ERIC L. TRIST - 1911 - 1993

GUILTY OF ENTHUSIASM An autobiographical view of Eric Trist, a founding member of the Tavistock Institute of Human Relations, published in "Management Laureates" edited by Arthur G. Bedeian, Volume 3, Copyright © Jai Press, 1993 *

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FAMILY FORTUNES, EARLY LIFE AND EDUCATION

My father was Frederick James Lansdown Trist and my mother was Alexina Middleton. Father's family was Cornish and he was a sea captain. His family did a mix of farming and fishing and – as most families did in those parts - smuggling. In the nineteenth-century they had three clippers in the China tea trade and, coming back from China, they would take their clippers over to the Brittany coast, and smuggle French lace to sell in Plymouth. Great-aunt Rachel's crinoline provided a safe hiding place! The British navy suspected them, and forbade the family to take their ships anywhere south of a certain point in the English Channel. But my great-uncle, Phil, a very rambunctious character, said, "To hell with that!" When he took the ships south toward Brittany the British Navy went after him, seized the ships and broke them up in Devonport shipyard. Phil then went to the gold rush in Western Australia, into coastal trade, and made a fortune. He came back to England looking for my grandfather, whom he didn't find. Instead, he found another branch of the family, and they got his money. That was the end of any family fortunes on my father's side. My mother's side of the family is highland Scot – so I'm Celt, again – and her people had a small estate in Kincardinshire. During the early period of the Industrial Revolution there was a reconciliation between her family and their neighbors who had supported opposite sides in the 1745 rebellion. My great-grandfather stood bond for his neighbor in some enterprise which went bankrupt. So again the whole family was in ruins. He started building up again, and became manager of a bleach field in Brechin north of Dundee.

My mother, who was about three years younger than my father, had been a governess in a military family in Shoeburyness where my father was stationed – thatÕs how they met. Both parents were the youngest in large Victorian families. They were married in the 1890s, and had given up the idea of having a child by the time I came along in September 1909 when they were in their forties. They had, unofficially, adopted a cousin of mine on my father's side. Her father had been drowned at sea in the tropics. She was in her teens when I was born. My cousins were all years and years older than me.

I went to the local elementary School, St Martin's, in Dover where we lived. World War I was a very dramatic experience, very vivid to me in 1916. Bombardment and air raids all the time.

Education was the central thing in our Scottish tradition. My father was as inclined towards education as my mother, but he wasnÕt as emphatic. He certainly didn't want me to follow his example and go to sea, which I might otherwise have done.

I attended the local secondary school because there was not enough money to send me away to boarding school. I tended toward the arts side rather than to science, and took English, French, history and Latin, keeping geography as my science option in the sixth form. English and French literature were my favorites. My French master, Thomas Watt, was absolutely magnificent, a great personal friend, and utterly exceptional in all respects. He was my main influence at school. The other very influential master, W.E. Pearce, taught physics, and wrote a textbook, School Physics, which became nationally adopted.

Two special friends at school were Henry Garland, who was a year ahead of me and went to Emmanuel College, Cambridge, became a Fellow and eventually the Professor of German at the University of Exeter; the other was my contemporary, Clifford Jarrett, the most brilliant scholar I've ever known. He came top in his year in the examination for entry into Division I of the British Civil Service. He became a Permanent Under-Secretary and was awarded a knighthood.

My secondary school wasn't one where people went on to university, so my generation was the first to go to Oxford or Cambridge. I had no notion of going to a university. It never entered anybody's head at school except this French master who selected Clifford Jarrett and myself, and told the headmaster he must enter us for State Scholarships. I didn't understand what that meant. The French master explained it to me, and told us a bit about what it would be like to go to university. He said that you would have a future, especially if you went to Oxford or Cambridge.

There was nothing immediately around in Dover that I wanted to do. I liked school up to the last year or two, but then I found it very provincial. I had got beyond the school, and was ready to leave in May 1927, but I had to stay on for a year to be sure of getting a scholarship.

LIFE AT UNIVERSITY

I went to Cambridge – Pembroke College – in the fall of 1928. Pembroke didn't accept me very well because I was from a Grammar School rather than a Public School, although they did take a number of people, like me, from Grammar Schools, as scholars. I didn't have a pre-existing circle of friends among the college students. I didn't have the culture of people who were born well, had gone to good schools and were well off.

There was the games business, but to play you had to have a reputation of having been in a first eleven, say at cricket, somewhere that was recognized. And, of course, I didn't. Though to play for the university was beyond my aspiration, I should have like to play for the first or second team of my college, but the standards were beyond my reach. The fifth team was my level and not much fun. So I had to give up the passion for games that I once had at school. Also, I wasn't good enough to get anywhere scholastically without putting in a tremendous amount of effort and time. I had to get First Class Honours, and I had to work very hard to do that and so I dropped out of games at Cambridge. Our College was a 'hearty' college, but I wasn't one of the 'hearties.'

I had a certain interest in the stage and dramatic work. I let all that go, too, because I wasn't in the same league as others such as Michael Redgrave and Alistair Cooke. There was a tremendous amount of conversation and interaction in the societies that were newly established, such as the film society. One was over-busy, going to things, and it was often hard finding private time to work. I was not religious. I was a member of the university Labour Club, and used to go there regularly. I had several friends there, and I was known, but not well known.

I didn't have much of a social life, but managed with a small coterie of friends, including some people in other colleges. One of my best friends, Grey Walter, was at King's College. He became very famous for work on the human brain. He was one of the people I greatly admired, and who helped me change from literature to science. Also, while I was still doing English, I was very influenced by I.A. Richards, the most famous English don at that time, who linked philosophy, literature and linguistics. With F.R. Leavis, Richards had a tremendous influence on the study of English at Cambridge, which in those days was world famous.

From September 1928 to May 1931 I read English literature. It was very exciting because of the reading, the tutors and the subject itself, which was very modern. I wrote an essay every week, but I didnÕt publish anything, or speak at any of the undergraduate societies because I wasn't confident enough. I went regularly to hear debates at the Cambridge Union, but I was very shy, and only listened.

After I graduated – First Class Honours in both parts of the English Tripos – I wasn't sure what I wanted to do. I thought of doing either philosophy or psychology. Through Richards I got interested in psychology, especially Gestalt psychology and psycho-analysis. I remember going to see Broad, the philosopher in Trinity, and him asking, "Why do you want to do philosophy?" I replied, "I was wondering between philosophy and psychology." And he suggested, "You read psychology. Go and see Professor Bartlett." I went, and Bartlett accepted me to read psychology which was then a Part II in the Moral Sciences Tripos.

Of course, at Oxford and Cambridge one knew perfectly well that some of the best of one's generation were there, and that quite a number were going to become famous, so one was always comparing oneself with these others. I did as well academically as anybody could, but I never thought of myself as very outstanding.

The Psychology Tripos comprised a small group where everybody knew everybody else. I was more interested in psycho-analysis than experimental stuff, but psycho-analysis was not very popular in Cambridge. You had lectures, and you read, but it was up to the scholar to involve himself. For the two years I had P.E. Vernon as my tutor. He was excellent. He didn't tell me what to do or anything and we were not really close, though he did help me with my career afterward. I read pretty broadly in experimental psychology, social psychology, and Gestalt psychology.

I was very much influenced by Kurt Lewin in those days, and there was an incident which was very negative to my future in the Cambridge psychology laboratory. I came in one afternoon, very excited, and Professor Frederick Bartlett asked, "Well, what's the matter with you?" I answered, "I've just read Lewin's article on the Galilean and Aristotelian methods." I was feeling very excited. This went against me – young Trist had shown himself guilty of enthusiasm, of being uncritically over-impressed, not detached enough, too involved.

I once met Kurt Lewin in Cambridge. When he left Germany, he went to Israel, and then he was invited to the United States. On his way he visited Cambridge. It was thought that he might stay for a while but that didn't happen. The last day he was there was one of the high points of my life. I was invited to tea with Bartlett and other professors. When the tea party ended Bartlett said, "Trist, you have an hour to show Professor Lewin around Cambridge before his train." I asked him, "What is it you most want to see?" He replied, "I want to see the statue of Isaac Newton." So I took him to Trinity and there was Newton's statue. Kurt stood gazing at Newton and started to gesticulate, just like the fan-tracery in the roof. This was the kind of diagram that he was doing for the book on topological psychology, which he was then writing. So, I got an advance view of what it was going to be about in front of the statue of Isaac Newton. Then, we had to rush to the train. We almost missed it because he had been so enthralled with Isaac Newton. It had started to move when we got into the station and I just managed to open the carriage door and push him in. I always treasure that memory of Lewin being thrust, by me, into a railway carriage.

I was very impressed with Bartlett as a thinker and as a teacher. His book on remembering came out while I was there, and he was elected to the Royal Society. The first psychologist ever to be so. He was, in hindsight, a great man of profound originality, but he was also an extremely pragmatic individual, and he went with the times.

The only funds for psychology that you could get at Cambridge in those days were for physiological psychology. Because I showed all the signs of becoming a social psychologist I was out.

I graduated in Psychology in May 1933. 1 got not only First Class Honours, but also a Distinction Star - the first time it had been awarded since World War 1.

The Fellowships were highly competitive, and I think the awards themselves were given whimsically. I don't know whether they were just, although a lot of the outstanding people got them. No exam. You had a big panel interview and your referees were very important. Bartlett supported me and so did I.A. Richards. Those two together got me through.

My interview was hilarious. Sir James Irvine, the Principal of St. Andrews, was chairman that year. During my interview one of the main members of the committee, Lord Somebody-or-other, went to sleep. He woke and asked me a question I had already answered, and I wasn't quite sure how I should play it. Sir James interrupted, "The candidate answered that question while you were asleep." The whole place went up in mirth, including me. I always thought that was the reason why I got a Commonwealth Fund Fellowship. My subject wasn't in line; I was the first psychologist to get one; and what I planned to do at Yale - to amalgamate anthropology and social psychology - wasn't really of interest to any of the people on the committee.

TO AMERICA, 1933-1935

The scholarship paid my return fare, university expenses and a personal income of $150 a month, which in those days was quite handsome. I sailed in September 1933 with the other Commonwealth fellows, and found I was paid great attention to by the fund director, Edwin Bliss, because of my Distinction Star. Apparently he thought that they had got hold of someone special. The M.S. Britannic left from Liverpool, went to Cork, Galway Bay, Boston and then down to New York in ten days. Most of the passengers were very wealthy Americans so I didn't move outside the company of the Commonwealth Fellows.

At Yale I greatly admired Sapir, whom I went to study with. He was the biggest influence on my intellectual life, ever. I attended a lot of classes in psychology, went to Clark Hull's seminar as well as Sapir's. But I didn't get on very well because I didn't have any clear direction or specialization, but I knew I had to do something in social psychology. I became close friends with the then Professor of Social Psychology at Yale, Spike Robinson. But he was killed in a car accident. It was terrible.

Sapir's concept of culture was important to me. It came from the internal world of the individual, and was shared with others, and was not a fixed thing which you passively absorbed. You actively, selectively got it, so no two people got it quite the same. I was very influenced by that, and by the experience of a field trip to one of Sapir's post-doctoral people, Walter Dyke, and his wife on a Navaho reservation. They were taking down autobiographies of Navaho Indians, translating them and comparing one man's account of his culture with another's. That was what was being done at that time in Sapir's anthropology. He used to say that any language will do its stuff but you can't escape from the instrument. I was deeply bitten into by this set of beliefs and the Whorff-Sapir hypotheses.

At Clark Hull's seminar at Yale, which was where I had my initiation into behaviorism, there was an experimental situation set up such that the subject (presumed to be a child) would see a piece of chocolate in the middle of the table but would only be able to reach it by turning right around and moving in the opposite direction before finding a way to secure it. Various big noises came down to the seminar but no one – not even Lewin – could work out how to get the chocolate.

I had some time with Lewin in the United States, and I was thoroughly hooked. He was at Cornell, and then later at Iowa at the Child Development Center where the big experiments were done with Lippitt and White – the democratic climate experiments.

At Cambridge, in England, I.A. Richards had said to me that there was a very bright behaviorist at Harvard, Fred Skinner, who had written on Gertrude Stein, an interesting chap, a very nice man, and I should see him. So I went up to Boston, and Skinner took me to see his laboratory. There was more apparatus and more money in Fred Skinner's laboratory than in the whole of psychology in England. It was simply breathtaking. It took me a long time to realize what he was doing. He kept talking about a "lever" and I didn't know what a "lever" was. Eventually I managed to deduce that a "lever" was a "leever," as we pronounce it! He took me to lunch with the girl to whom he was then engaged. She was a very beautiful girl. Later we went back to his lab and he said his experiments were the most interesting thing in his life. And I said, "My God, if I were you there would be something else that would be more interesting in my life!" I was never able to embrace Skinner's views, except that I was influenced by a paper that he wrote – was it 1932? – called "The General Character of the Stimulus and the Response." It was very good and I used to teach that four years later at St. Andrews in Scotland.

I rejected the Hull seminars, but I was very interested in attending them. You had to do the politics of your ticket. The idea of graduate studies, as such, was not known in Britain, but in America it was pretty well established. And I wasn't organized for it. To get a Ph.D. I would have had to stay three years and my scholarship was for only two. Sapir nominated me because I did want to stay a third year, but I wasn't accepted.

When I was in America, like all Commonwealth Fellows, I travelled around the United States and wrote a report. In the summer of 1934, in my Model A Ford, I went south along the east coast, then west to Denver and the Rockies, from Denver down to New Mexico to Santa Fe, and then across the desert to Los Angeles. Then I went up from Los Angeles to Berkeley where I stayed for a while, and then up the north west coast and back across the mountains through Montana, to Chicago and back to New Haven.

I began to get interested in the world and politics and the Depression. It was a tremendous shock coming back from New York one night when I picked up a guy who was starving. I took him into a diner for a meal. I had never seen anything like that. This made the Depression real for me. And when I was travelling in the south that summer, there was a textile workers' strike, and several people were killed. That was a terrible thing which upset me very much. And when I was in Arizona there was big trouble in a company said to be owned by the Rockefellers. One of the organizers, a communist, was chased out of the place, and left to die in the scrub on the Navaho reservation. I was there when he was picked up. These sort of things disturbed me. I had never experienced any violence, or seen how bad it could get.

Previously I had no concept of the realities of politics. Later in San Francisco I walked in a big parade in memory of the dead from certain incidents of several years before. It was a very, very moving experience.

Back in New Haven, I joined The Hunger and Strike Committee which supported strikes in Connecticut. I used to go out onto the picket line, and once I went out in the drenching rain in Hartford when there was a big arms strike. One morning we were all hosed down. The workers were very badly treated. That was the first time I was politically active, and the first time that I read any Marx, which both enlightened and confused me. I was confused by what seemed to be a metaphysic, and therefore nothing I could subscribe to. Wittgenstein had convinced us that metaphysics was 'nonsense.' On the other hand, in the Depression, Marx made every sense to me as an analyst of society.

EARLY RESEARCH IN SOCIAL PSYCHOLOGY

When I returned to England in 1935 I had a hell of a time. Just before I came back Sir Frederick Bartlett had sent one of his people to tell me that there was no job for me in psychology in Britain, not even a corner in Cambridge, nowhere! I knew Bartlett had control of all the appointments in psychology in England. Nevertheless, I went up to Cambridge to find out about jobs and I met Sir Frederick in the corridor to the psychology lab. He didn't recognize me though I had been his student for two years. Then he asked, "Look, I know you, don't I?" I replied, "Yes, I was here for two years." And then he said, "Oh, I remember now." That was how vanished I had been. That evening I was allowed to go to High Table in my College, but the Fellows thought that I was an odd joke. Going into psychology! Going to America! That wasn't their way.

Then I got a break. Oscar Oeser, whom I had met in 1932 in Cambridge, came back from Germany having finished his Ph.D. He had transferred from physics to psychology, and then gone to Germany to finish his degree. He had got some money from the Pilgrim Trust for interdisciplinary work on longterm unemployment in a Scottish area. Oeser interviewed me for the job of social psychologist in his three year project, and was interested in what I had to say about Kurt Lewin, and my political experiences of the Depression. Oscar Oeser was a committed academic and an action researcher who wanted to study unemployment and was a very enlightened social democrat at that time. So that was an enormous break for me, otherwise I would have had to fall back on my English and be a school teacher.

There were two industrial psychologists in Oeser's team who went off on their own. The economist, the sociologist and I worked pretty closely. We lived for the two major years of the project in Dundee. Occasionally we used to go over to St. Andrews where Oscar Oeser headed the Department of Psychology. He wasnÕt an effective organizer of our work, and the team never really worked as a complete whole.

With the economist, I was analyzing a large sample of Department of Labour records about the long-term unemployed. I had got married in the United States to a very intelligent girl, and she worked on this too.

The amount of work we had to do in finding data, and transcribing it, was monumental. The data showed that long-term unemployment – not so much unemployment itself – was due to a whole constellation of factors in Dundee. I analyzed records of juveniles' attitudes to getting jobs as the lads were coming out of school. They went to the 'low mill' in the jute industry until they were eighteen and then tried to get other jobs but met barriers in the local community. A lot of them would join the army. They would go in for seven years, come out again, and then have another big struggle to find employment.

The sociological side of their life was met by a psychological reality which bound them into long-term unemployment, and they couldn't get out. I didn't write it up, but I had analyzed the data, and I sent a report to Oscar Oeser who was going to write a book about it in Australia.

POLITICS BEFORE WORLD WAR II

I was politically involved with Spain before World War II, and with the unemployed. We knew what was coming up, and it was very, very hard to just be a psychologist. In 1938 I was beginning to get worried when I saw the Popular Front was not going to be a success in Britain. I sweated my guts out to help that one along, especially when Stafford Cripps came up to a big meeting in Dundee. But I just had no heart left to believe in these things any more. War was coming, and I had just got to prepare for war. Had Britain gone into the Popular Front in Spain the rot may have been stopped and we may not have needed to have had World War II. It was stoppable in Spain about the time of Guernica in the late 1930s. One was disgusted with one's own government in those days. It was awful. I read a book on the diplomacy just before World War II broke out. I was amazed at how everybody bluffed everybody else.

After my last year on the Dundee unemployment project at St. Andrews, I stayed on as Acting Head of the Department of Psychology. I replaced Oeser who had gone to Harvard for a year. When he came back war broke out. That period at St. Andrews was pretty rough. I was administrating this Department, never having taught in it, from September 1938 to June 1939. Then I left St. Andrews and lived in Dover with my family and my American relatives who were visiting us, and had got caught in Britain at the outbreak of war.

WORLD WAR II AND THE MAKING OF A CLINICIAN

What was I going to do in the war? I simply didn't know. I didn't volunteer for the army although I had had some association with the military in the cadets when I was at school. Then, just as I had given up trying to think about what to do with the War, there was an advertisement in The Times for a job in Columbia for someone with a background in English. I applied and, mercifully, was turned down. Philip Vernon, my tutor at Cambridge, who had been the clinical psychologist at the Maudsley Psychiatric Hospital, had moved into the armed services as a psychological adviser. He recommended me to Sir Aubrey Lewis at the Maudsley, as a clinical psychologist, and Sir Aubrey accepted me. So for the first two years of the war I was a clinical psychologist in the part of the Maudsley housed in Mill Hill School, London. The first war casualties came from Dunkirk and most of the mental casualties were sent to us. In the summer of 1940 when the London blitzes started, some very frightened people came out of their rooms, ran all over the grounds and we had to go and find them.

One very interesting assignment came from the National Head Injuries Committee which was looking at similarities with what had happened in the trenches in World War I when soldiers would put their heads up from the trenches and get head injuries. I was asked to do a study of closed head injuries, especially the psychological repercussions of those injuries, which Sir Aubrey Lewis suspected might be picked up by a psychologist before being identified neurologically. I analyzed the data in great detail and gave a paper on closed head injuries at the Royal Society of Medicine. That was one of the first papers I published. My wife worked with me on it. Her family was back in America by this time.

After two years at Mill Hill I was very well experienced in clinical psychology because every kind of psychiatry was there, including psycho-analysis. It was a teaching hospital and I learned a lot from its seminars. I was one of few people very well grounded in clinical psychology in Britain at that time.

While I was at Mill Hill, people from the Tavistock Clinic, who had gone into the army, visited the hospital, saw what I was doing, were impressed and asked me to join them. I was in a reserved occupation and couldn't be released. Sir Aubrey Lewis wouldn't let me go. But no one could prevent my volunteering to join the armed services. So I volunteered and joined the Tavistock group in the army. At the Maudsley people were furious because they didn't at all approve of the Tavvy. To them I had committed treason, I was a deserter. But they could do nothing. I moved into the military because there was very much more scope there than at Mill Hill and I wanted to be with the Tavistock people. I had got stale at Mill Hill. I recommended Hans J. Eysenck as my successor and he performed incredibly well.

My wife and I went to Edinburgh where I was the psychologist for the experimental work on War Office Selection Boards (WOSBs). I then became Senior Psychologist for the whole development of WOSBs. I was a Captain, but within eighteen months I was promoted to Lieutenant Colonel.

In the WOSB experiment I worked with Majors Jock Sutherland and Wilfred Bion. The scheme was first suggested by the late Ferguson Rodger, a psychiatrist. He had the idea of a group of selectors working with a group of candidates. But the form of the Boards was developed by Sutherland, Bion and myself. A very good account of this is in Hugh MurrayÕs article in Volume I of the history of our work - The Social Engagement of Social Science (SESS)

. My first job was to devise a psychological test program with intelligence tests, projective tests like the Thematic Apperception Test (TAT) and a life history questionnaire. At that time I had the only copy of Henry A. Murray's TAT in Britain and I kept it longer than I should out of the Cambridge library.

This was 1942 and my son, Alan, was born in Edinburgh. We were then transferred to Pierpoint MorganÕs country house near London, and the family came back to London. We worked on WOSBs for three months before they became operational – see the Introduction to The Social Engagement of Social Science (SESS).

During the last two years of the war, I was the Chief Psychologist to the Civil Resettlement Units (CRUs), for repatriated prisoners of war. The CRUs were the second therapeutic community. The first, at Northfield Military Hospital, was based on a proposal by Wilfred Bion in a memorandum in 1940 – the Wharcliffe Memorandum – which was never fully carried out.

In CRUs we first interviewed repatriates and escapers from 1943 onwards. Tommy Wilson conceived the scheme when doing morale studies in the Middle East; he had found that when people were separated from their relations they tended to go a bit haywire, especially after eighteen months away. Suspicions and other symptoms arose. Tommy Wilson asked for me to work on its planning and development. The aim was to devise a therapeutic community for helping repatriated prisoners of war to adjust to their home society from which they had been absent for five years. They were at first those from the German camps, most of whom had been captured at Dunkirk early in the war; later, others came from the Japanese camps. In Britain they lived in a special residence. It was very, very carefully worked out and an account of the CRUs appears in Volume I of The Social Engagement of Social Science.

My time as Senior Psychologist in the WOSBs was very exciting because I had a lot of development work to do, designing the first follow-ups and being a policy adviser. But the CRUs were probably the most exciting single experience of my professional life. It was a tremendous success and broke very new ground. I invented the terms 'social reconnection' and 'de-socialization.' I wanted to introduce a new terminology which was neutral, psychiatrically. We couldn't call these people 'patients' or 'clients', or anything with therapeutic overtones. We had to train ordinary soldiers to do this work because there were very few technical people available in psychology or psychiatry. We had twenty CRUs with an average attendance of 240 at any one time and an average stay of one month. Also there was an extension scheme for the people who didn't or wouldn't come to the CRUs. We would visit every one of those people in their homes. Altogether it was a very moving experience.

During the war we created these social systems, such as WOSBs and CRUs, within the military for the solution of key problems that werenÕt solvable by ordinary military methods. I wasn't demobilized until September 1946. By this time a group had formed of young psychiatrists who had gone into army psychiatry, and, as a group, had spearheaded all of these social system creations. They drew in many other people and became known as "the Tavistock Group."

Before World War II the Tavistock Clinic had become a professional democracy. Towards the end of the war there was a postal ballot of members of the Clinic asking them who they wanted on the Post War Planning Committee. The key people in the army group were elected to that Committee, and they asked Jock Sutherland and myself to join them. From the beginning I was in on all these plans. The Tavistock Institute of Human Relations, as distinct from the Tavistock Clinic, was formed. We had a starting grant from the Rockefeller Foundation in February 1946 when the Institute and the Clinic were one. Then the National Health Service came into being in Britain, so we had to prepare the Clinic to enter that scheme and establish it as an 'out-patients' psychiatric facility. It was based theoretically on depth-psychology, particularly the object-relations approach in psychoanalysis. One of the first appointments was John Bowlby who was to be Head of the Department of Children and Parents. Three or four leading army psychiatrists, who weren't at the Tavistock but who were in London before the war, were appointed.

Among the army group I had experienced psychoanalysis as an important way of viewing the wartime projects. We found that the object-relations approach linked the social and psychological fields. Not many of the people at this time were analysts – they were trained after the war – but they were psychoanalytically inclined people, and they had the understanding and skills which had worked in practice.

Immediately after the war we began to enter psychoanalytic training. At that time it was a rule that everybody at the Tavistock went into psychoanalysis. I am not an official Kleinian, though much influenced by her views, particularly her theory of the two developmental positions: the theory of manic-depressive states and schizoid mechanisms and the envy and gratitude theory. But I have also been very influenced by my colleague, John Bowlby, and his work on mother-child separation; by Winnicott on the concept of the facilitating environment; and by Bion and Sutherland.

As Melanie Klein aged she turned more inward and paid less attention to the environment. Meanwhile at the Tavistock we paid major attention to the environment, and became interested in social applications of psychoanalysis. As I developed, I didn't confine my attentions or sympathies to any single form of psychoanalysis. Also I became interested in Jung. I always had an independent mind in social psychology, and I tried to link it to the object-relations approach. In classical psychoanalysis Oedipus had number one place, but for me now, as the field of mother-child relations opened up, it wasn't number one.

The British government was very worried about the economy and, under Sir Stafford Cripps, formed a Productivity Committee which had a Human Factors Panel administered by the Medical Research Council. The Tavistock had three projects with the Council: the Glacier Metal project, which studied group relations in depth at all levels; a coal-mining project; and a project to develop a method for training people in postgraduate fieldwork in industry. We had six fellows for two years, one of whom was my pupil, Ken Bamforth. It was a very elaborate scheme based on experiential learning. All the fellows were in the Glacier Metal Project where they had a common field experience; they were all in some other project in the Institute; they were all in a therapy group; and, finally, they had their own group which looked at their own prejudices and problems. Universities would have nothing to do with us. There was great hostility to both the Tavistock Clinic and the Institute. How the hell we survived was a miracle.

Wilfred Bion and I were very close throughout the whole of the War and I was in BionÕs original therapy group as his assistant. In the late 1940s he wanted me to go into practice with him working with groups. That was impossible for me, because I was Deputy Chairman of the Institute, committed full-time to its projects. It would have been a big mistake to join Bion because he left groups in the 1950s – which flummoxed everybody – and got completely absorbed in psychoanalysis, though he didn't lose his sense of the social field. Very few people knew exactly what group work he had done; even so all the psychiatrists in the Tavistock Clinic started taking groups. For the psychiatrists one-on-one treatment wouldnÕt do. They had to develop a flow of enough patients to be cost effective in the National Health Service. Developments of the Tavistock always were highly pragmatic and linked to the realities of the society. Group therapy was not, in the beginning, a theory; rather it was something we did. And nobody exactly followed Bion. He was only followed exactly in Bethel-type labs that we developed with the University of Leicester. That was when the cult of Bion – a wrong cult in my view – became established.

The first studies that led the Tavistock Institute to find an identity were in industry – the Glacier Metal Project and the coal project. The first coal study was in Ken BamforthÕs original pit, and was stopped by the Divisional Board in Yorkshire because it did not wish to have attention drawn to work in autonomous groups. This was an early intimation to us of the resistance and the strength of the opposition to organizational change.

We got going again in East Midlands Division, but were again stopped when the Divisional Board wouldnÕt support us. So we had to go all over the British coal fields until we found, in Durham, one with a sympathetic Area General Manager. He was James Nimmo, an outstanding individual who had been at my college in Cambridge. Sir Sam Watson, Regional Secretary of the National Union of Mineworkers in the Durham Area, actively supported our work from the beginning.

The original paper on our coal project study was published in 1951. 1 had also delivered papers in 1950 to the Industrial Section of the British Psychological Society and to the British Association. But we weren't allowed to publish on the autonomous groups. Again the Divisional Board didnÕt want it referred to. We had the choice then of either playing along with the industry or not; if we had once left, we would never have got back in. So we kept our mouths shut for a time. Tommy Wilson mentioned the work in his Lewin Lecture, when the Institute, as a group, was given the Lewin Award in 1951.

Another major project was with the Family Welfare Association. In Britain with the inception of the welfare state, their previous work, which had been for the material alleviation of extreme poverty, was no longer relevant. They were besieged by people with emotional and social problems. The staff werenÕt able to cope with the new problems, so the head of the Association, Enid Eichholz, who had been head of the Civil Assistance Boards during the war, consulted Wilson. This led to the formation of what was first called 'The Family Discussion Group,' which later became the Institute of Marital Studies. Its methodology was developed by Michael Balint, a senior psychoanalyst; later he worked with general practitioners and with all health professionals, and marital studies became a major undertaking of the Tavistock Institute. There was also the beginning of Bowlby's world famous studies on mother-child separation and the establishment of family systems therapy. The creativeness in the early years was very, very great.

1951 saw the end of the Medical Research Council grants for industrial research at the Tavistock Institute. We weren't recognized as fit to receive funds from any British source, foundation or government, at that time, The Tavistock Clinic got extra funds from American foundations. The

Rockefeller Foundation's funds went largely into the clinical field, while the new Ford Foundation funds were to go to the social and industrial field. In 1951 we had put up a proposal for a grant from the Ford Foundation which unexpectedly fell through.

So we had to do consulting for industry and find out if we could pay our way. The great project which was our salvation was with Unilever. Lord Heyworth, who was then Chairman of Unilever, had become interested in WOSBs during the war; and, because Unilever were going to expand, he had a huge problem of selection of managers. At the time there was a lot of nepotism and he wasn't going to let it continue. We developed conjointly with them what became known as the Unilever Companies' Management Development program. Selection procedures were derived from WOSB techniques and training utilized group methods and related techniques. This became the big bread-and-butter line for the Institute. Today it is a network organization, and has been developed in a most amazing way by my colleague, Harold Bridger.

Then we were asked to start consumer studies by people who had got to know us during the War. In fact all our early projects came from wartime contacts. It was because we werenÕt generally approved of at the time that our work had to come by that kind of route. The first consumer study we did was with Mars. I was highly involved in that one and created a concept that was both Lewinian and psychoanalytic. It was called the 'pleasure foods region,' and it referred to products, such as confectionery, alcohol, and tobacco, that were not of much nutritional value, but met psychological needs. The extended studies and theoretical development awaited the arrival of my Australian colleague, Fred Emery.

We had to do something to get a reputable name for the Tavistock Institute. Our policy was to establish the journal, Human Relations, with Kurt Lewin's group in the United States. His notions of action-research were parallel with our socio-clinical, action-oriented work and I was regarded as his representative in Britain. A lot of his field theory was very congenial to some of my colleagues. Establishing a connection between Lewin's group, later situated at the University of Michigan in Ann Arbor, and our work was primarily my endeavor. If I hadn't been to the United States in the 1930s it wouldn't have happened. Lewin was enthusiastic and wrote two celebrated papers for the first two numbers of Human Relations. He died just before they were published. His people in Ann Arbor carried on after his death.

Human Relations succeeded in establishing us internationally, especially in the United States; and it gave us an outlet for our kind of work. Its articles wouldn't have been accepted by any of the other British psychological journals. For the same reason, we also had to establish a publishing company – Tavistock Publications – otherwise Elliott Jaques's book, The Changing Culture of a Factory, would not have been published in 1951.

After World War II, in the early days of the Tavistock, I was the first nonpsychiatrist. I was essentially a clinical social-psychologist, and nobody had my particular tradition. I was very lonely, and although I had most cooperative colleagues, I didn't have anyone that I could test my thinking with. Also I was so busy and so occupied with institution-building and policy matters, that I got out of date. I'd already had the bulk of the war period getting out of date. I was very quickly thrust into a policy-making role in the army and had been promoted very quickly. Then the Tavistock grew so rapidly that I felt I couldn't maintain myself technically to the extent that I might have done. But they weren't the only reasons. I had dreadful trouble with my personal life; my wife became very ill and eventually died. I remarried in 1959 and our daughter, Carolyn, was born in 1962.

In Britain my career had four phases. The first phase was becoming a social psychologist with the study of the social and psychological factors in long-term unemployment in Dundee; the second was really in group dynamics, which I learned during the war and afterwards in a psychoanalytic context; third came the socio-technical system ideas from the coal project; and fourth, development of the idea of socio-organizational ecology which dates from a joint paper with Fred Emery in 1965 on "The Causal Texture of the Organizational Environment."

BACK TO AMERICA – FROM 1966 TO THE PRESENT

I had developed close connections with a number of people in the United States, especially during 1960-61 when I was a Fellow at the Center for Advanced Study in the Behavioral Sciences at Palo Alto. And I had two colleagues in the Behavioral Sciences Group in the Management School at the University of California at Los Angeles (UCLA). One was Bob Tannenbaum, a close personal friend of mine, with whom I trained a T-Group at Bethel. Although we used entirely different methods, we managed to complement each other, and worked very well together. Another member of the UCLA staff was William McWhinney who came over to Britain to the Department of Industry at Leeds University. I became a consultant to their studies and used to go up to Leeds University at least once a month. I liked McWhinney tremendously. When he went back to the United States he proposed me as Regent's Lecturer at UCLA in 1964. When I came back to England after that month I received a letter from the then department chairman asking if I would be interested to come to UCLA permanently.

At the time Beulah, my wife, had just been told that for her health she should live in a drier and warmer climate. After a lot of meditation I decided to accept the appointment, and for about six months I worked on it with the Tavistock people.

My appointment at UCLA started in July 1966. To go to America permanently wasn't part of my career plan, but I had been at the Tavistock since the beginning, and I felt it was time I left.

The irony of the decision to go to a drier climate because of Beulah's health appeared when one of my medical friends from London came to UCLA during the second year I was there. He said he would like to see her X-rays. He said the diagnosis was wrong, and was absolutely furious with the Harley Street specialist who had made it.

At UCLA I was professor of Organizational Behavior and Social Ecology in the Graduate School of Business Administration until 1969. I had been asked to go there by the Behavioral Science people, but I found that I wasn't in their group. I was put in a group called Management Theory, with people I'd never heard of. I was in a new country, in a new department and I didnÕt know the politics – so there wasn't very much that I could do.

Then Russell Ackoff from University of Pennsylvania came out with his Dean and asked me why I hadn't come over to him instead of UCLA? He was very upset and offended. Earlier Russ had come to England on a sabbatical leave and we saw a great deal of him when he played a major role with the British Operational Research Society, getting together their social science inclined group with our people in the Tavistock. Fred Emery met him and they discovered their common interest in Singer's ideas and Sommerhoff's theory of directive correlations, and began their book On Purposeful Systems. That was a very big development. I stayed at UCLA from September 1966 to July 1969, working at a distance from, but in collaboration with, Lou Davis and other people. I taught a Ph.D. seminar and worked with MBAs. At UCLA they gave me a very good deal financially and, had the dean been quicker and got me a named chair, I would probably have stayed. He talked about it, but was too slow. I was also worried about my son, Alan, who had become very ill in London.

ACROSS AMERICA TO PENNSYLVANIA

You're much nearer to London on the east coast of America. So I was drawn to the east coast and in 1969 began work as Professor of Organizational Behavior and Social Ecology at the Wharton School with Russell Ackoff. I became Chairman of the Management and Behavioral Science Center at the University of Pennsylvania and had a large Ph.D. program there for almost ten years.

I started a big project in the medical school center at the University with one or two of my colleagues, and others with Russ. Very soon, I got a project at Rushton, a coal mine in Pennsylvania. We wanted to see if the methods we had developed in England's coal mine studies could be transferred to the United States where the technology and culture were different.

It was an independent mine, not part of a big outfit. Arthur Miller had become general secretary of the mine workers union. Theyd had a huge tragedy and a row with the previous secretary – there had even been murders. 'Miners For Democracy' came out of all of this trouble. So I got that wave of union support. The owner of Rushton Mine was a Christian Scientist with extremely advanced social views about industrial organization. He was very charismatic and gave the project his complete support. Nevertheless, we did meet resistance to the changes we were seeking. In that project I had two staff not from the University of Pennsylvania: Gerry Susman, whom I had known at UCLA, and Grant Brown, a mining engineer from the Department of Mines and Minerals at Penn State. I used to go up there twice a month. In a way this project failed, and in a way it succeeded, and the incredible story about that is reported in Volume II of SESS

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Then I did the Jamestown Project in a manufacturing town in northwestern New York State. I wanted to move to another system level. Jamestown was the first small town where innovative industrial cooperation took place. This was initiated by the then mayor, Stan Lundine, who has since been in Congress and is now Lientenant Governor of New York State. I heard about this through a colleague, Neal Herrick, who directed me to Jamestown. I went up there to see them, and they became interested. So I took one of my graduate students up for the summer and we made an anthropological survey of the whole place, and presented proposals which were accepted. In Jamestown there existed an institution, the Jamestown Area Labor Management Committee, that was at a higher system level than the individual companies concerned. We found that having the commitment of this overall body had a stabilizing effect. Local small projects would go up and down but they would hold because of the Committee. This led me to what I called the Õfunction of a continuant.Õ I introduced the concept publicly in Oslo in 1987. The term comes from a book on logic by W. E. P. Johnson, the Cambridge philosopher, written in 1924. It was then mentioned in an article by Maurice Ginsberg in the mid-1930s. I had a new use for it, namely the need for a point of stability in a change-making organization.

One of the big troubles in change-making organizations is that they have no resources, they are very unstable, and the field that they are concerned with often just collapses. The Tavistock projects were full of this, with one or two exceptions, like the Unilever project. So I made this into a theory. It was the first time any socio-technical work had been done at the community level, while the mining project was actually the first research-funded, socio-technical study started in the United States.

We had another big project going in the public sector. At the time there was trouble about laws like Proposition 13 in California, that is, a policy of cutting back on government expenditure. What we called Project Network comprised projects in twelve cities at that time. Again, this will be reported in Volume III of SESS.

Also there was a major project with a large international engineering company which spanned ten years from the early '70s. They had sixty or seventy socio-technical studies going in various places, and nothing was known about these at the center of the firm. They all failed eventually, or were phased out, except two. The first was in another country – Canada – and the second was in a new plant in the United States with new technology. But all the other projects faded even though some of them were marvelously successful for three or four years. My interest was to find out why the projects failed, and why there was no communication of the studies to the center. We found that the projects had been initiated by managers who had picked up something at a conference, got things going, and when they were transferred the project would collapse. This project was published in 1982 and will be republished in Volume III of SESS.

In 1970-71 Fred Emery came over and we decided to put our work together in the book, Towards a Social Ecology: Contextual Appreciations of the Future in the Present. Unfortunately, its publication was delayed for three years because of a big row between publishers in Britain and New York. Otherwise, our book would have been out before Don Schon's Beyond the Stable State, and Alvin Toffler's Future Shock. That delay damaged our appeal very much, as did the conceptual difficulty of the work.

From 1973-74, after becoming separated from the work of Russ Ackoff, I carried on my projects at the University of Pennsylvania until 1978 when I was made emeritus.

TO CANADA

Meanwhile I had developed a long association with people in Canada. Michel Chevalier and others had known me at Tavistock, and some of them had been to the University of Pennsylvania. I was invited to go to Toronto and join the Faculty of Environmental Studies at York University as Professor of Organizational Behaviour and Social Ecology. They were interested in developing their institutional, organizational side. So from 1978 to 1985, I went there, and I was very happy.

At York, I was adviser to Labour Canada. I had projects with them, and for two years was going all over the country getting very involved with people starting up Quality of Working Life (QWL) projects. We had a community group in Sudbury in northwestern Ontario, a regional project in Nova Scotia, and started Search Conferences in Alberta. My major task was to consult and advise, and set up projects and see them through.

In 1979, 1 found out that I had coronary artery disease. A triple by-pass operation was done in May 1983. I stayed at York University until 1985.

I went to Minneapolis for the fall term as a guest professor, at the invitation of Andy Van de Ven, Chairman of the Department of Management at the University of Minnesota. I was invited to stay but the climate made it impossible. I retired to Gainesville, Florida.

SCIENCE POLICY AND FUTURE STUDIES

I had introduced future studies at York and taught a course for two or three years which meant an immense amount of work. I was headed in that direction and that's where I ended academically.

During the '60s concern regarding recognition of, and government support of, social research had led to the formation of a Social Science Research Council in Britain. Sir Hugh Beaver, the Chairman of the Tavistock Council, wanted the Institute to take a public position on these issues and asked me to do a monograph on our experience. This attracted the interest of OECD and then of UNESCO. I became a member of the latterÕs committee on Research Trends in the Human and Social Sciences and produced a report on Social Science Policy: The Organization and Financing of Social Research. This was published in 1970 as part of the overall UNESCO study and as an independent monograph. In it, I included a critique of academic individualism and an account of a new type of social science illustrated in the work of the Tavistock – in addition to basic research and standard applied research there is a third type concerned with

emerging societal problems and involving the stakeholders in projects of actively inquiring into them (cf. SESS).

This led me to extend my empirical interests from socio-technical projects to the wider field of social ecology and future studies.

Methodologically, I moved from action research to the search conference and then into action learning. I would start micro-exploratory processes in the field, then get people concerned with them, and in turn I would get involved with them. Always I believed that the methodology would lead to new insights, and that in the social sciences not everything would be found out or done by conventional methods.

In the early days of action research and action learning we couldn't always find organizations that had a 'continuant function.' Often we got support from people higher up, but that led to problems when they didn't follow through. We found, repeatedly, that the political problems of the action researcher are monumental. Otherwise every project would be a success!

Now - with Hugh Murray and Fred Emery - Beulah, and I are editing The Social Engagement of Social Science

, a three-volume collection of writings giving an account of the work of the Tavistock Institute. The first volume was published by the University of Pennsylvania Press in June 1990 and the second in July 1993. I conceived the book in three perspectives. The socio-psychological perspective is about our studies in groups and organizations. It came from our wartime work. Then comes the socio-technical perspective which grew from work of my own in the early '50s and, finally, the socio-ecological perspective, which is more recent and expands the earlier work to wider systems. I am trying to put all the work together in one standing collection with over thirty contributors, and co-editors from Europe, Australia, and America.

Eric L. Trist died on June 4, 1993

* **"Guilty of Enthusiasm"** was based on interviews between Trist and Richard C. S. Trahair in 1989. Trahair, who teaches and does biographical research at the School of Social Sciences,La Trobe University, Melbourne, Australia, edited the interviews for inclusion in "Management Laureates."]

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