

**Replies to reviews of vol two of the Tavistock Anthology by Passmore and Scarbrough (HR, 1995).
Contrary Perspectives of Socio-technical Theory.**

F. Emery, April 1995

Scarbrough

There would seem to be something archaic about British social science if, as Scarbrough asserts, the very mention of 'a socio-technical system' 'still prompts a frisson of recognition from those who have not encountered it before'. (p24). The concept of socio-technical systems has been around for almost as many years as the concept of cybernetics, and I doubt that Scarbrough would claim that the mere mention of cybernetics causes "shudders, quivers, chills or tingles" (Webster's, 1961, definition of frisson. Not given in O.E.D.). The article by Trist and Bamforth (1951), which introduced the concept, and explored its implications at length, has been reproduced so frequently in books of readings that I have long since lost count. The last edition of Eijnatten's bibliography, with two supplements, of English language articles and books about socio-technical systems, 1994, listed 2,920 items: and the number of contributions has increased enormously.

I had my own 'frisson of recognition' when Scarbrough proceeded immediately to discuss the concept of socio-technical systems in terms of its 'spoons'. To my limited knowledge spoons are used to characterize species that are rarely seen or are extinct. It was seemingly unknown or irrelevant to Scarbrough that these were not spoons left by socio-technical theory but the spoons of competing systems that sought to occupy the same niche without disturbing the basic authoritarian structures of industry ;like their now forgotten predecessors of 'individual job enrichment' and 'quality circles'. The plateau in the eighties, which is shown on the following graph, appears to reflect the 'miracle' of Japanese. I was not mistaken in my frisson of recognition. Scarbrough was indeed afflicted with the English academic love of Bishop Berkeley. In the very next paragraph I am informed that socio-technical theory is "at best stalled, at worst moribund". Moribund means 'dead or dying'. No wonder that we are supposed to need a study of its spoons.

A dying theory would attract less and less scientific attention. The Eijnatten bibliography shows an increasing number of references (in the English language) to socio-technical theory. This fact flatly contradicts Scarbrough's basic premise.

-insert graph-

Far from being moribund socio-technical theory, after a slow start, is very much alive and growing exponentially. In the nineteen sixties management, union officials, communists and academics assumed that industrial efficiency could be achieved only by strict and narrow individual job specification and close supervision. Nowadays one will not be taken seriously unless one assumes that work will be done by more-or-less self managing groups of multi-skilled individuals. That is quite a change, given that industrialisation was built on the opposite assumptions.

In the rest of the review Scarbrough devotes himself to the task of explaining why sociotechnical theory necessarily became 'moribund'. Arguments that prove the existence of non-existent facts are crooked arguments. That is certainly true in this case. The only point worth comment is his additional argument to the effect that 'if sociotechnical theory is not yet quite dead then the new information technology will certainly kill it'. That is worth comment as the Pava paper and Trist's epilogue for the Pava book are both included in this volume and lend weight to Scarbrough's argument. Both Pava and Trist were so mired in North American academia, and its demand that a Ph D student be seen to do something original, that they overlooked the last two field experiments in the Norwegian program. At both the pulp department at Hunsfoss and the new fertiliser plant at Porsgrun the major task was with the processing and communication of information. The new Teesport refinery was also overlooked although it was Shell's first venture

into a computer controlled refinery. Those examples were very successful examples of what our theory enabled us to design in the sixties. There was no hang-up about instrumentation, automation and computerization because we had long regarded those as the challenges we had to meet (Emery, 1959; Emery and Marek, 1962). Quite early we realized that the critical matter was the location of decisions about control and coordination. Analysis of the technology guided us to where those decisions had to be made but could not tell us where those decisions had to be made in the social system. For high level non-routine tasks we recognized that full multi-skilling was not feasible so we designed for self-managing groups where this was not a prerequisite, and tested the design in practice. We, Merrelyn Emery and myself, published that extension of socio-technical theory in early 1974; having satisfied ourselves and our Australian colleagues that it worked in practice. That extension (in this volume pp599-613) was presented in the context of arguing for participative ST designs. Participative designing assumed a degree of mutual trust between management and the workforce that was not easily envisaged in the USA. I presume that that was why Trist and Pava ignored what was before their eyes. That is just the sort of contradiction that a conscientious independent reviewer should pick up. It was not an excuse to weave another story.

The above explains why Scarborough's review is so peculiar. His sense of proceeding from such a sure assumption might also explain his off-handed treatment of the book he was reviewing and his own review. He twice confuses the original Haighmoor, South Yorkshire, study with the later Durham studies (p23): he quotes me as the editor who wrote the introduction (It was Trist. As far as I can recall I had nothing to do with the editing of that volume). He interprets that quote as an admission of "intellectual marginality". Trist was doing nothing of the sort; he was referring to the mainstream of managers and trade unionists (pp55-56). Like me, Trist felt that most of the academic contributions to organizational theory were much more marginal to that mainstream than was sociotechnical theory. A certain casualness might be read into the fact that two of the references he gives in the text do not appear in his list of references.

What I have said above does not explain why Scarbrough was invited to review this volume. Independence is valueable and even some ignorance is tolerable provided the reviewer is prepared to study what he/she is asked to review. Scarbrough is certainly ignorant of the field. Evidence of this is the above graph, the fact that he has not qualified for a single entry in Einjatten's exhaustive bibliography and that he still regards Silverman (1970) as a reliable judge of systems theory. What is unforgivable in a reviewer is pre-judgement. Scarbrough did not need to read the volume he was asked to review because he already had very firm ideas on all the matters it could be expected, by him, to refer to.

Appendix.

Nothing in Scarbrough's reasons for the moribund state of sociotechnical theory can be given credence but they do give a lead to the thinking prevalent in Warwick and other such British universities. I am assuming, in what follows, that Scarbrough's peculiar review of Vol II reflects prevalent views in his sub-culture.

1. "...the socio-technical perspective stands indicted of reifying complex social interactions and neglecting human agency" (Silverman, 1970). How do we stand with regard to this criminal charge? First, reification is a matter of explaining observed behaviour by reference to the inherent nature of the object. We were amongst the first social scientists to reject that Aristotelean mode of explanation. Second, Trist and Bamforth dealt at length with human agency in response to the conditions of long-wall coal mining. In "Characteristics of Socio-technical Systems"(1959) I specifically dealt with that dimension and formulated preliminary hypotheses. In 1964 we had advanced to the point where we could specify the six human requirements of a job (what they should afford to people). Silverman's judgments in his Ph D thesis, although under the supervision of Tom Burns, show total ignorance of these developments. His references to Rice and Miller are

quite misleading. We separated from those people in 1961 because we had deep theoretical disagreements about just these matters. When Silverman published in 1970, these developments were public knowledge, readily available to him and his supervisor. That Silverman could be quoted so confidently in 1995 can only be put down to the readiness of British sociologists to accept Silverman's claim to have killed the systems dragon and restored sociology to the sociologists. That done, one does not have to think about systems; one just quotes Silverman's juvenile indictment.

2. "...the assumption of consensual organizational goals..".

That 'assumption' exists only in the eyes of those academics who sought to demonize the Tavistock Institute. We worked with both unions and management to convince them that there were win-win possibilities in just about any workplace. These possibilities concerned workers' alienation from their activity. We always made it explicit that these measures did not effect the alienation of workers from the product of their activity. The latter was a matter for so-called economic democracy and that was a matter with which we were never involved. In case that explanation is not understood let me say that we never assumed that what we were doing ensured consensus in the labour market.

3. "...their theoretical tools left them ill-equipped to explain the dynamics of such bureaucracies" (p25). On the contrary our conceptual tools, and our practice of getting involved with re-designing bureaucracies, private as well as government, enabled us to get to the genotypical features of bureaucracy (Emery & Emery, this volume, pp599-613). It is disgraceful but throughout this century academic sociology, in its 'critical, non-interventionist mode', has not advanced one whit from Weber's phenotypical description of the Prussian bureaucracy. It would have been irresponsible of us to have fallen back on that 'theoretical heritage'.

Scarbrough's comments on open system theory and action research would not have arisen if he had read the volume he was reviewing. It would appear that the only thing which could match Scarbrough's ignorance is his arrogance.

Dear Bill,

10 May 95.

My copy of the Jan. issue of Human Relations arrived only last week (sea mail to Australia was just as fast in the age of sail and only six weeks in the age of the mail steamers).

Your response to Scarbrough's review of Vol 2 was probably pretty much like mine. Enclosed is my review of his review. The interesting part is page two, the graph I made of Eijnatten's exhaustive review of STS literature. My feelings are expressed in the last sentence of the last page.

I will send this to the new editor of HR. My guess is that he deliberately chose Scarbrough as a foil to your very positive review. I will ask him for space for a temperate reply to Scarbrough.

Passmore.

There are some matters I would like to discuss but they are not at all the sort of matters that divide me off from Scarbrough.

1. PD & SC. They share a common philosophy of learning and both proceed from design principle two but are quite different methodologies, with quite different, although complementary, purposes. The employment of one of those methodologies does not necessarily entail employment of the other. When both are used the order depends on circumstances. They involve quite different criteria for selection of participants, and different group processes.

2. The persistence of the nine-step model in Lou Davis' sphere of influence is for the same reason as Davis sought to go under the label of QWL. (p 614. Vol.2). American corporations

wanted to minimize employee involvement and Davis wanted to calm corporate fears in order to get a hearing (even if it was not in their longer term interest).

3. Legislation. The comments on this seem to have missed my point that this would have to be a different kind of law - educational and without policing or punitive sanctions (p617, vol.2).

4. Eric's pessimism (yours, p16). You confirm what I had heard from a couple of other persons who were close to Eric. It reflects his personal position in the States since 1966. As I pointed out in the obituary he found no institutional base to give him leverage, Davis and Ackoff denied him this and at York Uni he was an ancillary. We others had no cause for pessimism (I wish that before he died Eric could have seen my Turkish paper and the graph of Eijnatten's bibliography).

Those are just odds and ends. More serious discussion should take place about views of the future. We are all guessing about the future. In writing about what is useless baggage you make three points:

- a) problems with wide acceptance.
- b) length and costliness of intervention
- c) the art of group dynamics.

I think that these three matters could be considered as three facets of the strategic problem.

The strategic problem has been around from the early days, although much more clearly spelt out over the years. The document "On some characteristics of socio-technical systems" (Feb 1959) summed up what we had learnt from our studies and what could be gleaned from the general field of industrial sociology and psychology. It was our 'bible' through the sixties. In that I took care to point out that STS only addressed matters that arose in the workplace from 'the alienation of people' from their activities ; it created conditions for pride in the quality and cost of the products and services created but did not address any of the general problems associated with 'alienation from the product'. This was easy for us as we had seen first-hand that the nationalization of the British coal mines had not in any way improved conditions at the coalface.

The pursuit of democracy in the workplace and 'economic democracy' are two very different things. In Europe the critics of STS have accused us of avoiding the big problem of economic democracy and in North America they have accused us to aiming at solving the big problem by indirect means! At the enterprise level there is currently little or no overlap. At the national level, and over a longer time-span, it does seem to me that there must be considerable overlap. On that scale the two processes still do not become one process but both contribute to the cultural transformation of society that both need.

Whatever the reason for opposing STS solutions the tactics of resistance have had to change with the general recognition that workforces, at both enterprise and national levels, have to be based on self-managing teams of multi-skilled workers. Over investment in training for multi-skilling and expert analysis of socio-technical systems have been emerging as the favored tactics. These over-investments 'prove' that management are taking the challenge seriously even when they know themselves that these over-investments are self-defeating. The boards of the corporations get a picture of a conscientious and progressive management and the workforce get the message that they are still just tools. Just in case in the later have any illusions they find that when organized into teams they are responsible to a 'team leader' and the team leader, in the last analysis, is the one who is responsible to management for team performance. The realities of supervisory control and consequent de-skilling on the job have not changed for the workforce.

You refer to the social dynamics of groups as a frontier for STS. You clearly mean a frontier for social scientists. I could not agree less. In no time social science would, once again, get a reputation for manipulating team membership for managerial purposes. Legitimate managerial purposes are adequately catered for when teams are allowed to accept or dismiss individuals solely for their ability and willingness to contribute to the negotiated group tasks (not for gender, ethnic

origin etc). People are good at working out whom they can work with. When social scientists think they can do better I suggest that they first prove their case on academic social science departments.

"What must we still invent?"

The reviewer puts his final points under this heading. Up to this point I thought I was discussing matters of difference with a reviewer who had a long and thorough knowledge of the field. However, I cannot see how his final points follow from his review.

First I would like to remind the reviewer that the Emery and Trist who published 'Causal texture of organizational environments' were the same Emery and Trist who contributed to the emergence of socio-technical

We did have enough sense not to reduce environments to the lower level of the socio-technical analysis of organizations. Perhaps this caused some trouble for the reviewer but we did point out that different level of causal texture favoured different kinds of individual learning. At no point that I can recall did we ever fall for the fallacy that organizations can learn. Organizations may provide a habitat for individual learning and provide circumstances whereby those learnings might be given attention but it cannot even be said that they learn to do this. The reviewer unwittingly admits this in his "Final Thought" when he describes a visit to the new Tavistock.

Second there is the tiresome question of STS and 'nonroutine work'. Certainly I tire of answering this question. In 1974 Merrelyn Emery published a clear answer to this question after work had been successfully done on the redesign of top management teams and of professionals engaged in R & D groups. Thorsrud and I re-published this answer in our English language report on the critical Norwegian experiments so that there is little excuse for serious scholars of STS missing the contribution.

Last, I find it difficult to understand why a reviewer who teaches STS in a university should be criticizing the founders of STS for not discussing such matters as "The socio-technical foundations of a new social order". It is possible that he needs something more suited to the US market?

Regarding the future is not the problem that of finding new ways to manage a totally different workforce? The MBA courses offered by universities do not even recognize that there is such a problem.