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# Market Measurement and Analysis: The First “Marketing Science” Conference\*

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A few years after I had joined the faculty at Stanford University's Graduate School of Business in 1975, I received a call from Frank Bass. During the conversation, which may have focused on our response to a comment on a paper we had published in the *Journal of Marketing Research*, he suggested that I consider organizing a conference on applied econometrics in marketing. Frank had been my thesis advisor at Purdue University, and he knew of my interest in this topic. However, I also had concerns about the extent to which data limitations prevented econometric model results from having much impact on marketing decisions. In the 1970s, researchers used Nielsen's *bimonthly* store audit data (aggregated to represent geographic areas) or SAMI's four-weekly *warehouse* withdrawal data to model how sales or market share depends on a few marketing variables. Each of these data sources was subject to severe limitations, especially compared with the scanner data we now have available.

## Dave Montgomery's Involvement

Perhaps the natural thing to do for anyone who is confronted with the dilemma of an appealing suggestion that presents considerable uncertainty is to share the idea with a senior colleague. For me, the logical person was Dave Montgomery. His immediate response was that a conference with a broader focus would have promise. Dave's heavy involvement with applications of management science in marketing and the birth of the College on Marketing (see Montgomery 2001) provided direction, and we agreed to co-chair a conference. Instead of confining the conference to econometric applications, we chose Market Measurement and Analysis as the title. I do not remember

whether, during many lunch meetings, we considered “Marketing Science” as a possible title. If we did consider it, we might have rejected it as an oxymoron. Actually, to contributors and readers of the journal *Marketing Science* there is nothing oxymoronic about it, but it is a controversial expression for both physical scientists and many marketing practitioners.

Once both of us decided that organizing such a conference had merit, we contacted a modest number of potential attendees. We learned that one reference point was the annual AMA Educators' conference, which had lost appeal for many researchers publishing in the *Journal of Marketing Research*, especially for those publishing in *Management Science* or *Operations Research*. Another reference point was the semiannual ORSA/TIMS meeting, which had a substantial number of marketing sessions. These reference points suggested that there should be a high amount of interest on the part of our target market if the theme of the conference was sufficiently broad. Importantly, we envisioned organizing a conference just once.

## Conference Focus

We chose Stanford University's spring break in 1979 as the time for the conference. This guaranteed the availability of classrooms across campus at little or no cost. We intended for the conference to provide a forum for the exchange of ideas, problems, solutions, interests, and experiences. Consequently, we issued a call for abstracts with the promise that we would screen all submissions for quality and fit. Our plan was to have between 50 and 80 attendees, and we wanted most of them to make a presentation. We ended up rejecting a large number of the submitted abstracts, and of course we received the usual litany of complaints (“You are ruining my life,” “This is the only conference I plan to attend this year,” “The uni-

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versity president just approved my request for funding," etc.). The difficulty of dealing with complaints and the need to define criteria for acceptance/rejection may have contributed to the current practice of acceptance of all abstracts for the Marketing Science conference. Another argument potentially relevant to this change is that relatively unknown researchers may find it difficult to get abstracts accepted. In addition, the conference is more likely to be successful financially if the number of attendees is unrestricted. However, in our case, we also used the screening process to make sure that the presentations would fit the theme of the conference and to achieve a high degree of interaction between people with different perspectives on common problems.

We planned to organize sessions with an empirical or analytical focus applied to marketing topics. Management science and operations research as well as statistics and econometrics were the primary fields of reference. We were especially interested in having presentations about applications of models or model-building approaches to marketing problems. Ideally, researchers would start with a marketing problem and apply appropriate tools or create new approaches to solve it. To help achieve substantive relevance during the conference, we decided we would make a special effort to attract many practitioners. Our interest was in having roughly a 50/50 split of academics and practitioners. Because the objective functions of these two groups differ in important respects (see Pringle 2001), we knew that it would not be easy to get practitioners to present a paper, let alone have them submit a paper for the conference proceedings. Yet 11 of 40 papers in the conference proceedings are authored by practitioners, 3 of those jointly with an academic (Montgomery and Wittink 1980).

We organized three panels to enhance the formal involvement of practitioners who could not present research due to confidentiality issues. The panel on data systems had five practitioner members, the panel on experimentation had three practitioners and two academics, and the panel on forensic marketing had one practitioner and three academics (see the conference program in Montgomery and Wittink 1980). Thus, across the three panels, we had perspectives from nine practitioners and four academics.

## Sponsorship

Before Dave Montgomery and I formally agreed to organize the conference, we considered the financial requirements. In retrospect, the risk of running such a conference is very modest. For example, having the conference on campus during spring break meant the absence of additional hotel costs. The financial risks were primarily the contracts with bus operators and food providers. To offset these costs we would need to attract a minimum number of paying attendees. Stanford's Graduate School of Business (or we personally) could easily have absorbed the risk. Nevertheless, we submitted a proposal to ORSA and TIMS to request their sponsorship and financial support. It was actually the sponsorship that was most important to us. Surely we would be able to attract the right kinds of people to a topical conference at Stanford even without the sponsorship. However, having the support of ORSA/TIMS characterizes the conference in an important way, and it may have, fortuitously, facilitated the financial viability of the journal *Marketing Science* (see Little 2001 and Morrison 2001). Our proposal was approved, subject to a few changes (such as the conference registration fee). Paid attendance was as high as we desired; the costs were modest (even after a celebratory dinner with our spouses at a French restaurant, later judged "too expensive" for Stanford business school functions); and attendees' enthusiasm was great. The conference was declared a great success.

I note that the 1979 conference at Stanford differed from other conferences held at about the same time. Specifically, Al Shocker organized a conference, "Analytic Approaches to Product and Marketing Planning," at the University of Pittsburgh in 1977 (Shocker 1979), cosponsored by the American Marketing Association and the Marketing Science Institute. He co-organized a second conference with the same title at Vanderbilt University in 1981 (Srivastava and Shocker 1982). Importantly, these conferences were not sponsored by ORSA or TIMS. Thus, there is no similarity in sponsorship between these two conferences and the conference organized at Stanford, which became an annual event. It is also clear that the research focus at Al's conferences was more narrow than the broad set of topics covered at what is now known as the Marketing Science con-

ference. The Product and Marketing Planning conferences were discontinued, at least in part because the behavioral and management scientist contingents were unable to engage constructively. It is noteworthy, however, that Al Shocker subsequently co-organized two Marketing Science conferences, one at Vanderbilt University in 1985, and another at the University of Washington in 1988. Many conference organizers mentioned that they intended to promote their universities and business schools through their efforts. Montgomery (2001) mentions earlier conferences sponsored by the TIMS College on Marketing.

## Content

I show in Appendix A an incomplete list of publications of papers which had been presented at the Stanford conference. It is interesting that the outlets with more than one publication include *Interfaces*, the *Journal of Marketing*, and the *Journal of Consumer Research*. This may reflect both the orientations of journal editors at the time (Ferber at *JCR*, Wind at *JM*) and the intent for the conference to have broad appeal to both academics and practitioners (*Interfaces*). The papers listed in Appendix A cover such topics as econometric applications, decision calculus models, consumer decision process models, market segmentation, new product diffusion (Bass) models, consumer purchase behavior models, models of advertising effects, hierarchical choice processes, and the relation between profits, prices, and advertising. The conference proceedings (Montgomery and Wittink 1980) show that other papers cover resource allocation, market share models, conjoint analysis, time series analysis, and forecasting.

The proceedings are of course an incomplete representation of presentations. For example, I vividly remember John Little's talk, which covered new findings from scanner data. He emphasized that weekly scanner data showed huge variations in unit sales and in prices, variation that was hidden in the bimonthly audit data available at that time from AC Nielsen.

Dave and I did not plan for the conference to become an annual event. Apparently the Stanford event generated sufficient interest for many people to volunteer for the organization of subsequent conferences. I show in

Appendix B a list of the conferences through 2001. Of the 23 conferences, nine were held at private universities in the USA (Stanford, NYU, Wharton, USC, Chicago, Vanderbilt, Duke, Washington University, Syracuse); nine at public universities (Texas-Austin, Texas-Dallas, University of Washington, Illinois, Delaware, Arizona, Berkeley, Florida, UCLA); two at private business schools in Europe (London Business School, INSEAD); two at public universities in Europe (HEC, Mainz); and one at a public university in Australia (South Wales).

We can be proud of completing almost 35 years of the TIMS College on Marketing, more than 20 years of annual Marketing Science conferences now attracting about 400 attendees, and almost 20 years of high-quality publication of *Marketing Science*. Indeed, the number of conference attendees, shown in Appendix B, has grown dramatically from 100 or more through 1984 to more than 300 since 1988. The largest by far was the one organized at Berkeley in 1997 with 491 attendees!

## What Next?

Today, a conference on econometric applications should attract many people and feature a lot of research with potentially practical relevance. Frank Bass deserves credit for stimulating research by many of his Ph.D. students in this area. Even though the research several decades ago was restricted to data with enormous weaknesses, I am convinced that the results even then had substantive relevance. Thus, despite the imperfections, marketing managers did or would have been able to improve their decisions based on econometric models.

In addition to research represented by models of historical data, marketing has a rich history of field experiments. Anyone who needs a classic example of the impact of experimental results on decisions should consult the Newfood case (Eskin and Montgomery 1975). Management of the firm presumably was interested in learning whether it should use a high price/high advertising or a low price/low advertising marketing campaign for a new food item. These two alternatives were favored based on an affordability argument. Logic of course dictates a demand-based argument, and the

field experiment was designed by Eskin to show the best of all possible price and advertising combinations, not just the two favored by management.

The field experiment showed convincingly that a higher amount of advertising attracted more demand (e.g., more trial) that, at the aggregate level, showed more price sensitivity than the demand resulting from a lower amount of advertising. The observed interaction effect in aggregated data does not imply that individual households become more price sensitive due to exposure to more advertising. Instead, households are heterogeneous in price sensitivity, and different amounts of advertising attract different groups of households. Importantly, the field experiment result suggested that management should either use high advertising/low price or low advertising/high price. Thus, while an accountant might use the affordability argument to approve an advertising budget based on the price and "miss the boat" entirely, a marketer would choose a target market and find the (optimal) price consistent with the advertising budget (see Eskin and Baron 1977 for results on four field experiments, including the Newfood item).

Although in the 1970s marketing did not have an abundance of promising empirical applications, we now do (see Leeflang et al. 2000 and Hanssens et al. 2001). In my contacts with senior managers at AC Nielsen in the late 1970s, I had raised the issue of a latent demand for econometric model output. Such discussions ended up nowhere, perhaps because Art Nielsen said that his firm was in the business of providing highly accurate scores (sales, market share, retail price), but it was not in the business of explaining nor forecasting the scores. Today, AC Nielsen and IRI provide data to stimulate academic research in the anticipation that the firms will learn about advances in theories, models, and estimation methods that can be commercially exploited. We also know that scanner data have allowed academic researchers to pursue a wide variety of important issues. Much of this research influences the commercial practice (e.g., see Bucklin and Gupta 1999), and it seems straightforward to claim that academic researchers have made meaningful contributions with substantive relevance. Important progress has been made on a wide range of issues

based on scanner-based household data (which are vastly superior to the diary panel data in the 1970s) and scanner-based weekly store-level data (which are vastly superior to the bimonthly data in the 1970s).

One of the purposes of a conference on econometric applications held today should be, I believe, to foster interaction between academics and practitioners (see also Pringle 2001). An excellent example of the applied work that results from the collaboration I have in mind is Eastlack and Rao (1986). The authors discuss a lengthy series of experiments and analyses conducted on a vegetable juice item, and they report a rich set of results on advertising and price effects. It is published in *Marketing Science* under the heading Applications. This designation may suggest a negative connotation, as in: "It is only an application." Indeed, papers categorized as Applications always appear after other papers (but before Notes), implying a secondary status. This unfortunate practice may well discourage academic researchers from submitting such papers in the first place. I should mention, however, that *Marketing Science* has also published many commentaries on papers, a practice I believe to be conducive to the growth of our profession. In particular, given the dearth of papers authored or coauthored by practitioners, I am a strong proponent of asking managers, decision makers, and users of models to provide such commentaries on articles describing new approaches.

Yet, it is easy to overstate the impact of what is increasingly academic research published in *Marketing Science* on the real world. Hermann Simon, who changed "shirts" by starting his own consulting firm, found the applicability of econometric models to solving real-world problems dismal. This experience contributed to his critique of academic research, especially research based on econometric models of historical data (Simon 1994). It suggests that no matter how strongly we as academics believe that the research we publish is of high quality, we cannot assume substantive relevance or impact on marketing decisions without evidence. Although others (Little et al. 1994, Parsons et al. 1994) have rebutted Simon's arguments by providing many examples of substantively meaningful empirical research, there remains a considerable amount of skepticism. If Simon, who has

extensive skills and experience, finds it difficult to create substantively useful econometric applications, we must intensify our efforts.

In a provocative essay, Pringle (2001) observes that the interplay between academics and practitioners is critical. He also argues that the minimal amount of cross-fertilization that occurs today is due to the fact that the two groups have very different, essentially non-overlapping, objective functions. As a result, simply bringing academics and practitioners together, what he eloquently terms the *Propinquity Model*, will not accomplish the desired amount of integration. Indeed, even though it is easy for anyone to claim that the Marketing Science conference is a success, and there is no doubt there are many positive aspects, such a claim is actually a very narrow one. The original intent, clearly stated for the Stanford conference, was to have heavy involvement by practitioners. Thus, the Marketing Science conferences have failed miserably on this dimension (obviously, the intent was modified over time).

One might ask whether the active participation by practitioners matters. For example, as academics we may claim that our objective function is to publish in the most respected journals where we present our latest findings. Such an objective function may be virtually indistinguishable from that of other academics. Certainly *Marketing Science* qualifies as a high-quality journal, and the Marketing Science conference is a splendid forum to spur us on. However, most of us are *business school* faculty members for whom there is more to life than academic prestige.

Business schools were strongly criticized in the 1950s as espousing little more than institutional knowledge and lacking discipline-based research (Gordon and Howell 1959). Today, some observers argue that business schools have become "too academic." For years, business school deans advocated or tolerated the hiring of senior faculty members, especially from economics and psychology, without regard to the ability of new hires to produce substantively relevant research. If one pushes this mantra to the limit, business school faculty members will be indistinguishable in research from colleagues across campus. Of course, as faculty members we must satisfy the tenure criteria established by the university.

The push for relevance today exists primarily in the courses we teach. For some business school faculty members, research and teaching are virtually unrelated activities. Yet, the more intertwined these two activities become, the more productive we should be. Thus, we need to address the substantive relevance question continuously in our research. The MIT model, referred to by Pringle (2001), may serve as a prototypical example. Little, Urban, Silk, Montgomery, Hauser, Lodish, and Lilien do the type of research, usually problem based, that is worth emulating.

### From Propinquity to Encapsulation

Perhaps the only way to bring academics closer to practitioners is by changing the objective function for academics. I propose, therefore, an *Encapsulation Model* in which academics are engulfed by business problems, and are rewarded for developing solutions that get implemented. For the *Encapsulation Model* to be adopted, academics at business schools need to have an objective function that specifies the publication of research with real-world relevance. Requirements for promotion would include the demonstration of implemented results in one or more organizations. To accomplish such a change in promotion criteria in many universities, we need a champion. I propose that the College of Marketing consider adopting a recommended set of criteria for *marketing scientists*. A document that contains the proposed criteria, along with the arguments and the expected benefits, should be sent to all business school deans with the request that the proposal be debated, considered, and approved by the school and by the university with allowance for local adaptation. Some agreement, especially among the top business schools, on new promotion criteria is critical, because it is easiest for all schools to favor the status quo. Without broad support, innovators run the risk of becoming isolated.

The College also needs to be involved because individual faculty members lack the incentive to promote change, because all faculty members obtained tenure under the existing criteria. Deans will not easily advocate change either, because they may alienate senior faculty members, and the university provost may prefer strict adherence to academic success. Such

a situation calls for a "prestigious consulting firm" to recommend change, in case the College on Marketing. See Little (2001) for other related and unrelated suggestions contained in his straw manifesto.

I believe that most business school deans will welcome such a proposal, the creation of which requires extensive discussion and deliberation, and will use the proposed document to generalize the implications to other faculty members. If promotion criteria are meaningfully modified, in yet-to-be-determined specific ways, I expect the following types of changes for marketing science:

1. Marketing Science Conference organizers will invite practitioners to attend the conference and to comment on research presentations.
2. Academic researchers will contact practitioners informally to obtain feedback on ongoing research and to identify problems in need of solutions.
3. Academic researchers will request opportunities to speak at MSI-sponsored conferences, which attract many practitioners.
4. Academic researchers will present more of their research more frequently to executive and regular MBA students (the latter having 5 or more years of business experience) to solicit feedback.
5. Academic researchers will want to spend part of sabbatical leaves working in business organizations or consulting firms.
6. Deans will stimulate faculty members to obtain outside funding for research from industry associations, from individual firms, and from other sources, including NSF (see Bass 2001).
7. *Marketing Science* will institute an annual prize for the paper with the most dramatic, positive impact on managerial decisions.
8. Practitioners will have an incentive to attend and participate in sessions at Marketing Science conferences.

I contend that a change in only one objective function, that of academics, is necessary to accomplish a substantial increase in cross-fertilization between academics and practitioners. The natural place for a change is the objective function for academics at business schools. Even though I propose the change only for *marketing scientists*, it is easy to argue that analogous changes are desirable for virtually all business

school faculty members' objective functions. Importantly, the change, if implemented widely, will stimulate the active involvement by practitioners in presenting research and in collaborating on publications.

I also believe that businesses are clinging to outdated notions of confidentiality with respect to research sponsored privately. Employees are increasingly mobile, which necessarily implies some dissemination of insights gained from private research. However, more important is the fact that all insights provide temporary benefits in an increasingly dynamic environment. By the time papers are published, any firm learning about the research from the publication will be 3 or more years behind. The Marketing Science conference can and should be the forum showcasing the best approaches to current marketing problems.

I note that despite the fact that the Marketing Science conference is currently almost exclusively an academic affair, other venues exist with broad participation from academics and practitioners. The Marketing Science Institute does an excellent job by stimulating exchanges in a variety of forums. And the New York City Modelers, a group started by Lew Pringle in April 1968 when he joined BBDO, continues to have monthly meetings. The six founders of the group were Lew, Ed Deming (quality control), Paul Lazarsfeld (sociologist at Columbia), Charles Jacobson (research director, Lifesavers), Jerry Greene, and Steve Stock (partners at MarketMath). Don Morrison, Ambar Rao, and Charles Jacobson relaunched the group in the 1970s. Ed Brody (see Steckel and Brody 2001) manages this activity by having a presentation by an academic one month, followed by a practitioner's presentation the next month. The audience includes members of both groups. However, the MSI forums and the Modelers' meetings, as interesting as many are, definitely fit Pringle's *propinquity model*. There is rarely a presentation that resembles the type of work done by Easlack and Rao (1986). The latter exemplifies what I expect to see a lot more of if the *encapsulation model* is adopted for marketing science.

## Conclusion

The Marketing Science conference has in many ways been a phenomenal success. The research presented at

the conference often finds its way into premier journals, and is of a quality that demands recognition by our peers within and outside business schools. However, the strong focus on academic, discipline-based research, stimulated in part by the Gordon-Howell report, also means that marketing scientists are often removed from problems in the real world. We are increasingly writing for other academics or just for ourselves. In *Marketing Science* we often write in such a way that few others can identify the substantive relevance of our results. We need to enhance the connection with the world of practicing managers.

Attempts to merely bring academics together with practitioners, as was done at the Stanford conference in 1979, will have no long-term benefits unless the objective function for academic marketing scientists is modified. If business schools, through the efforts of the College on Marketing, adopt promotion criteria that stipulate substantive relevance and impact for marketing scientists, we can expect to see desired changes. Business school deans will offer reduced teaching requirements to faculty members who do the best applied research, partly supported by institutions, foundations, organizations, and firms. New executive education programs will be problem oriented where high-level executives work with faculty members to discuss alternative approaches to solving critical problems. These same executives will quickly learn that traditional protection of private research is without merit. The real world moves so fast that by the time a paper is published, a sponsoring firm has already updated and modified the approach, while critical components with a longer shelf life can be disguised.

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### Appendix A: Publications of Some Papers Presented at the First "Marketing Science" Conference

- Bass, Frank M. 1980. Some case histories of econometric studies in marketing: What really happened. *Interfaces* 10 (1) 86–90.
- Chakravarti, Dipankar, Andrew Mitchell, Richard Staelin. 1981. Judgment based marketing decision models: Problems and possible solutions. *J. Marketing* 45 (Fall) 13–23.
- Elrod, Terry, Russell S. Winer. 1982. An empirical evaluation of aggregation approaches for developing market segments. *J. Marketing* 46 (Fall) 65–74.
- Farley, John U., Donald R. Lehmann, Russell S. Winer, Jerrold P. Katz. 1982. Parameter stationarity and "carryover effects" in a consumer decision process model. *J. Consumer Res.* 8 (March) 465–471.
- Farris, Paul W., David J. Reibstein. 1979. How prices, ad expenditures, and profits are linked. *Harvard Bus. Rev.* 57 173–184.
- Horsky, Dan, Leonard S. Simon. 1983. Advertising and the diffusion of new products. *Marketing Sci.* 2 (Winter) 1–18.
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- Weiss, Doyle L., Pierre M. Windal. 1980. Testing cumulative advertising effects: A comment on methodology. *J. Marketing Res.* 17 (August) 371–378.

### Appendix B: Chronology of Marketing Science Conferences

Year	Location	Chair(s)	No. of Attendees
1979	Stanford University	David B. Montgomery/Dick R. Wittink	120
1980	University of Texas, Austin	Robert Leone	100
1981	New York University	John Keon	100
1982	The Wharton School	Vijay Mahajan/Yoram Wind	120
1983*	USC	Fred Zufryden	119
1984	University of Chicago	Steven M. Shugan	144



**Appendix B: Continued**

Year	Location	Chair(s)	No. of Attendees
1985	Vanderbilt University	Allan D. Shocker/Russell S. Winer	212
1986	University of Texas, Dallas	Ram C. Rao	256
1987	HEC, France	Gilles Laurent/Dominique M. Hanssens	292
1988	University of Washington	Allan D. Shocker/Robert Jacobson	336
1989	Duke University	John M. McCann/Richard Staelin	312
1990	University of Illinois	S. Sudarshan	365
1991	University of Delaware/DuPont	Ajay Manrai/John Frey	374
1992	London School of Business	Mark Uncles/Gerald Goodhardt	353
1993	Washington University	Chakravarti Narasimhan	335
1994	University of Arizona	Ambar Rao/Dipankar Chakravarti	429
1995	University of South Wales	John Roberts/Pamela Morrison	356
1996	University of Florida	Steven M. Shugan/Bart Weitz	393
1997	University of California, Berkeley	Tulin Erdem/J. Miguel Villas-Boas/Russell S. Winer	491
1998	INSEAD	Erin Anderson/Hubert Gatignon	412
1999	Syracuse University	Amiya Basu/S. P. Raj/Tridib Mazumbar	403
2000	UCLA	Randolph E. Bucklin/Donald G. Morrison	397
2001	University of Mainz	Oliver Heil	(554)

\*The 1983 conference is the first one officially called the Marketing Science Conference. The first four conferences were labeled as Market Measurement and Analysis conferences.

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