

# PRESIDENTS' SYMPOSIUM: CREATING GROWTH IN OR/MS

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These remarks, adapted from remarks made at a symposium entitled "Perspectives on the Future of OR/MS" at the May 1984 ORSA/TIMS meeting in San Francisco, use a view of the history and current state of OR/MS as the basis for projecting possible futures for the profession, with special attention to ways of achieving growth.

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As operations research/management science professionals, we realize that the past is an imperfect predictor of the future; even when we put confidence intervals around our predictions, they may not capture reality. In spite of this caution, I would like to examine some aspects of our history. Although such an examination may or may not offer insights about the future, it will help describe just where we are now. The data I have are for ORSA, but I believe that TIMS data are similar.

## Some Indices of Growth

From an initial membership of 560 members in 1952, the year of ORSA's founding, the Society grew at a rate of about 325 members per year through 1965. Between 1965 and 1970, new members joined at a rate of 580 per year, a significant jump. After 1970, however, a fundamental change occurred: during the next decade and a half the growth rate was essentially zero. During this period, membership levelled off at about 6900 members, with small variations from year to year. This zero growth rate masks the fact that membership turns over at a rather steady 10 to 15% each year.

A similar pattern of growth can be discerned in the length of our flagship journal, *Operations Research*. Beginning in 1952 with a base of around 328 pages per year, it grew at a rate of about 60 pages annually until 1968, when it reached 1200 pages per year. It remained at this level through 1983. In 1984, 200 pages were added to accommodate the new OR FORUM and OR PRACTICE sections. During the

15 years of the Journal's zero growth, however, ORSA and TIMS started several new journals: *Transportation Science* in 1967 (with 527 pages in 1985), *Interfaces* in 1971 (696 pages in 1985), *Mathematics of Operations Research* in 1976 (717 pages in 1985) and *Marketing Science* in 1982 (436 pages in 1985).

Yet another similar growth pattern can be seen in university educational programs in OR/MS. In the 1950s, degree programs in the United States increased from three to only five. But this number grew to 80 during the 1960s, and to 110 by 1980; the growth rate recently has been zero.

One indicator of the success of OR/MS however, has been the propagation of its methodological and modeling concepts into many other disciplines in both university and professional settings. For example, we now see mathematical programming and stochastic modeling being taught in agriculture and regional science, in systems, chemical, electrical, and industrial engineering, in psychology, economics, medical sciences, finance, marketing, management, accounting, statistics, mathematics and in other fields. The development and the use of OR/MS methods have also proliferated, as shown in the content of such journals as *Mathematical Programming*, *Networks*, *Stochastic Processes*, *Applied Probability* and *Decision Sciences*.

## Four Concepts of the Current State of OR/MS

Given the history of very rapid growth in the 1950s and 1960s, with a decline to a much lower rate during the next 15 years, what can we say about the current state of OR/MS? For the sake of argument, I would

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like to consider the possibility that the profession is in one of four possible states: stagnation, a long-term steady state, maturity, or a short-term equilibrium between supply and demand.

**Stagnation.** Paralleling A. H. Hansen's economic stagnation theories of the 1930s (see, for example, Hansen 1938, 1951 and 1954), a case can be made that OR/MS is in a period of stagnation. Hansen characterized a stagnant economy as one lacking new (geographical) frontiers, at the limits of its natural resources, with a slowing technical growth and a stable population. Some observers who argue that "OR/MS is dead" build their thesis on allegations that the profession and its societies exhibit stagnation in ideas and basic paradigm, in leadership and in membership. In Hansen's terminology, other disciplines have adopted our paradigm and by applying it elsewhere have limited our "geographic" expansions. For our part, we have made little use of our "natural resources"—that is, the study of physical phenomena—to produce new theories and concepts. The "technical growth" of significant new ideas has stopped (or greatly slowed) since the 1950s and 1960s. And, as noted, our membership has been stable for more than 15 years.

History has shown that Hansen's theories, as applied to the United States of the 1930s, were not appropriate for that stage of our economic development, since, although the geographic growth of the country had indeed ended, in the past 50 years our natural resources have expanded owing to new technological materials and discoveries, our technical growth has exploded, our agriculture has produced huge surpluses and our population has grown. Thus, I believe it is too early to judge the validity of the stagnation theories applied to OR/MS during the 1970s and 1980s.

**A Long-Term Steady State.** In deriving its basic knowledge, OR/MS essentially has followed a paradigm that can be called *analytic normative modeling*: *analytic* since we try to solve a whole problem or system by separating it into its component parts and then deducing the validity of the solution by reversing the steps (the analysis); *normative* since we seek to find the correct (often optimal) actions to guide, control or regulate the system; and *modeling* since we construct a (usually mathematical) representation of the real system. This paradigm may have reached the limits of its growth in its *current* applications to decision making problems. Also, it may not be sufficiently robust to venture into new areas that involve major

behavioral, strategic or societal policy issues. In marketing science parlance, OR/MS may be milking its cash cows rather than breeding them in order to increase the size of the herd. This long-term steady state differs from stagnation in that, while it allows for the generation of new ideas, it does so at a modest—perhaps constant—rate. While its moderate advances may continue to convey a sense of success, major developments are unlikely to emerge. This steady state scenario would be illustrated by mature OR/MS members leaving the profession since the paradigm no longer suits their needs or interests.

**Maturity.** This state views OR/MS as being in the process of refining its grand, youthful ideas. The field has borne children who have gone off into other fields of endeavor, creating new knowledge there and finding their own successes. But within the OR/MS community, further development has become a process of refinement—making marginal changes by polishing, sifting, and judging the acceptability of minor improvements. This state is a comfortable one, characterized by a conservative, non-risktaking approach to the future. Those in this state have stayed inside the profession—now in its third, fourth or even fifth generation—and they feel comfortable with its paradigm and context since, unlike the pioneers of the profession, they have not been exposed to other approaches and problems.

**A Short-Term Equilibrium.** In this state we can see continuing profit from old successes, the growth of new product lines that have so far contributed to the system's equilibrium, and new markets that do not yet demand novel methods. Here, too, we see the merging of some of our older ideas, and the integration of some ideas from other fields, but the primary emphasis remains on past successes as cornerstones of the future.

### Future Scenarios

Given these four assessments of our current circumstances, what can be said about possible futures for OR/MS? Just as other characterizations of our current situation could be made, many possible futures could be described—but I will content myself here with three scenarios: decline, a continuing zero growth rate, and growth.

**Decline.** My a priori estimate of the probability that this scenario accurately describes the future of OR/MS is between 0.2 and 0.3—in other words, I

see a reasonable chance that OR/MS will move from a zero to a negative growth rate. This could happen if there is a continuing exacerbation of the divisions within our professional societies rather than a focus on the real needs that our profession should aim to meet. For example, our societies have for many years maintained a delicate balance between members who spend their lives generating new techniques and methods and those who devote their attention to solving the real problems of society. If this balance is seriously disrupted, the societies could move either toward becoming more self-centered, academic and mathematical, or toward becoming trade associations whose national meetings and publications chiefly address members' day-to-day, practical concerns. In either case, they would not attract the brightest minds necessary to develop the ideas and methods to meet the new challenges of significantly difficult problems in the real world. Nor would they support new, risky ventures, or—and this is my major concern—broaden the underlying science of OR/MS, a science built on practice *combined with* methodological and conceptual development.

**A Continuing Zero Growth Rate.** This scenario envisions that the OR/MS profession will stay at a roughly constant size for the next 10 to 15 years. My a priori estimate of the probability that this scenario describes the future is 0.6. This state offers a comfortable prospect: we could continue with our paradigm unchanged, teaching its methods and techniques and applying it to appropriate problems. In this scenario we would continue to tinker with our journals, our meetings and the problems we work on. Our societies would develop new technical sections and new colleges on the basis of occasional member initiatives. But this scenario is essentially reactive, rather than proactive; it requires minimal leadership.

**Growth.** This scenario envisions that the OR/MS profession will grow significantly over the next 10 to 15 years; my estimate of the probability of this outcome is only 0.1 to 0.2. This scenario faces severe handicaps. Our current paradigm controls all of our publications, as well as most of our practice in applying OR/MS to real problems; with the modest exception of the National Science Foundation's program on Decision and Management Sciences, our research funding has been significantly eroding—driving researchers toward other disciplines and diminishing the attractiveness of our profession to the brightest masters' and doctoral candidates. For our practitioners, it is often the case—especially in our academic

community—that short-term payoffs for research activities are emphasized: in academia, where the tenure system is based on getting many publications out quickly, the temptation for faculty to secure their futures by making small advances well within the limits of the standard paradigm is often overpowering. Finally, we face severe competition from other disciplines for our ideas and our colleagues, in a technological environment in which every day brings exciting new areas of application and basic concepts.

Against this background of constraint and competition, how can OR/MS grow? I do not believe that significant growth can be achieved merely by staying within our current paradigm and improving what we are now doing. If we are to grow, we must reach out to new areas of knowledge and to new approaches, and integrate them into our field. For many decades we have been a major exporter of people and ideas. It is now time to become a major importer.

Two areas fundamental to the future growth of OR/MS stand out as examples. The first is that of experimentation and data analysis through controlled and quasi-experimental designs. We must get used to more descriptive analysis; we must use more real data to develop fundamental scientific concepts as new parts of our fundamental paradigm of analytic normative modeling. The second area from which we need to gather more nourishment is that of understanding human behavior. The relevant methods currently come from psychology, sociology and political science, but these methods are essentially descriptive and are not yet based on understanding the processes of human behavior, both individually and in groups. It will be difficult to integrate knowledge from this area into our work because nonquantitative methods do not fit nicely into quantitative procedures. However, artificial intelligence and cognitive science, and other areas, have made progress along these lines. If OR/MS is to grow, it must deal analytically and realistically with human behavior.

### Achieving Growth

In order for operations research/management science to grow, what must we do? I believe we must broaden our discipline to create a new science of OR/MS. Although today's profession contains much science (that is, accepted understandings of real phenomena), the bulk of the work appearing in our journals is devoted to mathematics that may or may not eventually contribute to such understandings. Fundamental underlying laws and principles are few; much more

research needs to be done that is aimed at comprehending common processes. An April 1984 workshop in Dallas sponsored by the NSF's Decision and Management Science Program (for an account of its proceedings, see Little 1986) identified some of the real-system procedures needing extensive scientific investigation: homeostatic processes (stability-instability), evolutionary processes (growth, resistance, adaptation and decay), design processes (flow, production, sequencing, routing and communication), and group-conflict processes. Almost all of these processes involve human behavioral aspects, and work in any one of them will call for experimentation.

I do not propose that we create a new science of OR/MS solely out of a fabric of innovative thought—although it would be nice if a genius would contribute some fundamental new insights. Rather, I suggest the more pedestrian approach of entering, creating or coopting new areas of reality, of understanding other paradigms and relating them to OR/MS work, and of taking the risks of looking to our old problem areas with fresh eyes and of doing more large-scale experimental research in these areas. For example, three fields—operations management, manufacturing, and services—are poised on the verge of major growth, and each must be integrated into larger decision areas. The underlying processes that will guide the outcomes in these areas are extremely important.

To achieve the goal of growth as I have described it, our educational programs, our research and our publications must all change. In each of these professional activities we must exhibit a willingness to take risks, combined with a vigorous attack on the handicaps I have mentioned. Perhaps the key role our societies can play is in encouraging risktaking while at the same time helping to remove barriers and handicaps.

Many of our educational programs—and, indeed, some regarded as the very best—are locked into a very rigid view of OR/MS, with the result that it is difficult for anyone enrolled in them to achieve exciting, broad-

based breakthroughs from their background of an already highly structured core of knowledge. A first practical step toward reaching out to other fields would be to require all OR/MS students to minor in an area that involves quite different paradigms.

We should expect—and indeed require—our publications to include papers whose substance and power emerge from new contexts and paradigms. This goal will not be easy to achieve, because the research workers who write such papers often have a narrow view of the world, just as most of us do—and they may not be overly keen to display their ideas outside what they see as their world. Exceptions exist, however, and they often turn out to be either the giants or the mavericks of their fields. We should seek them out in order to publish some of their work in our journals.

As has happened a few times in the past, some of these innovative areas will need the encouragement of new journals. Because of the high quality of the work done by OR/MS researchers and practitioners, these new journals would gain wide appreciation and attract first-class work. In the short run, they would most likely lose money, but in the longer run they would define their fields and help to redefine our profession.

All of us need to learn more, to broaden our interests, to be more willing to experiment and to expect and accept losses, while hoping and planning for successes that will engender the growth of operations research and management science.

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