

Reminiscences of J. F. Brown, Psychology & the Social Order.1936.

F. Emery Oct 94.

It is an unexpected pleasure to have in my possession a copy of this book. The last of my three successive copies was borrowed before I left Melbourne at the end of 1957. The copy at the ANU library had disappeared before I returned to Australia in 1969.

A little history might explain my attachment to this book.

As I recollect I first discovered Brown during the couple of weeks I spent in the uni library in late January 1944. I was between my agricultural jobs and taking a bike tour of the south-west; before starting my second year at uni. I knew I was going to be doing social psychology so I delved into that small section of the library, and found Brown. I got my first personal copy of Brown through Alberts Bookshop in Perth about six months later.

During my first year at uni I had done physics, chemistry and maths but had been allowed to also do psychology. By the end of the year I had decided to major in psychology. What had been presented to me as psychology was very disappointing. Stuff from McDougall's books about instincts, Spearman's theory of the structure of intelligence and Fechner's psychophysics just did not ring true with my uneducated experience and unguided reading. I had learnt that psychology was divided into many schools of thought but, to a very young fellow, that seemed only to indicate a pre-scientific stage. In making the decision to major in psychology I had no idea of what career might follow (and fortunately I had not heard of the mind-deadening careers that were emerging for psychologists in mental testing and vocational guidance). The careers open to physicists and chemists were clear, as we then thought, and awfully dull.

Coming across Brown was quite unbelievably exciting.

Brown was spelling out in some detail the feelings I was only beginning to formulate about psychology and such of sociology that I had chanced to read. Brown agreed that psychology and sociology were full of scientific pretensions but unscientific in their theories and their methods. I had read quite a few such criticisms from leading figures in the field. But those critics were vague on the reasons and offered no alternative. He spelt out cogent reasons for believing that they were stuck in this state because they would not, or could not, free themselves of the Aristotelean ways of thinking that Thomas of Aquinas stamped on Western thought. That was music to my ears. I did not know enough to spell out this feeling but Brown had this knowledge.

Brown went beyond this criticism to spell out the Lewinian program for the development of a science of behavior. This program certainly reinforced my acceptance of the criticisms. I had no difficulty at all in accepting that the space and time in which humans act is not that of the clock and the foot-rule (ie of the physicists). I had no difficulty in accepting that psychology would have to make do with ordinal measures. I did have difficulty with the notion of only using ahistorical explanations and with Brown's rejection of free-will. I accepted these as the lesser of presenting evils and matters to be eventually sorted out. (Chein was to do precisely that, for both issues). Brown himself argued that to make use of Freud's brilliant insights psychology had to attend to historical facts as we did not have the means of measuring many contemporaneous determinants.

I had no trouble with Brown's extended treatment of nations, classes, minorities etc. That seemed the natural context for social psychology. In 1943, and for quite a few years after, peacetime society seemed very much like that which Brown was depicting in 1936. The points of view that he was countering with his field-theoretical interpretations were very real and powerful points of view in those days. "Class-theoretical" (Aristotelean) explanations abounded, particularly in the texts for university students, of national antipathies, prejudices race riots and lynchings, religious intolerances, hatred of unions etc. No student of social psychology could ignore these things. The staff of the WA psychology dept were riddled with these old fashioned perceptions

The same applies to Brown's long section on political science and his briefer sections on economics. Already at that time I shared his sympathies, liked the way he formulated the issues and much appreciated his reading lists because he pointed to his major theoretical antagonists. Spengler I had already caught up with but had not realized the strength that Pareto brought to his conservative position. (Only later was I to learn how central Pareto had been to the Harvard school of sociologists Homans and Parsons, and at second hand, Elton Mayo).

The fact that Brown was taking a minority position did not bother me. What did impress me was that in 1944, only eight years after first publishing, his predictions seemed to be borne out and his views were much more widely acceptable.

Lastly, I was very impressed by Brown's discovery, after he had developed his field-theoretical approach (from Lewin), that it corresponded in most of its features with dialectical materialism. (Foot-note. p485) The main differences were over the role of history and the need for mathematical formulation. I had been introduced to dialectical materialism in 1943 so it was a relief to find that my interests were not tugging me in opposing directions. I had gone into logical positivism in 1943 because of its strong anti-theological and anti-metaphysical stance but found it too contradictory in claiming that the results of scientific activity were label changes and not the identification of new levels of reality (this was a couple of years before Hiroshima).

Looking back on Brown's contribution from 1994 what does it look like?

The critical remarks about the social sciences that Brown quotes could be repeated today, without change or qualification. (See Emery, "Australian Psychology", 1994). The reasons for the lack of development do not seem to have changed - an addiction to nominalism in practice, in measurement and experimentation, (crass inductionism) and, where theory is attempted, a reliance on nominalistic definition of 'class-theoretical' concepts. The language and the focii have changed in seemingly erratic ways.

What of the positive program that Brown (and Lewin) were trying to introduce?

The three volumes of the Tavistock Anthology show that all was not forgotten. But Tavistock was small, and marginal to the great burgeoning world of university based social science. When founded in 1947, with a significant Rockefeller grant, Tavistock was small but it was central to a widespread movement of universities to establish similar multi-disciplinary and practically oriented institutes. Within a couple of years that post-war hubris evaporated and the goals of social science were being defined by cold war warriors. The Ford Foundation emerged in this climate and instead of taking over the funding commitments temporarily assumed by the medical division of the Rockefeller struck the Tavistock from the list of 'suitable' European applicants. Movement to a field-theoretical position has taken place outside the universities, and despite them. Tavistock refused to be involved with the Cold War research tasks that overwhelmed Lazarsfeld's Bureau of Applied Social Research at Columbia University. It survived only because the peaceful pursuits of industry, commerce etc were becoming unexpectedly troublesome. The major influence appears to have been the welling-up of the counter-culture in the late sixties, and its challenge to just about all 'basic truths', institutional sanctities and traditional statuses. The Aristotelean mode of thought had reified these traditional forms of the distribution of social powers. Note, however, that the counter-culture had no positive alternative theory only a practice of communitarism, small group encounters, street theatre etc. The theorists they quoted, but did not read, like Marcuse, Laing and Watts, also presented no positive alternative theories.

I have discussed this question at some length in my paper on "The next generation of issues". Some further reflections seem warranted.

The parallel that Brown noted between his philosophy of science and dialectical materialism is no longer surprising. Brown was unaware of the debt his teachers, Nagel, Dewey

and Stebbing, owed to Peirce and the similarity of the problems Peirce and Marx faced in the mid 19th century of trying to grasp the significance of the scientific advances into organic chemistry and biology (eg Liebig and Darwin). He was unaware of Peirce's influence because American academics had buried him deeper than they had managed to bury Marx. Neither Marx nor Peirce seemed ever to think that the other was relevant to them. Marx was increasingly into economics and Peirce into the experimental sciences, logic and semiotics.

Of the two major differences that Brown noted, history and mathematical formalisation, Lewin's program has come out the worse for wear (this might seem a silly comment to make when Marx's program, as interpreted by Lenin and Stalin in Russia, has proven to be a quite unmitigated disaster :but I am referring to the scientific program). I would have added, even then, a third problem. That was the lack of clarity about the nature of the 'constructs' were displacing Aristotelean 'concepts' in science. Almost in passing Brown noted a fourth problem that he had, namely, how a vector (a directed force) was to be associated with a postulated tension within a person. This seemingly technical question was at the heart of describing the dynamics of the O-E system, as distinct from imposing on that system the imputed dynamics of the organism.

It was the persistence of these problems that lead to me taking on my honeymoon (Sept 1948) Mead's Philosophy of Time and Cassirer's Substance and Function. I still think that these four problems are still central.

On the question of time I think Mead and Chein gave us the necessary lead. Our reference point had to be empirically established temporal gestalts and overlapping temporal gestalts. The concept of a dimensionless 'instant of time' - present instant- literally defines time out of the picture. That concept of time has been at the roots of Western thinking. Aristotle built his logic on the premise that A is A and not not-A. Writing at the time Zeno spelt out the paradoxes into which that assumption led us. In particular Zeno pointed out that Aristotle's assumption ruled out the reality of change or development. The reply to Zeno was to torture him to death and declare that all change was in fact illusory. Reality was defined as the unchanging Forms (attributed, dubiously, to Plato), the phenotypical manifestations of unchanging entelechies (such as Hawkins' 'Selfish Gene") or the endless repetition of identical cycles of events. Both Peirce and Marx saw that that assumption had to be dropped by science. Lewin and Brown remained struggling on the Aristotelean hook.

Sorting out the problem of time was critical to fully spelling out the nature of scientific constructs. There was no difficulty in identifying the 'generic' nature of Aristotelean concepts. It was, however, short of satisfactory to simply identifying the serial nature of scientific constructs. Seriality in the form of A including B, B including C, and so on, could be simply a spatial phenomenon existing at 'an instant in time'. What Cassirer was observing in the emergence of modern chemistry were SERIAL-GENETIC constructs. It did not matter that many of the processes were reversible eg that H₂O could be broken down to its hydrogen and oxygen components or that hydrogen and oxygen could be brought together to form H₂O. What was important is that a temporal genetic component was inherent in the scientific definition of water as H₂O. The scientific definition was recognition of the fact that we had determined at least one temporal path to decomposition and at least one temporal path to making water.