

The Foundations of Psychohistory: Positivism, Humanism, or Both?

Donald L. Carveth, Ph.D.

dcarveth@yorku.ca

John Hartman (*Clio's Psyche* 13, 1 [June 2006]) offers a warning to psychohistorians regarding some of the “methodological pitfalls” to which workers in this field are exposed. He argues that while it is true that “People make history” and that “People have childhoods,” it by no means follows that “Childhoods ... exclusively and directly make history along an easy to see linear path.” He reminds us that “The dynamics of the individual are not exactly the same as those of the small group and those of the small group not the same as the large group, and those of the large group not exactly the same as what we call history.” He points out that many of our hypotheses and conclusions in psychohistory rest upon creative analogies and offers the caveat that “arguing by analogy has its limits.” He urges us to pay greater attention to “what constitutes evidence in psychohistory.”

While these are certainly points worthy of our serious consideration, I think it is also important to consider the background methodological attitudes and assumptions informing Hartman’s concerns, a philosophical framework reflected in the following list of phases he employs: “empirical, systematic research”; “empirical evidential base”; “complex graphing”; “moving averages”; “percentage profiles”; “empirical findings”; “raw data”; “statistical analysis”; “factor analysis”; etc. He reports that his research involved “forty boxes of IBM cards, each holding 2000” totalling “80,000 group interactive scores,” and that his research team was “the second largest computer user at the University of Michigan in 1969” while “The entire Physics Department, including researchers at the cyclotron, was first!”

One doesn’t have to “listen with the third ear” to detect in this the kind of admiration of the natural sciences that, until fairly recently, was quite common among positivistic social scientists, especially psychologists. Rejecting the distinction made by hermeneuticists in the tradition of Dilthey and Weber between the two different types of disciplined scholarship (*wissenschaft*)—the *Naturwissenschaften* (natural sciences) and the *Geisteswissenschaften* (humanistic or cultural studies)—positivism holds that the scientific method evolved with such success in the former is equally appropriate for the latter which require no special methods of their own. In contrast, and without rejecting the naturalism requiring science to adopt concepts and methods appropriate to the nature of its objects of study, adherents of the anti-positivist, interpretive perspective point out that whereas in the natural sciences these are, quite literally, *objects* that must be studied *objectively* from an external point of view (because atoms and molecules, like rocks, trees and animals, cannot be interviewed regarding their *subjective* experience), in the human sciences our objects of study happen to be *subjects* as well—they are *subject-objects*—and, therefore, to be faithful to the nature of our field of study we must adopt both positivistic and humanistic methods. In addition to natural science *objective* approaches,

we need to employ such *subjective* methods as Weber's "understanding" (*verstehen*), Charles Horton Cooley's "sympathetic introspection" or Heinz Kohut's "empathic-introspective" approach.

Hartman points out "the group process aspect of psychohistory rests on an empirical evidential basis." Small-group research confirms that in leaderless groups revolt against the leader is followed by attempts to create new values of closeness and honesty, but competition, sexuality and rivalry emerge and lead to disappointment and eventual sadness associated with termination. Hartman acknowledges "This kind of study demonstrates empirically what anyone who attends any kind of mass event experiences." If so, were all those boxes of computer cards really necessary? Why attempt to prove what anyone who attends a group can easily observe? Hadn't the psychoanalyst Wilfred Bion (*Human Relations*, vols. I-IV, 1948-1951) already laid much of this out on the basis of participant observation, "empathic-introspection," and the imaginative interpretation and ordering of his "experiences in groups"?

Hartman's positivist conscience leads him to describe as "speculative" and "potentially problematic" his and his colleagues' venture into the making of an analogy between the stages of group development and those of revolution as described by Crane Brinton. Did Brinton arrive at his historical stages through the use of computers, complex graphing, moving averages, etc.? Or did he come up with them by means of an old-fashioned historical, hermeneutic method: attending to known historical facts; empathically identifying with one's historical subjects; and then working all this over with imagination? Of course, historians have traditionally recognized their discipline as an "art" and as part of the humanities, whatever aspirations they may occasionally have had to make it into a science. Brinton seemed to have little compunction about making an analogy between revolutionary conditions and a fever in an infected individual, but Hartman clearly feels nervous working with what he calls "colourful analogies." He is very aware that in working this way he has adopted "a different kind of methodology" relying on "a different kind of evidence," that he has entered into "the realm of interpretation of meaning," i.e., into hermeneutic as distinct from positivist science.

With respect to such psychohistorical practices as linking the peaks and valleys of distress observed in groups to shared fantasies and then labelling these as utopian, revolutionary or apocalyptic, Hartman writes: "It may not be impossible to do this, but it is not yet a part of a systematic methodology of evidence in psychohistory any more than this kind of inference is an integral part of psychoanalysis itself." I find this sentence problematic. Is Hartman suggesting that the interpretation of meaning, or linking observable emotion to underlying fantasy, is not an integral part of psychoanalysis? Or is he saying only that such interpretation remains unsupported by any systematic methodology of evidence?

Certainly, if it were the former, I would have to vigorously disagree, for the interpretation of meaning has always been integral to the psychoanalytic method, as has the interpretation of conscious and unconscious fantasy. Freud did not title the dream-book the *explanation* but rather the *interpretation* of dreams. Throughout his career he relied

on analogies between psychoanalytic work and processes of linguistic translation and the decoding of arcane messages and texts. Psychoanalytic listening has always sought to uncover the latent fantasy content beneath what is manifest and, like literary interpretation, it has always been attentive to image clusters, recurring themes and, especially, to processes of condensation and displacement of meaning—processes recognized by Jacobson and Lacan as none other than metaphor and metonymy respectively. Transference is nothing other than a type of metaphor (analogy) in which subjects turn their objects into metaphorical fathers, mothers, sisters or brothers.

In his classic paper, “Notes on Transference: Universal Phenomenon and Hardest Part of Analysis,” Brian Bird (*J.A.P.A.* 20 [1972]: 267-301) argues that transference (i.e., metaphor or analogy-making) is “one of the mind’s main agencies for giving birth to new ideas, and new life to old ones” and that it represents a kind of ego function. It is an ego function that must be highly developed in anyone who seeks to practice “the talking cure” which depends on alertness to the often more or less unconscious role of such transferences or metaphors in patient’s lives—just as it must be highly developed in anyone engaged in creative artistic or scientific work. Freud himself developed his discipline through the use of elaborate analogies drawn from archaeology, medicine, warfare, and other fields.

It is true that we need to remain alert to the danger of literalizing, reifying or concretizing our metaphors and, thus, mistaking the menu for the meal or the map for the territory. I have argued that psychopathology may fruitfully be viewed precisely as such literalization and analytic therapy as deliteralizing or “melting” “frozen” metaphors (“The Analyst’s Metaphors: A Deconstructionist Perspective.” *Psychoanalysis & Contemporary Thought* 7, 4 [1984]: 491-560). This, of course, is itself an analogy. When Hartman bemoans the fact that “there is a lack of a coherent methodology of evidence to assert the truth claims of what began as analogies and increasingly have taken on the aspect of causal links,” I think he may be attempting to get at this very tendency of creative analogies to regress in our thinking into identities—in which case we lose awareness that we are comparing two different phenomena that, in our regressive thinking, have come to be thought of as one.

If in that ambiguous sentence above, instead of suggesting that interpretation of meaning is not integral to psychoanalysis, Hartman meant only that we lack a systematic methodology for the evaluation of clinical evidence, I wonder if this is more of a problem for positivism than for psychoanalysis? Certainly if one regards psychoanalytic interpretation as an art bearing similarities to the arts of literary and historical interpretation, then one naturally expects some analysts to be more talented at its practice than others. Just as we are unlikely to seek rules of painting through the following of which anyone can become a great artist so, in this view, we are less likely to be perturbed by our inability to reduce the art of analytic interpretation to a manual. This is not at all to devalue attempts, such as Hartman’s and Boesky’s, to study “the actual criteria that are being used to assess the fit between the assertion and evidence as well as to evaluate the fit itself.” But this enterprise strikes me as essentially hermeneutic, though the positivistic language of “evidence” obscures this. Hermeneutic scholars, say, of

Shakespeare's plays, have long been concerned with criteria by which different interpretations may be rationally ranked from unsophisticated, one-sided and misleading to more adequate, comprehensive and convincing.

I think the questions Hartman raises about psychohistorical claims positing childhood origins of historical events are well founded, especially in light of the shaky evidence supporting psychoanalytic hypotheses about childhood in general. Freud felt free to work out a developmental psychology of childhood exclusively on the basis of his work with adults and without any direct observation of children, and Klein speculated about the minds of infants on the basis of her work with older children. I have gone so far as to suggest that we ought to stick to what we really know in psychoanalysis from our direct clinical hermeneutic explorations of the psychic worlds of our patients ("Leaving Development to the Developmentalists." *Modern Psychoanalysis* 25, 1 [2000]: 43-51). I think Hartman's illustration of the pitfalls of blurring different levels of analysis by linking the paranoia in groups to that of individuals is well-taken, although his reference to the classical notion of the role of repressed homosexuality in individual paranoia is unfortunate precisely because it is itself a highly questionable generalization of the type that, in the field of psychohistory, it is in the spirit of his paper to question.

Biography

Donald Carveth teaches Sociology and Social & Political Thought at York University, Glendon College, in Toronto. He is a Training and Supervising Analyst in the Canadian Institute of Psychoanalysis (IPA) and a member of the Toronto and Canadian Psychoanalytic Societies. He serves on the faculties of the Toronto Institute of Psychoanalysis, the Toronto Child Psychoanalytic Programme and the Advanced Training Programme in Psychoanalytic Psychotherapy of the Toronto Psychoanalytic Society. He is a past Editor-in-Chief of the *Canadian Journal of Psychoanalysis/Revue Canadienne de Psychanalyse* and a member of the Editorial Boards of *Psychoanalysis & Contemporary Thought*, *Free Associations*, *PSYART: A Hyperlink Journal for the Psychological Study of the Arts* and the *Journal of Psycho-Social Studies*. Several of the publications listed and/or available for download on his website (<http://www.yorku.ca/dcarveth>) fall within the purview of psychohistory.