Comment on Young's 'Alternative Realities'

Stanley Young speaks of the need for paradigmatic analysis in management systems. One of the main features to emerge from his paper is a strong sense of commitment to the idea of culture. In calling the represenations of subjectively shared views of social reality *paradigms*, the author suggests that they can be transformed according to a cybernetic scheme. I can go with him quite a long way, though not all the way.

That growth of knowledge is not a cumulative process but one of error-elimination, is now a well-established fact. As far as I know Karl Popper (*Objective Knowledge,* Oxford Univ. Press, 1979) used first the diagram: problem—tentative theory—evaluative error elimination. Having said that evolution means error-elimination, Sir Karl immediately adds that the incredible thing about life 'and mental growth is self-transcendence. Laboring the difference between science and the humanities has long been a fashion, and has become a bore. The method of conjecture and refutation is practised by both areas of inquiry. The task of science is to find satisfactory explanation, one which is not *ad hoc,* and the idea of independent evidence can hardly be understood without the idea of progressing to deeper layers of explanation. There is an increasing number of paradigms, but pure knowledge and fundamental research grow in the opposite direction, towards increasing integration and unified theories. The tree of knowledge springs from countless roots which grow up rather than down, and which, ultimately, tend to unite into a common stem. One remarkable thing about this position is that anybody who holds it ought to reject the law of excluded middle. For it is obvious that neither paradigm p nor the *not-p* can be fully supported by the evidence available.

It is then doubtful, that the paradigmatic analysis can replace the integration of paradigms because a deliberate use of a particular paradigm in management can be seen as a special case of pendularity, similar to the use of 'corridors' criticised by Sutherland (*Societal Systems,* North-Holland, 1979). In fact, in the theory of scientific revolutions developed by Kuhn there is no place for progress or for direction.

The hope for escaping conflict rests on an argument for abadoning the naive model of paradigmatic transformation and substituting an alternative model for transcending conflicting paradigms (*HSM* 1 (1980) 71—76).

This is perhaps the place to stress that the ideal of total control over the production of a human system is a crude rationalistic myth in management. No human system is ever totally determined by the manager's paradigm. Any organizational result of even minimal complexity will exibit more significant features than could conceivably result from a particular paradigm of the manager. Thus, any argument that attempts to fix the legitimate paradigm in management is destined to be a slippery-slope argument, crippled by the impossibility of judging which one is the best.

Modeling the growth of knowledge by continuous synthesis seems to avoid this difficulty. In fact, this could be the difference between ideology and science.

Borowing from Althusser, we should not forget that the frontier separating ideology from science was crossed a long time ago, that this great undertaking has been inscribed in the conceptual system of a knowledge whose effects have little by little transformed the face of the earth. We cannot and must not renounce the benefits of this gain, the benefits of these theoretical resources which far transcend in potential the use that has so far been made of them. We must not forget that an understanding of what is going on in the world today is only possible if we do not fall behind the uncertain frontier between ideology and science. We can give help to all those who are near to crossing that frontier, but only on the condition that we have crossed it ourselves, and have inscribed in our methodology the irreversible result of this change of scene.

And this is what Stanley Young tries to do.

Constantin Virgil NEGOITA

Simple as a little suggestion box?

Comment on Uhr's 'Toward using everybody's intelligence and knowledge'

Leonard Uhr points out in his editorial (Human Systems Management 1(3) (1980) 201–203) that we now have communication tools available which could substantially lower the effort and cost of obtaining citizen input in the local government planning process. Information and opinions from citizens, he feels, would vastly improve the planning process because citizens have a clearer view of their local micro-environment than any planner could hope to obtain.

It is true that citizens know better than any planner what is currently happening in the neighborhood. Developing communication technology also makes the solicitation of citizen input easier. For such an idea to be completely fruitful, however, some substantial human systems challenges will have to be met. As currently envisioned, the technology will widen the gap between those who are already citizen participants and those who are not. The current citizen participant is more often the economically secure, highly educated and socially integrated person than the economically, educationally, and socially marginal individual. And participation by the marginal person is likely to decrease if we move toward a high technology which employs impersonal communication techniques.

It would be a cruel travesty of open government to implement and utilize a technique which is supposed to allow more citizen input into government but says to the already disadvantaged, "Yes, we know you prefer personal communication systems over these impersonal, computerized communications and, yes,

we know you are not terribly articulate, especially when it comes to written communication; but in the interest of letting you participate more fully in government we are implementing a new technology which requires you to use the sort of communications channels you are least likely to want to use."

Moreover, the planner who is truly out to serve the community does not plan simply for what is; the job requires attempting to project what will be and altering development so that what should be is more likely to come about. Citizens may reliably report on how things should be done for their own best current interest, but what of the interests of the rest of the community, those who don't know that neighborhood? What will cause them to send inputs into the decision-making process on a particular item, that of traffic light? How will they know that new information is wanted? And what of those who are not part of the current population of the area, those who will arrive in the future? What will be the information input for those to come, those who will have to live with the long-term consequences of what is done now?

It is not that computer and communications technology can't help in planning or that the problems I have suggested can't be overcome, it is just that human systems for citizen participants are not as simple as a little suggestion box.

Robin CRICKMAN
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HSM in Europe

I am thinking about the apparent HSM difficulties to involve more Europeans in Human Systems Management Project. I think you [M. Zeleny] are partially right in observing Europe as "uncertain of its future, scrambling for short-term rewards, without dreams and visions ...". But I presume that North

America is not so different in its basic culture – and I expect that I am not so different in my perceptions than any other European scientist (or citizen). So let me try another explanation.

We in Europe are now assessing the same basic questions as you in America. Seven years ago, when

you started with HSM, we enjoyed very much to realize that it was feasible and acceptable to raise such questions in our 'normal' activities. As you did, we all did! Today we perhaps feel that we are more able to assess those same questions, in terms which are more adequate than you in the States do. Afterall, we are reading E. Morin, Y. Barel, H. Atlan, J.-P. Dupuy ... before you do! And some of us even think that J.-P. Dupuy is a better interpreter of Varela's self-reference calculus than most american scientists [1].

In am just reading, for instance, your paper in the *Interfaces* [2] and I consider that we, in France at least, were making exactly the same basic diagnostics in 1978. Robert Faure presented roughly the same arguments as Gene Woolsey and I was presenting the same view as Ackoff, and the discussions lead roughly to your own perspective [3]. In practice, it was our 1979 OR Congress on "Small Groups and Large Systems" which was the practical answer.

And as we (in France) feel "in roughly the same terms" about the same problems and the same project, we are less keen about outside exchanges. It is so difficult for us, in practice, to express ourselves in good English. We *now* have enough opportunities to publish directly in French: Have you heard about the tri-lingual German review of "Analyse de Systeme Appliquée", partially managed by Jacques Lesourne? At least two new French magazines (inspired by Stewart Brandt's *Co-Evolution*) have started in June 1980. The various AFCET colloquia and congresses give us enough opportunities to interact — the openess is perceived as urgent only when we don't find enough opportunities 'at home'!

This is to say that I presume that part of HSM's 'international problem' is also a logistical one: the translation problem. I need approximately 2500 FF to have a paper correctly translated and typed for HSM, together with various day-to-day practical operational problems. I can try to find a good bi-lingual student — but when and with what delays? In practice, as you see, I lose the final motivation!

In my personal situation, since I agreed to be active in HSM, I presume that I should find the motivation to stand up to the task. But what about the others? I presume that the silence of Y. Barel, J. Mélèse, J.-P. Dupuy, and so many others, have the same origin: as we have immediate and relatively cheap publication solutions available, we postpone the 'american' one. Let me add that, for most of us, the aim of life is not to be quoted in an american paper!

I am able to formulate a part of the problem as I see it now, but I am not able to suggest any practical solution for the time being. We probably should live with it for many years to come.

I too, however, remain optimistic. I congratulate you on a beautiful set of articles we have now in our HSM portfolio. I shall do my best to convince, again and again, my French-speaking colleagues and friends to put their best writings into good English!

Jean-Louis LE MOIGNE

[1] In "Flaws of Form", Internat. J. General Systems 5 (1979) 201–211, Paul Cull and William Frank claim that Brown and Varela have merely reinvented Boolean algebra but in an obscure notation; their 'solutions' to the problems of self-reference are based on a misunderstanding of Russell's paradox.
[2] M. Zeleny, The last mohicans of OR: or, it might be in the 'genes', Interfaces 9 (1979) 135–141.
[3] E. Heurgon, Editor, L'avenir dela Recherche Opérationnelle – Pratiques et Controverses, Monographie de l'AFCET (Hommes et Techniques, Susernes, 1979) 19–58.