BÜYÜK BRİTANYA'DA YENİ PSİKOLOJİ CEREYANLARI

SOME RECENT DEVELOPMENTS OF PSYCHOLOGY IN GREAT BRITAIN(*)

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Sir Frederic Bartlett, İstanbul'da 1956 yılında vermiş olduğu konferansların bu ilkinde 1914 savaşında İngiltere'de psikolojinin genel durumunu anlatmaktadır. İngilizlerin, fizik ve kimya dışındaki bilimlerde, genellikle, deneysel metottan ziyade vakıaların gözlenmesine dayanan bir yol takibedegelirken, 1914, 1939 savaşlarına ve İkinci Dünya Harbinden sonra ortaya çıkan ekonomik ve politik krizlere bağlı olarak deneysel ve uygulamalı psikolojiye nasıl yöneldiklerini kendine has, son derece enteresan bir üslûpla dile getirmektedir. Bütünü daha önceden yayınlanmış bulunan bu konferans serisinin birincisi, bir hatırlama ve hatırlatma gayesi ile, 80 inci yaş gününe armağan ettiğimiz "Çalışmalar" ın bu sayısına alınmıştır.

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I — PSYCHOLOGY IN THE 1914 WAR

1. British Empiricism.

It is always difficult to decide where to draw the line between what can and what cannot be regarded as «recent». So far as developments of psyhology go in any particular country, however, it seems as if «recent» must always be defined in terms of those special topics and problems which attract the greatest amount of contemporary, or near contemporary, attention and research. The more general aspects of approach to such problems appears to remain extremely consistent within any large group from period to period, once the group has achieved a fairly stable level of culture. In one very important general respect, for example, the kind of approach to psychological studies most favoured in Great Britain has remained precisely the same from the days when **John Locke** and **David Hume** exercised a great influence upon European thought.

The characteristic British approach has remained, with few and mostly unimportant exceptions, predominently and stubbornly empirical. There must be little or no theorizing until the appropriate facts have been collected in adequate numbers, studied and analysed. If the range of appropriate facts changes, as indeed frequently happens with the natural expansion of knowledge and society, it is the theoretical formulations that must give way if they lack the newly required range. People who set great store by scientific consistency may not like this very much. Certainly it has led, in psychology and in a good many other directions, to a sort of scientific opportunism. If we are British, or fond of the British, we generally call it «enlightened opportunism.» If we are neither of these we generally use some less pleasing epithet.

It might be interesting, and I am sure it would be profitable to give more consideration than has been usual to those characteristic and very persistent differences of approach which develop in different large national groups. Although British empiricism, for example, has often been set into sharp contrast with the apparently irresistible tendency of the German towards systematisation, less consideration seems to

have been given to what I think to be the more interesting differences between characteristic British and American approaches. Almost as soon as psychology was accepted in America as a more or less independent branch of knowledge, a wide, and somewhat uncritical belief became evident in the virtues of psychological experiment - almost any sort of psychological experiment. With this, however, was combined a tremendous hurry to get to theories, so that it happened over and over again, and is happening to-day, that experiments get designed and then are quickly set into a frame of schools and somewhat sectional «modes of thought», in ways a little disconcerting sometimes to the relatively slow-moving mind of the Englishman in England.

At first it may seem a little odd that empirical Britain should for many years have stayed sceptical not only of the value, but even of the possibility, of psychological experiment. As everybody knows, it was in systematising Germany that experimental psychology was first established and given respectable educational sanction, and it has been in school-forming America that psychological experiments have been pursued, in almost every possible direction, with greater enthusiasm and less constraint than anywhere else.

These were by no means accidents. British empiricism always has claimed to rest firmly upon an acceptance of facts, events in a natural world, as they appear to what we call commonsense observation. Very often the experimentalists' facts are decided by the experimenter himself, created, so to speak, in his experimental design and practice. The difficulty does not arise very much in physics and chemistry, because the ultimate components of the processes which these sciences study do not present themselves for commonsense observation at all. There is, therefore, no bias whatever against treating physics and chemistry, in all their many forms, as fully experimental. Indeed, even now, when, for instance, the British House of Commons discusses what is called «natural science» which, with some reluctance it may occasionally do, it is comparatively rare for sciences other than physics and chemistry to get much consideration. As soon as ever biological, and particularly psychological, problems have to be dealt with, the characteristic British attitude is to say «Well, we must first make sure of the facts. That is, we must take the facts as we find them. And anybody who looks earnestly enough can find them.»

I rather wish that I could attempt now to follow up these reflections more fully, for it is very likely that they might help us to understand,

at least by illustration, some of those persistent differences in the approach to life and its problems, which are characteristic of one large social group contrasted with another one. But this is not the place or the time to try to do that.

2. Three periods of social crisis.

What should already be recognised as implicit in the remarks I have made is that it must take a pretty considerable social crisis to convince the English, on any large scale, that unaided commonsense observation is not by itself a good enough guide to conduct, or for the explanation of conduct. During the last 40 years there have been three such crises for Great Britain. The first was the war of 1914. The second was the war of 1939, and the third was the economic and political consequences of both of these wars, linked particularly with the discoveries in physical science made during the second war. These brought about enormous industrial and social changes which nobody could fail to recognise. All of these three crises, but particularly the last one, have forced the rather unwilling English, to admit that there must be something not entirely sufficient about straightforward, unassisted empirical observation of human behaviour if we want to use it as a guide for behaviour.

3. Submarine Warfare in 1916.

For the purposes of these lectures, then, «recent» can be said to go back just about 40 years. By 1916 many influential people, and a mass of those with less direct influence, were realising that the exercise of tradition and commonsense could not win, but might lose, a war. It is both interesting and significant, that the decisive step, so far as experimental psychology was concerned, was taken, not by the statesman or the politician, or, indeed, by the military experts, but by a civilian body composed mainly of business men, and led by a physicist, who was to achieve world wide renown, for he was to become **Lord Rutherford.** No doubt Rutherford was greatly influenced by two British experimental psychologists, **C.S. Myers** and **T. H. Pear.** But it was he and his group who made possible the first organised psychological effort in England to use experiment in order to make the best possible human use of the instruments then available for a specialised form of behaviour - the detection and, if possible, the eventual destruction of enemy submarines.

It was my fortune, - good or ill, I think, according to how you look

at the matter - to be in charge of that effort. Was it an accident that this development should, from the first, have been associated both with physical research and with industrial interests? If so, it was an accident that has been most peculiarly persistent and effective.

In those days practically all we were trying to do was to pick out, from a volunteer population of naval ratings, those whose senses and intelligence best fitted them to use with success the physical methods of detection and search which were the basis of Britain's defence against submarine attack. We did it and I think it is fair to say that we did it successfully, mainly by using established psychophysical experiments thrown into a test form. Looking back upon them now, it is intensely interesting to me to realise that we put all our experiments and tests that we possibly could, into a form which could make use of the «just noticeable difference». This was the psychophysical unit that we never thought of questioning. We used it to examine auditory acuity, pitch identification, response to loudness, auditory localisation. We got into trouble with noise which could not be ignored but would not fit the patterns at all well. But I think now that perhaps in a way the most important thing of all was one which at the time we thought little about.

One of the most important characteristics of the patterns of sound which anybody listening for a submarine had to identify was its rhythm. And one of the members of the little group who were working as experimental psychologists was Sir Hugh Allen. He was not a psychologist at all, but a Cambridge musician who had become Professor of Music at the University of Oxford. He annexed rhythm, so to speak. Nobody minded that. Nobody else wanted to deal with it. What did he do? He did not attempt any minute analysis into beats, stresses, precisely measured off temporal intervals, and exactly controlled rates. He needed no instrumentation other than his own extremely mobile fingers and his sort of bark of a voice. He would hum his required rhythm or tap it out on a table top, and then, with improvised variations, that might differ every time from what they had last been, he would pick out the people who most quickly spotted the required rhythm and hung on to it longest in spite of distractions.

Truth to tell it was not particularly scientific. His standards were subjective. His stimulating situations and the responses which they evoked were extremely complex. If any of the rest of us fell ill or got knocked out, somebody else could come along and do our job, and there

would be little or no difference. But nobody else could do Sir Hugh Allen's job like Sir Hugh Allen. Maybe one of the main differences between the scientist and the artist is that the former is all the time making himself more and more dispensable; but the latter is not. However this may be, it now seems to me that perhaps it was at this time. and in this way, that a seed was planted in two or three of our minds which was to go on growing during the uneasy interval between the two wars and then spring up, flower and fruit during the second critical period of recent development of experimental psychology in Great Britain. It was perhaps then that we began, seriously and effectively to believe that psychology must find a way of dealing, in an accredited scientific manner with the complex situation and the complex and patterned response. We must perhaps find measures that are somewhat more like the «subjective standard» than they are like the alleged simple «just noticeable difference.»

Now, perhaps, one thing which I said at the beginning may be a little more clear. I said that so far as psychological developments go, what has to be regarded as within the range of «recent» in any country has also to be identified in terms of special topics and problems. What the group I was myself most concerned with did in the case of submarines, others were doing for the Royal Flying Corps, as it then was, and particularly for night flying. And very soon the neurologically oriented psychologists in England were becoming preoccupied with experimental studies of the effects of defined central nervous injuries, especially in their possible bearings upon the use of language and upon thinking.

4. Psychologists and other Experts.

That which makes this period both «recent» and of very outstanding importance is, to my mind, the fact that it was then, most markedly, that psychologists began to venture out of their own castles, and to associate freely with other experts and with everyday people. Especially it seems to have been then that psychologists and physicists, and very soon psychologists and engineers, began to be able to understand one another better and to work together. If the association, during this first war period had been with other experts alone — or particularly if it had happened that the psychologists had themselves attempted to take over and practice the other specialisations — psychology in Britain might have toppled over into a form of physics or mathematics. There have been threats of this; but it has not happened. I believe that this is mainly because these developments arose out of

the most urgent practical problems. And so the other association of the British psychologists as they emerged from their fortresses was with the common man. That, more than anything else, I believe, in so far as British psychologists are both sane and genuinely psychological, is what has kept them so.

One other thing must be said about psychology in Britain in this period. It was now that all sorts of people began to agree that special, controlled methods of observation and experiment are needed to discover how human behaviour is determined in special circumstances of wide practical urgency. But they and we also, who, being psychologists, were trying to develop the required methods, saw them then in one way only. The physicists, the engineers, they would provide the tools, the instruments with which these special circumstances would be met. And we, the psychologists — we would merely pick the people to use the instruments. Nobody then realised properly how inadequate, how prejudiced, such a view was.