Dr. David Wadley 10/12/87

Dear David,
Thank you for informing me of this developing situation. Thank you also for enclosing a copy of Jim Walmsley's letter.

It is good to know that there are others out there who feel "frustrated and angry" when a good idea is lost in the pursuit of the 2x2 table. For the teacher or the consultant the 2x2 is perfect. It requires only that they remember that this goes with that, but does not go with the other. Without a third variable there is no theory, no explanation, no suggestion of dynamics. (see enclosed notes).

Before I try to offer any advice let me review the broader picture because Stubbart is only the latest of a long line of trivialisers, and will not be the last. I will number my points for ease of correspondence, not because there is rigor. I will of course try to develop some order, but no promises.

1. A lot of the trouble has arisen from the fact that commentators have taken the 1965 paper in Human Relations as the source paper. It was not. It was a simplified version of "second progress report on conceptualization", F.Emery, Doc. T125, Tavistock Institute of Human Relations, March 1963. (the first progress report explored the limits of Bertalanffy's concept of open system, i.e., L11 + a 'transport equation for L12, L21, and concluded that that concept, and General System Theory, bogged down in the insoluble problem of trying to represent boundaries, dynamic relationships, as if they were substances, like walls, fences and skins. The reports, by the way were to the Informal European Group, which subsequently, because of boundary problems, labeled itself the Non-existent Informal European Group: they were not reports to the Tavistock). The source was a publicly available document but was not mentioned in the 1965 paper for a very simple reason. My chairman had been invited to give a paper in Washington; he did not have time to start from scratch and was much taken with the ideas in my document. However, the detailed arguments in the source document were published in the same journal just two years later, "The next thirty years: concepts, methods and anticipations." Human Relations, 20, 199-237. It would not have been a major feat in scholarly research to have discovered this article if one was really interested in the ideas in the 1965 paper.
2. The simplification of the '65 paper encouraged distortion in two ways:
   a) it failed to hammer home that the critical innovation was the introduction of the L22. In the source paper it was made explicit that this was the step beyond Bertalanffy and GST and it was claimed that what this meant was that "we cannot characterize a system unless we can characterize its environment". Because of my interests at the time I did not spell out the other implication, namely, that "we cannot characterize an environment unless we can characterize the organisms (organizations) that inhibit it." It was made very clear, as it was in the referenced papers by Schutzenberger, Simon, and Toda, that we were granting to the L22 the same epistemological status as granted to the L11, namely an autonomous, objective reality. It was unfortunate that this stress was missing as only one of the four world hypotheses that Pepper identified allow this of the

L22, and that is the contextualist hypothesis, which is not all that prevalent in academia. This has allowed people to assert that all we are talking about is a state of mind of executives. A chap writing in Futures in 1981-2 asserted this on the strength of an address that Eric Trist had given to a USA management conference, (Futures, 1980) and added that it was a good theory for scaring managers into using consultants. Eric asked me to join him in replying to this fellow but after reading Eric's paper I declined. We could of course argued that the chap should have gone back to the original papers, but then he could have come back and asked why Eric did not.

b) It put verbal labels on what had been a numbered series. I was strongly opposed to this change but Eric insisted that it was necessary if the ideas were to be got across in a verbal address. As it happened that did not work. Warren Bennis who chaired the meeting at which Eric delivered the address felt compelled to write to us, some three months later, to apologise for having failed to direct discussion appropriately – it had taken him that length of time to realize the radical nature of what we were suggesting. Yet Warren was seen as being at the forefront of organizational thinking, and a friend of the Tavvy. My reason for wanting to hang onto the numbered series was that it was a SERIAL GENETIC concept not a generic concept (Aristotlean classification of substances). I did not start from two variables that I thought to be of great significance and then develop names for the cells in the table. I started from a detailed analysis of environments and their historical evolution. There was no built-in assumption that the series would stop at 3,4,5,7 or whatever. In fact, from the mid-sixties there was a definite hunch that a type V environment could be specified. (footnote, 1967 paper). How on earth could someone think of encompassing this in a two variable, 2x2 table?

The labels themselves have given us some trouble. Once I had accepted Eric's demands we worked hard at finding appropriate labels. No matter what we did any such verbal labels would land us with connotations that were alien to the phenomena we were dealing with in a way which would not arise if we could have gone on simply referring to the series by number, a series that retained the essential character of a continuum (e.g. allowing for type 2.3 or 3.2), and did not exclude the possibility that the series might turn back on itself, as in fact type 5 turns back to type 1 at a higher level.

The use of the term placid is a first example. We were trying to express the fact that those environments are quite indifferent to the systems living within it. The usual use of the term connates a benevolent indifference. In type 1 environments I wished to suggest no assumption of benevolence e.g. the example of the concentration camp. In type 2 environments it is quite in order to assume benevolence or indifference.

The use of the term turbulence is the prime case of what Whitehead would have called 'misplaced concreteness'. It was, I am sure, the use of that term that made the 1965 article a 'citation classic'. It was, I am sure, that the term effectively short-circuited thinking about the theory I was advancing.

When I went to the Uni of Penn in 1982 I found that Eric had been filling his students up with a story of how we arrived at the ‘concept’ of turbulence (actually no more than a label)- ‘we got the notion as our plane was coming down through turbulent air’. It was actually a little more serious than that. I was concerned with the problem of what military structures could survive the radical transformation of a battlefield with tactical nuclear weapons and, at the same time, with how scientists were trying to find rules governing the development of turbulence in fluid flows. Turbulence carries the additional connation of chaos. That is a connation I would have wished to reserve for type 5 environments, if we had done enough study of that at the time. All that I wished to convey with the concept of a type 4 environment is that the L22 had to be taken into account because its autonomous actions were becoming part of even the short-term considerations of systems in that environment. The type 4 environment does not offer much latitude to the individual system (contra to Terrebury’s otherwise fine restatement) but it does offer room for adaptation. Chaos, type 5, offers only the luck of finding a safe cave. It is possibly of passing interest that when I arrived at Penn in 1982 there were two mature aged Ph.D students working under Eric on the question of a type 5 environment, McCann and Selsky, “Hyperturbulence and the emergence of type 5 environments”. The Academy of Management Review. July 1984.9.460-471. They were locked into failure to adapt to a type 4 environment and simply could not even begin to envisage a type 5 environment. I think that this was because they had been brought up on the labels and hence thought that chaos was part of its nature. At the same place and time a lonely Turkish gentleman (remember that the Saracens had a better record as gentlemen than the Crusaders) was confronting type 5 (historical examples of type 5 tend to be concentrated between the Bosphorus and the Euphrates). His thesis makes a nonsense of the 2x2 boyo’s. (Baburoglu, O.N., “A theory of stalemented social systems and vertical organizational environments”. Uni.Microfilms International, 1987. 8714004).

3. The Miles’ diagram that Stubbart uses distorts the fact that 2 is larger and includes 1. L22 is larger and includes L11. Taken literally Miles’ diagram would suggest that the focal organization had only to relate itself to the B organizations. The so-called corporate planning departments of Anheuser-Busch and other such corporations have been set up to do this. The tactic corresponds to Ackoff’s view of the future as the future of the corporations (“Redesigning the Future”). I did not think that way. I have always held that the relation between organisms (organizations) and their environments could not be treated as a part-part relation within a larger system. The O-E relation is not reducible to just another system problem. The O-E relation is intransitive; the E encloses the O, not vice versa. It encloses the O in both time and space. In speaking of the O’s we must expect to speak of classes of O’s, not a particular O, unless it has a monopoly of its niche.

In speaking of E enclosing O's over time the implication is that O's can only develop in ways determined by E. I only intend to say that one can always find a time span long enough to prove that this is so. In shorter runs O's might determine E. What is happening at any given time is always a matter for empirical determination.

I think that that might be enough of generalities.

It should be obvious that I have no sympathy with what Stubbard has attempted. If the environmental problems could be reduced to problems of complexity and rate of change we would have no problem that could not be solved by additional investment in information technology. I have been trying to say something different. Relevant Uncertainty is increasing at a rate that cannot be matched by any amount of computer modelling. We do not know what to put in the model. My suggestion was that in the face of increasing relevant uncertainty it was necessary for us to define our world, the world that we would accept as our operational sphere, in terms