The Future of Operational Research is Past

RUSSELL L. ACKOFF

Silberberg Professor of Systems Sciences, University of Pennsylvania

After a brief discussion of the diagnoses of others of OR's ailments, a detailed examination is made of the impacts of academic OR on its practice. These impacts include the dispersion of OR in organizations, the displacement of OR workers, and the dissolution of its interdisciplinarity. Then the changes in OR's environment which should have evoked adaptive responses from it, but didn't, are considered. The increasing inappropriateness of OR's methodology is discussed by focusing on the deficiencies of its concept and practice of optimization, and its pursuit of objectivity. These deficiencies, it is argued, can only be overcome by a comprehensive reconceptualization of the field, its methodology, the way it is practised, and the way students are educated to practise it.

INTRODUCTION

A FEW YEARS ago I was asked to speak at the Joint Annual Meeting of the Operations Research Society of America and the Institute of Management Sciences. I characterized that occasion as a wake for the profession, whichever name it chose to use. In my opinion, American Operations Research is dead even though it has yet to be buried. I also think there is little chance for its resurrection because there is so little understanding of the reasons for its demise.

This lack of understanding is well reflected in a recent article by John R. Hall Jr. and Sydney W. Hess entitled "OR/MS Dead or Dying? RX for Survival" that appeared in *Interfaces*.[1] The authors prescribed five treatments:

(1) ... practitioners could be more effective if more of the academics' new discoveries in OR/MS theory were made truly accessible to them. The use of short (2 page) readable summaries—refereed to protect the academics' interests—could help to move theory into practice.
(2) It would also help if some academics would show less disdain for problems they have "solved before" and recognize the importance to practice of steps outside the problem-solving process (such as preparation of examples and demonstrations of techniques on real problems).
(3) Assistant professors could be brought in to help write up the case studies that older practitioners are too busy (or too lazy) to write up.
(4) Academics can be given part-time or on-site-jobs with companies in their areas.
(5) Internship programs can also foster closer relationships between academics and nonacademic professionals.

These recommendations make it clear that the only thing that Hall and Hess find wrong with OR/MS is the relationship between academics and non-academic practitioners. This position is in sharp contrast with that taken by K. D. Tocher. In a recent article[2] he questions deeply the adequacy of what Thomas Kuhn would call "the paradigm of OR," and doing so Tocher sows the seeds of revolution, not of merely superficial changes. Less generally, but no less tellingly, Jonathan Rosenhead recently called into question the suitability of the OR paradigm in the area of health-services planning.[3] For a number of years now Neil Jessop, John Stringer, John Friend and their colleagues at IOR have done likewise in areas involving the interactions of autonomous organizations.[4,5] In a very recent issue of this Society's journal, K. D. Radford offered a sketch of a new paradigm for OR.[6]

Because there is more questionning of the paradigm here than in the United States, I have the impression that Operation*al* Research, unlike Operation*s* Research, may not yet be dead. Perhaps in the land of its birth OR may have a renaissance, but, in my opinion, not without a radical transformation. In this, the first of my presentations, I deal with the death of OR. In the second,[7] I consider how it might be resurrected.

THE DENOUEMENT

The life of OR has been a short one. It was born here late in the 1930's. By the mid 60's it had gained widespread acceptance in academic, scientific, and managerial circles. In my opinion this gain was accompanied by a loss of its pioneering spirit, its sense of mission and its innovativeness. Survival, stability and respectability took precedence over development, and its decline began.

I hold academic OR and the relevant professional societies primarily responsible for this decline—and since I had a hand in initiating both, I share this responsibility. By the mid 1960's most OR courses in American universities were given by academics who had never practised it. They and their students were text-book products engaging in impure research couched in the language, but not the reality, of the real world. The meetings and journals of the relevant professional societies, like classrooms, were filled with abstractions from an imagined reality. As a result 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. This obsession with techniques, combined with unawareness of or indifference to the changing demands being made of managers, had three major effects on the practice of OR.

First, practitioners decreasingly took problematic situations as they came, but increasingly sought, selected, and distorted them so that favoured techniques could be applied to them. This reduced the usefulness of OR, a reduction that was well recognized by executives who pushed it further and further down in their organizations, to where such relatively simple problems arose as permitted the application of OR's mathematically sophisticated but contextually naive techniques.

According to Hall and Hess[1]

> "... this decline in visibility has not stemmed from a wave of firings or other reductions in OR/MS ranks. Instead OR/MS talent is increasingly being dispersed to the various corporate functions. The original reason for centralization was that OR/MS was felt to need protection to establish itself. Today, the only companies still maintaining central OR groups are those that are large enough to be able to use "internal consultants" who are available to back up the analysts down the line."

The dispersion that Hall and Hess note is a fact, but their reasons for it are a fiction. I submit that OR was once a corporate staff function, because corporate executive's believed it could be useful to them. It was pushed down because they no longer believed this to be the case, and they correctly perceived that if it had any use, it was in the bowels of the organization, not the head. My observation of a large number of American corporations reveals that when it could no longer be pushed down, it was pushed out.

A second effect of the technical perversion of OR derived from the fact that its mathematical techniques can easily be taught by those who do not know where, when and how to use them. This, together with the fact that in the late 1960s use of quantitative methods became an "idea in good currency", resulted in these techniques being taught widely in schools of business, engineering and public administration, among others. This has deprived OR of its unique incompetence; an increasing portion of it is done by those who do not identify with the profession.

These nonprofessionals are not as obsessed with the techniques nor as immune to reality as the professionals. When required to make a choice, they are more likely to embrace reality than techniques; therefore, they tend to move up in their organizations, not down.

According to Hall and Hess, the decline of visibility of professional OR in organizations "is more a sign of institutional acceptance than a sign of real decline." If one were to take this criterion seriously, one would be forced to conclude that such profes-

sions as economics and engineering, which have not declined in visibility, enjoy less institutional acceptance than OR. No. What this dispersion signifies is that 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.

The third effect of OR's immersion in techniques is that those who either practise or preach it have come to be more and more like each other. The original interdisciplinarity of OR has completely disappeared. In his recent presidential address to this Society, Professor Michael Simpson correctly referred to OR *as a discipline.*[8] OR's isolation from other disciplines was (and is) encouraged by professional societies. In several countries, including this one, serious consideration has been given to registering only qualified practitioners and to accrediting academic programmes. Nothing could be more effective in removing whatever vestiges of interdisciplinarity there are in the practice of OR.

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. The nature of the problems facing managers has changed significantly over the last three decades, but OR has not. It has not been responsive to the changing needs of management brought about, to a large extent, by radical changes taking place in the environment in which it is practised. While managers were turning outward, OR was turning inward—inbreeding and introverting. It now appears to have attained the limit of introversion: a catatonic state.

Many practitioners do not accept my characterization of the state of OR or the environment in which it is practised. Supporting evidence and argument are required. I tried to provide them in my book "Redesigning the Future,"[9] but here I summarize very briefly some of the essential points made there.

THE CHANGING ENVIRONMENT OF OR

I believe that World War II marked the beginning of the end of what might be called the *Machine Age*, an age that began with the Renaissance. In the Renaissance, man's attention shifted from death to life and the world in which it took place. Like a child, man sought understanding by means of a three-step process: first, taking apart the things he wanted to understand, then trying to understand how these parts worked, and finally assembling his understanding of the parts into an understanding of the whole. This process is called *analysis*. It was the dominant mode of thought in the Machine Age.

The use of analysis raised several fundamental questions around the answers to which the world-view and science of the Machine Age were organized. First, if in order to understand something we must take it apart and seek understanding of its parts, how do we acquire understanding of its parts? To analysts the answer to this question was obvious: by taking the parts apart. But if we can only understand something by understanding its parts, and if we can only understand its parts by taking them apart, how can we gain ultimate or complete understanding of anything? Machine-Age man, who believed in the possibility of complete understanding, also believed it could be derived from understanding ultimate indivisible parts, *elements*. The doctrine based on these beliefs is called *reductionism*. Its many manifestations are familiar; for example, atoms, chemical elements, cells, directly observables, basic needs, instincts, phonemes, and so on. Elements were the holy grail of the analytical crusade.

Once an understanding of parts is acquired, an understanding of the whole can be achieved only if the relationships between the parts are understood. It is not surprising that Machine-Age man believed that one elementary relationship was sufficient for this purpose: *cause and effect.* His exclusive commitment to this relationship yielded a *deterministic* concept of the universe, one in which everything that occurs is taken as the effect of a preceding cause. The possibility of complete understanding of the universe

required postulation of a first cause, God. The universe and everything in it, including man and society, were thus conceptualized as machines or machine parts created by God to do His work.

It was natural for men who so viewed the universe, and who believed that they had been created in the image of God, to try to imitate Him, however unconsciously, by creating machines to do their work. The product of this effort was the Industrial Revolution.

Formulation of a new world-view was brought about in part by the growing preoccupation, in the second quarter of this century, with *systems*, their growing complexity and the increasing difficulty of managing them effectively. This preoccupation led to the realization that systems are wholes which lose their essential properties when taken apart. Therefore, they are wholes that cannot be understood by analysis. This realization, in turn, gave rise to *synthetic* or *systems thinking*. Three steps are involved in this process. First, a thing to be understood is conceptualized as a part of one or more larger wholes, not as a whole to be taken apart. Then understanding of the larger containing system is sought. Finally, the system to be understood is explained in terms of its *role* or *function* in the containing system. Analysis of a system reveals its structure and how it works; it yields know-how, knowledge, not understanding. It does not explain *why* a system works the way it does. Systems thinking is required for this.

Such thinking has produced the doctrine of *expansionism* which, in contrast to reductionism, asserts, first, that ultimate understanding of anything is an ideal that can never be attained but can be continuously approached; and, second, that understanding, in contrast to knowledge, flows from larger to smaller systems; not, as analysis assumes, from smaller to larger.

At the turn into this century the American philosopher of science, E. A. Singer Jr.,[10] formulated an alternative to the deterministic cause-effect relationship and called it *producer-product*. Others have called it *nondeterministic* or *probabilistic causality*, or *directive correlation*.[11] Whereas a classical cause was both necessary and sufficient for its effect, the Singerian producer was only necessary, not sufficient, for its product. Singer showed that slicing the universe with producer-product yielded a view of it as different from that obtained by slicing it with cause-and-effect, as the view of the interior of an orange obtained by slicing it horizontally is from the view of it obtained by slicing it vertically. The universe that appears to be mechanistic when sliced by cause-and-effect appears to be *teleological* when sliced by producer-product. Moreover, the teleology revealed is *objective*: such properties and phenomena as free-will, choice, function, role and purpose can be operationally defined, observed and measured.

Systems thinking, expansionism and objective teleology provide the intellectual foundation for what may at least tentatively be called the *Systems Age*. The world-view they yield does not discard that of the Machine Age but incorporates it as a special case. Machines are understood as instruments of purposeful systems; purposeful systems are no longer conceptualized as machines. The Post Industrial Revolution is as logical a consequence of systems thinking as the Industrial Revolution was of mechanistic thinking. In this second revolution man seeks to develop and use instruments that do *mental* rather than physical work: artifacts that *observe* (generate symbols), communicate (transmit symbols) and *think* (process symbols logically). Together these technologies make possible the mechanization of *control, automation*.

Systems thinking brings special attention to organizations: purposeful systems that contain purposeful parts with different roles or functions, and that are themselves parts of larger purposeful systems. This focus reveals three fundamental interrelated organizational problems: how to design and manage systems so that they can effectively serve their own purposes, the purposes of their parts, and those of the larger systems of which they are part. These are the *self-control*, the *humanization* and the *environmentalization* problems, respectively.

Now OR has been and is almost exclusively concerned with organizational self-control. It has virtually ignored the other two types of problem and the relationship

between them and self-control. Furthermore, it employs a Machine-Age approach to the self-control problem. Its method is analytic and its models are predominantly of closed mechanical systems, not of open purposeful systems. This is clearly revealed when one considers OR's use of two concepts: *optimization* and *objectivity*.

OPTIMIZATION IN OR

I begin this discussion of optimization by retelling one of what my students call 'Ackoff's Fables'.[1,2] Two years ago, an OR group that was highly placed in an important public agency asked me to review one of its major projects. Most of the members of the group had received degrees in OR from three of the major centres of OR education in the United States.

The project involved a very large intrasystem distribution problem. Those who worked on it were very proud of the number of variables and constraints included in the LP transportation model they had developed. As usual, the researchers complained of the fact that they had not been able to obtain either the quantity or quality of data that they would have liked. As a result it had been necessary for them to engage in a bit of data enrichment.

After the team members had presented the problem, the model and their way of solving it, they showed me what they referred to as their "evaluation of the results." Over a reasonably long period of time they had recorded the decisions actually made and implemented by the responsible managers and had fed these decisions into their model and calculated the total related operating costs. They had then compared these costs with those associated with the optimal solution derived from the same model.

Lo and behold! The optimal solutions were consistently better than those of the managers. Using these differences they had estimated an annual saving which they had used successfully in convincing management to adopt their model and optimizing procedure.

Of minor significance was the fact they had not taken into account the not insignificant costs of their research and its implementation. Also of minor significance was the fact that they also had not taken account of the unreliability and inaccuracy of the data about which they had complained. Of *major* significance, however, was the fact that even if these costs had been taken into account, their evaluation would have been a *farce*.

The optimal solution of a model is *not* an optimal solution of a problem unless the model is a perfect representation of the problem, which it never is. Therefore, in testing a model and evaluating solutions derived from it, the model itself should never be used to determine the relevant comparative performance measures. The only thing demonstrated by so doing is that the minimum or maximum of a function is lower or higher than a non-minimum or non-maximum.

All models are simplifications of reality. If this were not the case, their usefulness would be significantly reduced. Therefore, it is critical to determine how well models represent reality. In this project the team had not done so.

I put the following question to its members: Suppose you could not conduct an adequate test of your model, what would you do? After considerable squirming the group's leader said he would ask managers to accept it *on faith. Voilà* and Q.E.D.!

This was not the end of the matter. The researchers went on to tell me how the managers had invariably modified the optimal solution to take into account factors that were not taken into account in the model. Furthermore, the team confessed that it had not carried out any analysis of the nature of the adjustments made by managers or their effects. When I pressed for an explanation of this oversight, I was told that the nature of the factors considered by the managers precluded their inclusion in a mathematical model. *Voilà* again!

Then came the climactic revelation. After about six months the managers had discontinued use of the model because of a significant change in the environment of the

system. This change was political in character. When I asked why they had not tried to incorporate the relevant politics into their model, I was told that such changes are neither quantifiable nor predictable.

It should be apparent by now that if the researchers had, in fact, solved a problem, it was not the problem that the managers had.

The need for learning and adaptation

The structure and the parameters of problematic situations continuously change, particularly in turbulent environments. Because optimal solutions are very seldom made adaptive to such changes, their optimality is generally of short duration. They frequently become less effective than were the often more robust solutions that they replace. Let us call this cross-over point the moment of death of the solution. Donald A. Schon[13] has convincingly argued that the life of solutions to many critical social and organizational problems is shorter than the time required to find them. Therefore, more and more so-called optimal solutions are still-born. With the accelerating rate of technological and social change dramatized by Alvin Toffler[14] and others, the expected life of optimal solutions and the problems to which they apply can be expected to become increasingly negative.

For these reasons there is a greater need for decision-making systems that can learn and adapt quickly and effectively in rapidly changing situations than there is for systems that produce optimal solutions that deteriorate with change. Most Operational Researchers have failed to respond to this need. As a consequence, the application of OR is increasingly restricted to those problems that are relatively insensitive to their environments. These usually involve the behaviour of mechanical rather than purposeful systems and arise at the lower levels of the organization; hence the movement of OR down to them.

The omission of aesthetics

There is a second very subtle deficiency in OR's concept of optimality that derives from the concept of *rationality* on which it is based. The conventional concept of an optimal solution to a problem is one that maximizes expected value. Expected value is expressed as a function of three types of variable: first, the *probabilities of choice* associated with each of the available courses of action; second, the *efficiencies* of each of these courses of action for each possible outcome; and third, the *values* of each of these outcomes. The probabilities of choice that maximize the expected value are said to be both a rational and optimal choice. Rationality and optimality so conceived are seriously deficient because they do not take into account two other types of variable. Let me explain.

A positively valued outcome is called an *end* or *objective*. Any course of action that has some probability of producing an end is called a *means*. In OR's concept of optimality the value of a means is taken to lie exclusively in its efficiency for ends; that is, the value of a means is taken to be purely *instrumental, extrinsic*. On the other hand, the value of an end is taken to lie in the satisfaction its attainment brings, to be purely *non-instrumental, intrinsic*. OR does not acknowledge, let alone take into account, the intrinsic value of means and the extrinsic value of ends.

Means and ends are relative, not absolute, concepts. Every means is also an end and every end is also a means. For example, going to school is a means of obtaining an education, an end. But obtaining an education is also a means of increasing one's income, an end. Increasing income, in turn, is a means of supporting a family, and so on.

Every means has intrinsic value because it is also an end, a potential source of satisfaction. And every end has extrinsic value because it is also a means; it has consequences.

That means have intrinsic value is commonplace knowledge. For example, we have preferences among shoes that are identical in all respects except colour. We like certain colours more than others. Such preferences among means have nothing to do with

their efficiencies but with the satisfaction their use brings. We often prefer a less efficient means because of the satisfaction its use brings. Persistent efficiency-independent preferences among means are called *traits*. Traits, in turn, are elements of that general characteristic of personality called *style*. Style has to do with the satisfactions we derive from what we do rather than what we do it for. It is apparent to all but (apparently) an optimizer that the pursuit of an objective is often more satisfying than its attainment. Herein, of course, lies the attractiveness of games.

To the extent that OR's concept of optimality fails to take the intrinsic value of means, traits and style into account, it is seriously deficient. How they can be taken into account is much too big a subject to deal with here.[15,16] But let me say a little about it.

In 1936 the eminent psychologists G. W. Allport and H. S. Odbert identified 17,953 traits.[17] It is apparent, therefore, that it is not feasible to measure, let alone include in a model, every relevant aspect of the styles of the decision makers and all others who hold a stake in a decision. This presents a problem that is taken up in Part II.[7]

Now consider ends. Every end that is less than an ideal has consequences with respect to which it can be considered a means. Every consequence of an outcome is itself a means to further consequences, and so on to an ultimate consequence. A maximally desired ultimate consequence, of course, is an ideal which is the only kind of an end that can have purely intrinsic value. Therefore, the instrumental value of any end that is less than an ideal lies in the amount of progress towards one or more ideals that its attainment represents. For example, if an ideal of science is omniscience, then at least part of the extrinsic value of the outcome of any scientific research lies in how much it advances us toward this ideal.

Again, to the extent that OR's concept of optimality fails to take extrinsic value of ends, progress towards ideals, into account, it is seriously deficient.

In a recent article[16] I tried to show that noninstrumental values of means and instrumental values of ends are *aesthetic* in character, and that aesthetic, as well as ethical, values should be incorporated into our theories of decision making. My reasons for doing so were not philosophical; they were pragmatic.

I believe the current world-wide concern with deteriorating *quality of life*, and of such aspects of life as work and education, derives from decreasing stylistic satisfaction and loss of a sense of progress. More and more people are coming to realize that optimization of all the quantities of life does not optimize the quality of life and that growth is a limiting objective.

In addition, there is a widespread belief that much of the accelerating rate of change is getting us nowhere. There is a diminishing sense of progress towards such ideals as peace of mind, peace on earth, equality of opportunity, individual freedom and privacy and the elimination of poverty. A sense of progress towards ideals gives meaning to life, makes choice significant. But today more and more people believe either with Jacques Ellul[18] that they are no longer in control of their futures, or, as the Nobel laureate George Wald[19] wrote, with part of the younger generation, that there may be no future. For them choice is meaningless. Quality of life is degraded by resignation to a future that is believed to preclude progress towards ideals.

Those of us who are engaged in helping others make decisions have the opportunity and the obligation to bring consideration of quality of life—style and progress—into their deliberations. OR has virtually ignored both the opportunity and the obligation.

Beyond problem solving

My third point about problem solving is as obvious as my second point about style and progress was obscure. Managers are not confronted with problems that are independent of each other, but with dynamic situations that consist of complex systems of changing problems that interact with each other. I call such situations *messes*. Problems are abstractions extracted from messes by analysis; they are to messes as atoms are to tables and chairs. We experience messes, tables, and chairs; not problems and atoms.

Because messes are systems of problems, the sum of the optimal solutions to each component problem taken separately is *not* an optimal solution to the mess. The behaviour of a mess depends more on how the solutions to its parts interact than on how they act independently of each other. But the unit in OR is a problem, not a mess. Managers do not solve problems; they manage messes.

Effective management of messes requires a particular type of *planning*, not problem solving. The inappropriateness of OR modelling to the type of planning required has been presented effectively by K. D. Tocher and Jonathan Rosenhead in the articles already referred to.[2,3] Furthermore, the *design* (or redesign) of organized systems so as to reduce or eliminate messes is not a preoccupation of OR. Planning and design are predominantly synthesizing, rather than analytic, activities; they involve putting things together rather than taking them apart. Moreover, there is no such thing as an optimal plan for, or design of a purposeful system in a dynamic environment. The objective of such efforts should be to produce systems that can pursue ideals effectively and do so in a way that provides continuing satisfaction to the participants.

The paradigmatic dilemma of OR

Now for my fourth point: the type of model employed in OR implies a particular paradigm of problem solving. It consists of two parts: *predicting the future* and *preparing for it*. Clearly, the effectiveness of this approach depends critically on the accuracy with which the future can be predicted. It helps us little, and may harm us much, to prepare perfectly for an imperfectly-predicted future. It is important, therefore, to understand the conditions under which perfect prediction is possible in principle, if not in practice.

Perfect prediction is possible under two sets of conditions; first, when nothing changes. Of course, if nothing could change, choice, hence problems, would not exist. At best we would be restricted to changing our own behaviour and only that behaviour which had no effect on anything external to us. Moreover, others in our environment would not be able to change any of their behaviour that affects anything other than themselves, because this would constitute a change in our environment. Clearly the world is not like this. If nothing else, what we do affects others, what they do affects us.

The second set of conditions under which perfect prediction would be possible is that the behaviour of that which we predict occurs in accordance with deterministic causal laws, and that we know perfectly these laws and the structure of whatever it is that we are predicting; that is, if the world were like what the Machine Age thought it was like, and we had such complete knowledge of it as that Age believed was within our grasp. But if what we can predict perfectly is necessarily determined, then we can do nothing about it; that is, we cannot change what can be predicted perfectly. But can we not prepare ourselves for it?

Some will point out that we can prepare for the weather although we cannot affect it; and it is conceivable that the weather behaves mechanistically even though we do not yet understand it well enough to predict it accurately. This argument holds only because apparently we are not a part of the weather system. The organizations and institutions for which we work, however, are a part of the socio-economic system that we try to predict, hence their preparations and those of others affect that system. This is why the behaviour of containing systems cannot be predicted accurately.

To avoid the dilemma deriving from the predict-and-prepare paradigm, the Operational Researcher implicitly assumes that the environment of the system he deals with, the containing system, is deterministic in nature—hence is predictable in principle if not in practice—but that the system he is dealing with has choice, is purposeful. This amounts to assuming that the containing system behaves mechanistically, but that the contained system being manipulated is teleological. Now one of the things we have never been able to conceptualize is a machine that has purposeful parts, and for good reason: it involves a contradiction. Thus there is a critical type of indeterminacy inherent in the paradigm employed by OR: to the extent we can predict accurately the behaviour

of a system of which we are a part, we cannot prepare effectively for it; and to the extent that we can prepare effectively, we cannot predict accurately what we are preparing for.

OR lives with this paradigmatic dilemma by keeping it unconscious and continues to try simultaneously to improve its predictions and its preparations.

There is a way out of this dilemma, but it requires a change of paradigm. If we can prepare for the future, we can affect it. Therefore, the paradigm of OR should be one that involves "designing a desirable future and inventing ways of bringing it about." The future depends at least as much on what we and others do between now and then as it does on what has already happened. Therefore, we can affect it, and by collaboration with others—expanding the system to be controlled—we can increase our chances of "making it happen." The wider the collaboration, the more closely we can approximate the future we have jointly designed. It is this perception by Fred Emery and Eric Trist that gave rise to their work in social ecology.[20]

Prediction and preparation were the principal modalities of the Machine Age; design and invention are emerging as the principal modalities of the Systems Age. Prediction and preparation involve passive adaptation to an environment that is believed to be out of our control. Design and invention involve active control of a system's environment as well as the system itself.

The models currently employed by OR are evaluative in nature; they enable us to compare alternative decisions or decision rules that are "given." In design and invention, however, the alternatives are "taken," created. Creative solutions to problems are not ones obtained by selecting the best from among a well- or widely-recognized set of alternatives, but rather by finding or producing a new alternative. Such an alternative is frequently so superior to any of those previously perceived that formal evaluation is not required. If it is, however, then the evaluative models of OR may have a use. The challenge, therefore, is not so much to improve our methods of evaluation, but to improve our methods of design and invention.

The point of the views I have expressed up to this point is not that OR's concept of problem solving is useless, but that it should have been taken as a starting-point of OR's development, not as its end-point. To have taken it as the end-point was to have aborted OR's development and to have initiated its retreat from reality.

The disciplinarity of OR

My fifth point relates to an allegation I made earlier: that although OR began as an interdisciplinary activity, it has become uncompromisingly unidisciplinary. This, I believe, has contributed significantly to its decline.

Colin Cherry[21] observed that up until the time of Leibniz one person could know the entire body of scientific knowledge. As science expanded it became necessary to subdivide both its pursuit of knowledge and the knowledge it produced. Disciplines began to emerge, slowly at first, but with increasing speed. A few years ago the U.S. National Research Council had identified about one hundred and fifty of them.

Subjects, disciplines, and professions are categories that are useful in filing scientific knowledge and in dividing the labour involved in its pursuit, but they are nothing more than this. Nature and the world are not organized as science and universities are. There are no physical, chemical, biological, psychological, sociological or even Operational Research problems. These are names of different points-of-view, different aspects of the same reality, not different kinds of reality. Any problematic situation can be looked at from the point-of-view of any discipline, but not necessarily with equal fruitfulness. The higher in the evolutionary scale is the object of study, the larger is the number of disciplines that are likely to make a constructive contribution to that study. For example, a doctor may see the incapacity of an elderly women as a result of her weak heart; an architect, as deriving from the fact that she must walk up three flights of steep stairs to the meagre room she rents; an economist, as due to her lack of income; a social worker, as a consequence of her children's failure to

"take her in"; and so on. Planning such an old lady's future ought to involve all these points-of-view and many others. Progress in handling messes, as well as problems, derives at least as much from creative reorganization of the way we pursue knowledge and the knowledge we already have as it does from new discoveries. Science's filing system can be reorganized without changing its content, but doing so can increase our access to and understanding of that content.

The fact that the world is in such a mess as it is is largely due to our decomposing messes into unidisciplinary problems that are treated independently of each other. Effective treatment of messes requires the application of not only Science with a capital "S," but also all the arts and humanities we can command. OR provides no such treatment. Its interdisciplinarity is a pretention, not a reality.

OBJECTIVITY IN OR

OR does not incorporate the arts and humanities largely because of its distorted belief that doing so would reduce its *objectivity*, a misconception it shares with much of science. The meaning of *objectivity* is less clear than that of *optimality*. Nevertheless, most scientists believe it is a good thing. They also believe that objectivity in research requires the exclusion of any ethical-moral values held by the researchers. We need not argue the desirability of objectivity so conceived; *it is not possible*. Since my argument to this effect appeared in this journal,[22] I repeat only its main points here.

Most, if not all, scientific inquiries involve either testing hypotheses or estimating the values of variables. Both of these procedures necessarily involve balancing two types of error. Hypotheses-testing procedures require use of a significance level, the significance of which appears to escape most scientists. Their choice of such a level is usually made unconsciously, dictated by convention. This level, as many of you know, is a probability of rejecting a hypothesis when it is true. Naturally, we would like to make this probability as small as possible. Unfortunately, however, the lower we set this probability, the higher is the probability of accepting a hypothesis when it is false. Therefore, choice of a significance level involves a value judgment by the scientist about the relative seriousness of these two types of error. The fact that he usually makes this value judgment unconsciously does not attest to his objectivity, but to his ignorance.

There is a significance level at which any hypothesis is acceptable, and a level at which it is not. Therefore, statistical significance is *not* a property of data or a hypothesis but is a consequence of an implicit or explicit value judgment applied to them.

The choice of an estimating procedure can also be shown to require the evaluation of the relative importance of negative and positive errors of estimation. The most commonly used procedures are "unbiased"; therefore, they provide best estimates only when errors of equal magnitude but of opposite sign are equally serious—a condition I have never found in the real world.

Those who conduct research in which the consequences of error are very difficult to identify often excuse themselves from giving these errors conscious consideration by calling their research "pure," "fundamental," or "basic." These labels do not change the fact that in drawing any conclusion from any type of research, a judgment about the relative seriousness of different types of error is necessarily involved.

The prevailing concept of objectivity is based on a distinction between ethical-moral man, who is believed to be emotional, involved and biased, and scientific man, who is believed to be unemotional, uninvolved and unbiased. The scientist is expected to deposit his ethics and morality, his "heart," at the door to his workplace, but to take his "head" with him. This expectation obviously assumes the separability of head and heart, of science and values. I find this assumption incredible in light of what science and philosophy have revealed about the nature of man. To make this assumption is equivalent to assuming that because we can look at and discuss the head and tail of a coin separately, we can separate them.

Immanuel Kant[23] showed that thought and observation could not be separated.

C. G. Jung[24] argued that neither thought nor observation could be separated from feeling, the source of value judgments. C. West Churchman,[25] I believe, showed the validity of this argument. To think of objectivity in terms of thoughtless and feelingless observations, as some classical empiricists did, is to think of the scientist as a camera or tape recorder. To think of objectivity as observationless and feelingless thought, as some classical rationalists did, is to think of the scientist as an unprogrammed computer. The scientist can no more act like a machine than a machine can act like a scientist.

Objectivity is *not* the absence of value judgments in purposeful behaviour. It is the social product of an open interaction of a wide variety of subjective value judgments. *Objectivity is a systemic property of science taken as a whole, not a property of individual researchers or research.* It is obtained only when all possible values have been taken into account; hence, like certainty, it is an ideal that science can continually approach but never attain. That which is true works, and it works whatever the values of those who put it to work. It is *value-full*, not value-free.

This concept of objectivity has an important implication for OR. The clients of OR are usually organizations. These organizations have purposes of their own, and so do their parts and the larger systems of which they are part. Therefore, organizations clearly have responsibilities to themselves, their parts and their containing systems. These purposes are often in conflict. Such conflicts are frequently conceptualized by managers and the researchers who serve them as games to be won. In my opinion, such a formulation is irresponsible, unprofessional, and unethical.

It seems to me that it is the responsibility of managers and their researchers to try to dissolve or resolve such conflicts and serve all of an organization's stakeholders in a way that reflects the relative importance of the organization to them, not their relative importance to the organization. This cannot be done without involving them or their representatives in the organization's decision making. To fail to take all stakeholders into account, as OR usually does, is to devalue those who are not considered or involved in the decision process but who are affected by it. Their exclusion is a value judgement, one that appears to me to be immoral. Science has a moral responsibility to all those who can be affected by its output, not merely to those who sponsor it.

SUMMARY

Now let me attempt a constructive summary. I have tried to make the following points.

First, there is a greater need for decision-making systems that can learn and adapt effectively than there is for optimizing systems that cannot.

Second, in decision making, account should be taken of aesthetic values—stylistic preferences and progress towards ideals—because they are relevant to quality of life.

Third, problems are abstracted from systems of problems, messes. Messes require holistic treatment. They cannot be treated effectively by decomposing them analytically into separate problems to which optimal solutions are sought.

Fourth, OR's analytic problem-solving paradigm, "predict and prepare," involves internal contradictions and should be replaced by a synthesizing planning paradigm such as "design a desirable future and invent ways of bringing it about."

Fifth, effective treatment of messes requires interaction of a wide variety of disciplines, a requirement that OR no longer meets.

Sixth and last, all those who can be affected by the output of decision making should either be involved in it so they can bring their interests to bear on it, or their interests should be well represented by researchers who serve as their advocates.

In Part II[7] I consider what can be done to take these points into account.

POSTSCRIPT

Let me close with a very personal postscript. I doubt the persuasiveness of the arguments I have presented here. In the early 1970s I persistently argued similarly with the faculty

in OR that I had assembled at the University of Pennsylvania. Despite three years' effort I was unable to convince them of the need for radical change. A minority of the faculty and I felt this need for change so deeply that we separated from the OR faculty and initiated a new graduate programme in what we called "Social Systems Sciences." This name was selected for three reasons. First, it was the only one we proposed that no other department of the University objected to, for obvious reasons. Second, we could not conceive of a profession, a discipline or a society using such an awkward name, and we wanted to preclude such use. Finally, it suggests, however vaguely, what we are about. Nevertheless, we would not have changed the name if we could have changed OR.

If I could not persuade a faculty that I had assembled to change its concept of OR, you can understand why I am not very hopeful here. But I would point out one thing. The OR Programme at Pennsylvania is only a fraction of the size it was when the separation occurred and it no longer has a research centre associated with it. The new programme in Social Systems Sciences is now much larger than the programme in OR is or ever was. Of course this does not prove I was right; but it does suggest that if I was wrong, it was not a costly error.

REFERENCES

[1] J. R. HALL JR. and S. W. HESS (1978) OR/MS dead or dying? RX for survival. *Interfaces* **8**, 42–44.
[2] K. D. TOCHER (1977) Systems planning. *Phil. Trans. R. Soc. Lond.* **A287**, 425–441.
[3] J. ROSENHEAD (1978) Operational research in health services planning. *Eur. J. Opl Res.* **2**, 75–85.
[4] J. K. FRIEND and W. N. JESSOP (1969) *Local Government and Strategic Choice.* Tavistock, London; J. STRINGER (1967) Operational research for multiorganizations. *Opl. Res. Q.* **18**, 105–120.
[5] J. K. FRIEND, J. M. POWER, and C. J. L. YEWLETT (1974) *The Inter-Corporate Dimension.* Tavistock, London.
[6] K. J. RADFORD (1978) Decision-making in a turbulent environment. *J. Opl Res. Soc.* **29**, 677–682.
[7] R. L. ACKOFF (1979) Resurrecting the future of operational research. *J. Opl Res. Soc.* **30.** (To appear.)
[8] M. G. SIMPSON (1978) Those who can't? *J. Opl Res. Soc.* **29**, 517–522.
[9] R. L. ACKOFF (1974) *Redesigning the Future.* John Wiley & Sons, New York.
[10] E. A. SINGER JR. (1959) *Experience and Reflection.* University of Pennsylvania Press, Philadelphia.
[11] G. SOMMERHOFF (1950) *Analytical Biology.* Oxford University Press, London.
[12] R. L. ACKOFF (1977) Optimization + objectivity = opt out. *Eur. J. Opl Res.* **1**, 1–7.
[13] D. A. SCHON (1971) *Beyond the Stable State.* Random House, New York.
[14] A. TOFFLER (1971) *Future Shock.* Bantam Books, New York.
[15] R. L. ACKOFF and F. E. EMERY (1972) *On Purposeful Systems.* Aldine-Atherton, Chicago.
[16] R. L. ACKOFF (1975) Does quality of Life have to be quantified? *Gen. Sys.* **IX.** 213–219.
[17] G. W. ALLPORT and H. S. ODBERT (1936) Trait-names: a psycholexical study. *Psych. Mon.* **211.**
[18] J. ELLUL (1967) *The Technological Society.* Vintage Books, New York.
[19] G. WALD (March 8, 1969) A generation in search of a future. *The Boston Globe*, Boston.
[20] F. E. EMERY and E. L. TRIST (1973) *Towards a Social Ecology.* Plenum Press, London.
[21] C. CHERRY (1957) *On Human Communication.* John Wiley, New York.
[22] R. L. ACKOFF (1974) The social responsibility of operational research. *Opl. Res. Q.* **25**, 361–371.
[23] I. KANT (1929) *Critique of Pure Reason.* Macmillan, London.
[24] C. G. JUNG (1926) *Psychological Types.* Harcourt and Brace, New York.
[25] C. W. CHURCHMAN (1961) *Prediction and Optimal Decision.* Prentice-Hall, Englewood Cliffs, N.J.