



Management Science

Publication details, including instructions for authors and subscription information:
<http://pubsonline.informs.org>

Commentary

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To cite this article:

Stafford Beer, (1965) Commentary. Management Science 12(2):B-10-B-12. <https://doi.org/10.1287/mnsc.12.2.B10>

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COMMENTARY

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The taxonomy proposed by the authors is particularly interesting because it is descriptive of attitudes and neutral towards motives. They analyse for us what they find that people say about implementation; in what they say, these people betray motives which are not themselves challenged. I would unhesitatingly challenge what I take to be these motives in most cases.

The management scientist faces, it seems to me, an ethical imperative. It is *to serve* the manager, because (for our purposes) the manager holds power as of right. If we, as men, seek to overthrow the manager, or to supplant him, or even to criticize him, we do so in the role of anarchist, or usurper, or commentator: we must accept the appropriate role and the concomitant responsibility. But these activities are not actions taken qua researcher. There can be only one legitimate reason for undertaking research into decision-taking, policy-making and control—namely to improve the way these things are done. Since it is the managerial prerogative to do them, the researcher's role must be that of hand-maiden.

This position is taken, I repeat, as an ethical imperative. The concept of service, however, has to be interpreted. In the first place, I should not equate the role with that of an adviser. Suppose we say: 'course A appears to yield a 75% chance of \$1m. profit; course B offers a 90% chance of bankruptcy. However, I am no more than an adviser: you must take the decision'. Clearly the decision is nugatory. We researchers very often take decisions, although we say we do not, in modelling the situation as we have modelled it, in selecting the evidence as we have selected it, in using the particular techniques of analysis and synthesis that we have used. It is important to note that the manager who accepts my proposal accepts *me* more definitely than he accepts the proposal as such. Therefore, to cut out of the implementation phase is immoral.

On these bases, I think that the separatist position ought to be discarded outright. The separatist can exist only as an academic observer of the field (*ars gratia artis*). Moreover, he can by definition be no more than a poor management scientist. For management is *about* change, *about* successful implementation. If the separatist is genuinely separate, he can know no more of the situation than can the physicist who seeks to know the characteristics of a particle without in some sense observing it. If he does observe it (I invoke Heisenberg) he changes the situation; he is therefore not a separatist. The separatist position, it seems, is methodologically absurd as well as socially unethical.

The remaining three postures indicated ('communication', 'persuasion' and 'mutual understanding') are not necessarily mutually exclusive. With the authors of this paper, I unhesitatingly adopt the 'mutual understanding' position, notwithstanding that communication and persuasion each play some part

in this. The main offence committed by the protagonists of these two pure positions, insofar as they are quoted, is their attitude of ineffable condescension. The communicator who believes that management has a duty to understand him, while he has no duty to understand the manager, has a strange concept of service. Moreover, he is surely not a very observant scientist; if he were he would know that some large proportion of the management problem in real life is a projection of the manager's personal dilemma—which becomes in consequence highly relevant to any problem under discussion. Secondly, the pure persuader begins by assuming that he is 'right' and the manager 'wrong'—an attitude not at all consonant with scientific humility, nor let it be said with the facts.

Personally I believe that it is both wise and opportune to begin with the assumption *that the manager knows his job*. If the researcher knows his job too, the two men may well create a higher synthesis in implementation than either could hope to encompass alone. The manager's shortcomings are that he is not trained to look for systemic structure within the situation he has to govern, and that he has no formal means of quantifying such structure anyway. His strengths are knowledge and experience, leading to an intuitional grasp of what to do. It is high time we researchers recognized intuition as a strength, instead of branding it a weakness—which it becomes only when it is obsessively pursued against real evidence. Surely it turns out that the computer in the cranium is heuristically programmed to do a very fine job? My own criterion is this: when the manager *knows* he is right, I take a very long look before contradicting him. Trouble arises only when the manager who has no idea what to do *pretends* to know, because his image of himself seems to demand that he should know. Thus, if the manager and the scientist is each frank with the other about what he really knows and what he does not know, the two will find their knowledge to be excitingly complementary. Between them they may forge new knowledge.

Writing from England, I would like to speculate on the national differences in attitude which may well have arisen, and indeed seem to me to arise, between American and British practice in this field. It seems a fair generalization to say that most American operational research has been university based or inspired. Hence the OR man carries an appropriate intellectual and social prestige, he enjoys a certain financial independence, and he escapes, on the whole, organizational sanction at the operating end. The British OR man, on the contrary, has been almost entirely institutionally based: our universities have taken an interest in OR comparatively recently, and academic activity in the context of real problems is very small indeed to this day. Thus the British OR man has been dependent on his employers to an alarming degree—even to the point where perversions and evasions of scientific objectivity have occurred. This judgment is a generalization, I repeat, but it suggests whence differences in attitudes to implementation arise. A very important test of its reliability is furnished by the startlingly different practice in the two countries in regard to publication. Is it not true that most American (non-military) OR is published in some form? I doubt if 5% of British OR has been published over twenty years of civil applications.

Why not? The answer is because OR has to be based on mutual understanding,

relying in turn on the motive of service. The British manager disbelieves in a collaboration which involves immediate publication of (as he sees it) his inadequacies. He disbelieves in an alleged motive of service which at once results in self-aggrandizement on the servant's part. I do not blame him too much. But I do consider that this block in the *general* dissemination of ideas and of results has retarded the OR movement in Britain. We have had to rely, for the most part, on heavily disguised case material and word of mouth to tell our story.

Yet there have been many advantages. First, the British worker discovers early in his career that the objectives of managers are not those to be discovered in most textbooks. The manager may well underwrite the objective of profit optimization if one emerges from a university to ask him. It is, after all, a good gambit to offer an academic who is supposed to be above such materialism. Work with the manager, on the contrary, and you discover the facts. He is more interested in such things as robust policies than in maximizing policies; he values smoothness more highly than spectacle; his public goals are highly correlated, albeit in concealed fashion, with his private—even secretly personal—goals. How could one *help* such a man from the isolation of the separatist or the arrogance of the pure communicator, the pure persuader?

Secondly, the researcher committed to institutional work soon begins to see that problems always exist in a wider context than that initially propounded, and that this context must affect the outcome. Hence, even though he abandon the ethic I have thrust upon him in this note, he understands at last that scientists too have limitations. To say: I could do this job better than the manager, and to seize the reins of power, is to fall into the trap of scientism. At this level, our preoccupation with rigour becomes a neurosis, and our plea for model-building a withdrawal symptom. These things are scientific capabilities for use as aids; they cannot be surrogates for the real world except in a madhouse.

For these reasons I conclude that the motive of OR is service, and that the mode of mutual understanding is the only mode of implementation for that service which will actually work in a viable enterprise. Protagonists of the other three pure strategies all run this ghastly risk: that they elegantly and sophisticatedly devise the optimal way of implementing disastrous plans.

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