Perspectives on the Conference

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I. Introduction

The technique of the 10-minute précis and the 2-minute discussion interval has produced a new time unit which makes it essential that thoughts move rapidly and be expressed fast. I shall, therefore, limit my remarks to only four points.

(a) What were the points of full agreement of this conference?

(b) What were those in which only partial agreement was attained?

(c) What were the disagreements?

(d) What points which should have been included were omitted from our discussions?

Initially I had hoped to make a content analysis of each paper, punch IBM cards for each contributor, and make a statistical analysis of the agreements through either a typological, dimensional, or like-mindedness approach; but unfortunately time ran out.

II. What are the points of agreement?

1. Diagnosis is in a bad way:

Present-day diagnostic categories are too heterogeneous, and, unless improvement is introduced, we will fail to meet the needs of the new types of patients in community mental health centers that are starting to flow into our facilities and demand diagnosis and classification.

2. Typology seems to be more congenial to the usual clinical way of classification, but the burden of proof lies on the typologist to demonstrate that his method has more merit and more power than the clinical or dimensional approach.

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III. Points of partial agreement:

1. Diagnosis has a multipurpose goal.
2. Better data than those now provided are needed.
3. Typology tends to reduce intragroup variability. This is a gain for experimental purposes.
4. Prior clinical knowledge could be helpful in the preparation of diagnostic schema. Of course, the difficulty here is that much of prior clinical knowledge “ain’t so” and how to distinguish between the reliable knowledge and unreliable knowledge is an important question.
5. Arbitrary decision must be resorted to often in the making of any classification and the nature of this arbitrary decision must be at least specified so that replication is possible.

IV. What are the points of disagreement?

1. Though clinical interviewing is loose and freewheeling, it is not yet clear whether it is inferior to the more systematic structured approaches, like the Mental Status Schedule or the Structured Clinical Interview. Work going on in our own laboratory seems to demonstrate that these techniques are at least more reliable. Whether they are more valid remains to be demonstrated.
2. The relative merits of factor analysis and typological analysis are not yet agreed upon. Back in the late 1930’s I wrote a paper entitled “Sociobiological Types and Methods for Their Isolation” and it appears to me that I did better then than I knew by introducing the term sociobiological types. The types that we got then and those we get today are ad hoc types and not biological types. That is why it is important that we look into the social cultural background of the people for whom profiles are developed. High degree of hostility in a person coming from a low socioeconomic level where aggressive behavior and overt violence are more often seen than in other sociocultural groups, must be separated from a similar score for a person from another socioeconomic level. This may be the reason why our types do not hold up upon reexamination and unless we include the social cultural factors as well as other factors that are necessary, such as educational background, we will not be able to make much progress.
3. The relative merits of disease entities versus behavior patterns is not agreed upon. No definition of disease was arrived at although a rather tentative definition was suggested which runs something like this: A disease is a progressive condition which unless attended to would lead to premature death or to excessive lowering of efficiency. A defect is a stationary condition which leads to an extreme reduction of efficiency. It was pointed out at the conference that such factors as military service or poverty or delinquency or old age would qualify as diseases according to this definition. I doubt this since these terms are too global. Only by analyzing these global concepts into their components can any understanding of their relationship to disease be attained. In the case of old age it is necessary to see what biochemical changes are in back of the gradual lowering of vitality. With regard to poverty, unless we look into such factors as malnutrition or overcrowding which comes with poverty, we do not really deal with a quantifiable or definable concept. Furthermore it is quite clear that only in mathematics can definitions be foolproof and rigid. In biology rigidity of definition falls by the wayside, and the power of the defined concept to integrate observations becomes the criterion of a good definition. Take, for example, the concept of species, which Julian Huxley points out could not be defined rigorously at all. It is a combination of several indicators plus a little flair which makes the definition acceptable. Thus the presence of many open-ended definitions in our field should not be regarded as a hazard.
5. Clinical psychological tests have not fared too well at the conference nor in actual usefulness in everyday work.

6. The medical versus the behavioral model has not yet been discussed fully enough to arrive at any conclusion.

7. In dealing with profiles the distinction between level and pattern has not been sufficiently considered, and it's quite clear that following Penrose one can break up \( \bar{d}^2 \), the average square deviation, into two components: (1) \( \bar{d}^2 \), the square of the average distance between profiles, and (2) \( \sigma^2 \), the variance of these discrepancies. The square of average, \( \bar{d}^2 \), of course refers to the discrepancy in level between two profiles and the variance refers to the discrepancy in shape. If complete parallelism exists (all the \( d \)'s being constant) the variance drops out completely. To the extent that there is some variance present there is some discrepancy in shape.

8. Specification of the mathematical model underlying clustering met with a good deal of discussion. It is quite clear that some people do not care to specify ahead of time the assumption underlying clustering, and are satisfied with blind clustering techniques. As a result only ad hoc typologies emerge. On the other hand, if you specify normal distributions or rectangular distributions, and specify also the range of correlations between the variables in each cluster, you may be specifying too much. Only by trial and error or by some better systematic approach can this problem be resolved.

9. It was not quite clear as to how one crosses boundaries from the concepts used by one discipline, such as nursing, to another, such as the psychiatric or psychological disciplines. The fact that the same words are used does not necessarily assure us that the concept behind the words is the same.

10. One of the criteria often used by the newer approach to diagnosis is that the obtained results in a cluster or a profile agree with previous clinical knowledge. Since there is no guarantee that the older clinical formulation is correct, this constitutes a doubtful criterion. In our own work we are most suspicious when a finding corroborates a previously held clinical opinion. Agreement per se is pleasant because it leads to a friendly feeling, but it is no proof.

11. Linear versus nonlinear relationships. It was pointed out that whenever relationships are obtained between variables from two different disciplines—e.g., psychology and physiology, the relationship usually turns out to be nonlinear. Whether this is a result of the units used or whether it represents the influence of more than one underlying factor is an open question. To be sure, not all the correlations within a given discipline are invariably linear—that is why typology is a useful concept.

V. What matters have been either omitted or not discussed sufficiently?

1. One of the major difficulties in the area of improving diagnosis is the lack of criteria for validation of diagnosis. At the present time there are two aspects that are most urgent, namely the severity of the illness and prognosis of the illness, and these two aspects could be utilized as a basis for validity. At the present time validity criteria for the severity of the illness is available only in the judgment of other clinicians, and utilization of concurrent validity for this purpose is the only way out. With regard to prognosis, the only solution is to have good followup studies. In the meantime the best that one can do with regard to validity is to utilize consensual reliability.

2. The investigation of the decision process involved in diagnosis is of the utmost importance since at the present time the basic tool for the development of diagnosis is the clinician himself. The investigation of this instrument—the clinician—is most appropriate, and methods are
already available for studying decision processes.

3. Insufficient time was allotted to the discussion of the role of personality in its relation to psychopathology. Here the three possible connections between personality and psychopathology ought to be surveyed: (1) psychopathology and personality as one and the same, (2) psychopathology as an interference with personality development, and (3) psychopathology as independent of personality.

Apparently personality assessment, regardless of whether it is independent of psychopathology or dependent on it, is an important element in diagnosis and prognosis.

4. Very little consideration was given, surprisingly, to the distinction between primary and secondary symptoms in such illness as schizophrenia and other illnesses. At one time this was one of the major issues before diagnosticians, but now it seems to have been relegated to the background. It would be interesting to know why that is so.

5. Freud's primary process and secondary process have never been mentioned, and that, too, raises an interesting question.

6. Although much attention was paid to drug therapy in consonance with the nature of the conference, little if any discussion of psychotherapy and the newly developing behavior therapy was engaged in.

7. The mixture problem, of finding the natural lines of cleavage in a heterogeneous population, was left unattended to, probably because of the absence of Professor Barnard.

8. It is interesting to raise the question why the role of psychogenic versus other factors was little heard of in the discussion.

Conclusions:

Conferences such as these, to which we each bring our prejudices and sharpen them on the whetstone of opposition, are not the best way to take diagnosis out of its present-day doldrums. Because we need better diagnoses today as a result of the tremendous number of therapeutic approaches from which we have to choose, it becomes necessary to take a drastic step. I would like to propose the establishment of assessment centers in various regions of the country where intuitive clinician and objective biometrician confront each other not across the conference table, but in front of living case material or, at least, in front of video tapes of living case material. Only by a direct confrontation with living clinical material can we hope to make progress. Perhaps, following Dr. Klein's suggestion, a combination of assessment centers with community treatment centers is the next step in the development of improved diagnostic schema.