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Comments on "Models and Managers: The Concept of a Decision Calculus"

Managerial Models for Practice

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Managerial models for practice have undergone remarkable growth in the past 50 years. My paper on decision calculus, published in 1970, was both a progress report and a prescription for improvement. This commentary describes why I wrote the paper, my perception of why it has been considered influential, and a brief overview of what has happened since. The overview starts by tracing the trends and ideas of the 1960s into the 1970s. The 1970s blend easily into the era of decision support systems (DSS). Starting late in the decade and still continuing, DSSs have evolved rapidly due to an explosion of data, increased computer power, and advances in modeling methods. I review highlights of this evolution from the point of view of managerial models, giving special emphasis to marketing, since that has been my window on this world. The good news is that more managers than ever are using models. The bad news is that many managers do not even realize they are using models! (But we should ask whether this is really bad.) The commentary concludes with thoughts about the future of managerial models.

The selection of my paper as one of the most influential in the first 50 years of *Management Science* was a significant honor for which I am grateful. I take it as an indication by the members of INFORMS of the importance to our field of the issues addressed.

Key words: decision calculus; managerial models; decision support; DSS; marketing models

1. Why I Wrote the Paper

This was a protest paper. The first sentence put the problem on the table:

> The big problem with management science models is that managers practically never use them.

The trends in the OR/MS literature didn't seem right in the late 1960s when the paper was written. Ideologically, I came from the OR/MS tradition of the 1950s. I went to college right after WWII and entered graduate school at MIT in physics in 1950 but, while I was there, became attracted to the opportunities in operations research, obtaining a Ph.D. in 1955. I greatly enjoy scientific research but am also strongly motivated by the challenge of solving live problems and feel that scientific research is most interesting when it supports the improvement of real-world enterprises. My paper on decision calculus (Little 1970) reflects that OR/MS tradition. At the time it appeared, 17 years had elapsed since TIMS was founded, 18 years since ORSA. The energy and enthusiasm of the 1950s for spreading WWII successes in operations research into peacetime activities had begun to subside. The founding generation of OR/MS professionals was being replaced during the 1960s by baby boomers. The latter were well trained and just as energetic as their predecessors, but often embraced different values. Many of them wanted to "do science" and develop theory. The result, at least in North America, was that OR/MS went mathematical. A side effect was attrition of industrial OR consulting groups and, although the number of pages in OR/MS journals went up, the number of papers reporting implemented OR/MS work decreased, at least on a percentage basis.

During the 1960s I had the good fortune of working with colleagues and former students building marketing models in several major companies. We dealt directly with marketing managers and their staffs. I and my academic colleagues intended to stay academic and so published our work whenever appropriate. This undoubtedly affected the way we worked and what we expected of ourselves and the models. We had no objection to new mathematical theory and were happy to use it ourselves when needed. It wasn't what people were doing that bothered us but what they weren't doing. I should also say that we found what we were doing to be exciting and novel, as well as useful and needed.

2. The Paper's Message

Although Little (1970) starts off with a criticism of the field, it is not a negative paper. It describes what

I felt was wrong and what I thought could be done about it. The illustrations, drawn from my colleagues' and my experiences, gave the paper a credibility that, combined with the enthusiasm of a believer, made it easy to read.

Here is what I thought were the obstacles to the use of models by managers:

(1) *Good models were hard to find*. Many published models of the day failed to contain relevant variables for managers to control.

(2) *Good empirical estimation of parameters was even harder.* Measurements and data were big challenges.

(3) *Managers didn't understand the models.* They naturally tended to reject black boxes they didn't understand and revert to mental models of great simplicity, often considering one variable at a time and ignoring interactions, nonlinearities, and dynamics.

(4) *Most models were incomplete on critical issues*. Often because of lack of credible measurements, key aspects of a problem were simply omitted.

We faced these issues in the field and still felt that models should be able to help. We stressed fidelity to the manager's problem and sought to provide a structure for thinking that was open and transparent. After a few years and some degree of success, I wrote Little (1970), which proposed a set of guidelines for building models for managers. A "decision calculus" was defined as "a model-based set of procedures for processing data and judgments to assist a manager in…decision making." The paper advocated that a decision calculus be

(1) simple,
(2) robust,
(3) easy to control,
(4) adaptive,
(5) complete on important issues,
(6) easy to communicate with.

Simplicity fosters understanding. *Robustness* prevents inconsistency and guards against absurdity. *Ease of control* implies model transparency so that a manager knows what is happening when input knobs are turned. *Adaptability* wards off any complacency that might be generated by falsely assuming the decision environment won't change. *Completeness* requires that the model builder and the manager face the true requirements of the decision task at hand. *Ease of communicating* with the model is obviously desirable and by 1970 had received a big boost with the spread of time-shared computing.

Having stated the problems and suggested ideas that might help solve them, the paper illustrated decision calculus with a consumer-packaged-good example for setting advertising spending over time.

3. Why the Paper Seems to Have Been Influential

Little (1970) hit a resonant chord among many academics and practitioners in OR/MS. In part, this was because we weren't the only people concerned about the state of affairs. I do recall that some academics wanted to dismiss our work as "consulting." Consulting is a devastating word in the academic promotion process. It was true that many of our models were created and tested in consulting contexts—that's how we knew they were working—but these efforts produced new concepts and new knowledge that were publishable in their own right. We started to notice that our papers were turning up on required reading lists in Ph.D. programs and that doctoral students were requesting copies. We were pleased to help sensitize a new generation to the research potential of live problems and real managers.

The concept of a decision calculus was liberating for many practical model builders. Measurement issues were particularly difficult in that era. The decision calculus idea permitted the rejection of constraints that most academics had tacitly accepted. Traditional econometric methods assumed that the model builder had a single consistent database containing all the input and output variables. Decision calculus, on the other hand, thrived on eclectic data sources in a drive to obtain model completeness on important issues. One prominent liberation was the potential acceptance of judgmentally estimated parameters when other data were not available. This was not without precedent—subjective probabilities were common in the growing field of decision analysis—but the mandate here was broader and provided a way to achieve greater completeness in addressing critical phenomena.

4. What Has Happened Since?

Much. In providing a brief overview of events related to managerial models for practice, I shall focus on areas that bear a relation to Little (1970) and for which I have direct knowledge or contact with principal participants. These are mostly in marketing.

4.1. A Flowering of Applications

Decision calculus ideas appear in many published models directed at practice from the 1960s through the 1980s. MEDIAC (Little and Lodish 1969) deals with selecting media options to create a media schedule. CALLPLAN (Lodish 1971) guides salespeople in allocating their own time to customers. BRANDAID (Little 1975) assists the setting marketing-mix variables over time. SPRINTER (Urban 1970) dynamically allocates effort to marketing activities in a new product introduction. ASSESSOR (Silk and Urban 1978) is a seminal system for forecasting new product success prior to test market. DEFENDER (Hauser and Shugan 1983) addresses the marketing-mix strategy for an established brand that faces a competitive brand about to be launched into their common market. DETAILER (Montgomery et al. 1971) focuses on the marketing-mix in the specialized world of pharmaceuticals. The custom model of Lodish et al. (1988) was used to size the sales force at Syntex Laboratories and determine its allocation to market segments. Many but not all of these models had applications that included judgmental inputs.

Published papers were the tip of the iceberg. By design, only initial studies involving new concepts in new contexts went to journals. Repetitions belonged to everyday practice. Some models had hundreds and even thousands of applications, many of which continue in an evolved form to this day. A discussion of the amount of field application of these and related models appears in a rejoinder by Little et al. (1994) to Simon (1994) who had criticized marketing science as having had little impact on practice.

4.2. New Learning

If I were asked to name the principal way in which decision calculus applications differed from previous practice, I would point to the drive for completeness on important issues. This has two notable consequences. First, coupled with the goal of simplicity, it implies suppressing unimportant issues. Second, the inclusion of important phenomena can force a desirable search for new data. Historically, however, the use of judgmental inputs when hard data are not available attracted more research attention than any of the other guidelines, although the interest abated as data availability increased. I briefly summarize the work done at the time: Two test-and-control field experiments reported improved decision making when a model that included judgmentally estimated parameters was used to assist the decisions—Fudge and Lodish (1977) and Edelman (1965). Two studies were also done in behavioral laboratory settings. One, McIntyre (1980), reported improved decision making when subjects used judgmental estimates in a decision model. On the other hand, Chakravarti, Mitchell, and Staelin (hereafter abbreviated CMS) performed two sets of experiments in which subjects did not make better decisions when they used a judgmentally calibrated model and, in fact, made poorer decisions on the average. CMS (1981), in reporting their results, suggested reasons why this may have happened and proposed ways to improve subjects' ability to use such models. For an extended discussion, see CMS (1981), and Little and Lodish (1981).

4.3. The Growth of Decision Support Systems

Starting in the late 1970s, a new wave of real-world applications emerged under the name decision support systems (DSS). Large amounts of data had started to be collected through the automation of commercial transaction processes. An early example was the use of bar code readers installed at checkout in supermarkets. Meanwhile software had become much more powerful for handling such data. The net result was the growth of decision support systems. A DSS can be defined as a collection of data, models, statistical packages, and optimization routines designed to assist management in its decision making. I was heavily involved in this in marketing, as were some of my colleagues, and wrote a paper describing the developments (Little 1979). The paper forecast great increases in the amount marketing data that would become available within the next 5–10 years, as well as corresponding increases in computing power available for marketing analysis. Perhaps more significantly, the paper predicted a shift from *market status* reporting to *market response* reporting. Market status includes items like sales, shares, prices, and advertising exposures, broken out by time period, geographic region, and SKU. Market response reporting goes the next step and also includes measures such as price elasticities, promotional response, and advertising effectiveness.

These predictions had largely come true in the consumer-packaged-goods industry by the mid-1980s. The growth in data and computing power was about a factor of 1,000. The bread and butter business of a marketing DSS is a steady stream of requests for market information and analysis ranging from the sales of 40 ounce Cheerios in the latest quarter in Buffalo to a sophisticated evaluation of the performance of a new product in a test market. Although marketing managers have easy access to such systems, the major hands-on users are typically management scientists or market analysts who act as intermediaries to front-line managers.

DSS was the natural step beyond Little (1970). Managerial decision models still benefited from being simple, robust, and complete, but new attention was focused on how to ask questions that might be answered by the big new databases. For this purpose, a new class of models emerged that may be called *imbedded models*. Examples appear in the methods developed by management scientists at data supply companies like Information Resources, Inc. (IRI) and ACNielsen in packaged goods. These firms collect a weekly gusher of data from individual supermarkets, process it, and repackage it into clean, standardized databases to be sold to grocery manufactures. The imbedded models enter the processing as implicit assumptions about how store promotions, pricing, and display activities work in supermarkets.

They make possible the calculation of response measures like price elasticity and promotional lifts. Such models and procedures have been documented in the marketing science literature and form part of the fundamental research of the era, e.g., Abraham and Lodish (1993). For better or worse, a characteristic of imbedded models is that managers tend not to realize that they are there and speak of the resulting measures as "data."

By 1990 managerial models were at work in the marketing of durable goods, for example, automobiles, (Urban et al. 1990). The same was true of industrial products (Choffray and Lilien 1980) and services. Examples of the latter were the design of a hotel by conjoint analysis (Wind et al. 1989) and a decision to introduce a satellite TV service (Bass et al. 2001).

A distant dream in 1970 was the possibility of modeling the response of *individual customers* complete with control variables. By the early 1980s, commercial data suppliers in the packaged-goods industry, had started to provide multiple markets with voluntary panels of 2–3,000 households, whose grocery purchases were identified at checkout and collected into time series. These contained not only the purchases, but also related records of in-store prices and promotional activities. An early paper by Guadagni and Little (1983) introduced the multinomial logit model for analyzing household brand choice as a function of marketing control variables. Since then, academic research in choice models in marketing has blossomed and still continues. Some of this has become part of commercial practice. For example, Little (1994) describes a method for evaluating cents-off coupons using panel data.

The data deluge was often frustrating because it created opportunities that seemed hard to seize. In 1970 a standard bimonthly Nielsen report was a modest deck of 50–100 pages of charts and tables in hard copy. A manager or marketing analyst could reasonably thumb through looking for significant events. This doesn't work when the amount of data is multiplied by 1,000. In response to this situation, models have been supplemented by rule-based expert systems to comb through the data in a directed way to search for important market changes. CoverStory is one example (Schmitz et al. 1990).

It is infeasible to review all that has happened in the arena of marketing models in practice, but I want to mention one book, now in its second edition, and two major reviews of marketing models that have appeared within the past five years in top journals. The book is *Marketing Engineering* by Lilien and Rangaswamy (2004). As its title implies, the book is directed at assisting marketing analysis and planning in practice using contemporary marketing science tools. It is the most ambitious effort to date to combine text, hands-on cases, and supporting software to educate a new generation of master's-level students for practice.

The first of the reviews is a 1999 special issue of *Marketing Science* on "Managerial Decision Making." The lead article, "The Success of Marketing Management Support Systems," (Wierenga et al. 1999) offers a framework of factors that determine the success of such systems, as well as a brief history. The same issue contains a valuable survey of commercial use of UPC scanner data (Bucklin and Gupta 1999).

The *International Journal of Research in Marketing* produced an issue in 2000 on "Marketing Modeling on the Threshold of the 21st Century" that is even more focused. It begins with a comprehensive article, "Building Models for Making Decisions: Past, Present and Future" (Leeflang and Wittink 2000), which is followed by 13 shorter commentaries on the main article and a concluding response by Leeflang and Wittink.

4.4. Managerial Models Outside of Marketing

Fortunately, for more than 30 years, CPMS, the Practice Section of INFORMS, has been running a prize competition for the Franz Edelman Award for Achievement in OR/MS. The papers of finalists are published in *Interfaces*. This has produced a rich and varied literature of successful implemented managerial models.

The record speaks for itself, but begs the question of what attributes characterize a successful model. On this subject, I had an exchange of e-mail with Marshall Fisher about my commentary. (Marshall has an optimization paper is in this issue *Management Science*.) Besides doing theoretical research, Marshall has built models for practice in the areas of logistics and scheduling. In his email Fisher (2004) says:

> Everything in your comments resonates with my own experiences, working with United Shoe Machinery while in graduate school and with two startups in the last 20 years. I have come to the conclusion that models can be deployed in one of two ways—either fully automated, untouched by human hands, or as a decision support system under the direction of a manager. I have been involved in the application of the 1st in vehicle routing applications that ran overnight to schedule delivery of orders received the preceding day and in retail inventory applications that set order points for millions of store-sku replenishment items. I have found that these applications require a very accurate model and powerful optimization algorithm, but, after a validation phase, can be run as black boxes. In the second mode, I have found that simplicity and transparency beats complex optimization every time because it enables a better coupling with the heavily involved manager. In fact, most of my failures have come from trying to deploy sophisticated, black box optimization models in DSS environments, because the

managers with responsibility were unwilling to implement recommendations they didn't understand. However, after a simple, transparent application has been in use for a while, a user may tire of the care and feeding of the DSS, and evolve to a more precise understanding of what he or she wants. Then one can sometimes transition to the automated optimization mode.

This view corroborates the ideas expressed in Little (1970) but makes an important further point, essential to the future of decision models, that complex models with sufficient demonstrations of validity can gain the trust of managers.

5. Managerial Models for Practice in the Future—Peering into the Mist

Preparatory to looking ahead, I attempt to characterize the environment in which managerial model builders operate today. The following outline sketches what has changed and what hasn't since the data explosion started in the early 1980s.

What Has Changed

- Data availability
- The Internet invasion
  - Collapse of geography
  - Portals: Yahoo
  - Internet retailing: Amazon
  - Search engines: Google
  - Comparison engines: MySimon
  - Auctions: eBay
  - Shopping advisors: autochoiceadvisor.com
  - Computer viruses, identity theft
  - Streaming rich media
  - Web-based marketing research (faster, better, but not cheaper)
- Computer technology
  - Continued growth in speed and storage (Moore's law)
  - Graphical user interfaces (GUIs)
  - Desktop and portable computing
- Information technology, artificial intelligence
  - A computer becomes chess champion
- Growth of marketing knowledge
  - Improving measurements
  - Uncovering behavioral phenomena
  - Empirical generalizations
- Growth of knowledge in supporting fields
  - Behavioral decision theory, judgment and decision making
  - Choice modeling
  - Statistics, econometrics
    - Data mining
    - Structural models
    - Hierarchical Bayes
    - Approaches to endogeneity
  - Economics
    - Game theory, agency theory
  - Optimization
    - Faster, better algorithms
    - Problem specific methods

What Hasn't Changed

- Managers
  - Organizational inertia
  - Academic promotion criteria
  - The advantage of proactive design in data collection
  - The rewards and challenges of addressing interesting and relevant problems

The list of developments takes my breath away. What does it mean for practice? Do any of the guidelines for desirable model characteristics in Little (1970) have relevance for the future? For example, has simplicity been made more difficult by the huge databases and the powerful but complex statistical and econometric techniques that relate models to data? There is hope. In many marketing situations the managerial control variables that affect choice, sales, and profit are fairly standard, so that despite the database sizes and modeling sophistication a what-if dialog by a user can look simple. Needed, however, will be off-line demonstrations of the validity of underlying models. Guidelines like simplicity, robustness, completeness, and ease of use, as perceived by the user, should not be surrendered easily.

Returning to the crystal ball, what do we see?

5.1. Creeping Automation

There will be more imbedded models. As an obvious example, an internet retailer must have all its Web interactions with customers preprogrammed. As already mentioned, imbedded models of customer behavior enter into most repetitive measurements of market response or sales effectiveness and will do so in new situations. Furthermore, not every managerial problem deals with high-level strategy. Many tactical and operational tasks, such as developing weekly sales promotions, rebate programs and the like, can well be automated, subject to periodic managerial review. See Bucklin et al. (1998)

5.2. Web-Based DSS

Lilien and Rangaswamy (2000) put forth a credible vision of Web-based decision support under the title "modeled to bits." Their ideas represent a needed technological upgrade of many classical DSS concepts. They envision databases, model banks, and statistical tools distributed around the network and forming a global workbench for managerial problem solving. Mostly, such a DSS would be used by management scientists on a project basis to answer managerial questions. But, as part of the project, they may wish to fashion transparent online what-if summaries to be used by management to test changes in alternatives and assumptions.

5.3. A Grandiose DSS Vision

Most of us, I suspect, can't imagine doing a research project today without Google. Google gives us a huge virtual library with a clever, but rather literal-minded, librarian. Google sits idly around doing nothing by itself, but Google and I together make a great action team. Now suppose I am a manager or a member of the strategic staff of a corporate CEO. Imagine I have SuperGoogle. In SuperGoogle I don't type in a keyword or phrase, but a *scenario* and my questions. For example, suppose I work for a pharmaceutical company and want to introduce our new stent in Europe next year. What resources would this require and what would be the likelihood that our major competitor would get there first? I put into the scenario knowledge I have that may be relevant and any assumptions I want to make, as well as the questions I would like to answer. SuperGoogle does a content analysis of my input and goes to work. It scours the Web and my corporation's private databases for relevant information. The system contains the Web-based DSS described in the previous section. SuperGoogle presents me with information and options that I never would have thought of by myself and so stimulates my imagination and gives me new ideas. I am not passive. On the contrary I give direction to the search and analysis by changing or adding to the scenario and developing subscenarios as I work. For a big project I would be part of team, each member of which has access to SuperGoogle. The end result would be a richer, deeper strategy analysis than is possible today. Such a SuperGoogle does not seem out of reach in the next few years. (A step in that direction is the DOME system (Wallace et al. 2000), which can connect globally dispersed models and people in engineering new product development.)

In any case, the synthesis of knowledge, databases, hardware, and methodologies into support systems for managerial (organizational) use offers almost unlimited challenges and opportunities for management scientists in universities, corporate strategy groups, software firms, and entrepreneurial consulting firms. Managerial models will be part of this but much more will be added. It should be exciting.

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