O.R., SYSTEMS LEVEL AND THE 'OPERATIONAL ENVIRONMENT'

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Abstract

It is the contention of the author that O.R. has been steadily 'painting itself into a corner' and thereby reducing its utility.

Whilst granting some pressures toward academic respectability the main conceptual traps are seen as (a) failure to identify the system level of the operating system (b) failure to identify the level of causal texturing of the operational environment.

These shortcomings have led a great deal of OR into the mode of thinking that Ackoff identified with planning for optimization.

It will be suggested that the current turbulence in organizational environments requires an adaptive response in O.R. An alternative for the paper could well have been 'OR for ideal seeking systems in a turbulent social field'.

Cf. Intro to G. Heald (Ed.)
Last Chapter of Futures.

The exponents of O. R. appear to have been steadily 'painting themselves into a corner'. Their universe of discourse has increasingly become an esoteric academic universe, fit only for academic discussion. It is a universe far removed from the world of operations described in the pioneering O.R. work reported by Morse and Kimball, Crowther, Waddington and Blackett.

Unlike the highly successful pioneering work in O.R. this modern universe of discourse scorns discussion of actual operations in any walk of life or of ways of extracting information about such operations. We are unable to judge whether the operations of O.R. itself are highly successful or highly unsuccessful because there is little room in this precious modern universe of discourse for discussion of such things as what actually happened after modeling indicated that such and such changes would be beneficial. Were the changes not implemented? Were they implemented but botched up in the implementation? Were they well implemented but produced funny results? It would seem that these questions are now out of place in Operations Research, the foremost journal of O.R. (in term of prestige, size and subscriptions). In volumes 21 and 22 of the journal, 1973-4, I could find only thirteen articles (less than fifteen per cent of the space in the two volumes) that attended to the fore or aft end of the O.R. paradigm – observations and verifications. The rest were all huddled amidships – playing checkers out of the wind, or up the mast dangling sale – signs that pathetically read as "Of course, no claim is made that the model presented here reflects the complicated phenomena of reality. It may, however, offer some insights that have practical value." (quoted from Vol. 21) One can imagine what a contribution such a sales line would have made to the mutual trust and understanding between Blackett, Bernal, Ellis Johnson and Co, and their 'Service Chiefs'.

Mine is a view from the sidewalk. However, I think you will grant that Hugh J. Miser, as editor of Operations Research 1968-74, has been as well placed as any to observe the trends from within the main-stream. I do not find our views in disagreement. He observes that despite a deliberate policy of editorial encouragement:

> "…it is nowhere near as common (for a paper to report actual observations) as the importance of this activity would suggest it should be…." (p.907)

> "The literature reporting verifications is scarcer even than that showing observations, in spite of the obvious fact that, without it, we cannot truly say we have a science." (p.908)

> "…at times it has almost seemed as though operations research and modeling (admidships!) were synonymous." (p.907) (Vol. 22, 1974)

It is clear that it is not the journal staff who have dictated the universe of discourse defined by the journal's contents. This has been defined by forces within the profession that are beyond the editor's control.

It could be that there is a simple rebuttal to the hard line I have laid down. This would be, I think, that,

a) the pioneers had not, at that stage, understood what O.R. is essentially about.
b) O.R. can only become a science, and a respected science, by developing general theories of optimization, allocation, queuing etc. It is through the modeling process that such general theorems and theories will be arrived at.

My argument would certainly be much set aback if either of these points were established.

I do not think they can be.

O.R. is <u>not</u> a science and the pioneers were quite correct in recognizing this. Let there be no misunderstanding. I am stating that O.R. is not a science: I am <u>not</u> for one moment suggesting that there is not science <u>in</u> O.R.

These are 'fighting words'. How can I justify them? First, and at the simplest level, let us take note that many of the pioneers in O.R. , e.g. Bernal, Blackett and Zuckerman, in the U.K., were already scientists of repute before they came into O.R. They had no illusions that they were making a science of military operations in combat, logistics or training. They did know that they were bringing the learning of science into collecting information, analyzing information, setting up explanatory hypotheses and designing verification procedures. They also found that bringing science into these matters, made a significant improvement in men's ability to achieve their desired objectives in real life situations. Furthermore, they knew that they were not performing as physicists, chemists etc. but as operational researchers. They were not, as physicists, trying to devise better performing magnetrons nor, as chemists, trying to invent new chemical agents of war. They knew perfectly well that they were doing something different

The reports of what the pioneers did and their own attempts to generalize on what they did all point to a single main thing. They went into <u>the grey areas of human experience</u> where knowledge and ignorance are at best in the proportions of 70:30 or 30:70 in mixture. This is not the world of the classical sciences. Within the paradigm of the classical sciences, it could be said that laboratory controls were aimed at achieving a 95:5 ratio of known and unknown. That way real scientists could make sound unerodable contributions to the general corpus of knowledge. Little wonder that scientists typically display a convergent, not a divergent, style of thinking. Little wonder that the most successful of the pioneers in O.R. were those that came from such indeterminate disciplines as biology and psychology (e.g. Gordon Zuckerman and Trist in the U.K.) or were somewhat crazy classicists (e.g. Blackett and Bernal).

The pioneers were well aware that what was common in their O.R. successes was not a common feature of the classical sciences within which they had gained prior distinction. As Morse and Kimball put it,

> "Large bodies of men and equipment carrying out complex operations behave in an astonishingly regular manner so that one can predict the outcome of such operations to a degree not foreseen by most natural scientists." (my emphasis)

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Or Crowther and Whaddington, from the other side of the Atlantic,

"Warfare is an extremely complicated activity… After many years of experience, the good military commander acquires skill in judging the value of weapons, tactics and strategy. But he has not the time, or the scientific training, to submit this qualitative judgment to quantitative analysis. It is found that experienced executive judgment in many professions is most likely to go wrong when quantitative analysis requires the application of the theory of probability. (my emphasis)

The above, to my mind, is clear enough evidence that the pioneers grasped the essence of O.R.: that is that, as Shewart showed in 1939, it is possible to establish relevant truths in the grey areas; things that cannot be taken into the lab may still be examined scientifically; in matters that are not black or white probability theory might provide a scientific basis for inferences.

What has been said so far might correct any notions about whether the pioneers were naïve about the O.R. discipline they were producing. It might, however, still be held that the best current strategy for O.R.'ers is to model away like beavers till general theorems or theories are achieved, and hence O.R. more rapidly approaches the status of a genuine science. Even if it was the way the sciences were made it is a doubtful recipe for O.R.

We have already indicated that the pioneers were quite clear in their minds that what they were doing in O.R. was very different to what they were used to doing in science. Even those who came from sciences such as biology and psychology that were used to relying on probability theory were well aware that they were in a very different ball-game.

I would conclude from this that the pioneers were very well aware of what the essentials were of O.R. They were not floundering.

It might still be argued that as a strategy, not mind you the grand strategy, O.R. should be doing what it is doing now i.e. concentrating on the modeling phase even to the point where it seems 'synonymous with O.R.'. This argument seems to me to be saying 'let us make the science in O.R. so good that when we put it back into O.R. it will make the whole practice look really good, and scientifically respectable'.

My first, short tempered, answer to this is, 'would not O.R. look good if it made real contributions to the currently significant affairs of man as it made to the military affairs of W.W.II?'

Let that pass.

Let us more deliberately explore the difference implied in O.R. and science. In the process we might get some sense of the implicit claim that modeling is scientific theorizing.

The parallels and contrasts between the scientific endeavor and O.R. can be seen by setting up a table like the following:

Table I

Science	O.R.
Start: from problems defined by the scientific paradigm (e.g. from the peculiarities of previous experimental findings).	From a problem arising in practices outside the scientific paradigm (even for instance the planning of an O.R.S.A. conference, see Vol. 22 of O.R.p.)
Entry phase: Establish what is already known about that problem to science - increase knowledge of the general, the universal - library work.	Establish the context of the problem – increase knowledge of the particularities of the problem – field work.
Reactive phase: mathematical modeling of the 'general' that will permit rigorous extension to cover the problematic particular.	Statistical analysis to help establish a genotypical representation of the problem situation.
Response phase: laboratory experiments or critical observations.	a) re-design of context b) evaluation of re-designs.

These contrasts arise because science is concerned to establish equations of the form $x = f(y)$, where both x and y are abstracted dimensions of reality; O.R. is concerned to establish equations of the form $X_{t2} = f(X_{t1})$ where X is a concrete bit of the real world. That is, O.R. can, hopefully, find some way that an existent concrete reality can be transformed into another more desirable concrete reality. These concrete realities do not have the universality of H_2O, neutrons or fluorospar. If there is any science in O.R. it is not to be found in the immutable properties of its subject matter. And, if the subject matter does not have these immutable properties then no amount of modeling will ever make it otherwise. Models can invoke equations of the $X = f(y)$ form till the cows come home and still make no contribution to the O.R. problem of solving the function in $X_{t2} = f(X_{t1})$. The way they are going most O.R.'ers will not even be able to define X at t_1 or t_2. What in their journal, in the last twenty years, would help them decide on when a description of a situation is genotypical and not just phenotypical? What would instruct them on how to go from a theoretical solution to implementation testing and conversion to a standard operating procedure?

Woolsey, Ackoff and myself have attempted to outline ways in which people could be educated to be effective O.R.'ers. I do not want to go into that here. I want instead to try and sort out why O.R. drifted in this fashion.

For a long time I thought it was just the drift to the universities. Certainly there are extended career opportunities when O.R. becomes recognized as a suitable discipline for university teaching. Certainly, to achieve and extend this recognition it is necessary that O.R. look like a special branch of applied mathematics. Also, it is easier to build acceptable university curricula around applied mathematics than around messy case work. Lastly, a publications performance can be much more easily maintained by the staff if they cut themselves loose from the fore and aft ends of the O.R. paradigm.

It would almost seem that no other explanation is needed for the course O.R. has followed. But I have still been wondering. When O.R. has been successful it has been so successful that I feel that O.R.'ers would have stuck to this course come hell or high water, regardless of the temptation to a quiet secure life in academia.

What I am suggesting is that O.R. was not seduced by prospects of academic respectability but in fact embraced academia, and all that that meant, because they really believed that that was the direction O.R. had to go for its own sake. I think that I am, furthermore, implying that the present shape of <u>Operations Research</u> is not due simply to the academic O.R.'ers. I am suggesting that for the practitioners the model is the name of the game: Woolsey is the odd-man-out.

It is my guess that O.R. bogged down because its practitioners commonly shared the belief that: -

1. there was nothing basically distinctive between their subject matter, operating systems, and the subject matter of other scientific disciplines e.g. physics, geology, astronomy, physiology
2. There was nothing basically distinctive about their methodology.
3. The action-research side of their work involved no more than the sales skills of the usual management consultant.

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