
The Rorschach Test in Clinical Diagnosis— A Brief Commentary



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In this commentary, an attempt has been made to put the 1947 article by Garfield in historical perspective, and to make some critical references to emphases made in the review article on the Rorschach Test in Clinical Diagnosis. © 2000 John Wiley & Sons, Inc. *J Clin Psychol* 56: 431–434, 2000.

I am pleased to have this opportunity to respond to the article on the Rorschach Test in clinical diagnosis since my 1947 article seemed to be at least a partial focus of the critical comments. I also will try to comply with the editor's request for a "short rejoinder" to this lengthy article. In addition, although I felt the tone of the manuscript was overly haughty in criticizing an article published 52 years ago, I will attempt an objective response.

First, it is important to remember that the Rorschach was a relatively new test in 1946 when my report was prepared, and few research studies had been conducted. There were just a small number of clinical-psychology training programs in 1948, and the Division of Clinical Psychology had 482 members. The emphasis on the Rorschach was on personality appraisal and clinical diagnosis rather than on psychometric scores. It was the new era of projective techniques as contrasted with psychometric questionnaires.

After praising me for my contributions to clinical psychology, the authors state: "It is instructive, therefore, to see how this careful and thoughtful scholar was led to conclusions regarding the Rorschach that were almost certainly in error . . ." (p. 396). They then discuss three significant errors that they state can still be found in current Rorschach research. The first refers to the "set of dramatic results" that appear to support the value of the Rorschach for diagnostic purposes. The authors performed their calculations on the figures provided and have come up with a .90 correlation with staff diagnoses of schizophrenia where I stated that 71.9% of the Rorschach diagnoses were in agreement with the staff diagnoses. Whereas, it is stated, such high "validity coefficients" would arouse

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“intense skepticism” today, “in the late 1940s neither the author nor the editor seems to have suspected anything amiss.” They further state: “To a present-day reader it seems clear that the near-perfect correspondence between Rorschach and clinical diagnoses . . . was simply too good to be true, and must therefore have been largely or completely artifactual. Yet Garfield downplays the idea that artifact influenced the results” (pp. 396–397). Nowhere did I state there was “near-perfect correspondence.” What I did state was that my Rorschach diagnoses of schizophrenia agreed with the final staff diagnoses in 23 out of 32 cases, or 71.9%. Are 23 out of 32 cases near-perfect? Would medical diagnosticians regard such accuracy as near-perfect?

I do not know how many individuals would consider a 71.9% agreement as “near-perfect, as too good to be true, and therefore must have been largely or completely artifactual.” For example, in the February 1999 issue of the *Journal of Clinical Psychology*, Greenblatt and Davis reported a study of the “Differential Diagnosis of PTSD, Schizophrenia, and Depression with the MMPI-2” (pp. 217–223). “As shown in Table 3, 70% of all patients were classified correctly” (p. 220). Should we, in 1999, expect these results to be “too good to be true”? Earlier, they also stated that “A 50% correct classification should not be acceptable in clinical practice” (p. 219)—and I would agree.

The authors next mention the presence of other glaring methodological errors. “The Rorschach administrator and scorer was Garfield himself, who was not blinded to the hypotheses of the study” (p. 396). In fact, there were no hypotheses to the study at all. I simply compared my regular clinical report of the cases referred to me to the final diagnosis reached at the clinical-staff conference. I had no specific predictions but was essentially checking the agreement between the diagnoses I made from my examination of the patient and the final staff diagnosis. I was not studying blind analyses of the Rorschach, nor did I check any particular Rorschach scores or ratios. It also is stated that I could have received cues from the patients during the examination. In my view, the observations of the examiner and the rapport established during the examination are parts of all clinical psychological examinations. Essentially, in the clinical use of the Rorschach, the examiner is using a set of standard stimuli, but examiners vary in their clinical skills and knowledge (Garfield, 1983). The one comment that the authors make that merits agreement is that other staff members, sometimes before, but mostly at the clinical-staff conference, had access to the Rorschach report. It was at the conclusion of the staff presentations that the official diagnosis was made, and, as indicated in the report, the diagnosis did not always agree with the Rorschach interpretation, nor did the latter always agree with the official staff diagnosis.

Finally, with reference to the previous comments, one must consider the settings in which tests are evaluated. One of the methodological problems emphasized, the failure of blind test administrators, is irrelevant to appraisals made of actual evaluations in clinical settings. As already emphasized, I prepared my evaluation based on my examination of 75 clinical cases for which I also had staff final diagnoses. The criticism of criterion contamination technically is valid, but if one is working in a military or any other hospital and a case is referred for diagnostic appraisal, one cannot keep his findings secret. I also would question the authors’ statement on “failure to blind test administrators” (p. 397). Interpreters and scorers may be blind to the patient, but a blinded administrator would present difficulties.

It is obvious that there have been a number of developments concerning the Rorschach Test since the 1947 article. What seems most apparent is the more recent emphasis on “scores”, on more specific symptoms or disorders, the tie-in to the DSMs since 1980, and validation studies “conducted by unrelated researchers or research groups.” Although one might have expected some significant or important advances over a period of a

half-century, such progress readily is not apparent. In fact, I found the review rather depressing. I, myself, attempted to keep up with the research on the Rorschach, as well as other popular diagnostic procedures, in preparing for the publication of my three textbooks in clinical psychology (Garfield, 1957, 1974, 1983). I only referred to the 1947 article in the 1957 text when discussing problems in evaluating the Rorschach Test and did not even mention it in my later editions since there were many more recent studies to choose from. However, although I have been critical of most psychological tests of personality over the years, and have concentrated on the area of psychotherapy in more recent years, I have no feelings of guilt concerning the publication of my 1947 article. It was a very early attempt to appraise the value of the Rorschach as a diagnostic aid and using an imperfect but available criterion, staff diagnosis. As many of my writings have indicated, I have never been impressed with psychiatric diagnosis as a research criterion (Garfield, 1986, 1993), but it was available to me as a pragmatic criterion in order to attempt some appraisal of my work. I also can state without reservation that I certainly did not suspect “anything amiss.”

In concluding, I present some brief and critical comments:

1. No clear statement is given as to why the review is limited to 10 diagnoses when there are over 350 diagnoses in the most recent versions of the DSMs. Has the Rorschach been used mainly for such cases, or is this due to the use of criteria proposed by Wood and colleagues that emphasize “test scores” (p. 397)? The reviews in the article do appear to emphasize scores, scales, indicators, and the like, but shouldn’t one consider the total test performance? The emphasis on scores etc. make the Rorschach appear to be a psychometric test instead of a projective technique. In fact, the emphasis is on scores and relationship to a particular symptom or disorder instead of the total record, and this may be why there is so much emphasis on blindness in research and on the findings secured. Actually, practically nothing is said about the training and skill of the Rorschach psychologist, as if all examiners are equal.
2. In a similar fashion, very little is said about how the psychiatric diagnoses were made and who made them, and these constitute the validity criteria. For example, on page 405, it is stated that “If a Rorschach score is to be validated by comparing it with a psychiatric diagnosis, then that diagnosis should be established completely independently, by a diagnostician who has not been influenced either directly or indirectly by patients’ Rorschach scores.” I would agree that the two diagnoses should be independent, but how good is the psychiatric diagnosis? Who made it? How accurately has he performed? To compare any test interpretation with the diagnosis of a single unknown diagnostician will not necessarily advance clinical science. The entire issue of the validity or soundness of the diagnostic criterion used to evaluate the Rorschach is not discussed at all, and this is a serious omission. I, at least, used the official diagnosis secured at a staff conference where the majority of the psychiatric staff participated—not the opinion of one staff member.
3. The review appears to indicate that the level of research in clinical psychology is very poor and that the editorial standards for publication need a drastic improvement. The listing of one poor study after another did make me wonder what kinds of standards characterize our journals today.
4. On page 415, it is stated that “the present findings support the view that methodological issues are important in evaluating the Rorschach research literature.” Aren’t they important in evaluating *all research*?

5. I also would take exception to the pronouncement at the bottom of page 417, "that the Rorschach not be used to diagnose individuals in forensic contexts," and that it would be inappropriate to imply that a parent in a custody case is "narcissistic" or "dependent" based on Rorschach results. This is too severely judgmental. *No* conclusive recommendation should *ever* be based on the results of only one test. All tests need to be administered properly, interpreted adequately, and should be supplemented by other inputs, such as skilled interviews, observations, and other relevant sources of data. If the only indication of "narcissistic" is the Rorschach, and there are no other indications, I doubt it would carry the day. I also would question the recommendation of dropping the Rorschach from all training programs. Here again universal statements are made that imply that Rorschach is Rorschach, diagnosis is diagnosis, and say nothing about the variability of training and quality of students and faculty.
6. The material at the bottom of page 417 really has nothing to do with the Rorschach but with possible unethical behavior of the parties involved.
7. Finally, I would state that the article tends to stress a psychometric approach to the Rorschach, pays too much attention to obviously poor studies, and appears to have great faith in psychiatric diagnosis, regardless of who offers the diagnosis. At the same time, little attention is paid to the training and skill of the Rorschach clinician. This is in the psychometric tradition of objective scores and norms; however, is the clinician's observation and skill with reference to the patient's behavior to be entirely omitted? Little attention is paid to the psychology of individual differences, and I wonder if this indicates that there is something "amiss."

References

- Garfield, S.L. (1947). The Rorschach Test in clinical diagnosis. *Journal of Clinical Psychology*, III, 375-381.
- Garfield, S.L. (1957). *Introductory clinical psychology*. New York: Macmillan.
- Garfield, S.L. (1974). *Clinical Psychology. The study of personality and behavior*. New York: Aldine.
- Garfield, S.L. (1983). *Clinical Psychology. The study of personality and behavior*. (2nd ed.) New York: Aldine.
- Garfield, S.L. (1986). Problems in diagnostic classification. In T. Millon & G.L. Klerman (Eds.), *Contemporary directions in psychopathology: Toward the DSM-IV* (pp. 99-114). New York: Guilford.
- Garfield, S.L. (1993). Methodological Problems in Clinical Diagnosis. In P.B. Sutker & H.E. Adams (Eds.), *Comprehensive handbook of psychopathology* (2nd ed., pp. 27-46). New York: Plenum Press.