TESTING FOR INTELLECTUAL IMPAIRMENT —SOME COMMENTS RECONSIDERED

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As Piercy (1959) has pointed out, it is generally agreed that currently available psychological tests for detecting brain damage (Yates, 1954; Meyer, 1957) or early dementia (Shapiro, 1952; Inglis, 1958) are by and large poorly standardized and poorly validated. If the clinical psychologist attempts such detection he can make very few positive statements if all he does is administer those standardized tests which even approximately meet the commonly accepted standards of validity.

We agree with Piercy when he points out that a basic weakness in most of these tests is that they are founded upon the probably incorrect assumption that brain damage produces only one kind of abnormality. It is, in fact, highly likely that different kinds of brain damage can impair a number of distinct functions. In other words, brain damage should not be regarded as producing some easily measurable change along a single dimension. It would indeed be surprising if lesions of different extent, in different areas and of different aetiology did not have quite different effects upon behaviour.

A study recently reported by Meyer (1959) indicates, for example, the importance and behavioural relevance of which side of the brain is damaged in the temporal region alone.

Like Piercy, we cannot agree with Yates (1954) that the construction of valid tests must await the development of some generally applicable theory of brain function. Should we wait this long for satisfactory indices of impairment little practical work would be done for decades, perhaps even centuries.

Few, then, would dispute the fact that, in this area at least, the clinical psychologist has hardly any acceptable, objective tools which can be used routinely. A question often hotly disputed, however, is what the clinical psychologist should then do when he is called in to deal with the problem of ascertaining impairment. Piercy suggests that the only thing that can be done is to allow the clinical psychologist to interpret his test results in the light of his own judgment and clinical experience. He argues that, "Human error must be tolerated if other sources of error are greater" (1959, p. 493), and that the psychologist with his experience of cognitive tests and dementing patients is the best person to make a decision in these cases.

He draws an analogy between this kind of judgment and that required of a good chess player. He reminds us that in the game of noughts and crosses, where the rules and combinations of play are few, it is possible to programme

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a machine so that it will make the "perfect judgment" of each play. Chess, however, is so complex and the combinations of moves so many that the programming of an analogous machine is not practicable. Nevertheless good chess-players are able to weigh up sufficient alternatives to defeat players who mechanically, and with less foresight, follow a few of the formal rules of procedure.

This seems to be an exceedingly misleading analogy. In its use the many rules and relations of the chess game presumably correspond to the many parameters of brain function. It is then argued that no machine (or battery of tests) can in practice successfully examine all the combinations possible in a single game (or patient), although a good player (or clinical psychologist) can produce a better-than-chance prediction by a subjective assessment.

Unfortunately for both the analogy and the psychologist, unlike chess, where the rules are known and only the complexity of possible relations precludes a "mechanical" solution, in the case of brain damage the very rules of function and dysfunction are themselves unknown. The psychologist who makes a "subjective assessment" is therefore less like a good chess player who subtly comprehends a large complexity of known relations and more like an autocratic opponent who, without consultation, makes up his own private rules as he goes along. Such a player might soon put his opponent's King in check. We would not, however, necessarily agree that this is a satisfactory way of winning. Similarly, the "experienced clinician" can always appear to make valid judgments of impairment so long as the only criterion of their validity is the fact that he on the basis of his experience has uttered them. We would not necessarily agree that this is a satisfactory way of working.

However, even if all the facts *were* known, but were too numerous to collate for an individual case, there is some evidence that judgment in clinical psychology is not much like judgment in chess—even if we leave aside the fact that the stakes for which we play are incomparably higher. For example, in the well-known study by Kelly and Fiske (1951) it was shown that the overall judgment by clinical psychologists of a number of facts, some of which were individually valid predictors of a criterion, was not better than chance, although better than chance "mechanical" prediction from each of several simple facts (test results) was possible. Thus, there is some evidence against the notion that "the human brain is the best available instrument for assessing" (1959, p. 493) this kind of evidence.

Essentially, Piercy differs from other authors (Shapiro, 1951, 1957; Payne, 1953, 1957) concerning the kind of method the clinical psychologist should use; but this, in itself, rapidly decides what the clinical psychologist will come to be. Piercy believes that he should use his "clinical judgment" just as the doctor does. But it then becomes extremely difficult to distinguish between the functions of the psychologist and the psychiatrist or the psychologist and the neurologist; except, that is, for the fact that the psychologist commonly lacks the crucial training in physical medicine which the other specialists have had. He no longer has a unique contribution to make but tries to be a kind of pseudo-psychiatrist or neo-neurologist.

Not only does Piercy suggest that the psychologist should adopt part of the role of these others, he recommends the acceptance and use of their ideas and hypotheses. He argues that, "An alternative approach is to examine test results with respect to their degree of approximation to known syndromes of intellectual disability" (1959, p. 495). To whom, we may ask, are these syndromes "known"?

The objective psychological evidence for the existence of any easily identifiable "syndromes of disability" is slender indeed. How approximate, we may enquire, are we allowed to be in making guesses perhaps crucial to the patient's treatment and future welfare? Knowledge of the size of error of measurement alone which attaches to all psychological estimates should make us extremely wary of accepting the relevance of such vaguely defined approximations.

Surely it is better for the clinical psychologist to pursue his own independent objective line of enquiry which does not take psychiatric conceptions for granted. It would seem far more useful to be able to demonstrate a few indisputable facts about a patient and his dysfunctions rather than to accept without question some of the received ideas of psychiatry and try to apply them through tests.

In his paper Piercy makes a number of statements about the kind of estimates upon which the psychologist may base his judgment. A few examples may be quoted. Thus, ". . . on a test of rote verbal learning, one patient may be impaired as a result of a slight but strongly perseverative error whereas errors produced by another patient obtaining an identical score may be extremely variable. The distinction is not trivial since the latter is more typical of dysphasia and is thus of localizing value" (1959, p. 491).

"Again, if on this type of test (Kohs' blocks) the trial is discontinued as soon as the conventional time limit is exceeded, the opportunity to distinguish between depressive and organic intellectual impairment may well be lost" (1959, p. 492).

". . . the interpretation placed on the ability to repeat 7 digits forwards and 5 backwards should be quite different from that placed on the ability to repeat only 5 digits forwards but 7 backwards" (1959, p. 492).

It would, to say the least, certainly seem desirable for the psychologist to investigate such statements as these before accepting them as procedural guides. It may be that such indicators can be recognized "from experience", but if a statement founded upon the psychologist's "experience" is to be given more weight than that of any other clinician who may choose to dispute it, then it must be a demonstrable, objective and repeatable fact.

Finally, two extremely general points must be made in regard to Piercy's contentions.

In the first place, he does not seem to recognize sufficiently clearly that the "problem of dementia" often presented to the psychologist usually contains within itself a number of different questions which *must* be considered separately. At least two kinds of questions can commonly be distinguished, these are:

1. *Descriptive:* (e.g.) (*a*) Is there any evidence of a *general* falling off from a previously higher level of ability?

(*b*) Is there any evidence for some *specific* impairment such as amnesia, dysphasia or the like?

2. *Aetiological:* (e.g.). Is there any intracranial pathology or damage present?

Now, while we may agree with Piercy that most standard tests are of little use, this is partly because their constructors have similarly failed to recognize the different questions which go to make up the general problem. Once it is recognized what specific questions are being asked it is likely that more successful attempts may be made to answer them. One may, for example,

utilize available tests or make up new tests to *describe* specific abnormalities (viz. Inglis, 1957). It is important to emphasize that the psychologist should be capable of attempting the experimental description and analysis of any behavioural abnormality however "subtle" and "qualitative" this may seem at first sight to be. It is almost always possible to examine the significance of any apparent abnormality by comparing the patient's performance with that of a group of control subjects. Unless we do this we are bound to fall into the error of accepting as indicative of a specific disorder some apparent difficulties which even a brief investigation might show to be characteristic of at least some individuals who do not suffer from the disorder being investigated.

As regards the aetiology of disorder Piercy seems content to suppose that certain subjectively assessed descriptive characteristics must necessarily have causal implications. As Payne (1958) had previously pointed out, however, even if a test can be shown to have a certain number of valid descriptive implications it cannot simply be assumed that a limited set of aetiological implications follow. These latter implications must themselves be proven before the test results can be used to make inferences about causation. A similar argument may be put forward concerning prognosis. Valid descriptive characteristics are not necessarily valid prognostic indicators and the psychologist can and must tackle this problem in its own right.

The second general point is this. Throughout his paper Piercy is concerned to emphasize that while many "theoretical" objections can be raised against current practices in testing for deterioration the actual problem is a "practical" one and must be tackled with the means at our disposal. We, on the other hand, would argue that medicine has advanced not because the sciences upon which it is now founded have accepted its limitations, but because they have transcended them. The help that bacteriology has given to surgery was not produced by a more refined understanding of, or harder work based upon, the notion of "spontaneous generation" but came from an experimental approach to the problems of fermentation and infection. The principal contributions that the basic sciences have made to medical problems have often sprung from a refusal to plead the exigencies of "clinical pressure" or the urgency of "practical problems". Again and again in the history of medicine such pleading has been made the reason for continuing to do what has been done. Again and again the real problems have been outlined and solved by those who have refused to accept custom as a substitute for reason.

Summary

A critical examination has been attempted of an argument recently restated by Piercy (1959) that a "flexible" approach is necessary to psychological testing in the clinic. It has been suggested that such an approach, far from being advantageous, tends to deprive the psychologist of his ability to add anything to psychiatric and neurological opinion since his contributions tend to be less the providing of evidence and more the imitation of procedures already carried out by the other specialists.

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