Takeoff for Whirlpool's New Personal Valet" by Joseph A. Foster, Peter N. Golder, and Gerard J. Tellis and "Implementing a Prelaunch Diffusion Model: Measurement and Management Challenges of the Telstra Switching Study" by John H. Roberts, Pamela D. Morrison, and Charles J. Nelson (see Lilien 2004). The first commentary further develops Golder and Tellis (1997), an article previously published in *Marketing Science*. The second commentary further develops Roberts et al. (2005), a forthcoming article.

Golder and Tellis (1997) discover that initial new product growth or takeoffs tends to appear as an elbow-shaped discontinuity in the sales curve rather

than the typical smooth sales curve shown in most textbooks. The original article has already had considerable influence on the important area of new product research (e.g., see Srinivasan et al. 2004, Tellis et al. 2003, and Agarwal and Bayus 2002).

Roberts et al. (2005) produce a valuable and substantial advance to the literature on defensive strategy (e.g., Hauser and Shugan 1983, Kumar and Sudarshan 1988, Robinson 1988, Kalra et al. 1998). This article, as well as the commentary in this issue of *Marketing Science*, introduces solutions to many new issues in defensive strategy makes major inroads as well on the topic of implementing defensive strategy.

The finalists and the winner of the Practice Prize each provide very general and useful lessons and insights for all researchers. In fact, all three projects provide at least the following insights. First, sometimes a short-term tactic can produce significant short-term advantages. For example, Roberts et al. (2005) show that an incumbent telephone company, defending against a new entrant while facing a cost disadvantage, can drastically slow the progress of a new entrant by having cheaper rates during just part of the day. That tactic prevents the new entrant from claiming to be cheaper for every consumer (see Bergen et al. 1996 on the related concept of comparability of brands).

Second, each of these research projects illustrates that existing models in the literature can be made useful for everyday managerial activities, given some minor but critical modifications. For example, managing firm expectations regarding what can be done is an important part of implementation, as well as using data from similar prior situations faced by similar companies.

Third, state-of-the-art research is not needed for everyday managerial decisions but might be required for many infrequent decisions. In each of the three research projects, the firm faced a crossroads that required a major but infrequently made decision. In each case, the research project caused the firm to take a discontinuous strategic action.

5. Conclusion

In sum, it is unnecessary for every researcher to work on the interface between academics and consulting. Scholarly research and consulting perform different functions, and both functions are valuable. However, it is critical that some research does bridge the gap between theory and practice. It is important that some research provides a detailed microexamination of the fundamental marketing practices that we seek to understand and improve. When that research can produce a scholarly article that bridges academic theory and industry practice, we should enthusiastically acknowledge the "bridging" contribution and be sure to thoroughly read and use the knowledge created by that successful effort.

Marketing Science is pleased to publish the winner of the 2003 INFORMS Society for Marketing Science (ISMS) Practice Prize and Commentaries by the finalists, as well as a special introduction by the ISMS Practice Prize Competition Chairman. Beyond demonstrating how to solve specific problems in practice, in my opinion, this research also illustrates that short-term tactics can produce significant short-term advantages; that existing models in the literature can be useful with proper implementation; that state-of-the-art research is most useful for infrequent decisions; and that knowing the decision making context is essential for determining which variables to include in the analysis.

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