VOL3 ; THE SOCIO-ECOLOGICAL PERSPECTIVE.

Note by ME. This is original of introduction to Vol III of Tavvy anthology which normally I would not include here. However, this draft contains notes at the end which are not available elsewhere.

INTRODUCTION.

Tavistock's socio-technical perspective was, as it were, born over-night. It was born when Trist and Bamforth published their case study of "Some social and psychological consequences of the longwall method of coal-getting" (1951, Vol 2, chapter). The questions that that paper tackled had been raised for Eric Trist in the latter stages of the Glacier Metals study (Jaques, 1951) but had been shunted aside by the urgency to publish the first major project of the newly created Tavistock Institute of Human Relations. The case study was very much Eric Trist's own baby. A critical review of that study and the empirical studies that flowed from it was made eight years later (Emery, 1959, Vol 2, chapter). It was found that the initial study, despite or because it was only a case study (Chein, 19), had created most of the conceptual framework required for the analysis and design of socio-technical systems. The only major lack in this conceptual framework was the explicit identification of the values or requirements that people bring with them into the workplace. Implicitly it laid the major stress on personal autonomy and self-determination when the major body of professional writing could see no further than the apparent boredom of industrial work and the need for variety (Baldamus, 19).

The socio-ecological perspective had no such dramatic birth.

From 1969 through the early sixties a series of overlapping case studies convinced some of us that our conceptual framework was inadequate. (It did not concern some of the senior members of the Tavistock who felt that our existing conceptual framework was quite adequate to a consulting practice based on helping CEO's deal with interpersonal relations in senior management. See pp , this volume). These case studies involved us in helping very some large organizations, with international markets, re-define their missions. Our basic conceptual model for understanding organization-environment relations was that of Bertalanffy's open system (Bertalanffy, 1950). In 1952, when we adopted the so-called open system model, without dissent, it clearly met our then current problems with understanding labour turnover, and our emerging problems with socio-technical systems. It was a relief from the closed system model that had dominated organizational thinking because of its close approximation to the logic of isolates previously espoused by the experimental physical sciences. (Emery and Trist, 1959; Levy, 193). Looking back, it seems that the closed system model was not unduely constraining on the practices of the social sciences as long as they were studying individuals, institutions and cultures where the environment could be taken as relatively unchanging. At the theoretical level it contributed to an unfortunate schism between the academic disciplines as each theorized about its own closed systems.

Some of the case studies with which the Tavistock Institute were engaged after 1958 could not be handled within Bertalanffy's open system model. That model did not allow for the specification of an environment whose own evolution helped determine the terms of exchange between it and the organization in question. In each of the cases with which we were engaged it was clear that the environment was evolving in meaningful ways and changing the context within which those organizations could hope to hold their positions, let alone grow. Discussions of this conceptual challenge were not limited, within Tavistock, to the persons directly engaged with the projects nor to any particular disciplinary background. Nor did the discussions proceed without internal challenge. Some felt that the Tavistock should take as its

primary task the financially secure route of applying well-established knowledge from anthropology, sociology and psychoanalysis. They argued that 'frontier research', as in the case of socio-technical theory through the fifties, cost the Institute more than it was worth. However, the discussions flowed outward from the Tavistock to engage, and, in fact, largely inspire the formation of the "Informal European Group". Two "Progress reports on conceptualization" (Emery, 196 ,196) were specially prepared and circulated before two of the nine-monthly meetings of this group.

 [Footnote. The informal group emerged at the instigation of Jaap Kookebahke, the 'old man' of Dutch social psychology. He thought it inadequate that we just got together when we found ourselves at the same international conferences. So we got into the habit of meeting about every nine months at a place in Europe of our own choosing. For about three days and two nights we would argue about newly emerging concepts, methods and theories. We argued, but had no need to seek unanimity. We also ruled, very strictly, that business was not to be done at those meetings. Several people who tried to do business were not invited any more. The 'we' were about a dozen social scientists working in Europe, including two or three from the Tavistock. The group continued meeting into the early seventies; by which time it was defensively referring to itself as 'the non-existent informal European group'.]

The conceptual steps from closed systems to open systems and to ecological representation can be set forth quite simply.

Let L stand for some lawful, testable proposition, the suffix 1 to some component of the system and the suffix 2 to some part of the environment of that system. Then the focus of the different system models is as follows:-

          a) closed system L_{11}

          b) open system L_{11} L_{12} L_{21}

          c) ecological L_{11} L_{12} L_{22} L_{21}

The Extended Social Field and its Informational Structure.
        "No man is unto himself an island" Donne.

             Fred Emery and Merrelyn Emery.

The concepts of 'personality' and 'social structure' have come to represent the higher levels of theorizing in psychology and sociology respectively. It has always been clear that a very broad gap existed between these concepts. Unless, or until, this gap was bridged a great many socio-psychological phenomena were left in a conceptual no-man's land. Such phenomena included the so-called collective behaviors (mob behavior, fads and fashions), cultural phenomena and the characteristics of linguistic communities.

Early attempts to bridge this gap postulated a group mind. The concept of a group mind was short-lived. As a concept it drew together the phenomena to be explained but assumed an entity that was itself inexplicable. No one could suggest how an entity such as a group mind was to be scientifically validated, that is, how it could be proven to have an existence independent of the phenomena it was supposed to explain.

Since that failure in the early twenties psychology and sociology have gone their own ways; the former to regard the social nature of human beings as a secondary feature to be explained, eventually, by general laws about biological organisms; the latter to regard psychological phenomena as essentially epiphenomena generated by sociological processes. (Asch, 1952 and 1959, Trist, 1952). Even up to the present day there has been no change if we are to judge from a recent, and approving report on "Current trends in social psychology" (Argyle, 1994)

This paper suggests that there is a scientifically acceptable referent to perform the task that a group mind was supposed to do. Two steps are required. First, it is necessary to follow Asch (1952) in establishing the social nature of human beings. Second, it is necessary to invoke Sommerhoff's (1950) concept of directive correlation to explain the individual"s essential role in non-random social behaviors.

Asch's contribution was to identify the properties that arise in a situation where two people (A and B) enter into relations to each other with respect to some object or event (X) in which they are both interested. Invoking the concept of interest implies no more than Trotter (1916) postulated in his "Instincts of the Herd in Peace and War", namely, that A and B are of the same species and hence are

a) specially sensitive to the behavior of their fellows, and b) they will tend to resist separation from their fellows. Interest does not necessarily imply anything about physiological drives.

A time ordered series of four properties emerge within an ABX setting:-

1. The ABX setting presents an objectively ordered field open to both participants. (Emery, Chapter 10.herein) Each can see what X affords to them and what X affords to the other.

2. The mutual confrontation of A and B, vis-a-vis X, attests to their basic psychological similarity. Each comes to the situation with intentions, attitudes and beliefs that can be known by the other. In particular, each becomes aware of the other as a potential agent of change in the field.

3. Stages 1 and 2 lead to the emergence of a mutually shared psychological field. A's actions in the context of this shared field are the context of B's actions, and vice versa. In this mutual representation of one's own and of the other's orientation to the situation we have, as Asch stresses (1952, pp161-162), the primary social fact. It is only on this fact that we are able to establish persisting social relations of coordination and control.

4. Within mutually shared psychological fields the individual psychological systems more clearly take on the characteristics of an "open system". To achieve their ends with respect to X both A and B have to co-relate their behavior to the behavior of the other; they have each to consider where they start from, what each can do and where each wants to end up. In Sommerhoff's terms the behaviors of A and B have to become directively correlated if either, or both, are to achieve their goals with respect to X.

We have till now taken the simplest case of ABX. There is no problem in principle of extending the concept of directive correlation to ABCX or AB..nX. A field of directive correlations does not have the unitary perceptual qualities of a thing such as human being or an engine. None the less such a field is not an empty abstraction such as a group mind. A field of directive correlations can be specified with respect to the coenetic (starting) conditions, the focal condition in the processes that leads to their convergence over time and the outcomes of the processes that were set in motion by the coenetic conditions. This is not a summation of separate cause-effect chains as it involves the joint effect of processes that respond to the coenetic conditions according to their own laws. But, as Sommerhoff has demonstrated, such convergent, goal-directed sets of processes are typical of all living systems, and they can not only be specified but they can be measured and manipulated. The group mind concept failed to meet these requirements.

The increased openness of individuals that occurs in a mutually shared field is critical for what we will argue therefore we should be clear about what it entails. By greater openness we shall mean that the individual is responsive to a greater range of coenetic variables. The individual's range (repertoire) of responses is better treated as a measure of sensitivity than of openness, and the range of goals and sub- goals as a measure of system complexity. Thus a system might be very open but relatively insensitive and single-minded or relatively closed but highly sensitive, and complex because of the range of goals being served.

Beulah,25/4/95. The Jan issue of HR arrived Monday, only hours after I had posted back the Vol III materials. Reading Passmore's and Scarbrough's reviews of Vol II I can now see why Hugh was concerned to have a respectable paper about the Australian work scene. (I assume that Hugh received the Jan issue of HR in January!)

Passmore is generally informative and seemed to have read a lot of the volume. He is quite scatty about the future. Scarbrough is scurrilous throughout, and seems to have lost his reading glasses. He reminds me of Woodward.

Fortunately Scarbrough starts from the assumption that socio-technical theory is a failed intellectual project. The rest of his article gives his reasons for why it had to fail. If it is demonstrated that the theory has not failed, but made significant progress then his whole critique collapses.

My paper on the Australian experience punctures his balloon. It was published, at their request, by a refereed Australian academic journal (one that gets to the research officers of employer organizations, trade unions and relevant government departments). It is still my view but the refereeing insures that it does not involve falsehoods. It is a short article. Published as the first paper in part V it would be the most effective answer to the Scarbrough's. (I suspect that the new editor of HR is one of those as he did not hold Scarbrough to academic standards). Having this paper in the series makes it readily available to those who consult the series. Having it as a minor paper in part V is wholly appropriate. It does not detract from the vision and scope of Eric and Howard's article: it does assert the relevance of our claim, often repeated in Vol's II & III, that radical change in the workplace is the key to controlling our future.

Sorry that this comes at one minute to twelve but sea-mailing of Human Relations has its disadvantages.

If the article was to be used the title should probably be changed. A suggestion follows:-
Steps to the Future Workplace: the Australian experience.

An introduction, however brief, would be required to part VI. I suggest the following.
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" It is probably less perilous to envisage the future than to extrapolate it. When we extrapolate a future we do so from a more or less solid base of what has been. Unfortunately we are rarely informed about the assumptions built into the apparent solidity of the past. This is the plague that affects our econometric models. When we attempt to envisage the future we have to proceed from some consciously formulated assumptions. Other people can inspect those assumptions and judge those assumptions as well as judge the steps we take to envisage a future. The perils are reduced by being shared with others who can bring their different experiences to bear on both the assumptions and the derivations. The situation invites the sharing of learnings and does not proceed from an assumption of some 'handed-down' authority. I think this fairly describes the attempt by Trist and Perlmutter to envisage the future. Together they represent a vast body of experience but they do not use this to claim special standing. They use their experience to widen the horizon of their readers and to invite broader questions.

The first paper, by myself, is much narrower in its focus. It is concerned with workplace reform as a critical key to unlocking the doors and gaining access to alternative institutional futures. This is a concern that binds together volumes II & III and can be seen emerging in Volume I. I have chosen the Australian example because it is one of the nations in the forefront of this change and, more importantly, it is one that I know first-hand. This latter point cannot be stressed too much. These sort of changes are not first debated in academic

journals and, when that debate is resolved, then handed down to managers and trade unionists via MBA courses the likes of the Harvard Business Review. The changes are tested and diffused in the bowels of the enterprises in collaboration with marginalized social scientists who are dedicated to the 'social engagement of the social sciences". This collaborative role is not to be confused with either the academic role or that of the consultant (Churchman and Emery, Vol. III). As was the case with the spread of Taylorism the academic discussion tends to come later, if at all.