THE NATURAL DRIFT: WHAT HAPPENED TO OPERATIONS RESEARCH?

CHARLES J. CORBETT and LUK N. VAN WASSENHOVE
INSEAD (The European Institute of Business Administration), Fontainebleau, France
(Received August 1992; revision received March 1993; accepted March 1993)

"Crisis? What crisis?" could also have been an appropriate title for this paper. The OR/MS literature contains more than enough papers addressing the crisis in OR/MS to take the matter seriously, but it is not always clear exactly what is meant by crisis. The complaints usually concern the perceived gap between theory and practice, pointing out that there are too many theoretical and too few practice-oriented papers. This may well be true, but we suggest a slightly different view of the crisis, by hypothesizing that a 'natural drift' has occurred, i.e., that old-style OR has remained underdeveloped relative to its more purely theoretical and practical counterparts. To explain how this hypothesis arose, we provide an overview of the debate on professional concerns in OR/MS, and contrast it with *Harvard Business Review* papers providing a managerial perspective. We also explore the extent to which such a natural drift would be truly natural, by comparing the development of OR/MS to that of other professions. We arrive at a mixed conclusion: All is not well, but all is not lost either.

It is not everyone born during a world war, pronounced dead at age 41, only to go through a midlife crisis and simultaneously be the subject of a post mortem 8 years after, that still develops a plan for the next decade the following year, and even has his or her existence proved another 3 years later as if nothing had happened. The single fact that the discipline known as operations research/management science has pulled off this remarkable feat (as the titles of papers by Ackoff 1979a, Lilien 1987, Ackoff 1987, CONDOR 1988, and White 1991, respectively, suggest) is sufficient reason to take a closer look at some much discussed aspects of its development so far.

Particularly (though by no means exclusively) in recent years, the operations research/management science (OR/MS) literature has shown a growing interest in the history of the field, but also a growing concern about its future. Much has been written about the future of OR/MS, claiming that future to be bright, expressing some worries, or simply stating that the future is past and OR/MS is dead. Dando and Bennett (1981) note that the dominant feeling in the British OR community evolved from very optimistic in 1963, through optimistic in 1968 and unsure in 1973, to gloomy in 1978. Given that so much is written about the "current crisis in OR/MS," it is reasonable to ask to what extent this debate is truly justified.

Surprisingly, writings on the OR/MS crisis generally show little or no awareness of opinions expressed in management literature. In this paper, we frequently refer to articles from the *Harvard Business Review* (*HBR*), being the management journal most read by executives (and also widely read by TIMS members; see King, Grover and Nelson 1987), in an attempt to step outside this largely inward-looking debate Although the *HBR* obviously cannot be said to represent management attitudes in general, tracing its OR/MS-related articles provides some interesting insights. To begin, as Figure 1 shows, the crisis debate in the OR/MS literature took off soon after a dramatic drop in attention paid by *HBR* to OR/MS. A superficial perusal of the *Sloan Management Review* reveals a pattern similar to *HBR*. Such a graph is obviously no evidence in itself; it merely suggests that managers are hardly interested in OR/MS any more and/or that the OR/MS community is no longer paying attention to managerial literature, both of which would be cause for concern. However, a glance through recent

Subject classifications: Professional: comments on; OR/MS philosophy.
Area of review: OR FORUM

0030-364X/93/4104-625$01.25
© 1993 Operations Research Society of America

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

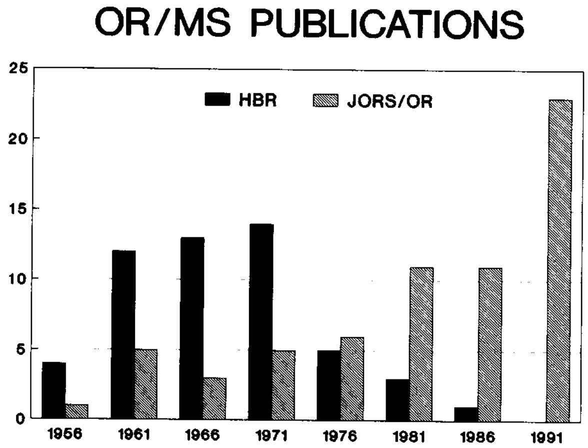

Figure 1. The number of articles on OR/MS in the *Harvard Business Review* contrasted with the number of articles expressing concern about the development of the profession in the *Journal of the Operational Research Society* and in *Operations Research.* For the *Harvard Business Review,* all articles largely devoted to methods or applications of OR/MS, considered in a broad sense, are included in the graph. For the *Journal of the Operational Research Society* and *Operations Research,* the graph includes all papers expressing concerns similar to those raised here and those explicitly referring to the crisis. Only *JORS* and *OR* are included, as hardly any philosophically-oriented papers have appeared in *Management Science* since the early 1970s, and the other major OR/MS journals were founded considerably later than the two considered here. Including these relatively new journals would inevitably reinforce the above pattern. Obviously, this graph should not be taken too literally, but the overall picture remains suggestive. Note that, as discussed later, it was in 1973 and more viciously in 1979, that Ackoff pronounced OR/MS dead.

editions of *Interfaces* suffices to demonstrate that this decline in attention in general management journals does not coincide with a lack of relevant and highly successful applications of OR/MS. A graph of sales of software containing OR tools will also show a very different picture than Figure 1; for example, Geoffrion (1992, p. 430) notes that by the end of 1991 over a million copies of a linear and nonlinear optimization code, modified to work with the leading spreadsheet packages, had been shipped. It is surprising that the apparent drop in managerial attention has occurred despite the vastly enhanced implementation possibilities offered by the advent of computers, decision support systems, etc.

The aim of this paper is to identify the main issues in the OR/MS debate, also taking into consideration the views expressed in *HBR.* We seek to hypothesize, in a simple way, a natural underlying cause of the development of OR/MS and the surrounding debate. Section 1 outlines the key hypothesis, while Section 2 is devoted to a systematic but necessarily far from complete discussion of the different viewpoints found with respect to the main issues distinguished. Having thus sketched, very briefly and incompletely, the perceived state of the field, obvious questions to ask are: What the deeper causes are of the current sense of crisis? and To what extent similar phenomena occur in other areas? We address this issue in Section 3. Section 4 draws the threads together and suggests some implications for the field of OR/MS.

1. THE NATURAL DRIFT HYPOTHESIS

The notion of the existence of a crisis in OR/MS has attracted enough attention to be taken seriously. The complaints voiced frequently in the literature usually concern the predominantly theoretical orientation of much of the literature in OR/MS, the lack of attention for practical work, and other related issues. While we do not necessarily disagree with these concerns, we are inclined to believe that they do not fully address the underlying cause of the current sense of crisis. By taking an historical perspective, we hope to be able to illustrate what we believe to be the fundamental source for the unease felt by many leading authors in OR/MS. We do so in this section by presenting a very rough sketch of the development of the field nowadays known as OR/MS.

We propose to classify the activity going on in what we now call OR/MS into three areas, according to the type of problem addressed in each. The terminology we employ in this section is not generally in use nowadays, but we believe it would have been accepted in the early years of the discipline. The distinctions we are about to draw are obviously not sharp, but will help to illustrate the natural drift hypothesis, which we formulate at the end of this section.

In the first area, the goal of solving problems is to develop new results to contribute to the body of knowledge in the discipline. This area of activity we call *management science.* In a second area of activity, the goal is to solve somebody's practical problems

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

using existing, standard methods; standard, that is, to an expert who has been trained in their use. We label this area *management consulting.* The third area lies between these two: The goal here is to solve those practical problems for which it is necessary to adapt existing tools or use existing tools in fundamentally novel ways. This area we call *management engineering.* We use the term *management* only for want of a better term; it is not meant to exclude OR/MS aimed at public policy making, or community OR, or other areas of OR/MS not strictly related to management.

Let us dwell for a moment on the concept of management engineers. Management engineers draw upon the knowledge and methods accumulated in management science and, in turn, indicate to management scientists which areas still need developing. Ideally, management engineers will have a wide range of theoretical knowledge to know which approach to use for any problem they encounter, combined with a thorough understanding of that specific approach to be able to apply it. They should also have a deep understanding of the real world, and how problems are embedded in complex environments. The main objectives of management engineers are to help managers understand the situation they are facing and/or to provide answers to specific questions, which are not solvable by standard means, by using an analytical approach. In general, the aim is to eliminate unintended irrational decisions and actions and to give the eventual decision a more "scientific" character. Dawes (1988) provides an excellent discussion of how even highly trained professionals frequently and unintentionally make errors of judgment, exposing them to systematic exploitation by others who have succeeded in banishing such irrational judgments. This is not to say that there is no room for intuition in management. On the contrary, one aim of management engineering is to increase managers' understanding and thereby sharpen their intuition, eliminating irrational elements. A management engineer studies the situation from an analytical point of view, and attempts to relate what he sees in the real world to concepts and frameworks developed by management scientists. Together with the managers, the management engineer performs thought experiments; they combine observation and theory, and, in doing so, attempt to grasp what is going on around them.

To see why we use this particular terminology, despite the fact that management science and engineering nowadays have different connotations, consider the following quote from Flood (1955, p. 179), in his Presidential Address for the Second National Meeting of The Institute of Management Sciences: "... we of TIMS need not be especially concerned with the grand effort at fostering basic science—our main effort should be to adapt existing scientific knowledge, *and the techniques of the scientist*, for the solution of problems of management" (author's italics), and (p. 183) "... the knowledge-oriented management scientists are sure to make many discoveries eventually of value to the problem-oriented management engineers and managers."

Having set up this framework, let us see how this relates to the development of OR/MS as we see it, from World War II to the present. In the early years, OR/MS was often described in terms of applying a scientific approach to practical problems. Although this is nowadays an inadequate description of the field, it roughly corresponds to our description of management engineering. In the 1940s, OR/MS did not yet have its own body of theoretical knowledge, so management science had yet to be developed; besides, OR/MS was still too young for parts of it to have become more or less routinized, so management consulting could not yet exist either. For a simple, pictorial representation, see Figure 2a.

Gradually, some applications proved successful and became increasingly routine, so that management consulting in OR/MS came into being. Simultaneously, early workers in OR/MS quickly realized the need for OR/MS to develop its own body of theoretical knowledge, if it was to be accepted as a field in its own right and not prove to be just a short-lived fad; among these writers was Ackoff (1962). In this way, management science was born. Figure 2b illustrates the situation as it was in the late 1950s and early 1960s.

The success of several tools proved to be lasting; linear programming and simulation enjoyed widespread recognition and use, and the area we call management consulting continued to grow. Meanwhile, OR/MS acquired increasing academic status, as the body of theory behind the field expanded rapidly. These trends still continue, and Figure 2c depicts what we perceive as the current state of OR/MS. From the figure, it is immediately clear that we believe that management engineering has stayed behind. As a result, the link between 'pure theory' (management science) and 'pure practice' (management consulting) has become thinner and thinner, causing widespread worrying about the 'gap' between theory and practice. From our perspective, there is no gap between theory and practice, but an underpopulated area of activity, the area of management engineering. Let us reiterate that the crisis in the OR/MS community is not due to the amount of attention paid to the theoretical,

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

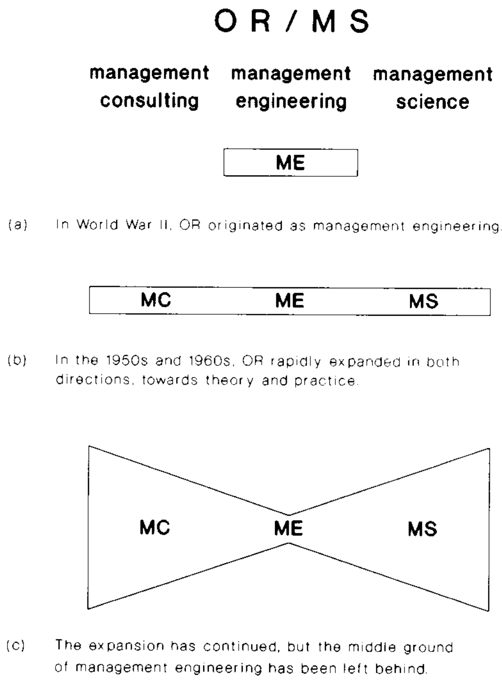

Figure 2. A rough outline of the development of OR/MS from World War II to now. Originally consisting only of management engineering, the necessary expansion into management consulting and management science was rapidly accomplished. While this expansion has never stopped, it seems that the middle ground has been left behind. This graph should not be taken to mean that theory and practice are perfectly balanced, but only that there is an area in between which is underdeveloped.

mathematical side of the field, but rather to a relative shortage of fundamentally novel approaches and applications, such as should arise from the domain of management engineering. In Section 3 we look at the underlying causes of this phenomenon.

Having outlined the necessary preliminaries, we can now formulate the natural drift hypothesis:

Operations research, as born during World War II, was equivalent to management engineering, but, as a result of the natural drift phenomenon, is undergoing an identity crisis; the ensuring polarization is leaving the middle ground underdeveloped.

Looking at the crisis in this way, hypothesizing an underdeveloped area in between two well-established polar extremes, rather than simply as a gap between two areas, obviously has some implications as to which remedies are called for. Before developing this theme any further, we look at the development of OR/MS as reflected by writings in OR/MS journals and in *HBR*.

The terminology in the remainder of this paper is inevitably somewhat confusing. We use the term OR/MS to span the entire range from management consulting through management engineering to management science, but the authors we quote in the next section sometimes have different interpretations of operations research and management science. Our aim here is definitely not to provide normative statements concerning the true meaning of the terms operations research and management science, but simply to point out an area (the one we have labeled management engineering) that in our view is not developing as rapidly as it should and, as such, is at the root of the current sense of crisis.

2. THE DEBATE

This section contains a systematic discussion of a selection of writings in the OR/MS debate, comprising both early and recent articles in the OR/MS literature and articles from the *Harvard Business Review*. To distinguish opinions from the OR/MS literature from those found in *HBR*, most of the references in the text include a mention of the journal involved. This discussion obviously can be nowhere near complete, and is intended merely as a rough overview. For an excellent recent discussion of forces, trends and opportunities in OR/MS as perceived by a leading author in the field, see Geoffrion (*OR* 1992).

We focus on six main issues of the debate. The fundamental issue is, in our view:

- striking the balance between OR as a knowledge-oriented science or a problem-oriented technology.

The five other issues we distinguish are each derived from this basic issue. They are:

- tool-orientation versus problem-orientation in OR/MS;
- client relations in OR/MS;
- the learning effect of an OR/MS study;
- the relevance of OR/MS at a strategic level;
- the interdisciplinary nature of OR/MS.

A separate subsection is devoted to each of these issues.

2.1. Science or Technology?

The fundamental issue in the "crisis" debate in the OR/MS literature is that of the orientation of the field;

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

one's individual views on most issues inevitably boil down to whether one sees OR/MS primarily as a knowledge-oriented science or as a problem-oriented technology. In the previous section we argued that both orientations are needed for the survival and growth of OR/MS. For example, research into interior point methods for speeding up linear programming may sometimes be purely mathematical in nature, but is of great relevance for management engineers when suitably implemented into novel software, and is therefore an important contribution to management science. On the other hand, developing a large-scale LP model for a particular refinery and then implementing the fastest available LP code is a typical case of management engineering, and signals to management science that there is a need for extremely fast LP codes. It is our belief that the crisis is partly a result of workers of both types not acknowledging the necessity and the achievements of those of the other type, and therefore not letting developments at one end be a guideline for development efforts at the other. This lack of mutual appreciation is particularly serious for management engineers who stand with one foot on either side of the divide. Precisely these valuable people, who could help in restoring the balance by bridging the gap, are often the ones who fall between the cracks.

The problem of the split identity of OR/MS was noted early by Herrmann and Magee (*HBR* 1953, p. 111): "As an applied science, the work is torn between two objectives: as applied it strives for practical and useful work; as science it seeks increasing understanding of the basic operation, even when the usefulness of this information is not immediately clear." Flood (*MS* 1955), quoted above, also clearly recognized this distinction. Symonds (*MS* 1957, p. 126) writes: "Although operations research and management science are now closely related, they are quite different but complementary in their purposes. Operations research represents the problem-solving objective; management science the development of general scientific knowledge. Nevertheless, much of our understanding of management science came through operations research, as well as industrial engineering and econometrics." Drucker (*HBR* 1959) recognized the need for both a knowledge orientation ("*The first need of a management science is, then, that it respect itself sufficiently as a distinct and genuine discipline,*" p. 30; author's italics) and also a problem orientation ("*The second requirement for a management science is, then, that it takes its subject matter seriously,*" p. 148; author's italics).

The question whether this second requirement is adequately addressed in modern OR/MS lies at the heart of the debate. Lathrop (*OR* 1959) did not view OR in itself as a science, and held that OR workers should always be concerned with applications. King (*OR* 1967, pp 1177–1178) expressed concern early that this was not happening enough: "... it seems apparent that the long-range acceptance and development of the field intrinsically depends on the solution of problems in the real world and that, in turn, such successes can be achieved in quantity only if we can develop and maintain a cadre of practitioners who are competent in mathematics *and* who display the creativity and ingenuity that is so important to the 'real world' aspects of problem solving." Beged-Dov (*MS* 1966) agreed: "Although they seem to pay lip-service to applications, it is quite easy to establish that the majority of the operations researchers best known in the field are openly proud of their learned papers and almost apologetic when referring to applications." Fierce criticisms of the neglect of real-world aspects of OR were made by Woolsey (*OR* 1972) and some well-known "OR is dead" papers by Ackoff (*OR* 1973, *JORS* 1979a, b). Dando and Sharp (*JORS* 1978) argued that OR was and should return to being a technology, drawing on a broad scientific basis; it is not a science in itself.

In the early 1980s, the British Operational Research Society established the Commission on the Future Practice of OR out of a "widespread apprehension of an impending, or even an actual, crisis confronting operational research" (Report of the Commission, *JORS* 1986, p. 831). The entire 58-page report is, as the name already shows, practice-oriented; in fact (p. 842), "The main methodological drive, as inferred by the Commission, is pragmatism." The contrast with the U.S. Committee on the Next Decade in Operations Research (*OR*, CONDOR 1988) is striking, as the U.S. Committee does not convey a sense of crisis, and seems mainly concerned with the mathematical achievements to date and their promises for the future. It would not be fair to suggest, though, that the CONDOR report is representative of opinions in the U.S., as is demonstrated by the comments on the report by Wagner et al. (*OR* 1989); a recurring theme in these comments is the need to learn more from and about the practice of OR. Particularly interesting is the remark by Rothkopf (p. 668) that ignoring what made "old style OR" successful will split the profession into two largely independent parts, each struggling desperately for existence.

An illuminating exchange on the subject of OR as science versus OR as technology recently took place between Keys and Miser. Keys (*JORS* 1989) started

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

the exchange with a paper titled "OR as Technology: Some Issues and Implications," which proposed that the area of activity, we have labeled management science above, should not really be considered as part of OR; OR is not a science, but a technology. Miser (*JORS* 1991, p. 430) responded to this, referring to Ravetz (1971), who distinguished three classes of problems in science:

- for scientific problems, the goal of the task is to solve the problem, and the function to be performed by the solution is to contribute new results to the field;
- for technical problems, the function to be performed sets the problem. The task is accomplished—and the problem is solved—if the solution enables the function to be performed;
- for practical problems, the goal of the task is to serve some human purpose, and the problem is solved when a means for serving the purpose has been devised and is seen to be effective.

By letting science include all three classes of problems, Miser argues that OR is a science, as the activity going on within OR nowadays covers all three classes. Keys' (*JORS* 1991) response is partly to agree with Miser, pointing out that the issue is partly semantic as he has more restrictive connotations with the term science. Of course, the issue of the extent to which OR is primarily concerned with technical problems, the standpoint toward which Keys seems inclined, remains a fundamental one. Part of the unease some people feel is presumably caused by the fact that the most well-known literature in the philosophy of science is not directly applicable to OR. Miser (*EJOR* 1993a) discusses how the philosophy of science could be made more recognizable for scientists in general and operations researchers in particular.

2.2. The Rule of the Tool, or Managing Messes?

Originally, OR was entirely oriented toward real-world problems, but at that time there simply were no OR tools. In the early days, there was a rush to develop the much needed tools, and that this development has been extremely successful cannot be disputed, as is demonstrated by the wealth of techniques OR workers currently have at their disposal. However, many of the contributors to the OR/MS debate seem to think that the focus has shifted too far toward "Analysis in Wonderland," and away from the real world; if true, the discipline has neglected Drucker's (*HBR* 1959) second requirement, that OR/MS take its subject matter seriously.

Wagner (*OR* 1971) indeed warned that there may be too much emphasis on techniques in OR, and too little on the problems they are supposed to solve. Ackoff (*OR* 1973) emphasized the distinction between mathematical models and managers' messes, and argued that the tools used in OR/MS are out of date, and no longer capable of dealing with the messes faced by managers. Note that Ackoff used the term problem to denote a well-defined, abstracted version of a messy real-world situation. In his words (pp. 670–671): "... accounts are given about how messes were murdered by reducing them to problems, how problems were murdered by reducing them to models, and how models were murdered by excessive exposure to the elements of mathematics." Morse (*OR* 1977, p. 188), the first President of the Operations Research Society of America, signals a trend away from the real world: "But if we stick to our original aim of matching our models to reality, rather than trying to make reality fit the preferred model, we can contribute in important ways to many of the serious problems facing this country and the world." Bishop (*HBR* 1979, p. 154) also observed excessive tool orientation: "The modus operandi of the operations researcher is to abstract from some real-world problem a mathematical problem for which he can find an answer. He then touts that answer as a guidepost to the manager."

Ackoff's (*JORS* 1979a) "The Future of Operational Research is Past" is a well-known and outspoken article in the OR/MS debate. He holds academic OR and the relevant professional societies primarily responsible for what he perceives as the decline of the discipline; they brought about that (p. 94) "... OR came to be identified with the use of mathematical models and algorithms rather than the ability to formulate management problems, solve them, and implement and maintain their solutions in turbulent environments." In other words (p. 95), "In the first two decades of OR, its nature was dictated by the nature of the problematic situations it faced. Now the nature of the situations it faces is dictated by the techniques it has at its command." He adds that (p. 95) "... OR has been equated by managers to mathematical masturbation and to the absence of any substantive knowledge or understanding of organizations, institutions or their management." Managers have lost interest in OR, because (p. 100) "[T]he unit in OR is a problem, not a mess. Managers do not solve problems; they manage messes." In his "OR, A Post Mortem," Ackoff (*OR* 1987, p. 474) pushes his discussion of tool versus problem orientation further: "The field's introversion drove it into a catatonic state in which it died mercifully, but it has yet to be buried." Ackoff's views are obviously not uncontroversial, but

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

what he describes does seem to correspond to the potential fatal outcome of a natural drift phenomenon, where management engineering gets left behind.

Many of the criticisms claim that OR/MS tends to abstract too much from real-world problems, in order to allow more analytical sophistry. The general (though not universal) warning against letting tools become the dominant orientation in OR/MS (the rule of the tool) seems justified, as this requires that potential clients are familiar with the OR/MS toolbox in order to decide whether or not to commission an OR study. In this way, OR/MS teams will only receive projects for which they already have the tools, thereby limiting growth opportunities for OR/MS in both theoretical and practical directions. Daniel (*EJOR* 1987, p. 271) concludes that "the adaptive OR group itself has proved to be the most enduring and useful 'tool' of all."

Of course, the importance of tools should not be underestimated either: The profession would never have got very far without them, nor will it get much further without continuous development of new tools. Weingartner (*OMEGA* 1987) signals that researchers will no longer consider individual problems as being independent of each other, but will attempt to develop solutions that can be used more than once. This development of general tools is necessary; designers, for example, also use general tools to create a unique design. The importance of tools is also emphasized by the U.S. Committee (*OR* CONDOR 1988), whose report focuses on past and future development of tools.

An important, but not often recognized, aspect of OR/MS tools, is noted by Blumstein (*OR* 1987, p. 98): "We must recognize, however, that our patents on the tools we like to think of as our own have largely expired." It is true that tools such as linear programming, PERT charts, and simulation, have been developed by the OR/MS community, but, due to their own success, have now become public property. These tools are now used by many people (management consultants) who are not necessarily trained as OR/MS workers, and straightforward application of such tools is no longer the exclusive property of OR/MS. This coincides with Telgen's (1988) view of OR; he also notes that OR tools are increasingly being used as black boxes, e.g., while linear programming methods are included in spreadsheet packages, most users have no idea of the underlying algorithm. Development of fundamentally new tools and new ways of applying existing tools is a necessary condition to prevent OR/MS from eventually being sold out.

2.3. Client Relations

An issue strongly related to dominant orientation, but specifically relevant for applied OR/MS, is that of problems in client relations. Here we look at three aspects of this issue:

- the frequently recurring theme of communication problems between OR analysts and managers;
- the need for better public relations (PR for OR) and the name of the discipline;
- the need for better understanding of how OR can be of value to clients.

2.3.1. Communication Between OR Analysts and Managers

The problems that OR workers encounter in communicating the results of their studies to managers were already recognized in the 1950s in the *Harvard Business Review*. Herrmann and Magee (*HBR* 1953) noted that communication between OR workers and executives is the most serious problem with applied OR. It is interesting to note the change in opinion in *HBR* as to who is responsible for breaking the communication barrier. Roy (*HBR* 1958, p. 122) realizes that "There is a vital need for a bridge between the two diverse frames of discourse represented by the prevailing loose terminology of the businessman and the precise language of the scientist," but stated that both business management and OR consultants should make an effort to overcome this gap. Bennion (*HBR* 1961, p. 100) gives reasons why "more businessmen than not are highly skeptical of, if not downright antagonist toward" OR, but, in contrast to Roy, he believes that (p. 101) "to correct this regrettable state of affairs, I am prone to feel that the econometrician and the programer [sic], rather than the businessman, must accept responsibility for the first step." Jones (*HBR* 1966) already finds that communication from the OR side is deteriorating; writing about OR articles for businessmen, he states (p. 180): "As a rough rule of thumb, the early articles tend to be better than later ones; the relationship between the business problem and the technique is frequently clearer in the former; and when they were written, there had been less time for a specialized jargon to develop."

That communication problems occur in both directions is remarked by Boulden and Buffa (*HBR* 1970, p. 66): "Unfortunately, the OR staff often did not understand the problem, nor could the manager clearly define it." Although the communication issue does not seem to have greatly bothered the OR/MS community until relatively recently, it certainly does

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

now. Smith and Culhan (*INT* 1986, p. 32) discuss the priorities indicated by teachers, researchers and practitioners of OR/MS: "... more than 50% of the 20 priority issues dealt with communications." Miser and Quade (1985, chap. 10) discuss some of the intricacies of communication in the context of systems analysis.

2.3.2. The Image of OR/MS

The image of OR/MS in the outside world is not what it could be. Compare the striking difference in tone between two *HBR* articles, 22 years apart. Baumol and Sevin (*HBR* 1957, p. 52) began their article by writing, "It is difficult to exaggerate the opportunities for reduced marketing costs and increased marketing efficiency, and hence greater profits, which are offered to management by the combined techniques of distribution cost analysis and mathematical programing [sic]." On the other hand, recall Bishop's (*HBR* 1979) less euphoric view of the "modus operandi of the operations researcher." The ongoing debate shows that the OR/MS community is aware of this problem.

One result has been to call the name of the field into question. Recall that Flood (*MS* 1955) had used the term management engineers, at a time when there was no particular reason not to use the term operations researchers. The first paragraph of Wagner's (1975) well-known textbook discusses why the name operations research is ambiguous and unfortunate. Lilien's "mid-life crisis" paper (*INT* 1987, p. 36) observes that "the term *operations research* has developed a somewhat negative connotation in the late '60s and early '70s. According to the panel, the term *management science* is seen by many managers as a contradiction in terms; managers know that much of what they do is messy, ill-structured, based on poor or biased data, and in need of almost instant resolution." As a possible remedy, he mentions a suggestion by Hoffman to use the term management engineering for the discipline of OR/MS. The Report of the UK Commission also recognizes the problem (p. 854): "An extreme but widely held view—conceivably as many as half OR practitioners hold it—is that the phrase 'operational research' is now understood by clients in so narrow a way, and in a way so unrelated to the actuality of OR in practice, that some new name should be found." The reason for not actually recommending a change of name is that the Commission could not agree upon a suitable replacement. And Weingartner (*OMEGA* 1987, p. 259) states: "The view is growing in acceptance; the name of the field, and the name of the society that champions it, ought to be changed."

Persistent management apathy toward OR/MS is one of the major trends noted by Geoffrion (*OR* 1992), who claims, however, that managers are not to be blamed for this, as their main concern is to manage their own businesses. He does concede that OR/MS has a visibility problem, and that despite all the success stories available the business press largely has ignored OR/MS. Rinnooy Kan (*EJOR* 1989) sees better visibility of the discipline ("PR for OR," p. 283) as the solution to the image problem.

2.3.3. How Can OR/MS Be of Value to Its Clients?

Many authors have lamented the relative shortage of papers discussing applications of OR/MS. Though there may still be cause for concern in this context, the OR/MS literature does contain an impressive number of excellent application papers. Perhaps the most outstanding example of this is given by the 19 special issues of *Interfaces*, so far, containing the finalist papers for the Franz Edelman Award competition. These and many other unpublished success stories suggest that many practitioners know very well what they are doing. However, most knowledge about the *process* of applying OR/MS is implicit, and generally seems to be learned by trial and error. This situation is not very satisfying and even unacceptable for a mature discipline. We believe, therefore, that there is a need for more fundamental research into the process of OR/MS, focusing not on the technical aspects of projects but on what analysts *actually* do. At the same time, we need to know more about clients' perceptions of OR/MS projects. Finally, descriptions of failed projects can be at least as instructive as success stories.

It would be incorrect to suggest that there is no literature on the process of OR/MS. There is a sizeable number of accounts of personal reminiscences from experienced analysts, providing valuable insights into the process of OR/MS. Several books, such as those by Schultz and Slevin (1975), Tomlinson and Kiss (1984), and Miser and Quade (1985), address the process and implementation of OR/MS. What we do not yet have, though, as Miser (*OR* 1987) observes, is an organized, comprehensive, coherent epistemology of practice.

One example of a simple framework attempting to capture the process of OR/MS is the OR-cycle, suggested by Fortuin, van Beek and Van Wassenhove (1992), which describes the phases a typical OR study would go through: from realization that there is a problem to problem description, model building and data gathering, selection of a solution method, validation, and, finally, implementation. Occurrences they warn against are solving the wrong problem (recall Boulden and Buffa's (*HBR* 1970) remarks

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

about OR teams not understanding the problem), and the fact that problems may change during the study, so that the implemented solution must be contrasted with the current problem, and, if necessary, the cycle must be passed through again. Obviously, preventing such occurrences, e.g., by keeping in close touch with clients throughout the study, belongs to the elementary craft skills of the profession. In fact, many skills required in OR/MS are craft skills (see, e.g., Miser and Quade 1988, or *OR*, Miser 1992). However, it is our belief that by establishing a comprehensive and coherent epistemology of practice, students of OR/MS will be able to acquire more of these craft skills before learning them the hard way, by making mistakes in the field, severely damaging the reputation of OR/MS in doing so.

Thinking about OR/MS in marketing terms may be helpful. Market segmentation is needed to distinguish between different types of clients (large vs. small firms, private vs. government, strategic vs. operational, etc.) in order to cater to their needs. Particularly, the personality characteristics of the client have often been found to be important (see, e.g., Huysmans 1970). Jackson and Keys (*JORS* 1984) discuss how different types of OR are suitable for different types of situations. Key questions are how OR/MS can provide a value-added service to the client, and how OR/MS should evolve to gain and hold a competitive advantage over other concepts managers could invest their scarce time and money in.

A basic question is how an OR study can be made to meet the client's wishes. In the next subsection we see that OR studies can provide anything between clear answers to specific questions and an increase in the client's understanding of his/her situation. Naturally, the entire organization of the project should hinge on whether the client expects to receive a black box computer program with some efficient problem solving algorithms, or whether he/she expects to gain more insight into his/her operations.

2.4. Learning From OR

Many *HBR* articles consider the learning effect of an OR study to be more important than the actual results. The Report of the UK Commission contains some highly interesting observations (*JORS* 1986, pp. 841–842):

34. The Commission found significant amounts of O.R. in practice with one or more of the following aims:

- to help structure 'messes' or messy problems;
- to research into the facts of an uncertain topic;
- to help an understanding of a sphere of activity;
- ...

35. It is noteworthy that the above list does not include, directly at least, optimizing operations or obtaining cost savings. While improving efficiency in some general sense is no doubt a driving force behind much O.R. in practice, it is achieved indirectly. Almost all practitioners with whom the Commission spoke think that the main benefits of O.R. stem from enhancing the client's understanding of his own problems. Practitioners commonly see their role as helping their client do his job better, not doing it for him.

These facts do not seem to have received much attention in the OR/MS literature, and little has been written about how this learning process works. Therefore, we believe that fundamental research is urgently needed into how and what executives can and do learn from OR studies.

Preparing for future decisions was seen by Salveson (*HBR* 1957) as the main use of OR. Baumol and Sevin (*HBR* 1957, p. 58) state that: "While a linear program usually will not compute a correct optimum because the changes it suggests will go too far, there is yet a very strong presumption that it will correctly indicate the best directions of change." An article probing deeper into the interaction between OR and management (Roy, *HBR* 1958, p. 120), says: "In short, and oversimplified, the OR method means getting behind the art of doing business and probing into what makes up that important but elusive thing called business judgment."

Bennion (*HBR* 1961, p. 100; author's italics) is more explicit; he considered the most valuable aspects of models to be "1) The capacity of the models to improve management's *understanding* of highly complex problems, which can scarcely fail to enhance the quality of the necessary value judgments management makes. 2) The undeniable fact that the most valuable use of such models usually lies less in turning out the answer in an uncertain world than in shedding light on how much difference an alteration in the assumptions and/or variables used would make in the answer yielded by the models." Hayes (*HBR* 1969, p. 108; author's italics) elaborated on this, and considered this the advantage of OR over other kinds of tools: "I believe that the greatest impact of the quantitative approach will *not* be in the area of problem *solving*, although it will have growing usefulness there. Its greatest impact will be on problem *formulation: the way managers think about their problems* ... In this sense, the results that 'quantitative people' have produced are beginning to contribute in a really significant way to the *art* of management."

According to Brown (*HBR* 1970), the main benefits of decision theory analysis are not the numerical results, but the fact that it clarifies the relevant issues, makes implicit assumptions explicit, and provides a

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

framework for communication. Boulden and Buffa (*HBR* 1970, p. 83) concluded that "Decision making is facilitated because the manager interacts with the model, not because decision-making logic is built into the model itself." In Jones's words (*HBR* 1970, p. 78): "In business decision making, 'getting there is more than half the fun.' Thus, many discussions of attempts to apply new mathematical methods to solving business problems end up with the conclusion that the real benefit was not the specific answer but the increased awareness of organizational problems and opportunities. This was achieved through the discipline of formulating the problem in a new vocabulary and structure." In Wagner's (*OR* 1988) terms, OR has become an international language for business strategy. Geoffrion (*HBR* 1976) too defends the use of OR by arguing that computer models can deepen managers' insights.

Ackoff (*JORS* 1979b, p. 189; author's italics) recognizes this fact: "*the principal benefit of planning comes from engaging in it.*" He suggests using the concept of "idealized design," giving managers a direction to follow, even if the target itself may be unreachable. (Compare this with Baumol and Sevin's *HBR* 1957 views on linear programming.)

Reading these comments, it is hard to escape the feeling that the OR/MS community has failed to recognize adequately how it could be of greatest value to managers, and it seems high time to develop in detail the implications of these remarks for applied OR. The British OR community has given these issues more thought, which has led to the development of the 'soft systems methodology' (note that this is very different from what is understood by systems analysis in the U.S.), as described by, e.g., Checkland (1981). Outside the UK, this approach would perhaps not always be recognized as OR, and it should be seen primarily as a complement to more traditional OR rather than as a way of solving the problems inherent in traditional OR.

Another approach was recently initiated by Jaikumar and Bohn (1992), who suggest that, in the context of production management, OR should focus on continually accumulating knowledge about a process, rather than on its static optimization. A third line of research is exemplified by a recent special issue of *EJOR*, devoted to the importance of modeling as a learning process; for an overview, see Morecroft (*EJOR* 1992).

2.5. OR/MS at the Strategic Level

It has been demonstrated beyond question that OR/MS can be highly useful in supporting tactical and operational decisions; whether or not OR/MS can be applied in strategic situations has been debated. Furthermore, a field that is not at ease with itself, that has problems communicating with executives, that is reputed to be predominantly mathematical in nature and tool-oriented, will generally not be considered a useful aid in strategic situations. Many well-known examples, dating back to World War II, show the incorrectness of such a view of OR. For a brief history of the role of OR/MS in truly large-scale projects, often under the name of systems analysis, see, e.g., Miser (1993b).

Salveson (*HBR* 1957, p. 93) even stated that OR is primarily suitable for strategic decisions: "Operations research has little, if any, role in making current operating decisions or actions. One sufficient reason is that, in the event of a current decision, there simply is not time for OR to help." A similar viewpoint comes from Platt and Maines (*HBR* 1959, p. 120). Hayes and Nolan (*HBR* 1970), however, take a different view; they believe that, while OR had proved useful in operating situations, trying to extend these smaller models to large corporate models led to "disaster by addition" (p. 105). Hansen (*EJOR* 1989) suggests that failures in the practice of OR, particularly those due to attempts to build too complex models, have caused the current crisis.

Ackoff (*JORS* 1979a) observes that OR has been dispersed to lower levels within organizations. Weingartner (*OMEGA* 1987, p. 258) is concerned that to have any impact on the higher levels within organizations, "We must certainly avoid coming across as members of a priesthood, as practitioners of occult arts." Rinnooy Kan (*EJOR* 1989, p. 283) also expresses his worries that OR ought to concentrate on achieving more strategic success: "For instance, what will happen if tactical, short-term planning procedures have been automated and organizations scrutinize their long-term planning problems? Will our profession have anything of substance to offer them beyond what is available today?" Zipkin (*INT* 1986, p. 88) sees a clear challenge here: "There are gaping holes in our collective abilities to model, especially at the level of strategy and policy, and therefore tremendous opportunities for energetic researchers and practitioners." Kirkwood (*OR* 1990, p. 750) feels strongly about the importance of performing strategic projects: ". . . we should encourage those who are applying OR approaches to strategic problems. If the OR community does not recognize and support this work, then the practitioners doing the work will migrate to other communities. This will mean lost opportunities for OR professionals to work on interesting issues, and

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

less visibility for operations research with corporate and government top management."

Naturally, the emphasis found in *HBR* on the learning effect of OR indicates that those authors believed that OR is a valuable support in strategic decision making; after all, for simple operational decisions, insight and understanding are perhaps less relevant. As an aside, one might feel that if the discipline wishes to play a larger role in strategic decisions, the name operations research is not so fortunate. Two remarks are in order here. First, it may be true that, whether justified or not, managers do not see OR/MS as a way to help them when they are involved in strategic decision making. On the other hand, the implicit impacts of OR/MS on strategy, or, in Mintzberg's (1985) terms, its contributions to emergent strategy, are undeniable. The Edelman finalists again provide sufficient evidence that OR/MS can help shape a firm's strategy, whether or not that was the intention from the outset. Second, it is particularly in such strategic cases that OR/MS can only function and be taken seriously if it is part of an interdisciplinary effort, the subject of the next subsection.

2.6. The Interdisciplinary Character of OR/MS

When OR emerged during World War II, there were obviously no specially trained OR workers; the field was inherently interdisciplinary, uniting scientists from diverse disciplines. From our earlier definition of management engineering, it is clear that very few individuals can hope to meet all these requirements; interdisciplinary teams are therefore a prerequisite. One could, perhaps, in a provocative mood, even suggest that an operations researcher is a contradiction in terms, arguing that OR can only be performed by interdisciplinary teams. Being at the heart of OR/MS, the question of interdisciplinarity has received much attention in the debate. A mathematical approach, whether this means using mathematical (but not necessarily sophisticated) techniques or simply an analytical style or logical thought, is an important characteristic of OR/MS, but it is precisely the dominance of mathematics over other disciplines that has attracted much criticism. It is by no means the case that every single individual in the OR/MS community should be interdisciplinary, only the OR/MS community as a whole. (Note the plural ending in The Institute of Management Sciences.) From the following, it becomes clear that, in particular, the behavioral sciences are deemed to have been neglected by the OR/MS community.

Beged-Dov (*MS* 1966, p. B586) expressed his concern: "Perhaps the single greatest obstacle to the establishment of OR as a powerful discipline noted for actual accomplishments is the fact that an ever increasing number of narrow specialists in mathematics and natural science impart to our profession undesirable dogmas and outlooks," while Morse (*OR* 1977, p. 187) also observed a "narrowing in outlook of many operations research workers." Dando and Sharp (*JORS* 1977, p. 943) are worried about the "present difficulties OR workers have with problems involving major Social or Behavioral [sic] features." The importance of interdisciplinarity is underlined by the UK Commission (*JORS* 1986, p. 844): "The Commission observed that much successful OR appears to be done in teams drawn from across an organization, seeded with one or more OR practitioners, who will play a key but not necessarily overtly central role in the structuring of the decision-forming process as the work progresses." And Pierskalla (*OR* 1987, p. 155) states: "If OR/MS is to grow, it must deal analytically and realistically with human behavior." It must "reach out to new areas of knowledge and to new approaches, and integrate them into our field."

3. PUTTING THINGS IN PERSPECTIVE

From the evidence presented in the previous section it should be clear that OR/MS has at least some cause for concern, whether or not one wishes to admit to there being an actual crisis. Although we do not consider the natural drift hypothesis as having been proved, it does remain, in our view, a possible explanation for the sense of crisis. This raises two important questions:

- What are the causes of the natural drift?
- To what extent do other fields have to cope with similar phenomena?

In this section we address these two questions, and attempt to answer them simultaneously. To do so, we turn to Abbott's (1988) book, *The System of Professions*. Abbott looks at a range of professions, including law, medicine, psychiatry, and operations research, and attempts to describe and explain their development by viewing all these professions as a system, each struggling to maintain or increase its jurisdiction, which he views as the central phenomenon of professional life. Abbott describes the arising of OR/MS as a separate discipline as (p. 237): "The OR professional community thus began as a hybrid between mathematics, various other branches of science, and the occupations within which OR was applied. It illustrates again the process of enclosure, in which a coalition of groups takes over a body of work previously

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

accomplished by members of environing occupations and draws jurisdictional lines around it." In the 1950s and 1960s, OR/MS made inroads on the jurisdiction of business management, being one of the few "formalizations that *have* worked" (p. 103). Nowadays, however, OR/MS is no longer the newcomer in the system of professions, but the incumbent, and finds itself having to fight off claims for managers' attention from other sources, such as total quality control, organizational renewal, and time-based competition. The shift from the natural optimism of a newcomer to the defensive posture of a besieged incumbent is, as Miser (*OR* 1987) suggests, undoubtedly one of the factors contributing to the sense of crisis.

Let us look at some of the observations Abbott makes about professions in general, and see how they relate to OR/MS. Rather than list a number of hard assertions here, we prefer to formulate the issues as questions, to indicate that these are issues to which we have no ready answers, and that need careful reflection.

3.1. Has OR/MS Become Too Broad?

The range of activities sometimes considered part of OR/MS has become very broad, if one includes fields such as systems analysis, the British soft systems methodology, and all the fields catered to by ORSA and TIMS. Abbott warns against such a trend (p. 88): "No profession can stretch its jurisdiction infinitely. For the more diverse a set of jurisdictions, the more abstract must be the cognitive structure binding them together. But the more abstract the binding ideas, the more vulnerable they are to specialization within and to diffusion into the common culture without." We have already mentioned the way various OR/MS tools have become widely diffused. Rather than continue to regard these tools as its own, should the OR/MS community be more concerned with developing a higher-level expertise, finding novel ways of applying them?

3.2. Has Education in OR/MS Become Too Theoretical?

We have not treated this as a separate issue, but much has been written about the role of education in the current crisis. Education in OR is in general a largely academic affair, and often accused of being too theoretical. This may be, in part, due to a defensive reaction: by ensuring that everyone in the field has undergone a thorough theoretical training, it is possible to uphold the claim that a formal training in OR/MS is necessary to be a successful practitioner, and that people with a different training can never become good practitioners, even after years of practical experience. However, Abbott observes (p. 68): "This is often a fiction, since the theoretical education in the dominant profession is often irrelevant to practice."

3.3. What About the Degree of Abstraction in OR/MS?

Abbott (pp. 102–103) distinguishes two types of abstraction: the first type "emphasizes mere lack of content; that is abstract which refers to many subjects interchangeably," whereas the second type "emphasizes positive formalism, which may in fact be focused on a fairly limited subject area; that knowledge is abstract which elaborates its subject in many layers of increasingly formal discourse." Physics is mentioned as an example of the latter type, being limited in subject but extremely formal in treatment; this abstraction of the second type apparently makes a jurisdiction quite secure, as for example, no one will try to explain particle interactions without mastering the abstract knowledge of physics. But: "extreme abstraction in the first sense . . . can make a jurisdiction weak. . . . Psychology, sociology, administration, economics, law, banking, accounting, and other professions all claim some jurisdiction in business management, each by extending its own abstractions, emptying them of content, and claiming that they cover the whole field." As much theoretical knowledge in OR/MS can be applied to a wide range of situations, and is not always limited in subject, there is a danger of accumulating too much abstraction of the first type. How can we in OR/MS prevent ending up with a huge collection of such abstractions, but no jurisdiction to apply them in? This insecurity may well be another cause for the sense of crisis.

3.4. What's So Difficult About Really Complex Problems?

We concluded that there is widespread concern that strategic and highly complex issues do not receive as much attention as they ought to. Abbott relates this to the issue of systematization (pp. 110–111): "Extreme systematization is a likely consequence of serious competition for jurisdiction." Such a competition is continually going on for the jurisdiction of business management, and OR/MS is involved in it. The way in which most textbooks in OR/MS are organized in terms of tools and the problems which can be solved with them may be considered an example of such systematization. According to Abbott, this is dangerous: "Systematization has decisive consequences for service. Where professional knowledge is

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

highly systematized, complex problems are likely to be ignored." This is reminiscent of Ackoff's (1973, 1979a) remarks that managers do not face the neat, systematized problems that OR/MS is accused of studying, but that they have to manage messes. Given the unease in the field about tackling strategic, complex problems, has such extreme systematization perhaps already taken place in OR/MS?

3.5. What Is the Cause of the Natural Drift?

Although we outlined the natural drift hypothesis in Section 1, we have not yet attempted to explain it. However, our natural drift is closely related to what Abbott (pp. 118–119) calls *professional regression.* Reading some of Abbott's observations on this subject, it is difficult not to think of OR/MS:

> Professions tend to withdraw into themselves, away from the task for which they claim public jurisdiction. This pattern results from internal status rankings. The professionals who receive the highest status from their peers are those who work in the most purely professional environments. . . . A profession is organized around the knowledge system it applies, and hence status within profession simply reflects degree of involvement with this organizing knowledge. . . . On this argument, the most pure professional work is academic work, which has nothing to do with clients at all, and, indeed, academic professionals generally enjoy high status within their professions. . . . Since professionals draw their self-esteem more from their own world than from the public's, this status mechanism gradually withdraws entire professions into the purity of their own worlds. The front-line service that is both their fundamental task and their basis for legitimacy becomes the province of low-status colleagues and para-professionals.

In the case of OR/MS, MBAs, having had an elementary introduction to, for example, linear programming and simulation, could be seen as such paraprofessionals. Specifically about OR, Abbott writes (p. 238): "Since the professions are founded on knowledge, admiration peaks when knowledge is most pure, that is, when it is least deformed by actual application. Hence mathematical preeminence." To top it all (p. 119) he states that: "The mechanism of professional regression is irreversible. Even under clear threat, regressing professions do not eradicate the internal status distinctions that cause regression."

It is tempting to feel relief at Abbott's observation that professional regression is common; even if OR/MS has seen a natural drift occur, maybe things are not as bad as they are sometimes made out to be. Alternatively, one could resign to professional regression, as it may seem inevitable and irreversible. However, there are two reasons why we believe such complacence would be highly misplaced. First, there is no reason to take it lightly (p. 238): "Professional regression seems to be extremely pronounced in fields like operations research where a small, but very elite, core maintains intellectual control over a much wider jurisdiction." So although the natural drift may be truly natural, it does indeed seem to be worse in OR/MS than elsewhere; the crisis has a reason. Second, there is no excuse for fatalism or resignation. Despite the quotes above, the general flavor of what Abbott has to say about OR/MS is that it is a profession that has managed to organize itself relatively flexibly and successfully.

4. CONCLUSIONS

This paper was largely motivated by the observation that OR/MS is not attracting managers' attention the way it used to, as reflected by, among others, the shift away from OR/MS in the *Harvard Business Review.* Although competition for managers' attention is fierce, a permanent stronghold there is essential for long-term survival of OR/MS as a practically relevant discipline (who else will pay?). We believe that the discipline can and should fight harder, and hope that this paper has identified some of the main areas toward which our efforts should be directed. Having illustrated the natural drift phenomenon above by systematically discussing various viewpoints on OR/MS, we can draw several conclusions.

First, one could perhaps think of the sense of crisis as if it were caused by OR/MS reaching the next stage in the product life-cycle, gradually changing from a highly innovative approach to a more mature management tool. In doing so, its members are finding the boundaries of the field, inevitably leading to some disillusionment. However, this does not constitute failure, but is a consequence of success. Analogously, one might ask whether alchemists, despite having laid part of the basis of chemistry, failed just because they never managed to produce gold?

Second, any individual in the OR/MS world is at liberty to choose where to position him or herself in the space defined by the six main issues distinguished above, but the field as a whole must take care not to stray from an acceptable balance of orientations. Not all current practical OR work (management consulting) would be considered as worthy of OR by the rest of the community. Theoretical work in OR seems to have shifted toward management science, leaving the middle ground of management engineering underpopulated and underdeveloped. The existence of different paradigms within the OR/MS world should be recognized by the professional societies, and each

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

paradigm should be catered to and its development stimulated. While a separation of paradigms into distinct fields (with their own societies) is potentially harmful, mutual acceptance is a must. The impression imposes itself that, in Great Britain, the Operational Research Society is closer to management engineering than the Operations Research Society of America, which lies further toward management science. Abbott (1988, p. 154) writes that professions as OR "must be flexible enough to move in directions that enable organizational survival." Finding these directions and initiating the corresponding movement is a major challenge facing the professional societies. The dispersion of many practitioners throughout organizations rather than being grouped in central OR/MS staff departments does not make this task any easier. In this light, the TIMS initiative to pay more attention to the needs of such Lone Rangers is laudable.

Third, some action is needed to improve the visibility and image of OR/MS, e.g., by aggressively pushing suitable versions of the Edelman finalist papers into the business press. *Interfaces*, after all, is only read by the OR/MS community itself, not by potential clients; the field may not be paying enough attention to the business world, it certainly is not grabbing enough attention from them.

Fourth, some areas in need of research can be identified, all typical, fundamental aspects of management engineering. The need to better understand the process by which managers can learn from an OR study has been mentioned above. It is equally important to know more about clients' expectations of OR studies and how these can best be met. Empirical research investigating factors influencing success of OR projects, such as Tilanus (*EJOR* 1985), can be valuable, but is not very widespread. The importance of research into the art of modeling has been noted by Mitchell (1973) and others. In short, the foundations need to be laid to arrive at a methodology of management engineering, or an epistemology of practice of OR.

Finally, it would be interesting to know how other professions are coping with their natural drifts, and who is responsible for their success or failure to do so. In the case of OR/MS, managers have no incentive, time or money to tackle the problem, and experience suggests that the academic community is not ideally placed to do so either. Maybe we should be looking for an "Operations Research Competitiveness Action" (ORCA), in the form of an international cooperation between academia, industry, the professional societies, and governments, who together can provide the people, the problems, the publicity and the funding needed.

To conclude, we repeat that natural forces operate to cause the natural drift described in Section 1. Where these forces have left OR/MS has been outlined in Section 2, from which it is impossible to emerge feeling that all is well. However, nearly all the problems mentioned can be avoided if ways are found to manage the natural drift; by clarifying, accepting, and exploiting the differences in orientation that exist and hopefully always will exist within the OR/MS community. The future of OR/MS is neither bright nor past, it is simply ours to shape.

ACKNOWLEDGMENT

The authors are grateful to Hugh J. Miser, the Area Editor, for his helpful and insightful comments on an earlier version of this paper, and for suggesting the term epistemology of practice. Acknowledgments are also due to William P. Pierskalla and Leonard Fortuin, although they are, of course, in no way responsible for the views expressed in this paper.

REFERENCES

ABBOTT, A. 1988. *The System of Professions: An Essay on the Division of Expert Labor.* The University of Chicago Press, Chicago.

ACKOFF R. L. 1962. Some Unsolved Problems in Problem Solving. *Opnl. Res. Qrtly.* **13,** (March), 1–12.

ACKOFF, R. L. 1973. Science in the Systems Age: Beyond IE, OR, and MS. *Opns. Res.* **21,** 661–671.

ACKOFF, R. L. 1979a. The Future of Operational Research is Past. *J. Opnl. Res. Soc.* **30,** 93–104.

ACKOFF, R. L. 1979b. Resurrecting the Future of Operational Research. *J. Opnl. Res. Soc.* **30,** 189–199.

ACKOFF, R. L. 1987. OR, A Post Mortem. *Opns. Res.* **35,** 471–474.

BAUMOL, W. J., AND C. H. SEVIN. 1957. Marketing Costs and Mathematical Programming. *Harvard Bus. Rev.* **35** (September–October), 52–60.

BEGED-DOV, A. G. 1966. Why Only Few Operations Researchers Manage. *Mgmt. Sci.* **12,** B580–B591.

BENNION, E. G. 1961. Econometrics for Management. *Harvard Bus. Rev.* **39** (March–April), 100–112.

BISHOP, J. E. 1979. Integrating Critical Elements of Production Planning. *Harvard Bus. Rev.* **57** (September–October), 154–160.

BLUMSTEIN, A. 1987. The Current Missionary Role of OR/MS. *Opns. Res.* **35,** 926–929.

BOULDEN, J. B., AND E. S. BUFFA. 1970. Corporate Models: On-Line, Real-Time Systems. *Harvard Bus. Rev.* **48** (July–August), 65–83.

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

BROWN, R. V. 1970. Do Managers Find Decision Theory Useful? *Harvard Bus. Rev.* **48** (May–June), 78–89.

CHECKLAND, P. B. 1981. *Systems Thinking, Systems Practice.* John Wiley, Chichester, England.

COMMISSION ON THE FUTURE PRACTICE OF OPERATIONAL RESEARCH, Report of the 1986. *J. Opnl. Res. Soc.* **37,** 829–886.

CONDOR. 1988. Operations Research: The Next Decade. *Opns. Res.* **36,** 619–637.

DANDO, M. R., AND P. G. BENNETT. 1981. A Kuhnian Crisis in Management Science? *J. Opnl. Res. Soc.* **32,** 91–104.

DANDO, M. R., AND R. G. SHARP. 1978. Operational Research in the U.K. in 1977: The Causes and Consequences of a Myth? *J. Opnl. Res. Soc.* **29,** 939–949.

DANIEL, D. W. 1987. Half a Century of Operational Research in the RAF. *Eur. J. Opnl. Res.* **31,** 271–275.

DAWES, R. M. 1988. *Rational Choice in an Uncertain World.* Harcourt Brace Javanovich, San Diego, Calif.

DRUCKER, P. F. 1959. Thinking Ahead. *Harvard Bus. Rev.* **37** (January–February), 25–152.

FLOOD, M. M. 1955. The Objectives of TIMS. *Mgmt. Sci.* **2,** 178–184.

FORTUIN, L., P. VAN BEEK AND L. VAN WASSENHOVE. 1992. Operational Research Can Do More For Managers Than They Think! *OR Insight* **5** (1) (January–March), 3–8.

GEOFFRION, A. M. 1976. Better Distribution Planning With Computer Models. *Harvard Bus. Rev.* **54** (July–August), 92–99.

GEOFFRION, A. M. 1992. Forces, Trends, and Opportunities in MS/OR. *Opns. Res.* **40,** 423–445.

HANSEN, P. 1989. A Short Discussion of the OR Crisis. *Eur. J. Opnl. Res.* **38,** 277–281.

HAYES, R. H. 1969. Qualitative Insights From Quantitative Methods. *Harvard Bus. Rev.* **47** (July–August), 108–119.

HAYES, R. H., AND R. L. NOLAN. 1974. What Kind of Corporate Modeling Functions Best? *Harvard Bus. Rev.* **52** (May–June), 102–111.

HERRMANN, C. C., AND J. F. MAGEE. 1953. Operations Research for Management. *Harvard Bus. Rev.* **31** (July–August), 100–112.

HUYSMANS, J. H. B. M. 1970. *The Implementation of Operations Research.* John Wiley, New York.

JACKSON, M. C., AND P. KEYS. 1984. Towards a System of Systems Methodologies. *J. Opnl. Res. Soc.* **35,** 473–486.

JAIKUMAR, R., AND R. E. BOHN. 1992. A Dynamic Approach to Operations Management: an Alternative to Static Optimization. *Intl. J. Prod. Econ.* **27** (October), 265–282.

JONES, C. H. 1966. Applied Math for the Production Manager. *Harvard Bus. Rev.* **44** (September–October), 20–182.

JONES, C. H. 1970. At Last: Real Computer Power for Decision Makers. *Harvard Bus. Rev.* **48** (September–October), 75–89.

KEYS, P. 1989. OR as Technology: Some Issues and Implications. *J. Opnl. Res. Soc.* **40,** 753–759.

KEYS, P. 1991. A Technologist's Response to Miser. *J. Opnl. Res. Soc.* **42,** 431–433.

KING, W. R. 1967. On the Nature and Form of Operations Research. *Opns. Res.* **15,** 1177–1180.

KING, W. R., V. GROVER AND A. NELSON. 1989. The Evolution of the Management Sciences: A Report on the 1988 Survey of TIMS Membership. *Interfaces* **19** (November–December), 10–24.

KIRKWOOD, C. W. 1990. Does Operations Research Address Strategy? *Opns. Res.* **38,** 747–751.

LATHROP, J. B. 1959. Operations Research Looks to Science. *Opns. Res.* **7,** 423–429.

LILIEN, G. L. 1987. MS/OR: A Mid-Life Crisis. *Interfaces* **17** (March–April), 35–38.

MINTZBERG, H. 1989. *Mintzberg on Management: Inside Our Strange World of Organizations.* The Free Press, New York.

MISER, H. J. 1987. Science and Professionalism in Operations Research. *Opns. Res.* **35,** 314–319.

MISER, H. J. 1991. Comments on 'OR as Technology.' *J. Opnl. Res. Soc.* **42,** 429–431.

MISER, H. J. 1992. Craft in Operations Research. *Opns. Res.* **40,** 633–639.

MISER, H. J. 1993a. A Foundational Concept of Science Appropriate for Validation in Operational Research. *Eur. J. Opnl. Res.* **66** (2), 204–215.

MISER, H. J. 1993b. The Global Challenge to OR/MS. *OR/MS Today.* (to appear).

MISER, H. J., AND E. S. QUADE (eds.). 1985. *Handbook of Systems Analysis: Overview of Uses, Procedures, Applications, and Practice.* North-Holland, New York.

MISER, H. J., AND E. S. QUADE (EDS.). 1988. *Handbook of Systems Analysis: Craft Issues and Procedural Choices.* North-Holland, New York.

MITCHELL, G. H. 1973. The State of Research in OR. *Opnl. Res. Qrtly.* **24,** 3–8.

MORECROFT, J. D. W. 1992. Executive Knowledge, Models and Learning. *Eur. J. Opnl. Res.* **59,** 9–27.

MORSE, P. M. 1977. ORSA Twenty-Five Years Later. *Opns. Res.* **25,** 186–188.

PIERSKALLA, W. P. 1987. Creating Growth in OR/MS. *Opns. Res.* **35,** 153–156.

PLATT, W. J., AND N. R. MAINES. 1959. Pretest Your Long-Range Plans. *Harvard Bus. Rev.* **37** (January–February), 119–127.

RAVETZ, J. R. 1971. *Scientific Knowledge and its Social Problems.* Oxford University Press, Oxford, England.

RINNOOY KAN, A. H. G. 1989. The Future of Operations Research is Bright. *Eur. J. Opnl. Res.* **38,** 282–285.

ROY, H. J. H. 1958. Operations Research in Action.

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.

Harvard Bus. Rev. **36** (September–October), 120–128.

SALVESON, M. E. 1957. High-Speed Operations Research. *Harvard Bus. Rev.* **35** (July–August), 86–97.

SCHULTZ, R. L., AND D. P. SLEVIN (eds.). 1975. *Implementing Operations Research/Management Science.* American Elsevier, New York.

SMITH, R. D., AND R. H. CULHAN. 1986. MS/OR Academic and Practitioner Interactions: A Promising New Approach. *Interfaces* **16** (September–October), 27–33.

SYMONDS, G. H. 1957. The Institute of Management Sciences: Progress Report. *Mgmt. Sci.* **3**, 117–130.

TELGEN, J. 1988. Verzin een list! (in Dutch). Inaugural Lecture, University of Twente, The Netherlands.

TILANUS, C. B. 1985. Failures and Successes of Quantitative Methods in Management. *Eur. J. Opnl. Res.* **19**, 170–175.

TOMLINSON, R., AND I. KISS (eds.). 1984. *Rethinking the Process of Operational Research and Systems Analysis.* Pergamon Press, Oxford, England.

WAGNER, H. M. 1971. The ABC's of OR. *Opns. Res.* **19**, 1259–1281.

WAGNER, H. M. 1975. *Principles of Operations Research.* Prentice-Hall, Englewood Cliffs, N.J.

WAGNER, H. M. 1988. Operations Research: A Global Language for Business Strategy. *Opns. Res.* **36**, 797–803.

WAGNER, H. M., M. H. ROTHKOPF, C. J. THOMAS AND H. J. MISER. 1989. The Next Decade in Operations Research: Comments on the CONDOR Report. *Opns. Res.* **37**, 664–672.

WEINGARTNER, H. M. 1987. The Changing Character of Management Science. *OMEGA* **15**, 257–262.

WHITE, J. A. 1991. An Existence Theorem for OR/MS. *Opns. Res.* **36**, 183–193.

WOOLSEY, R. E. D. 1972. Operations Research and Management Science Today, or, Does an Education in Checkers Really Prepare One for a Life of Chess? *Opns. Res.* **20**, 729–737.

ZIPKIN, P. 1986. Confessions of an Optimist. *Interfaces* **16** (March–April), 86–92.

Reproduced with permission of the copyright owner. Further reproduction prohibited without permission.