

Epilogue

Orillia: Searching for the Emerging Agenda

In 1985 the city of Orillia, 100 miles north of Toronto, was the site of a three-day conference to explore the future courses of a continuing engagement of the social sciences with the important practical affairs of mankind. The occasion was supported by a Canadian government department, Labour Canada, and brought together an international group of social scientists to honor, in a practical way, the lifetime that Emeritus Professor Eric Trist had devoted to pursuing this engagement. The conference was designed to be in the search mode (sort of), but Eric Trist was asked for his recollections of the historic 1949 Gerrards Cross (U.K.) meeting between Lewin's group from the University of Michigan's Research Center for Group Dynamics and the Tavistock Institute, and Fred Emery was asked to outline some of his thoughts (see the next two sections of this essay).

Present at that two-week meeting in 1949 were several other like-minded scientists from Europe and the United States. The meeting was held in order to set an international agenda for the social engagement of social science in the world emerging from World War II. It was the forerunner of Orillia. Unfortunately, although Eric Trist could not know this when he gave his recollections on the first night at Orillia, it was a forerunner in more ways than one. The difference this time was that it was not "scientific detachment" that was under the gun but "scientific action research," from those wanting less constrained forms of social engagement. They stressed the need for more feeling and free-wheeling imagination.

Not surprisingly, no agreement could be reached at Orillia about a future agenda for scientific action research.

The Last Time Around, 1949—Eric Trist

We held the conference at an old inn in Gerrards Cross near London in 1949. It was a setting of great beauty. The people were largely from the United States—mainly in group dynamics—and ourselves, the Tavistock group. We also had some very distinguished guests from Europe, particularly from France, and one from Czechoslovakia who was very important to us as he had suffered under Stalin and we had arranged for him to come to Britain.

This was the age when group dynamics first came into social science. At the conference we had this mixture. We had very different backgrounds but we all knew the work of Kurt Lewin, who had recently died in the United States. (The conference had originally been arranged for 1947 when he was to come to England.) Lewin was very excited by many of the things the Tavistock was doing and about developments among British psychoanalysts as they became interested in field theory and were attracted to exploring the social scene.

If that visit had come off in 1947, I think things would have happened between him and the Tavistock group. We had come out of the war with a background in socio-clinical psychiatry, social science and operational action research projects in the British Army. The Americans had done a lot of work on food habits and other subjects at the Commission for Community Relations in New York, out of which had come the term “action research” just after the war. Everything seemed right for the two groups to get together and to establish a trans-Atlantic bridge. We wanted a European connection, too, with people who wished to do something in the real world that involved groups—then a very new idea.

So we thought that even after Lewin’s death we would still have a meeting and it was arranged at Gerrards Cross. But the key person, the integrator, was dead and that was really why this meeting did not spark the kind of relations we had hoped for between our organizations on both side of the Atlantic.

By that time Lewin’s main pupils had separated into two branches, one had gone to set up the Research Center for Group Dynamics at Ann Arbor, which was the group that came over, and the other, the psychological ecologists, had gone to Lawrence, Kansas. We didn’t know enough about the split or we might have invited the other group. We didn’t know, even though we had had visits from them, that we and the Ann Arbor people were going to go on separate roads. We discovered this when we met at the conference.

It was an intense meeting lasting two weeks. Beulah and I planned it. There was no work in the afternoons. After lunch everybody had to play a game (cricket, tennis or golf) or ride a bike. Everybody had to do something and not work all the time. One of the Americans, Jack French, was very good at cricket and took on the local club. Work resumed after tea. Some of our best meetings occurred after this break.

What transpired was that our crowd in the UK were headed in the direction of taking up projects in the real world. We were already set and had started with a lot of confusion and much anguish. The Europeans, too, were also set in that direction, but the American group had now located themselves in a university. The Tavistock was not a university, it was an independent action research organization. In those days it was very, very hard getting anything going in the real world from a university setting in Britain. But in America, you could do it and they had plenty of scope if they wanted to. But we found that they had begun to turn in the direction of academic research on propositions dealing with group theory. The expositions that they gave of the concepts were extremely interesting but were going somewhere else. I could hardly recognize Lewin's field theory as I had come to understand it.

So what I think I should say to *us* is that I hope the fissure is bridged between the action researchers and people whose minds are largely set toward academic production. I have nothing against that, *per se*, but it is academic production which is taking the social sciences away from the commitment to, in Fred's famous phrase, the important practical affairs of mankind; taking us away from that important directive correlation. At Gerrards Cross we, in the U.K. wanted to get on with things which were of practical interest. The Michigan group had gone into methodology and concept development, testing these out under careful conditions. That was their direction. This was what we discovered during the conference.

I think we probably learned more from them than they did from us. We learned a lot from their conceptualizations, but they did not learn from us the commitment to go into the society—as Lewin himself had been doing in his time. We were moving into the society and they were moving away from it. We continued to publish together the journal *Human Relations*, which we had founded with Lewin, but we had discovered that we were different kinds of people.

I know most of the people in this room and I don't detect any signs of anyone wanting to uproot the connections between the social sciences and social reality. So I am very hopeful that we will do something here which we did not do in our meeting with the Michigan group. Let us be different from what we were 36 years ago.

On the Next Generation of Issues—Fred Emery

In addressing this topic I am primarily concerned with identifying those issues that are, or should be, on the agenda of the social scientists. Second, I am going to take seriously the notion of generation. Forty years ago social scientists emerged from the Depression and the war against fascism with a very clear agenda for their future. Human affairs had slipped disastrously out of human control and a powerful, influential body of social scientists were determined that such would never happen again if social science could prevent it. Since the early 1970s, human affairs have been again slipping disastrously out of human control. The agenda of the 1940s just yellowed and withered away. No interim agenda of comparable scope and integrity has emerged. If we have not lost faith in social science, then a new agenda must be prepared. Today is as good a time to start as any other day.

An appropriate place to start is with the last agenda. It was a unique response to a unique set of circumstances. It effectively disappeared in the sands of time but not before it had extensive effects.

I think that we should approach this part of our past with two questions in mind. First, we are rightly concerned with whether that event is repeatable. Second, if it is repeatable under current conditions, can we avoid such an early eclipse?

To help us answer these questions we might try to characterize that agenda. What was it about that so marked it off from the early decades of social science and allows us to judge so confidently that it subsequently ceased to exist?

The dominant themes of that agenda seem, to me, to have been

- “The integration of the social sciences” (the subtitle of the journal *Human Relations*, launched in 1947 as a joint venture by the Tavistock Institute and Lewin’s Michigan University Center for Group Dynamics).
- The advancement of social science through direct involvement in the significant practical affairs of people (action research; “community self-surveys”).
- Building democratic bulwarks against the reemergence of fascism (Commission on Community Inter-Relations; the UNESCO Social Tensions Project).

Within these themes there were particular concerns with combating social prejudice—with the human relations movement in industry; with the New Education Fellowship and the revival of concern with democratizing education

and with a humane nonmedical, psychiatry. The issues for these early post-war years of *Human Relations*, *Psychiatry*, *Journal of Social Issues* and *Applied Anthropology* amply document the range of the agenda.

It is difficult to recapture a sense of the drive behind this agenda—the extent to which it constituted and defined the leading edge of social science as it emerged from World War II. In many ways it was a social movement. It did not involve most social scientists; in fact, it did little to shape the professional life of the majority. However, it did define, for a few short years, the climate of opinion within which social scientists sought to build on the status they had gained during the war and sought a greatly expanded power base in the massive post-war expansion of tertiary education.

If anything comes close to being a manifesto defining that agenda it is Kurt Lewin's posthumous paper (1947), "Frontiers in Group Dynamics." His title directs us to the key to the euphoria of the 1940s. There was the belief that the social sciences had at last gotten a firm, though incomplete, knowledge of the dynamics of small groups and possessed ways of putting that knowledge into practice. This created a bridge between psychology and psychiatry, as sciences of individual behavior, and sociology and anthropology. It was the basis for the social sciences to work in democratic ways with groups, organizations and communities to create conditions conducive to individual mental health and personal growth, while providing positive feedback for the quality of group life. For those who believed that science should serve human ends this was a revolutionary improvement on the psychoanalytical couch and promised to be less bloody than revolutionary confrontation.

Lewin was speaking for many of his contemporaries when he wrote in his paper, "This development indeed may prove to be as revolutionary as the atomic bomb."

When the program—the agenda—of the 1940s is spelled out in this way I do not think we have any difficulty today in appreciating what it was about. Looking back and down from the "peaks" we have achieved in the 40 years since then, we could also condescend to accept this as a well-meaning agenda, appropriate to those times and that stage of development in the social sciences.

Lewin has been described by his biographer as the Practical Theorist (Marrow, 1969). There cannot be any doubt but that he thought that our success in building on the practical achievements depended, first and foremost, on the development of a *field-theoretical* social science. For all of his efforts to apply topology and evolve a non-Euclidean geometry, namely hodology, for the description of purposeful behavior, he failed. He failed because at that stage there was no way of translating field theory into the design of social research. Outside of analyzing artificially constrained data we were struggling with the three variable relations (Kendall and Lazarsfeld, 1950). Field-theoretical hypotheses

could not then be researched, except in case studies. With that failure, I suggest, the integrative force of the post-war program was lost. Without that integrative force, the traditional concerns of the social sciences occupied the high ground in the burgeoning world of the universities. The rash of multidisciplinary centers that had emerged in the first post-war years was blotted out.

I will jump the gun at this point to say that my conclusion will be the same as Lewin's. Once again we are faced with drawing up an agenda for the social sciences, and once again the item at the top of the agenda, I will argue, is that we evolve field-theoretical social science: that we make the same revolutionary break with the Euclidean/Newtonian paradigm that Faraday made when confronted with novel phenomena of a nonmechanical nature.

However, the comments above give only a brief description of events surrounding the last time an agenda was drawn up. I think that our present concerns are much more serious and more pressing. Let me spell this out.

As the title of this address suggests, ideas do tend to come in waves rather than emerging as a more or less steady flow. Or, rather, social issues tend to come in waves and at such times ideas become a material force.

Looking back over the past two centuries it seems that the high points of intellectual fever have been in the depression and early upturn of the Kondratieff cycles. This is pretty much where we are at now. The economic shape of this Depression lacks the cataclysmic features of 1929 but is increasingly looking like the long drawn-out misery of the Great Depression that started in 1873, was broken by bursts of recovery in 1880 and 1888 and continued into the mid-1890s. Since 1972 the world economy has been in deep trouble. Even the richest economies—the United States, Japan, West Germany, Sweden, Canada and Australia—have experienced little or no growth in net real family income over the past 12 years with large-scale and persistent unemployment, massive overcapacity in their major branches of industry and staggering burdens of public and private sector debt. While these rich countries have staggered from one “recession” to the next, with pitifully weak and brief bursts of “recovery,” the economic activity of the rest of the world has sunk deeper into depression with declining commodity prices and the drying up of petro-dollar loans.

This time, as in previous times of economic depression, it is the economic crisis that dominates the social agenda and hence dominates any agenda the social sciences might make for themselves. This does not mean, however, that the first item on the agenda is the discovery of another great economic theory. Quite the contrary. The loud and almost unanimous cry of the economists is that the economic system is beyond understanding and therefore leave well alone; any interference based on any economic theory is bound to be counter-productive. The issues that are competing for the top of the social agenda are social and political. The social expectations born of the growth period of the economic cycle are thwarted. This is true at the level of personal and family

expectations, community and national expectations and international expectations. The frustrations are not borne evenly and hence divisions between “haves” and “have-nots” sharpen at all of these levels. Existing social institutions and beliefs come increasingly under challenge as they fail in their supportive and explanatory roles. New ideas gain social currency and new, and hitherto peripheral, institutions gather social support. These challenges are met by a hardening of the defensive positions of those already in positions of power and privilege. Unless those ruling groups are deeply divided among themselves, or grossly discredited by military misadventures, any notion of social revolution is far from the top of the social agenda. Significant social changes, and hence a role for the social sciences, are only likely to arise in the phase of recovery. In that phase the ability to bring together the resources of the nation will pretty well determine the advantages, if any, that that nation gains in the reshuffle that follows such crises.

It appears that industrial civilization has five times gone into depression and five times recovered. Each time a larger proportion of the world’s population has been affected and, on the evidence available for the last three depressions, it seems that each crisis has been deeper and lasted longer than the previous one. On this admittedly patchy evidence, the best hypothesis would be that we still have some years to go before this crisis bottoms out and quite a few years (a decade?) before the world economy gets back on a strong growth curve. But a recovery is certainly to be expected.

Before we write a Kahn-Bell-IBM scenario for a “postindustrial civilization” (an Athens based on robots), we should take stock of the peculiarities of the present economic crisis.

Following the work of Joseph Schumpeter, a good deal of study has been made of the conditions required for recovery from worldwide depressions. One of the critical conditions—the emergence of new technologies to foster new markets—is well and truly met. The microchip is spawning markets we could not have dreamed of 10 years ago. New sources of raw materials and labor are also promising, partly due to the new technologies. However, a new source of cheaper and plentiful energy has been problematic ever since our hopes for nuclear energy were scotched as the true costs were revealed. To underpin recovery to anything like the growth curves of the 1960s, we are looking for a massive energy source that would also permit a doubling of supplies every seven years. Fusion, ultradeep geological sources of hydrocarbons and genetically engineered pathways to utilize solar energy are still dreams. They might be solutions to energy problems in the distant future but hardly for the 1990s.

The most serious constraint on recovery is imposed by labor productivity. At each of the preceding recoveries we have seen the emergence of more sophisticated forms of imposing asymmetrical dependence on the workforce; at least on that part of the workforce at the leading edge of new investments. This

time around we are witnessing a radical departure from the historical norm. The interface between technology and worker has increasingly evolved toward an interface between information processing and educated operators. Productivity is being achieved by displacing traditional forms of asymmetrical dependence in the workplace by new forms of symmetrical dependence, e.g., semiautonomous, self-managing work groups. Even the wages relation is increasingly redefined as a salaried relation (Hill, 1972/Vol. II).

It would be easy to see this undermining of industrial autocracy as a fulfillment of a major item on the Lewinian agenda. As one who was deeply involved in those efforts, I would agree that it was one of the most sustained attempts to fulfill that agenda, and a conscious attempt. On reflection, however, it seems to me that other social scientists would have continued to ignore that work and industrialists would have encapsulated it if it had not been for the youth "countercultural revolution" of the late 1960s. That challenged the legitimacy of autocratic relations in a way that our writings and field experiments could never have done (Emery, 1978). As the front edge of the "baby boom generation," the first of their generation in the 18–25 age group, collided with society, employers started to realize that here was a generation that did not accept the ground rules; yet they were the ones with the educational levels required for the information technologies. Things have been politically and socially quiet on that front for some time, but it is with this generation that any recovery will have to be negotiated. All the evidence is that they are no more moved by appeals to "God, king and country" than they were in the late 1960s (Naisbitt, 1982).

If this interpretation of history is correct, then this address is premature. If we are considering any early flow on into action, that is certainly true. However, it is not too early to try to identify what must be on the social agenda for recovery and hence to speculate on what role the social sciences might play.

It is just such speculation that I will now engage in. Because it is just speculation, I hope it will have no more influence on the proceedings of this conference than anyone else's speculations. There are just two qualifying remarks before launching on my speculations. First, I do not think that the productive potentialities of our new technologies can be fully realized without relations of symmetrical dependence in the workplace. As a corollary, I do not think such relations can be sustained in the workplace unless supported by similar relations in the family, schools and other institutions required for the reproduction of a labor force capable of sustaining such work roles. I could be wrong, but I do not advance that hypothesis in a merely speculative manner. Second, and definitely in the speculative manner, I must record that in the back of my mind are serious worries about whether (a) the cultural challenge we face goes much deeper than the challenge of the counterculture and (b) another "little ice age" is just around the corner.

Western civilization is the only known civilization that has claimed Design Principle 2, individual resourcefulness, as its ideal. All other civilizations have been unambiguously based on Design Principle 1, the replaceability of the individual part (Emery, 1967/Vol. III).

Design Principle 1, the usual Design Principle for getting stability in large civilizations, which comes with the emergence of urban areas and the network of mutual reciprocal relationships that are required to enable the urban centers to emerge, involves "redundancy of parts." One of the ways you can get reliability in a system of unreliable parts is by building in redundancy of parts so that if one part fails another is there to take over. For example, the American shuttle has four computers working in parallel, with a fifth on standby. As we see reflected in the price of labor and in the life expectancy of most people in Design Principle 1 countries, individuals are prepared so that if someone drops dead or is kicked to death there are still sufficient to get on with the job.

The alternative principle for getting reliability in a complex system is Design Principle 2, building in a "redundancy of function." You overeducate all the people who are constituent parts of the society so that if any one person fails to carry out a particular function someone else has the additional functions at his or her disposal—the capabilities to help out.

If you design on Principle 1, it is essential that you have a control body, some other specialized group of people who will decide when a person is allocated to one part of the system or another. The parts can only do their own bit, they cannot—not knowing the other bits—decide whether and when to move around. The epitome is the assembly line. In other words, you need some hierarchy of control, "a dominant hierarchy," in such a society; there is necessarily a stratification of the worth of a life and an elitism.

In moving to the second Design Principle, theoretically all that is required is multiple functioning parts, parts who are equipped to share a sufficient appreciation of the field within which they are mutually operating and a sufficiently extensive, but commonly known, range of values to enable them, individually and collectively, to decide what ought to be done in certain circumstances. The second Design Principle should result, in large measure, in a self-controlling society and not require a special control section—not require an elite or dominant hierarchy.

Differentiation of functions, heterogeneity, leads in the first design to increasing complexity of controls and an increasing diversion of the free energy in the system to the internal control function.

In the second design, differentiation of functions leads to an enrichment of the qualities of the parts without either (a) increased complexity of the control system or (b) diversion of system energy to the control function.

On the excuse of the scarcity of resources and the consequent threat of outsiders seeking to redress their own scarcities, we have always limited the extent

to which we practice what we preach. Thus for millennia the key institutions and statuses have been arranged on the principle of hierarchical dominance. Only with the Beatniks and then the Hippies was there widespread spontaneous awareness that scarcity and security through national strength were not unchallengeable facts of life. The millenarianism has temporarily subsided but not before raising serious doubts about whether our civilization is committed to its ideals. Potent external challenges to those ideals have recently come from the upsurge of militant Islam and the demonstration by the Japanese that modern technology can be very successfully put to the service of an Asiatic type of civilization.

If there is anything in these speculations, which do unfortunately have about them a ring of Spengler and Toynbee, then we may have to look sharply to the refurbishing of our ideals and the values which guide our pursuit of those ideals. A good half of Europe has given up on Design Principle 2, and Germany and Italy temporarily did so in response to the last depression. Let that path be taken—I am talking just of the next decade or so—and that is the end of the experiment in Western civilization and we can forget about agendas for the social sciences.

The other matter which might temper our optimism about the future is the growing evidence that world climate cannot simply be regarded as a lot of variation around a steady mean value. On that assumption a strong case can be made for rejecting doomsday scenarios based on population-food balances (Emery, 1977/Vols. II, III). However, in the past decade it has become apparent that the great crises of the seventeenth and fourteenth centuries were due to century-long “little ice ages,” not in the first place to wars, the Plague or “inherent contradictions in the feudal mode of production.” It is seen as increasingly probable that the vast disturbances associated with the Vikings and Moslems in the ninth and tenth centuries and the mass migrations of the fifth century had a similar cause. There is, to my knowledge, no explanation for this phenomenon, although there is some evidence linking it to properties of the solar system (Eddy, 1978). Hence we can attach no probabilities to the recurrence of such phenomena in the time scale that concerns us. All we know is that, on past reckoning, we are overdue for such a climatic shift and that such shifts tend to need only a decade or so to take place. The climatic disturbances since the early 1970s have clearly worried some of the experts but they could equally be within the range of “normal” variations. This is literally the joker in the pack. If it is played, the consequent struggle between “haves” and “have-nots” could be very destabilizing for world order. International buffer stocks would give us some chance of bridging the gap as crop plantings are relocated to match shifts in the rain belts but, although surpluses now exist to establish such stocks, there is little willingness among nations to sacrifice current market advantages.

Let us assume that neither of these possibilities eventuates, that is, that Western societies decide to build their recovery in ways consistent with the ideals of their civilization, and that there has been no great loss of agricultural resources. Within that scenario, it is not difficult to identify where the social sciences could make a significant contribution.

It is not difficult because, if we are to be consistent with the ideal of the inherent dignity of the resourceful individual (Forbes, 1971:94) recovery cannot be planned and administered from on high, as with Roosevelt's New Deal in the late 1930s. Steps to recovery must mobilize the resourcefulness of the citizenry and educate and develop that resourcefulness through their engagement in planning and carrying through those plans. Individual dignity would be enhanced to the extent that individuals are engaged in deciding and making their future.

I will list the tasks that I see for the social sciences and then make some comments on the reasons for listing each item and what it may involve:

1. Empowerment of the individual through
 - a. liberation of the human senses (Emery, 1981/Vol. III),
 - b. unfolding of the world hypothesis of contextualism (Pepper, 1942),
 - c. a field theory of language (Verbrugge, 1985).
2. Creating tools for self-liberation:
 - a. participative democracy as a viable institutional alternative to representative democracy,
 - b. matrix-type inter-organizational arrangements,
 - c. Search Conferences,
 - d. methods of participatory design,
 - e. action research.

This is not presented as an exhaustive list. There are some matters that I have considered and discarded, e.g., possible advances in economic theory and the dynamics of small groups. I do not think they are central, although I know many think otherwise. There are also undoubtedly many matters of real significance of which I am ignorant or which I have simply failed to appreciate. The fact that I have written on most of the listed items does not, I hope, prove that I think that what I write about must be important. Historically, I have been drawn to these matters by my involvement in action research, and I have had the freedom to follow up on them.

The first group of proposed agenda items simply recognizes the extent to which those who are dependent in asymmetrically dependent relations have their worlds defined for them. Historically, they have been in the position of the prisoners in Plato's *Allegory of the Cave* (Book 7 of the *Republic*): chained to the benches, head locked in forward position and trying to make meaning of events of which they see only the shadows projected on the wall in front of them. The emergence of mass education, even mass tertiary education, has

hardly changed this deeply inculcated belief in the fallibility of the senses. That education is premised on the notion that sound knowledge cannot be achieved by the senses, that knowledge can be achieved only by scholarly minds disciplined to long and arduous processes of abstraction, induction and deduction. For most of their lifetime the psychological sciences have lent their weight to this assumption. Only in recent years has the assumption itself been subjected to experimental investigation by the ecological psychologists (Shaw and Bransford, 1977). The assumption is false. Over a wide range of naturally occurring circumstances the human perceptual systems yield direct knowledge—provided we are free, physically and socially, to explore and probe what we perceive. The implications of this for our educational practices have yet to be worked out but clearly a lot of credence must be given to the radical proposals of Illich and Freire (De, 1984).

Our knowledge is not limited to what we directly perceive. We transpose knowledge from one situation to another by use of metaphor and thus build on what we already know, or what is best known by another. In relations of social dominance “it is the people in power who get to impose their metaphors” (Lakoff and Johnson, 1980:484). The reason is obvious. A knowledge of their metaphors is the best predictor of what they will do in any situation deviating from the norm. Over long stretches of Western civilization certain root metaphors, and their associated world hypotheses, have played a dominant role. Those root metaphors had such a deep and pervasive influence because they indicated, in shorthand fashion, the kind of thinking and reasoning which could lead to true understanding. Until the mechanistic root metaphor emerged with Descartes and Newton the dominant “Aristotelian world hypothesis” held that true knowledge had to take the form of the written proposition, more specifically the syllogism. For the past 40 years systems theorists have loudly claimed to have liberated our thinking from the analytic root metaphors of syllogism and machine and the sloppy synthesis of organic metaphors (Hegelianism). It has not proven difficult to write Aristoteleanism, mechanism or organicism into a so-called systems language—particularly when a system is defined as only “a set of elements and the relations between them,” without reference to a system principle defining a *unitas multiplex*. The net effect of the systems movement has been to confuse and mask the significance of the new root metaphor that emerged when Faraday rejected the mechanistic interpretations of the electromagnetic fields he had discovered in the laboratory. Through the efforts of Charles Sanders Peirce and Stephen Pepper this breakthrough evolved as the contextualist world hypothesis. The root metaphor of this hypothesis is, I believe, that of human discourse; that is, true understanding is that which can be averred to and will continue to stand up in human discourse. This offers no certainty of attaining absolute truth as no limits can be placed on changes in the practices that are the context of discourse or limits on who will be drawn

into the discourse over what period of time. It is, however, Peirce argued, all the reassurance we can expect to achieve.

This is the first world hypothesis to proceed from the assumption of direct perception; that, if others were in my shoes, they would perceive what I perceive. It is the only one that takes qualitative change as its primary category and it is the only one that asserts the irreducible character of some triadic relations without denying the brute reality of interactions (organicism accepts the former but not the latter). The methodology of “directive correlations” (Sommerhoff, 1950) derives from and exploits the essentially triadic nature of adaptive and purposive behavior. I suggest that this is the only world hypothesis that gives due weight to what is involved in people acting cooperatively to make their own future.

The third item on the list is only a belated awareness, on my part, of the significance of what Sapir, Whorf and Korzybski stressed in the intellectual turmoil of the 1930s, namely, that our accepted usage of language could blind us to reality if that language had been shaped to serve the contrary purposes of others. All I wish to say now is that we need to take up this challenge again, but to take it up where Peirce left off around 1900, not where “the semanticists” left off in about 1950. The semanticists rightly stressed that words like “God,” “capitalism” and “sex” were loaded with meanings that were misleading in practice. They failed, however, to challenge the roots of our traditional concept of language as a tool of social domination, that is, that (a) language is constructed from linguistic concepts that are used as building blocks and (b) collections of these words, grammatically ordered, spoken or written, contain sets of meanings that are there to be extracted by those learned in the “proper meanings.” The “building block theory” has us believing that a word has its meaning because it is associated with an idea more or less clearly held in the head of the user. How those ideas come to be the same in two or more heads, or in the same head at two different times, is left a mystery—particularly mysterious when, according to the traditional world hypotheses, neither could know whether they were sensing the same world. The “container theory” (also dubbed the “conduit theory” [Reddy, 1979]) is convenient for those who are dominant in a relation of social dominance (see *Alice in Wonderland*, Carroll, 1870). It hardly explains why the same words can be taken to mean such different things in different contexts, or why so many different combinations of words can be taken to mean the same thing in the same context. The answer is, of course, that communication depends on both sender and receiver striving for an homology of words and context. Peirce realized this in what was arguably the first formulation of a field theory of language. He realized that the individual word carries no inviolate meaning. We can put meaning into words (“real words”!) only in the context of signs, or words acting as signs, of an indexical or iconic character. We can extract a similar meaning from the words

in the message only insofar as we can identify what is indexical or iconic in the message. Where communication is such a cooperative endeavor it can serve to advance mutual enlightenment and understanding; where it is less than this, language may not go much beyond what is achieved in subhuman communication, that is, instructing or commanding, or informing, as in pointing.

The agenda items listed under “tools for self-liberation” all derive their scientific significance from the matters just discussed. In the best tradition of Koestler’s *Sleepwalkers*, most were developed because they seemed to fit the requirements of on-going action research and with only a hazy sense of what was theoretically at stake. Thus, in designing the first search conference in 1959, I was very conscious that we had to meet the conditions for an open, trusting relation that Solomon Asch (1952) had laid down for a ABX relation. We were not conscious that we were working on the first three items of a 1985 agenda for the social sciences.

Heightened theoretical awareness should enable us to considerably improve on, and extend, the kit of tools. One does not, however, sense the seriousness and sense of united purpose that characterized the post-World War II generation of social scientists.

Addendum—Fred Emery

By now (1993), it is clear that the agenda suggestions of 1985 failed to foresee (a) the continued loss of nerve of the social sciences and (b) two major shifts which, if foreseen, would have required a section 3 in the above agenda.

The “loss of nerve” was illustrated by the Orillia Conference. On an international scale we have had a “consultancy-led” backing off from participatory design and a retreat to a more cloistered, expert socio-technical systems design process and, on the other hand, from social scientists, a watering down of the concept of action research. In both cases, there appears to be an unwillingness to fight for a mutually respectful engagement between social scientist and those directly responsible for important practical affairs.

The STS (socio-technical systems) consultants justify their expert approach by pointing to what Emery and Thorsrud’s team did in Norway in the 1960s. They overlook the quite specific historical facts that dictated that approach. First and foremost was the need to establish, in workplaces pervaded with cyni-

cism and mutual dislike and distrust, that a particular speculative idea could become a practical reality. Before the end of the program—late 1960s—Thorsrud and I were well aware that the purpose of the field experiments had been achieved. We knew from our experiences that to continue with that approach would only serve to reawaken the conditions it had originally set out to overcome. The main scientific task had moved on from redesign of work organizations to that of demonstrations and the diffusion of knowledge. Participative, cooperative design processes were the way to go to reduce obstacles and resistance to diffusion (Emery and Thorsrud, 1969/1976: 156–57).

The watering down of the action research concept has taken two forms. One has been simply to slide over the difference between consultancy and scientifically motivated action research (Eldon and Chisholm, 1993). The fact that a consultant wears a professorial hat does not change the nature of the consultancy. The other form has sought a more sophisticated refuge in the theories of Habermass (Gustavsen, 1992). In that approach it is held that social scientific knowledge has no privileged position vis-a-vis “local theory,” and hence the most that social scientists can contribute to practical affairs is the setting up and facilitating of “democratic dialogues.” The premise of this argument is, I think, false and hence indefensible. One might say that it is a lawyer’s argument. In fact, both science and “local theory” have privileged—and hopefully complementary—positions; and participative design is based on that fact.

The matters just discussed should readily be coped with as normal business of social science. We have been over the ground before and divisions and ups and downs in our collective confidence and morale are not new.

The two recent changes in the social agenda have much more far-reaching implications for the formation of a social science agenda. First is the collapse of the Soviet Union and the dissolution of the post-war polarity of the two superpowers. Second is the emergence of a very serious national project for the transformation of a national workforce. It is an ironic historical juxtaposition—as the great nation that trumpeted that it was the “workers’ paradise” goes down the drain, a small, determinedly capitalistic nation sets about creating a multiskilled, career-oriented and self-managing workforce the like of which has never existed before.

Let me take the first matter first. When I listed participative democracy as agenda item 2a, above, I was thinking of tools that social scientists should create to meet future needs (Emery, 1991). I had no idea that the issue was about to come high on the agenda of practical social concerns. In fact, I was at that stage more concerned with the apparently tenuous hold in the world of the democratic ideals. With the collapse of the Soviet Union as a superpower the “iron curtain countries” are in the market for democratic forms of government. They have been exposed for decades to evidence and diatribes about the shortcomings of the so-called Western forms of democracy. Social science should

put forward a wider range of choices (Emery, 1991). In the Third World, we have a similar problem. Since they have been released from, or struggled their way out of, colonialism they have been locked into grossly unrepresentative forms of government. Their elites could ignore their people and still ensure their rule by entering into deals with one or other of the superpowers. Now they have to find their way to forms of self-governance that fit with their being sovereign powers, not colonies or pawns. As Robert Dahl (1989) has made clear, the existing models of democratic representation are less than adequate and the so-called political science is out of its depth. Whether social science can rise to the challenge has yet to be seen. The challenge is certainly there.

The second social change—the transformation of a national workforce—would pose social science with the biggest challenge it has ever faced; and a much bigger challenge than it ever thought it would face. Most likely a bigger challenge than social science with its many thousands of graduated PhDs and professors could cope with. If, of course, the change is genuine, sustained and spreading to other nations. The nation I am referring to is Australia. In 1985, at Orillia, I had no idea of what was going on beneath the surface in that nation. It is said that deep rivers run smoothly. This was a deep river. We well knew of the effort that had been spent in the 1970s, in field demonstrations and discussions, to acquaint leaders, in corporations, unions and politics, with new perspectives in democratizing industrial relations. In the mid-1980s, we could not tell what had happened. We had done what we could do and we had no role in the discussion and decisions of the powerholders.

In the late 1980s the changes broke surface. Very rapidly radical changes have taken place in the industrial relations law, in trade union structure and at all levels of the educational system. The publicly stated aim is to create “a clever country.” The connecting link is the transformation of the workforce from the traditional mass of narrowly semiskilled workers under close and direct supervision, to a workforce that is multiskilled, career oriented and, for the most part, deployed as self-managing work teams. This has entailed massive and deep-reaching changes in existing institutional structures. The scale and speed of the changes has caught the social science community flat-footed. Bureaucrats and industrial leaders, particularly the union leaders, have made the going. Such response as the social scientists have made has been largely knee-jerk reactions of providing more training (Emery, 1994). However, the changes have already reached so deeply that it is very unlikely that they can be reversed, and it is much more probable that they will lap over to the other key institutions. The challenge for social science is to muster its resources so as to engage in the changes in an appropriately effective way.

Looking back over the 50 years covered by these three volumes of the Tavistock Anthology, it is clear that the social agenda has always dominated any agenda set by social scientists for their engagement with the important practical

affairs of mankind. When democratic societies have wanted to change themselves there has been considerable interest in, and support of, practically oriented interdisciplinary social science research. This was true in the early post-World War II years; it was true in the late 1960s and early 1970s when the counterculture movement challenged traditional notions of hierarchical domination. It would seem that it is on again. Whether we have learned enough from our past experiences to successfully meet the present challenges is a question that cannot yet be answered. At very least, these volumes have made this past experience readily available.

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