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Commentary

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COMMENTARY

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As I understand it, this paper takes up the venerable problem of "knowledge utilization," focusing on the relationship between the scientist and the management-customer. The authors believe that rational answers fail to become real solutions to problems unless the user of research is willing to understand and adopt the research findings. The position taken by the authors is that "mutual understanding" between researcher and manager is the only true, though possibly least comfortable, path to the effective utilization of research.

This sounds like pretty mild stuff at first glance, something akin to One Worldism and Brotherhood and all the other eternal verities. But what the authors *mean* by mutual understanding is radical and goes deep. They argue that the main blocks to using the results of scientific effort (implementation) are the "unconscious" and "politics," commodities which are liberally available in all shapes and sizes in both the scientific and business communities. Mutual understanding comes about through a process whereby researchers and managers turn inward and recognize these problems and resistances in their own universe before they begin contemplating and manipulating the other. I presume, though the authors never say so, that this inward emigration is only a first-step which provides analogues to the relationship itself and the problems it is trying to solve.

The readers can make up their own minds about the persuasiveness of the paper. As one acutely interested in the same class of problems, I find it a gem, uncut and rough, but a gem all the same, because it identifies the major problem confronting the behavioral sciences (and OR) today (the effective utilization of research) and illumines the darkness surrounding it. Implementation *is* the problem and the relationship between researcher and user *is* its pivotal element.

I can be fairly concrete about my admiration. First of all, it locates the area of "resistance" not in stupidity, innocence, laziness, incapacity or any other futile label, but in terms of the unconscious and its social twin, politics. From there it takes a major step forward by implying that these automatic biases can be utilized, if understood to improve the relationship between consumer and producer of research goods. I liked this point enormously and recent work (of Rosenthal in particular, Orne, and Mills) demonstrates dramatically experimenter bias and the way in which our unconscious operates for and against hypothesis confirmation depending on whose hypothesis we're testing. The objective implacable facade of science has its subjective insinuations, too.

And science has its politics as well and makes equally strange bedfellows. I remember some years ago, after the Festinger, Riecken, and Schacter (*When*

*Prophecy Failed*¹) study was published, how social scientists reacted to the Apocalyptic group under study. How could this group increase their zeal and their proselytizing tactics after the date they predicted for the Second Coming failed to produce more than a sunny sky? How irrational and hence inhuman to deny clear, disconfirming evidence! How bizarre! A bunch of kooks!

Ever known of a scientist to give up his pet hypothesis? Despite lack of experimental confirmation? "It must be a design problem." "Damned instrument is weak." "Let's get some more graduate students and Guttmanize the scale; that'll do it." Well, maybe but we've got an awful lot in common with that bunch of kooks. At least we ought to understand them.

So, the Churchman-Schainblatt thesis is enlightening and reaches out in some fascinating and new directions for a viable answer to the problem of implementation.

But they were unwilling to go far enough. They dwelt on the first three solutions (separate functions, communication, and persuasion) which they rightly claimed were inadequate for 23½ pages and then spent about 3 pages on mutual understanding. Like detectives, they have identified and trapped the culprits (unconscious and politics) but on insufficient evidence and without quite knowing what they should do, now that they've got them.

The authors never define their terms. It sounds as if they intend politics to be the constellation of forces which determines belief. But politics can mean something else; like: pleasing authorities or relevant groups and officials; like: power maneuvers and status striving; like: cliques and cabals jockeying for power. You name it. Churchman and Schainblatt seem to think that once politics determines a decision, then the unconscious takes over and attempts to confirm and testify to it. I find this too narrow a view of both political and unconscious processes. But I believe the authors on the basis of my own experience, not by their arguments. So I ask them to clarify what they mean by politics and unconscious and their interaction. For example, is politics the social side of the personality's unconscious? Is "false consciousness" what is meant by politics? Are the processes of repression and suppression (and other defense mechanisms, for that matter) operative in an organizational framework as they are in the individual?

But let us go another step. Say one trusts their thesis, which I do. Does it lead to action or is it an example of what they deplore, namely, proposing unacceptable answers? They are asking the scientists and managers to turn inwards and make "an intellectual attack on the mysteries of management and science." How is this done? By themselves? It's hard enough to lower our defenses when some trusted and respected person interprets our behavior or "gives us feedback." Eaton has shown how valid data are resisted by subjects, particularly if the data are evaluative and negative. Psychoanalysts are continually saying that the "analysis of resistance" is the crux of therapy. So how can self-analysis overcome the defenses? Freud once made the charming observation that the only difficulty with self-analysis is counter-transference. And from a more philo-

¹ Minneapolis: Univ. of Minnesota Press, 1956.

sophical point of view, how can people or organizations or institutions overcome the reflexive dilemma? In short, the forces *against* analysis—without outside help—appear formidable.

What I really object to in the Churchman-Schainblatt argument is their emphasis on *blocks* (resistance) to a relationship rather than attending to the elements which *build* relationships. What are the ingredients of good instrumental relationships? We know some of the answers: valid communication, open and authentic; the ability to be dependent without fear of enslavement; the ability to fight without fear of disabling retaliation; the ability to take risks with others; the ability to influence; the ability to predict; the ability to be onself without harming the other and the ability to allow someone else to be himself; the ability to grow and permit another's growth. These are some of the ingredients which can go into a good relationship and which can be summarized with the word "trust." And perhaps one other: empathy. But empathy doesn't mean *becoming* the other. Professor Tolman used to say that in order to become a good rat psychologist, one had to *think* like a rat. He did not say that one had to *be* a rat! And to what extent *do* the authors want the scientist to become *like* a manager and vice versa? They are not clear on this point.

Now: Is it necessary to probe into the unconscious and explicate the politics in order to have a relationship of mutual understanding? Is it necessary to become empathic or similar to another? Certainly, I have observed many relationships of true collaboration between teachers and students, leaders and the led, researchers and managers, without mutual understanding. Of course, we still know very little about the formation and maintenance of trust, and indeed, about the whole subject of building good interpersonal relationships, but I doubt that self- and organizational analysis—particularly when there is a problem to be solved—is the easiest, best, or even most feasible way.

I should like to make a few other points before ending. None of these should be interpreted as criticisms, but rather as requests for clarification:

1. In their appendix the authors make evident that which they slighted in the body of their paper. There they continually talked of the manager-researcher relationship as if it was a discrete unit detached from institutional or organizational realities. All that was necessary to achieve was for person-scientist to mutually understand person-manager. But we all know that that person-scientist is a role-scientist embedded in an organization and responding to institutional imperatives. And the same is true for the manager. Furthermore, they come together within this complicated field to solve a problem or deal with some exigency which is the *raison d'être* of their relationship. And finally, there is an answer, or alternative answers suggested by the scientist.

Given these factors I would suggest to the authors that they examine the possibilities of a "balance theory" to explain the determinants of mutual understanding. I thought that Emery and Oeser used this well in their marvellous little book, *Information, Decision, and Action*, London and N. Y.: Cambridge U. Press, 1958, where they applied it to the adoption of farming techniques in Australia.

2. I think the authors believe, though they never state it, that the *type* of problem determines the quality and degree of the relationship. Admitting that mutual understanding may be crucial in some conditions and not others, I would say that the latter represents that class of chance events where the problem does not implicate human-value dimensions. To put this in hypothetical form, the more the problem involves human-value dimensions, the more necessary it is to develop a relationship of mutual understanding.

3. Another thing the authors do not consider is that the scientist-manager relationship is slightly "tilted"; I mean that the consultant change-agent, etc. is called in to help, by a client. The scientist *does not* call the manager in, though that may not be a bad idea. There is an important role definition here, one which implies a power differential, namely, expert power. This, by itself and for certain problems may be all that is necessary, particularly if the class of problems do not significantly involve human factors.

I have rambled so much that I should summarize my main associations to the paper. First is my belief that the authors shied away from really coming to grips with their conception of mutual understanding and left dangling and unclarified a sufficient amount of ambiguity, almost making it too easy to agree or disagree with their central ideas. Primarily I want my comments to have the effect of forcing the authors to clarify verbally and through example and evidence, not only by formal analysis, their idea of mutual understanding.

My main substantive criticism is this: The authors believe that the quality of the relationship determines the effectiveness of implementation. They argue that the relationship can work (and again they are obscure on this point) only if the vagaries of politics and the unconsciousness are understood (through self-analysis?) I would argue that viable and good relationships can be produced from building trust and valid communication without recourse to understanding unconscious motivation. In fact, it may be the only truly *operational* way to arrive at mutual understanding.²

I said earlier that this paper earned my admiration. I did not say fully why. Because it triggered many associations and relations, and because it will compel me to do my own thinking in an area which, if successfully understood, will provide a breakthrough, not only for OR, but for the entire range of management sciences.

² See *Interpersonal Dynamics*, Edited by W. G. Bennis et al., Dorsey Press, Homewood, Illinois, 1964, particularly part V.

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