

DIAGNOSIS AND MEASUREMENT: A REPLY TO LOEVINGER

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Loevinger's brief note (3) on my paper (2) is somewhat obscure in its import and its coverage; she seems as much concerned with the sins of the MMPI constructors as with the study of mine, which is the ostensible reason for her note. I shall not follow her into this broad field, but concentrate on her main points.

The problem I investigated was a very clear-cut one. Groups of psychiatrically normal, neurotic, and psychotic people may be differentiated along one dimension (severity of illness?) or they may be differentiated along two dimensions (neuroticism and psychoticism). Experimental results ruled out at a reasonable level of statistical significance the one-dimensional hypothesis. This seems to me an interesting, and possibly even an important, demonstration. I see nothing in Loevinger's reply that throws doubt on the result. The use of soldiers as a normal sample might be subject to criticism if our aim were the determination of population parameters. This, however, was not our aim, and Loevinger does not explain how the choice of this particular sample of normals would have produced two significant latent roots instead of one.

I attributed advocacy of the one-dimensional hypothesis to Freud on the basis (a) of my reading of the work of Freud and his adherents and (b) of lengthy discussions with leading psychoanalysts. Loevinger appears to disagree with my interpretation, although her writing is so elliptical that I find it difficult to know whether she does in fact disagree or not.

One would probably have to agree that Freudian writings are lacking in clarity and consistency to a degree that makes it possible to attribute almost any view to him that one pleases. Textual criticism, as in the case of the Sibylline Books, appears to do little but make confusion worse confounded. The reader who is familiar with the Freudian *opus* must decide for himself whether my interpretation was reasonable or not. It is quite possible that in this context, as in so many others, Freudian theory is too complex and imprecise to be amenable to any kind of scientific test or check whatsoever. (In that case, the results reported in my paper would, of course, still retain their importance unimpaired.) If Loevinger does not agree with this pessimistic view, perhaps she could state briefly and succinctly (a) precisely what the Freudian hypothesis does say in its dimensional implications and (b) how precisely a test of this hypothesis can be performed. It would also, of course, be necessary to show that psychoanalysts themselves agree, both with her interpretation and her suggested method of proof. Anyone reading Blum's (1) recent survey of psychoanalytic theories will await the outcome of such a referendum with the greatest interest.

REFERENCES

1. BLUM, G. S. *Psychoanalytic theories of personality*. New York: McGraw-Hill, 1953.
2. EYSENCK, H. J. Psychiatric diagnosis as a psychological and statistical problem. *Psychol. Reports*, 1955, 1, 3-17.
3. LOEVINGER, J. Diagnosis and measurement: a reply to Eysenck. *Psychol. Reports*, 1955, 1, 277-278.

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