

Kurt Back, "Field Theory, Truth & Beauty".

F. Emery, April 95.

Back makes two points up front in his paper:-

a) that Lewin's field theory was empty of scientific content and

thus received no more than cursory and transient recognition in psychology.

b) that the faddish acceptance of Lewin's field theory is to be explained by what was happening in the world of the arts - "the appeal of his work is closer to that of other creative products of contemporary society than of scientific deduction" (p14).

I do not agree with the first point and hence find the extended treatment of contemporary movements in the arts to be irrelevant. (Although I do not disagree with the broader point that acceptance of field theory is related to some of the movements in the social climate).

However, Back introduces a third point, "the Faustian bargain". With respect to this bargain he allows that Lewin's field theory might have been empty, but not useless. It was not useless because it made at least a gesture of defying the Faustian bargain that science had entered into and "perhaps in this gesture it symbolizes the spirit of modernism" (p15). This spirit of modernism, according to Back, was being carried forward mainly by the arts. I do agree with Kurt Back that science struck a Faustian bargain and that the reaction to Lewin's field theory had much to do with this. I think this point needs to be further developed.

Before discussing the Faustian bargain let me first explain my rejection of Back's first point, and hence his second point.

It is simply not true that Lewin failed to define what he meant by the concepts of 'field' and 'field of psychological forces'. Lewin (1936, p 216) gave the same definition of 'field' that Back uses to spell out Galileo's conceptual contribution (p2). At this point a quite inexplicable gap emerges. Back suggests that "It seems prudent not to attempt a more ambitious and complex definition...Lewin really did not go much farther in his basic writings in producing a definition of field theory; later field theorists in psychology and Lewinian scholars have avoided precise definitions as well" (p2). This is simply not true. Lewin did not assume that he had only to copy Maxwell's theory of fields of electro-magnetic forces nor the imaginary fields of gravitational forces. I was of the impression that Lewin went out of his way to argue that fields of psychological forces could not be reduced to the metrical fields of physical science. Lewin, as I read him, did go out of his way to go very much further, "in his basic writings", than anyone had been brave enough to go before. He, Lewin, insisted that $B = f(P \times E)$; just as the area of a rectangular figure equals height times base. A great number of combinations of height and base, or P & E, can produce equivalent results and in the absence of one of the basic factors we have neither any area nor any behaviour. Lewin went further to assert that new terminology, eg valences, was needed by psychology to refer to the field forces that acted to determine behaviour.

What I find inexplicable is Kurt Back's assertion that Lewin did not go much farther than Galileo's notion that "the properties of space itself are the determining factors" (p2). Quite to the contrary, Kurt Lewin asserted that the properties of psychological space are not the properties of physical space. How much farther can one go from Back's perception? Well it so happens that Lewin went further. Lewin asserted that change, the dynamics of human behaviour, was what should be the central concern of the social sciences, not the statics of structural arrangements. In asserting the importance of change and the psychological

environment Lewin placed himself directly against the vastly respected views of Stevens in psychophysics and Hull in learning theory. I think that Graumann's citation study should be interpreted in this light. Articles having the words 'field theory' in their title were unlikely to be published in other than the journal, Human Relations. The same concerns with change and the psychological environment could, and did, find expression under the labels of 'open systems', 'ecological psychology' and 'contextualism'. Even at the Tavistock Institute of Human Relations, a stronghold of Lewinianism, the label of 'open systems' took first place without in any way being seen as a rejection of field theory: it was just that Lewin's concepts for describing P in the equation $B = f(P \times E)$ seemed less than appropriate for a field-theoretical description (Rivera, 19).

There is one sense in which Back is quite correct in describing Lewin's field theory as empty. Lewin was espousing the new world view that was emerging with the rise of the sciences. Amongst the few that sensed this emergent were Charles Peirce, Ernst Cassirer and von Uexkull. (If the year 1910 has any significance for the development of Lewin's field theory it is not because of the art exhibitions organized by Roger Fry nor the musings of Virginia Woolf: it was because that was the year that Cassirer published Substance and Function which did so much to shape the new scientific world-view.) It was not until 1942 that Stephen Pepper was able to label this new world hypothesis as contextualism. For those who were locked into the traditional world hypotheses of Aristotelianism, mechanism or vitalism Lewin's message did seem empty of any meaningful messages. In fact, it seemed remarkably wrong-headed and threatened the very precarious foot-hold that the social sciences had achieved in some of the universities.

This brings us to Kurt Back's third point about the Faustian bargain. The history of the universities in Europe, Britain and the USA suggests that the critical Faustian bargain was reached in order to get science into those bodies. The price for sharing the protected and highly subsidized life of the universities was that the sciences stuck to their test tubes, labs and herbariums and did not engage in matters that traditionally were the preserve of theologians and the so-called humanities. Pepper pointed out that corroboration of findings required both multiplicative evidence, the repeatability of observations or experiments, and the structural evidence that comes from relating findings to what has been established in other related fields. The latter, structural corroboration and hypotheses related to that sort of corroboration, became verboten in university science. Only increasing specialization about increasingly narrow forms of multiplicative corroboration was left for further development of the sciences. I was made very conscious of what the Faustian bargain implied when I, with Merrelyn Emery, put forward an hypothesis about the difficulties humans had with receiving and processing televised messages. That was a hypothesis that arose from trans-disciplinary research and required structural corroboration. Within five days six professors from different departments in the Australian National University had banded together, in quite an unprecedented way, to publicly state that developments in their specialty could not possibly permit them to advance such a hypothesis for many decades to come. Within twenty years multiplicative corroboration for our hypothesis emerged, accidentally, from advances in tomography. Structural corroboration will probably never emerge from any of those six departments. Not least of the reasons will be the fact that by then the six will have become twelve, or more. As always they will not communicate to each other nor read each others special journals because they neither

understand nor wish to understand the special language and procedures of the others.

Lewin was challenging the Faustian bargain by real experimental research, not just gestures, and he paid the price. He was never invited to a chair in any major US university. If he had stuck to verbal gestures room would probably been found for a mind as fertile as his. His dream of trans-disciplinary research and an integrated social science was realistic in the years just following World War II. By the fifties the universities were strangling all of the multi-disciplinary offsprings of such a vision.

Despite the world-wide watering down of the concept of a university I do not think that things have changed for field theories in the social sciences. That this might be the scientific way to go is not just irrelevant to universities but is seen as poisonous to what universities have conceived as their traditional task- the selection and certification of social elites. The field theoretical approach would have to be thoroughly sanitized to get through the gates of academia and get a secure lodgement. Such sanitization would require strict adherence to nominalism, operational definition of its terms and restriction of its activities to conventional hypotheses that could be tested by standard statistical designs. Perhaps the Michigan Center for Group Dynamics would be the model for such sanitization.

Truth and beauty are relevant criteria but neither is a Platonic ideal. Each world hypothesis sees truth and beauty in its own way. Unlike Pepper I see contextualism, and hence field theory, as providing a much more adequate world hypothesis for judging such matters. It is simply an unfortunate fact of history that universities remain locked into the old world hypotheses. The Faustian bargain is still in place and the places for scientists in universities are still reserved for those that are pledged to the bargain. Structural corroboration might be the royal road for the progress of the sciences but university scientists who advance hypotheses that require structural corroboration are an endangered species.

Dear Ana and Bob,

Please feel free to delete any or all of the above. I do not know the context, political and otherwise, and rely on your judgement. But please fax me if you want a change of meaning.

Fred (fax: Australia,06 257 3421)
or e mail to Merrelyn.Emery@anu.edu.au

e-mail from Ana & Bob. (summarized).

Please expand on following points so that it is not just a reply to Kurt but something that stands as an independent contribution.

1. Field theory as a threat to normal social science.
2. Field theory has political implications.
3. Field theory has implications beyond science.
4. The effect of specialization in science and the social sciences.
5. The 1950's and McCarthyism.

Fax to Dr Marjanovic-Shane USA 215 843 2288.

Dear Ana & Bob,

26 Nov 95

Your e-mail message was received by Merrelyn and I readily agreed with your request. It seemed a very straight forward request. When I re-read my comments on Kurt's paper the matter was no longer clear, and the e-mail message was traipsing around the western USA in Merrelyn's computer ! I really do not know what to add to my comments on Kurt's paper, short of a book (which I am busy writing, anyway). Some of my

views are contentious but they seem to be clearly stated. The most contentious issue seems to be that of the 'Faustian bargain', and Kurt introduced that. I fully agree with Kurt's implication that a meaningful history of science cannot be written without this a central theme. I sought only to concretize this by referring to the role of the universities.

You will have to fax me details for me to know what to do. Merrelyn's computer is now on the New Guinea border so I do not recommend e-mail.

Fred Emery (fax: Australia 06 279 8066).