

What kind of research in psychoanalytic science?¹

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The kind of science that psychoanalysis is (can be), and the kind of research appropriate to it, qualitative and/or quantitative, have been divisive issues from the very inception of the discipline. I explore in detail the complexity of these issues, definitional and semantic, as well as methodological and substantive. A plea is made for the application of qualitative (idiographic) and quantitative (nomothetic) research methods, each to the extent that is appropriate, separately or in conjunction, across the entire spectrum of research domains in psychoanalysis, empirical, clinical, conceptual, historical, and interdisciplinary.

Keywords: psychoanalytic research, qualitative research, quantitative research

The place of quantitative *vis-à-vis* qualitative research, that has beset social and behavioral science over a now long and fractious history, has become equally contentious within the recent burgeoning of the empirical research enterprise in psychoanalytic science. Though most of us have a ready sense of what we seem to mean by the distinction between quantitative and qualitative, there are actually distinct definitional problems to begin with, when we make this juxtaposition, problems that are linked, though not bound, to the distinctions that we make between the natural and the human sciences within which the research is conducted, and the position of psychoanalysis within this spectrum of sciences, from the so-called ‘hard’ through the so-called ‘soft’.

Psychoanalysis as science

Within psychoanalysis there are even prior questions of the appropriateness of its claim to a place within the array of sciences at all, i.e. the questions of the nature of science, and within it then, the nature and place of research by which science intendedly, incrementally adds to its established and consensually agreed knowledge base. For Freud it was *a priori* evident that psychoanalysis is an evolutionarily-based, biological science, a science of the mind within the biological body, to be firmly anchored, as our knowledge increases, in its physiological and chemical matrix.

¹This paper has profited considerably from discussion with some of the members of a professional study group – Samuel Gerson, Peter Goldberg, Erik Hesse, Deborah Melman, Shelley Nathans, Harvey Peskin, Tzipi Peskin, Stephen Seligman and Judith Wallerstein.

Strangely though, Freud never linked the declared nature of psychoanalysis as a science to any need (requirement) for the formal systematic research by which science necessarily claims to grow. The only empirical research bearing on psychoanalytic concepts which Freud ever mentioned (and he did so approvingly) was a footnote reference in the *Interpretation of Dreams* (Freud, 1900, footnote added in 1919, pp. 181–2) to Otto Pötzl's experimental studies of subliminal perception (Pötzl, 1917). And when the American psychologist Saul Rosenzweig wrote to Freud in 1934, describing his laboratory support for some of Freud's central psychoanalytic concepts, Freud responded tersely and dismissively, stating that such confirmatory evidence was not needed, since psychoanalysis rested on such a wealth of positive clinical experiences. Freud ended acerbically with: "Still, it can do no harm" (Shakow & Rapaport, 1964, p. 129).

The argument about psychoanalysis as science (or not) and, if a science, what kind of science, and the place of organized research in relation to it, began, but did not end, with Freud. I have devoted two previous articles (Wallerstein, 1976, 1986) to a detailed statement of the many issues surrounding the placement of psychoanalysis in relation to the world of science. In the earlier paper (Wallerstein, 1976) I began with the statement that few theoretical issues were more constantly and passionately argued – among both adherents from within and observers and critics, friendly and otherwise, from without – than the status of our discipline as a science.

The attack from outside was stated most powerfully by Karl Popper (1963), who dismissively declared psychoanalysis to be only a pseudoscience, since its theoretical structure seemed to him elastic enough to explain any human activity or consequence as a confirmation of its postulates; therefore it did not allow for the possibility of falsification, i.e. of true testing of its theoretical tenets. From the inside, Edward Glover (1952), in his role as a polemicist on the shortcomings of psychoanalytic research, had declared even earlier that there is "no effective control of conclusions based on interpretation, [and this fact] is the Achilles heel of psycho-analytical research" (p. 405).

I will not repeat here any of my detailed responses to the variety of critics and of criticisms of the status of psychoanalysis as science. They have ranged widely, and from opposed and contradictory conceptual stances. There has been the hermeneutic critique which, in its extreme, has tried to remove psychoanalysis from the world of science altogether – and therefore from the need for any of what we usually mean by research, whether quantitative *or* qualitative, by which science necessarily grows. Though even here this has been qualified by Merton Gill (1983), someone lifelong committed to the need for empirical psychoanalytic research, who then sought to square this imperative with his increasingly hermeneutic viewing of the nature of psychoanalysis, by declaring it a "hermeneutic science" (p. 534), and adducing his reasons for this odd placement. While others in the *anti*-hermeneutic camp have seen the hermeneutic stance as a massive abdication of our scientific responsibilities as a discipline, labeled by Blight (1981) as a misguided and totally unnecessary "retreat to hermeneutics" (p. 150).

Oppositely, there has been what I feel to be a premature, and also an abortive, effort to transform the declared outmoded 19th-century energy and structure model of psychoanalytic theory (Freud's model) into a modern neurophysiologically-based natural science information theory and cybernetics model of the mind, functioning in a manner analogous to high speed computers. (See in this connection Peterfreund [1971] and Rosenblatt and Thickstün [1977, 1984].) Much of the current psychoanalytic fascination with contemporary neuroscience contains the same aspiration towards a transformation of the general theory of psychoanalysis into a presumably better, neurophysiologically-grounded (and more accepted) kind of science.

And from within the camp that declares psychoanalysis as is, a science, but fundamentally flawed in its efforts to function as science, is the sharp challenge posed centrally by the philosopher of science, Adolf Grünbaum (1984) who declared the data of psychoanalysis to be so hopelessly contaminated by the power of suggestion, that psychoanalytic findings derived from the therapy could have only 'heuristic', and not 'probative' value; and that therefore they were unable, however tested within the system, to be adequately confirmed. He declared that, to secure legitimate status as science, psychoanalytic findings would need extra-clinical confirmation outside the individual consulting room, experimental or epidemiological.

I argued all these positions in my 1986 article, *Psychoanalysis as a science: A response to the new challenges*, and presented my own conclusion, that psychoanalysis, as presently constituted and presently functioning, has successfully survived these challenges, and operates (still imperfectly to be sure) as a science, simultaneously developing general laws about how the human mind works, as well as pursuing the individualized working of these laws in the specific endowments and life experiences of the patients it treats. (This last statement was also the essential thrust of my 1976 article.) This overall posture is of course potentially modifiable in all its particulars – as all science is – with advancing data and concepts from all relevant sources, including, of course, neuroscience. My conviction is that the credibility of psychoanalysis as science – or at least its potential credibility as science – has survived its many challenges, and that its status as science is being increasingly accepted, at least within the psychoanalytic world.

The nature of psychoanalytic research

This conviction gives me warrant, I feel, to turn to the issue posed in my title for this article, *What kind of research in psychoanalytic science?* – since to me a central distinguishing feature of science, any science, is that it grows incrementally by research that systematically subjects its data and its tenets to as objective scrutiny as possible. And, in this connection, one reader of a prior draft of this paper recounted a pithy statement that he attributed to Robert Stoller, that psychoanalysis is (has been) the subjective study of subjectivity, striving to be the objective study of subjectivity. The question, that becomes then front and center, is of what *kind* of science psychoanalysis is, and the related, but far from isomorphic, question of what kind of research can best serve its scientific requirements. A German contemporary of Freud,

Wilhelm Dilthey, had divided the world of the sciences into *Naturwissenschaften* (the natural sciences, or the ‘hard’ sciences, prototypically physics and chemistry) and the *Geisteswissenschaften* (the spiritual sciences, meaning, in more contemporary idiom, the social and behavioral sciences). But over the years since, the declared distinguishing marks, supposedly clearly separating these two realms have become increasingly fuzzy or, even in some instances, broken down completely.

For example, Habermas (1968) strongly contended that causal accounts in human sciences, where he placed psychoanalysis, are always embedded in, and determined by, the uniqueness of history and context, whereas causal accounts in natural science are always universal, free of relationship to either history or context (p. 273). However, Grünbaum (1984) demonstrated convincingly that this is far from an invariant distinction. He gave homely examples of history and context-dependency in physical laws in the phenomenon that is called ‘hysteresis’ (pp. 18–19). For instance, the response of magnetizable metals to a magnetic field depends on the prior magnetization history of the given sample. Or, in elastic hysteresis, the response of a rubber band to stretching depends on its past history of being stretched. Or witness the breakdown of the very boundaries within the domain of natural science, through the rise of major hybrid disciplines of physical chemistry, of biophysics, and of biochemistry, all today of central importance to industry and to medicine, let alone of neuroscience, seeking to bridge the gulf between brain and mind.

Similar to the original distinctions created by Dilthey between the *Naturwissenschaften* and the *Geisteswissenschaften* were the roughly corresponding distinctions, elaborated by another German, Wilhelm Windelband, between nomothetic (generalizing, abstracting, seeking universal truths across the array of instances) research, declared to be applicable specifically to natural science, and idiographic (idiosyncratic, individualizing, seeking particular truth in the single, intensively studied instance) research, declared applicable specifically to the behavioral (social) sciences, embedded always in history and context. This nomothetic–idiographic distinction is still what is today conventionally meant by quantitative *versus* qualitative research methodology, despite the growing evidences that I will adduce, of a comparable growing breakdown of such invariant definitional distinctions.

Though some today would still try to place psychoanalysis as a biologically and cognitively based natural science – following Freud’s disposition – given current impetus by the enormous recent growth of neuroscience and the intense search for linking conceptualizations between the two realms, with even a new bridging journal, *Neuropsychanalysis*, nonetheless the great majority of those who have seen psychoanalysis as a science have understood it as one of the behavioral, or human, sciences, with the principal contentions being over the issue of what kind of research gives most promise to advance it as science. As already stated, Merton Gill (1983), a lifelong proponent of formal systematic research in psychoanalysis, who also became involved in the hermeneutic (and significantly anti-science) movement in psychoanalysis, carved out his own compromise position by labeling psychoanalysis a uniquely hermeneutic science which he defined as “obeying all the

canons of science [i.e. dependent for advancement on formal research] but dealing in the dimensions of human meaning, not in the dimensions of natural science” (p. 534). Earlier than Gill, Harrison (1970) in an article on the ‘scientific status of psychoanalysis’ made much the same point, describing psychoanalysis in quotation marks, as ‘our science’, implicitly our ‘peculiar science’, or declaredly in some way, our different *kind* of science, but nonetheless somehow still properly to be labeled a science.

This is not to speak of that still very significant sector of contemporary psychoanalysis which declares it not at all a science in any usual sense, in fact not even a psychology, which latter can be a science, but rather a discipline unique and *sui generis*, relying only on the conceptualizing of the findings that emerge in the psychoanalytic consulting room to enlarge its knowledge base, a position of which André Green has been a worldwide outspoken spokesman. This posture has been argued at length by Green in debate with Daniel Stern. (Green, 2000; Stern, 2000), and also with me (Green, 1996a, 1996b; Wallerstein, 1996). I have elaborated my own position on behalf of psychoanalysis as a (behavioral) science, one committed both to the elaboration of general laws of the mind and to their specifically differing particularization in the diverse individuals under study, and in description of the canons of psychoanalytic research, in detail, in a succession of articles spanning much of my professional career (see, for the main expositions, Wallerstein, 1976, 1986, 2006; Wallerstein and Sampson, 1971). In those articles I indicate what I consider to be the fatal shortcomings in the anti-science hermeneutic perspective on psychoanalysis, and argue my adherence to the belief in psychoanalysis as a science (and a profession), and to the requirement (and the sharp need) for the research necessary to sustain the science (see, in this last regard, Engel, 1968; Wallerstein, 1968).

However, as organized psychoanalysis, first within the American Psychoanalytic Association and, more recently, the overall International Psychoanalytical Association, have established (modest) funds for the support of psychoanalytic research – in implicit recognition of the status of psychoanalysis as a putative science – concern has come to the fore about what constitutes research appropriate to the subjectivistic and mostly private data of the psychoanalytic consulting room; and here the long-standing issues about the seeming exclusionary dichotomy between quantitative and qualitative research within the entire range of human sciences have clearly become central.

Sample size: One or many

One of the common assumptions about this distinction is that quantitative research is based on a sample of sufficient size so that confounding variables can be controlled out, and levels of incidence and prevalence can be established well enough, so that generalizations to the population at large can be confidently made. Qualitative research has, *pari passu*, been felt to focus more appropriately on single individuals, one at a time, where intensive study can more readily focus on individual factors of history and context. And yet research in the natural physical sciences, where quantitative meth-

ods prevail, can also be centered upon intensive study of the individual subject. A noted classic instance (from an educational context, however), was the teaching stance of the famed Swiss naturalist, Louis Agassiz, professor of zoology at Harvard University in the mid-19th century, who would give students a single specimen (a dead fish, for example) and ask them to study it and write down everything they could to describe it. However long they spent at this task, and however detailed their description, Agassiz would invariably send them back for second and third study sessions, to further amplify their observations and notations. The lesson was that even naturalistic study (and research) could profit from detailed individual observation, the qualitative method.

From the side of $N = 1$ studies, a hallmark of the qualitative method, there is a small but significant literature on its place in quantitative research. In 1965, William Dukes published an article entitled simply $N = 1$. In his abstract at the beginning of the article, he stated: “Despite the limitation on generalizing inherent in such studies, selected examples attest to their importance in the history of psychology” (Dukes, 1965, p. 74). He offered classic examples from earlier literature – Ebbinghaus’ famed 1885 investigations of memory, “a landmark in the history of psychology” (p. 74), based on experiments with only himself as subject; in abnormal psychology, Morton Prince’s 1905 account of Miss Beauchamp’s multiple personality, and Sigmund Freud’s case study with Breuer of Anna O’s hysteric illness, credited with containing “the kernel of a new system of treatment, and indeed a new system of psychology” (p. 75), among many other examples.

Dukes then elaborated his own four-fold division of situations in which single case observation can lead to generalized convictions. These are:

- i. “If *uniqueness* is involved, a sample of one exhausts the population. At the other extreme, an N of 1 is also appropriate if complete population generality exists (or can reasonably be assumed to exist) ... A variant on this *typicality* theme occurs when the researcher, in order to preserve some level of functional unity and perhaps to dramatize a point, reports in depth one case which exemplifies many” (p. 77, italics added).
- ii. “In other studies an N of 1 is adequate because of the *dissonant* character of the findings. In contrast to its limited usefulness in *establishing* generalizations from ‘positive’ evidence, an N of 1 when the evidence is ‘negative’, is as useful as an N of 1,000 in *rejecting* an asserted or assumed *universal* relationship” (p. 77, italics added to the words ‘dissonant’ and ‘universal’).
- iii. “a limited opportunity to observe ... a report of which may be useful as a part of a cumulative record (p. 77) ... “Situational complexity as well as subject scarcity may limit the opportunity to observe” (p. 78).
- iv. “Instead of being oriented either toward the person (uniqueness) or toward a global theory (universality) researchers may simply focus on a problem” (p. 78)

and here Dukes again referred to the classic memory experiments of Ebbinghaus.

Dukes ended his article with a quotation from McNemar, a statistician:

the statistician who fails to see that important generalizations from research on a single case can ever be acceptable is on a par with the experimentalist who fails to appreciate the fact that some problems can never be resolved without recourse to numbers.

(Dukes, 1965, p. 78)

The same overall support for single-case methodology, operative within the confines of the customary canons of quantitative research, has been offered by others, Harold Leitenberg (1973) in the case of psychotherapy research, and M.B. Shapiro (1961, 1966) in fundamental clinical psychological research – although, in each instance, from different perspectives than that of Dukes, in these cases, the perspectives of research design construction. Of this Leitenberg concluded: “Controlled experimentation and objective measurement of behavior are not only no longer antithetical to an interest in single cases; they have been demonstrated in behavior modification research to be particularly congenial to each other” (p. 88). Shapiro (1966) cited, in very early support of his declared ‘individual-centered research’, the observation of Claude Bernard who “pointed out 100 years ago, [that] the site of processes of change is in the individual organism; therefore, observations based on group averages and variances might be misleading” (p. 5). My own position here should be clear. An N of one (in distinction from an N of many) is not an essential hallmark of qualitative, as distinct from quantitative, research. On the basis of the considerations just adduced, an N of one can operate, depending upon how it is used, in both qualitative and quantitative contexts. Again the usual dichotomizing distinction does not hold universally.

Context of discovery or justification

Another major research divide is that between research in the context of discovery and research in the context of justification, with again the customary assumption that qualitative research, and, quintessentially, clinical psychoanalytic research within the case-study method innovated by Freud, must rest within the context of discovery; that is, able to generate fruitful hypotheses, but unable to substantiate them. It was this familiar contention, that Grünbaum so strongly built on, that Marshall Edelson, a psychoanalyst philosopher of science, questioned sharply in a series of articles (1985, 1986a, 1986b, 1989), as well as in an entire book (Edelson, 1984), all capped in the concluding section of his most remarked book (Edelson, 1988). Edelson’s overall thesis comprised his rejection of the value of the rival hermeneutic turn (or any presumed need for it) as an explanatory basis of psychoanalytic theory or praxis, with an impressive demonstration that – given essential conditions that he carefully spelled out – the traditional analytic single case study could be subjected to the usual canons of scientific scrutiny and should not be limited to the context of discovery. Psychoanalytic hypotheses could be tested rigorously within the framework of juxtaposition against plausible alternative hypotheses. And in this sense they could have (in Grün-

baum's phrase) 'probative value', advancing the psychoanalytic knowledge base within the context of justification.

In conclusion of his first article in the sequence cited, Edelson (1985, pp. 611–13) laid out what he called the "minimal standards" (p. 611) to make this possible within the single case study. Condensed down, these are: (1) the hypothesis needs to be stated clearly and prominently; (2) the author shows how the hypothesis explains the observations that are reported; (3) the author carefully separates facts (observable without knowledge or use of the theory being tested) from interpretations (based on the theory under test); (4) the author specifies what observations would be grounds for rejecting the hypothesis; (5) the author reports contradictory observations and indicates how he can deal with such counter-examples; (6) the author gives some argument as to why his observations are better evidence for his hypothesis than for a comparable rival hypothesis; (7) the author indicates what factors could have resulted in his obtaining his confirmatory data even if his hypothesis were incorrect – and why such factors can be properly dismissed as plausible alternative explanations; and (8) the author makes clear to what extent he proposes to generalize his hypothesis to other cases, and the argument justifying such generalization.

Admittedly, this is a difficult set of conditions to meet, and few extant case studies do meet such exacting standards, but in these papers Edelson, by argument and example, demonstrates powerfully that it is possible, and that it is a standard to which psychoanalytic case study should aspire, indeed must aspire, if it is to have 'probative value' in the advancement of psychoanalytic knowledge. If all that is adequately accomplished, the final sentence in the 1985 article would fully apply, and psychoanalytic case study could indeed operate within the context of justification, no less than can the rigorous deployment of quantitative study criteria, i.e. a large enough representative sample, control conditions, adequate statistical handling, establishment of stability, reliability, and validity of results, etc. Edelson's final sentence in that article says just that: "A case study meeting these standards would not have to appeal to subjectivity, meaning, and complexity and uniqueness as grounds for abandoning ordinary canons of scientific method and reasoning" (Edelson, 1985, p. 613).

Edelson (1986b) used an entire subsequent article as an exemplification of his tenets through study of Freud's account of the Wolf Man case, and the ways in which Freud did (or did not) live up to Edelson's requirements in creating a causal account of how early life experiences *caused* (produced) a later crippling obsessional neurosis. Edelson's concern was always with psychoanalysis as a system of causal understanding and explanation, i.e. a (causal) science. In his 1989 article he asserted at the end that:

All doubts expressed in the literature to the contrary, I believe that such a case study [as Edelson ideally constructed] can be used to argue that, given the *qualitative* data obtained in the psychoanalytic situation, the clinical *causal* inferences and *causal* explanations of psychoanalysis are scientifically credible.

(Edelson, 1989, p. 191)

Here we have a startling convergence of arguments from very opposed scientific traditions. Dukes (and Leitenberg and Shapiro) arguing from the tenets of quantitative, 'hard science', natural science research, embedded in number counting, give persuasive evidence that single case study ($N = 1$) can be used to generate new psychological knowledge in full accord with all their usual operative criteria; and Edelson, from the side of intensive single case study 'soft science' – in behavioral or human science like psychoanalysis – usually dismissively assigned only to the context of discovery, can nonetheless, if rigorously deployed, generate convincing causal demonstrations that should be persuasive to his 'hard science' counterparts. From both sides, single case study can be used to comparable ends, and by comparably rigorous methods.

This same distinction between research in the context of discovery (a supposed limitation of 'soft science' research) and research in the context of justification (the declared hallmark of 'hard science' research) has actually not been operative, even within the 'hard sciences'. Eric Kandel (2006), in his scientific autobiography, *In Search of Memory*, quotes the French molecular geneticist François Jacob's whimsical distinction between what Jacob calls day science and night science: "Day science is rational, logical, and pragmatic, carried forward by precisely designed experiments. 'Day science employs reasoning that meshes like gears, and achieves results with the force of certainty', Jacob wrote" (p. 240). Night science, on the other hand, "is a sort of workshop of the possible, where are elaborated what will become the building blocks of science. Where hypotheses take the form of vague presentiments, of hazy sensations" (p. 240). This can clearly be seen as a poetic description of the travel on the path – across the borderline twilight – from research in the context of discovery to research in the context of justification, written by a biologist reflecting on his genetic studies on bacteria, i.e. all in the realm of 'hard science'.

A signal example of this logical journey in psychological (and psychoanalytic) research across a porous boundary between the qualitative and the quantitative are longitudinal studies over significant time-spans, such as child developmental research, or psychotherapy process and outcome research over the entire span of the treatment, and into extended long-term follow-up periods. Here, periodic cross-sectional assessments, based on intensive depth study, can be followed through changing experiential (and maturational) vicissitudes over time into long-term patternings of outcomes. Such studies can be done with increasing (quantitative) statistical rigor through Markov stochastic processes, studies of sequences of trials, the outcomes of which are only probabilistically determined.

General versus aggregate-type propositions

Relevant here, in relation to the use of numbers, the *sine qua non* of 'hard science', is a methodological distinction made by the psychologist, David Bakan (1955) in a two-page article that seems to me to have been almost unremarked in subsequent literature. It has to do with the significance of

the next case in relation to what Bakan calls general-type and aggregate-type propositions:

A general-type proposition asserts something which is presumably true of each and every member of a designated class. An aggregate-type proposition asserts something which is presumably true of the class considered as an aggregate.

(Bakan, 1955, p. 211)

General-type propositions may be based on a single case study, as was Freud's proposition, derived from his study of the Schreber case, that repressed homosexual libido undergirds (and causes) paranoid illness. Aggregate-type propositions are basically statistical propositions, dealing with percentages, means, and correlations.

Where this distinction has methodological importance lies in the different role that is played in connection, within them, of the 'next' case. The 'next' case presents a fundamental threat to the validity of a general-type proposition. Bakan states that:

If a general-type proposition [like Freud's, cited above] fails to be confirmed by the observation of a member of the class to which the proposition presumably applies, then either the proposition must be rejected, or the class must be more closely delineated.

(p. 212)

Contrariwise, the 'next' case does not, in any way, challenge an aggregate-type proposition, the usual statistical study of an accumulation of instances. Rather it increases the 'power' of the test, no matter which way it comes out, and, therefore, the likelihood of the empirical proposition under consideration. Bakan's point was that failure to make this distinction can be at the root of considerable confusion in the handling of data, and in the signifying of results. Psychoanalytic propositions can be of both types, roughly corresponding to qualitative and quantitative research methodologies.

Conceptual and/or empirical research

A distinction specifically made within the psychoanalytic community is that between conceptual and empirical research, a distinction which the International Psychoanalytical Association has formally endorsed by creating two research committees, each devoted to the promotion of the one or the other of these two research domains. Basically, empirical psychoanalytic research *uses* psychoanalytic concepts in the empirical pursuit of psychoanalytic propositions, whereas conceptual psychoanalytic research makes the concepts themselves the *objects* of the research, the study of the origin, and the meaning and use of the concept, and its evolution, and often shifting meaning and use, over time.

This distinction was given strong impetus by an article by Sandler, Dreher and Drews (1991) which used the evolving history of the psychoanalytic conceptualization of trauma as its case in point, an article which in turn referred to a much earlier article by Sandler (1962) describing the Hampstead Index, developed at the Hampstead Child Therapy Clinic (now

re-named the Anna Freud Centre), which one can take to be the beginning of systematic conceptual research in psychoanalysis. Ulla Dreher (2000) followed the 1991 article, based on the study of the concept of trauma, with a monograph-length exposition of the possible scope of conceptual research, including a comprehensive delineation of the work previously done with the Hampstead Clinic (in London) and the subsequent trauma project (together with Sandler) in Frankfurt, Germany.

In a subsequent article, within a monograph entitled *Pluralism and Unity?*, describing the range of methods of research in psychoanalysis, Dreher (2003) laid out what can be considered the defining parameters of conceptual research. To begin with, “there is no conceptual research as a *standardized* procedure” (p. 109). Rather, “conceptual research is concerned with the *systematic* investigation of the meanings and uses of psychoanalytic concepts, including their change, in relation to both clinical *and* extra-clinical contexts” (p. 110). Here the concept of transference was used as the case example. After indicating the variety of usages and meanings associated with this concept, Dreher asked: “How is one to reconcile the various usages of the concept of ‘transference’ in such a diversity of contexts? Does everything that carries the label ‘transference’ actually contain ‘transference?’” (p. 104). This led to a definitional statement that “Psycho-analytic concepts have no exact definitions and thus no unequivocal and fixed meanings, but it is useful to describe them in an elastic meaning-space – a meaning-space with a core, but with indefinite and changing borders” (p. 116).

Research into these concepts can operate within an array “of different qualitative methods, literature analysis, interview techniques, and techniques of group discussion” (p. 116). But also, “meaningful empirical research, just like meaningful conceptual research, can be based on the use of both qualitative and quantitative methods” (p. 119). This becomes a salient point. Conceptual research can be quantitative as well as qualitative, just as single-case research can be quantitative (Dukes, Leitenberg, Shapiro) as well as qualitative (Edelson), and, in Edelson’s perspective, both in the context of discovery and in the context of justification, therefore also ‘probative’.

In a most recent article, Marianne Leuzinger-Bohleber (2006) makes a related, but somewhat different distinction. Calling the case-study method as innovated by Freud, and carried on by a host of psychoanalysts ever since, *clinical* research, Leuzinger-Bohleber separates off three other categories, *conceptual* research, *empirical* (and experimental) research, and *inter-disciplinary* research. She begins by defining the first, clinical research, as follows: “Clinical research is conceptualized as cyclic processes of observation in clinical situations, and unconscious processes of understanding or – in other words – of interpretation (2006, p. 1368) ... *Such research itself does not aim primarily at the development of concept or theories, but at deepening the understanding of clinical phenomena*” (p. 1369). From this starting point, she separates off the other three categories, though I would insert here the caution, that to be counted as research, not just search,

that the study be formal and systematic, and, to the extent possible, observable and replicable.²

Leuzinger-Bohleber takes her defining statement about conceptual research from Dreher's 2003 article (cited above):

As a working definition I would put it this way: conceptual research is concerned with the *systematic* investigation of the meanings and uses of psychoanalytic concepts, including their changes in relation to both clinical *and* extra-clinical contexts.
(Dreher, 2003, p. 110)

Empirical research, often quantitative, can, of course, also be qualitative (Edelson). "The difference from a clinical study is that the investigation is systematically planned, has a clear design and takes place 'extra-clinically' even if 'real' patients, from 'real (naturalistic)' therapies with 'real' therapists are studied" (p. 1372). Within this rubric, "*Experimental studies* are planned in a more rigid sense, analogously to 'experiments' in natural sciences (systematic selection of persons/patients to be investigated, design, tools, statistical methods, etc)" (p. 1372). An example of experimental research would be psychoanalytic dream research in a laboratory setting.

To illustrate the interconnectedness of these research categories, Leuzinger-Bohleber states:

In the context of this paper, it is important to mention that the *quality of empirical research* is always dependent on the *quality of the concepts* on which the research questions and hypotheses are based, and which are taken as reference points for interpreting empirical data. Empirical research without an interesting, clear and precise conceptual basis is empty and often not inspiring theoretically, empirically, or clinically. Thus, empirical, experimental and conceptual researches are interdependent and interwoven.

(p. 1372)

And to add a last category, there is interdisciplinary research. "*The focus of interdisciplinary research is the exchange of psychoanalytic knowledge with the non-psychoanalytic (scientific) world*" (p. 1373). This can, of course, be in exchange with any relevant branch of the cognate sciences of human (mental) functioning (child development, neuroscience, cognitive science, etc.). Actually, logically, what Leuzinger-Bohleber calls interdisciplinary research is of a different order from her categories, clinical, conceptual, and empirical. A study might be any of these three, and, in addition, also interdisciplinary if it crosses disciplinary boundaries.

²I state this caveat to distinguish psychoanalytic *research* from the extreme position advanced at one time by some (cf. Ramzy, 1963), that every clinical analysis is not only a search, but in its essence also a research. To quote: "One may hope that with more scrutiny of the requirements of logic and scientific method, and of what actually happens in psychoanalytic treatment, the standard psychoanalytic method will be considered the best research device so far suggested for understanding the human mind. It may turn out that every psychoanalyst who merely follows the method he was taught to follow will discover that he has been doing research, just as Monsieur Jourdain of Molière's *Le Bourgeois Gentilhomme*, suddenly discovered that he had been speaking prose for forty years without knowing it" (Ramzy, 1963, p. 74). I trust that it is completely clear from the entire text of this article that a search (for knowledge) which every proper psychoanalysis truly is, is not by itself a research, that the search must be formally specified and systematic, and, as indicated throughout this article, to whatever extent possible, observable and replicable.

Values for collective policy or individual intervention

An issue, implicit, and inferable from everything dealt with to this point, but not yet made explicit, is that of the kinds of knowledge that are derived from qualitative *vis-à-vis* quantitative research. This was the substance of an article by Paul Amato (2003), a family sociologist. His initial focus was that of counterposing Judith Wallerstein's researches into the impact of familial divorce on the children of the divorcing couple, with the approach of Mavis Hetherington, another prominent researcher in this area. The central question was of how severe, pervasive and long-lasting are the negative effects of the divorce upon the well-being and future development of the children, across the array of age ranges at the time of the parental separation. Wallerstein's decades-long studies (up to 25 years following the initial study at the time of the parental separation) have been variously faulted on the basis that her particular sample (60 families with 130 children) is not a truly representative sample, that she did not start with a proper comparison (or control) group (except one put together retrospectively at the 25-year mark), and that its method – the intensive individual study of each family member, father, mother, and each child, over many hours initially, and at each subsequent contact point, 18 months, 5, 10 and 25 years – made replication difficult. Hetherington (and numerous others) using more customary research methods – large samples geared to representativeness, established control groups, formal questionnaires, and appropriate statistical (correlational) methods – have likewise found detrimental impacts upon the children after the parental divorce, but, in the aggregate, less severe and of shorter duration.

Amato ended with the specific point that the customary quantitative methods, (Hetherington's) yielding nomothetic data, have their maximum applicability in the arena of public policy knowledge and recommendation, but are far less helpful in the individual clinical decisions that must be made – in regard to specific issues of custody, visitation, allowances for parental moves away, etc. – with regard to each different child at issue. Wallerstein's intensive individual clinical study, linked, in each instance, to history and context, yields idiographic data which maximize individual applicability, with significant but less precise value for public policy. Can the data from both perspectives, public policy knowledge and clinical intervention knowledge, be brought together to yield incremental, and more precise, overall value, whether in the arena of divorce, public policy, or studies in clinical, specifically psychoanalytic, theory and practice?

Qualitative *vis-à-vis* quantitative approaches

How can all this, recounted up to this point, be sorted out into the clearly interlocking relationship between qualitative and quantitative approaches in human science research? Dale Boesky (2002) has reminded us of the seemingly self-evident dictum enunciated by Aristotle more than 2000 years ago: "The same degree of precision is not to be sought for in all subjects ... it is the mark of an educated man to look for precision in each class of things

just so far as the subject admits” (p. 445). Though usually read as the advice to carry quantification as far as our knowledge permits, this famous quotation can also be seen as a caution, ‘but not to go further’. Malterud (2001), writing about ‘hard science’ *biomedical research*, has stated this opposite perspective: “The traditional quantitative research methods represent a confined access to clinical knowing, since they incorporate only questions and phenomena that can be controlled, measured, and counted” (p. 397).

It was Lawrence Kubie (1947) who had sounded this warning much more strongly in declaiming against the tendency in psychoanalytic writing to assess, and focus upon, the intensity of drive pressures, that Freud had originally emphasized as the quantitative aspect of his economic perspective – and, of course, at a time when the economic meta-psychological viewpoint was still at the forefront of explanatory efforts in psychoanalytic theorizing, and when current, far more sophisticated, quantitative research methods had not yet been developed. Kubie proclaimed: “It is my thesis rather that the easy assumption of quantitative variables as the only ultimate explanation of every variation in behavior is one of the seductive fallacies to which all psychological theorizing is prone” (p. 508). Such a presupposition is based on “a conviction that a science is not mature until it can count. Consequently to talk even of hypothetical and unmeasured quantitative variations [such as drive intensities] gives us a feeling of scientific maturity which may in fact be premature and illusory” (p. 511).

More strongly even:

Until it becomes possible to make quantitative comparisons of individual components in the complex stream of psychological processes, all quantitative formulations have at best only the limited value of descriptive shortcuts, and never provide a safe basis for explanations of behavior or of behavior differences.

(Kubie, 1947, pp. 517–18)

To Kubie, this made quantitative concepts in psychoanalysis only “descriptive metaphors” (p. 512), not explanations – a stern warning, certainly pointedly relevant in its time.

And from the perspective of academic psychology and child developmental research, the same warning has been sounded many years later by Herbert Zimiles (1993a, 1993b), at a point when quantitative research methods had advanced by a quantum leap in sophistication over what was available in Kubie’s time. Zimiles’s warning is about:

the adoration of ‘hard data’ (1993a, p. 369) ... The canonical bias [that] refers to a predisposition to assign inordinate importance to systematically gathered hard data, to exaggerate their potency and to selectively overlook their deficiencies and limitations ... The methodological orthodoxy that has evolved can be most tersely described as one that values ‘hard data’ (i.e. data that stems from operationally defined variables, have been systematically gathered, and are capable of being replicated and quantified), and dismisses ‘soft data’ (i.e. data that are subjectively derived and difficult to replicate and/or quantify). It is not merely that hard data are valued, they are primary; it is as if no other informational source or way of thinking matters.

(Zimiles, 1993a, p. 370)

This inevitably leads to:

restricting one's vision and curiosity to that which is observed and measurable..... We prefer to use a brief measure, however deficient, to gauge the impact of a specified environmental experience, because it fits the paradigm of empirical study, to an alternative strategy that would assemble the perceptions of experienced professionals with a long and wide-ranging history of observing the environments in question.

(1993a, p. 376)

In effect, researchers have adopted a special mode of discourse and function in accordance with an arbitrary set of conventions and rules. They devote comparatively little time to observing children as children, seldom engage in naturalistic observation, and are usually content to obtain brief and highly focused observations that are then subjected to extensive coding and statistical analysis.

(1993a, pp. 380–1)

And in response to spirited critical commentaries, Zimiles declared his avowed intent, not to separate researchers into 'good guys' and 'bad guys' but to advance knowledge in ways that, "are most likely to be achieved by the adroit use of processes of triangulation that capitalize on the consequence of multiple sources of knowledge, some clinical and/or nonsystematic, but nevertheless well-grounded in intensive observation" (1993b, p. 402).

This comes close to stating my own credo for the optimal research posture of human science, and specifically for psychoanalysis, marked by Gill as a 'hermeneutic science', and by Harrison as 'our science'. That the array of research methodologies marked out by Dreher and Leuzinger-Bohleber should all receive equal honor in the house of psychoanalysis, the clinical, the conceptual, the empirical, in whatever admixture (or parallelism) of qualitative and quantitative approaches best addresses the particular hypothesis under research scrutiny – with my difference from Zimiles, like my caveat with Leuzinger-Bohleber, that, in order to be counted as research, not just search, that the scrutiny indeed be systematic, and to the extent possible, observable and replicable.³

And it is within this both/and middle-ground, rather than the dichotomized either/or position, that commentators from our array of behavioral science perspectives have now increasingly defined the opportunities and the hazards. For example, the social psychologist Brewster Smith (1989), in a critical review of a book on the rise of experimentation in American psychology, used the occasion to embrace both directions, the quantitative and the qualitative approaches as equally validly grounded, and at the same time to warn against the dangers of scientism – what Zimiles had called the adoration of

³This is actually the official research credo of both the American Psychoanalytic Association and the International Psychoanalytical Association. Both the American's Fund for Psychoanalytic Research (FPR), of which I was the first chair (1976–1981), and the International's Research Advisory Board (RAB), of which I was the first chair (1997–2007), offer grant support to all varieties of psychoanalytic research, process and outcome therapy research, developmental, psychosomatic, neuroscience and cognitive science, conceptual, historical, archival, however quantitative or qualitative each proposal appropriately is.

'hard data' – that an unbalanced position can be vulnerable to. Although "the competition between natural science-oriented causal/explanatory approaches and humanistic meaningful/interpretive approaches will continue" (Brewster Smith, 1989, p. 16), it is nonetheless the balanced "interplay of causal and interpretive perspectives that seems most promising" (p. 16).

And from a somewhat opposed vantage point, the sociologist Immanuel Wallerstein (1997), talking of the natural sciences *vis à vis* the humanities makes the positive statement that:

We are in the process of overcoming the two cultures via the *social scientization* of all knowledge [for which he adduces evidence even from the writings of modern theoretical physicists, the prototypical hard scientists], by the recognition that reality is a constructed reality, and that the purpose of scientific/philosophical activity is to arrive at usable, plausible interpretations of that reality, interpretations that will inevitably be transitory, but nonetheless correct, or more correct for their time than alternative interpretations.

(p. 1254)

To some extent I agree. Everything is interpreted, whether the number on the blood pressure cuff, or the crying of the patient. In that limited sense one can think of all science as social scientization, or hermeneutic. But I use the word hermeneutic in its more conventional and narrower meaning, as reflecting one epistemological stance (as against others) in defining the appropriate advancement of knowledge.

It is in the context of these moderating and reconciling considerations that we should examine the stipulations expressed by a statistician J.B. Chassan (1953, 1956, 1957) in a sequence of three articles in the 1950s on *The role of statistics in psychoanalysis*, actually the title of the first of the three. Chassan's stance throughout was cautious and modest. His focus was on therapy research and the outcomes reported. He stated that the large number of possibly important variables, together with the comparatively small number of cases, and the overall complexity of the theory, sharply limited the degree to which the evaluation of results could "be made purely on a statistical basis" (Chazan, 1953, p. 160). Probability statements could "provide a means for an incipient quantification of psychoanalytic data" (1953, p. 161), but "such statements would have to be regarded as approximate" (1953, p. 161). This would be true as well, "with regard to the larger problem of determining the basis upon which a group of cases can be regarded as homogeneous" (1953, p. 161). Though a statistician, the great majority of Chassan's citations were to relevant (and supporting) aspects of the psychoanalytic literature.

In his second article, Chassan (1956) noted the limitations of statistically appropriate probabilistic statements:

The kind of problems to which the theory of probability and statistics are applied is distinguished by the circumstance that, either from principle or from practical consideration, it is thought impossible to predict with absolute certainty the 'final state' of a phenomenon from the 'initial state' and whatever other knowledge may exist with regard to the matter under investigation.

(Chassan, 1956, pp. 55–6)

This in turn leads to the thesis, in Chassan's third article (1957) of the (relative) unreliability of reliability in psychoanalytic therapy research when quantitative research methods are brought to encompass the phenomena:

Turning to the question of reliability, it seems to me that an almost direct consequence of the assumption of probabilistic patient-states is to cast serious doubt on at least certain aspects of the concept of reliability as these are measured by corresponding coefficients of correlation ... Whether one deals with precision as the reverse of the standard error, or with reliability as a coefficient of correlation, if the consistency of readings is to reflect validly the quality of a measuring instrument, then it must be implicit that the phenomenon under observation itself reflects a basic quality of consistency.

(Chassan, 1957, p. 165)

And "when one moves from the measurement of simple physical objects to that of the relatively complex characteristics of humans, one begins to get on less certain ground in a number of technical areas of research, and in particular with respect to various manifestations of consistent behavior or performance" (Chassan, 1957, p. 166). Which means that "a realistic view of many important research problems in psychiatry and clinical psychology demands a certain elasticity or comprehensiveness in design" (1957, p. 168). In overall summation, "the development of comprehensive data systems for the study of extensive psychotherapy accordingly must allow specifically for the study of variability in interpretation along with the variability in the more obvious aspects of the process" (1957, p. 170). And "the requirements of such research will ... be found in the development and application of more dynamic statistical systems and design concepts" (1957, p. 171).

This is the cautionary challenge, from pre-computer days, that many of today's burgeoning quantitative research studies of therapeutic processes and outcomes are designed to meet and, to the extent to which they succeed, can yield new knowledge that properly complements and enlarges upon the creative insights that have accrued within our qualitative researches based on the systematic application of our traditional clinical case studies. And it brings the presumably methodologically diverse methods closer in their premises, their view-points, and, hopefully, their findings.

Discussion: Models for qualitative research

Given this overall perspective upon the complementarity and mutual enhancement that comes from the combined (or parallel) use of both qualitative *and* quantitative research methods in psychoanalytic research, each to the extent appropriate, each trying to illuminate the types of questions it is best designed to answer and, to the extent possible, each helping to enlarge or complement the other, what further need be said to respond to the question that is my title, *What kind of research in psychoanalytic science?*

Actually, in regard to quantitative research, the epistemological framework, the methodology, and the kinds of results and conclusions that can appropriately be reached, have long been reasonably agreed and consensually deployed. It is all well known, mostly taken for granted within the research community, and needs no repetition here. By contrast, the govern-

ing assumptions and methods for appropriate qualitative research are far less known and agreed, and should be indicated here, drawn from within a much smaller established literature. Elliott, Fischer and Rennie (1999), writing from three different universities, wrote what they called 'evolving guidelines' for publication of qualitative research studies in psychological science. They did this under five headings, before their concluding caveats and promises: (1) definition of qualitative research, (2) philosophy of science considerations that undergird it, (3) the intended functions of the evolving guidelines, (4) a review of previous formulations of quality standards, and (5) the development of their own stated guidelines.

These authors start with the conventional distinction between the kinds of scientific questions that each research methodology can try to answer, that "qualitative research lends itself to understanding participants' perspectives, to defining phenomena in terms of experienced meanings and observed variations" (Elliott et al., 1999, p. 216), whereas "quantitative methods lend themselves to testing hypothesized relationships or causal explanations, evaluating the reliability, validity and underlying factor structure of psychological measures, and measuring degree of generalizability across samples" (p. 216). Within this division, quantitative research rests on positivism, maintaining the subject-object dichotomy, where "the role of subject is reserved for the researcher while the entity being researched is treated as an object that can be universalized" (p. 217). By contrast, qualitative research challenges this epistemological approach, and places emphasis instead, "on the particulars of human experience and social life (including discourse) by taking into account matters such as history, language and context that relativize the knowledge gained to the individuals and situations studied and to those doing the inquiry" (p. 217).

One needs, of course, to bear in mind the many considerations already adduced in this article that break down the distinctness and the exclusiveness of these conventional, distinguishing frameworks – as, for example, Edelson's demonstration that the usual quantitative standards of causal explanation, of reliability and validity, of generalizability are all achievable by (proper) intense qualitative study – as one examines Elliott et al.'s (1999) outline of evolving guidelines for publication of qualitative studies. After indicating a sequence of seven guidelines shared by both quantitative and qualitative research, such as specification of methods appropriate to the inquiry, clarity of the presentation of findings, etc., they list seven that they label specifically pertinent to qualitative research: (1) owning one's perspective, disclosure of values and assumptions, (2) situating the sample (describing the research participants and their life circumstances, which is, after all, the substance of reported therapy studies), (3) grounding in examples (allowing an appraisal of the fit between the data and the researchers' understanding of them), (4) providing credibility checks (multiple observers, 'triangulation' with external factors), (5) coherence (integration, while preserving nuances in the data), (6) accomplishing general *vs.* specific research tasks (an appropriate range of instances for the general *vs.* systematic and comprehensive description for the specific), and (7) resonating with readers (so that the reader feels the subject to have been validly clarified)

(pp. 220–4). Under each heading, the authors have provided what they feel to be examples of poor practice and of good practice.

Alvin Mahrer (1988) has also begun within the conventional dichotomizing framework between qualitative (discovery-oriented) and quantitative (hypothesis-testing) psychotherapy research, with the declared intent for the qualitative approach to displace the quantitative, as the more fruitful, as in fact the only one that, in his mind, will yield new knowledge. It is included here because of the detailed elaboration of two distinct kinds of qualitative approaches. To dismiss first his dismissal of the inadequacy of any hypothesis-testing (quantitative) methods in relation to psychotherapy, I will only cite his tendentious summary of his case against any place for numbers or statistical methods:

All in all, theories of psychotherapy are safely impervious from the findings of hypothesis-testing research. Psychoanalysis is in no grave danger from the analysis of variance. I suggest that hypothesis testing is essentially unable to fulfill its self-assigned mission of confirming or disconfirming psychotherapeutic propositions or theories. Indeed, in the field of psychotherapy the mission itself is fruitless. What is left for hypothesis-testing research in psychotherapy?

(Mahrer, 1988, p. 696)

Stated to this extreme, Mahrer's position can only be agreed with by that segment of the psychoanalytic world that is dubious about the place and the value of any formal, systematic research inquiry into the process and outcome of analytic therapy. It is certainly counter to psychoanalytic knowledge that has already accrued via quantifying studies (Dahl et al., 1988; Fonagy et al., 1999, 2002), and, of course, of hypothesis-testing approaches, even in qualitative studies (Edelson, 1984, and others) – which latter Mahrer has declared beyond achievement in either quantitative *or* qualitative approaches.

To switch to his positive side, Mahrer declares that:

the whole basis for designing discovery-oriented studies is the intention to learn more; to be surprised; to find out what one does not already expect, predict, or hypothesize; to answer a question whose answer provides something one wants to know but might not have expected, predicted or hypothesized ... Accordingly, the purpose of the balance of this article is to show why and how to carry out this alternative approach to psychotherapy research.

(p. 697)

Two major qualitative approaches are cited. The first, and more standard, is a five-step model: (1) selecting the target of the investigation (what do you want to study?), (2) obtaining instances of the target of the investigation (as many and as good instances as you can), (3) obtaining an instrument for taking a closer look (interviewing, audiotaping, describing within a group of judges, forming provisional categories from the instances), (4) gathering the data, applying the category-system, and (5) making discovery-oriented sense of the data (including a welcoming receptivity to the discoverable) and declining traps that mask the discoverable, of which Mahrer gives several examples. This is all called “taking a closer discovery-oriented look” (pp. 697–9).

Then Mahrer offers what he calls a second way to do discovery-oriented qualitative research in therapy. It is to organize the therapy into what he calls conditions, operations, and consequences, each of these terms being “taken as referring to more or less specific and concrete events rather than abstractions that are loose and general, and to events that occur in psychotherapy sessions rather than to anything occurring outside of these sessions” (p. 699).

Accordingly, “*conditions* refer to the patient in the session, to what the patient is doing and to how the patient is being ... *Operations* refer to what the therapist does right here in these statements ... [And] *Consequences* refer to what the patient does and how the patient is subsequent to the therapist operation” (p. 599). “The aim of this research is to discover the interconnections among in-therapy conditions, operations, and consequences” (p. 699). Out of this one emerges with three general questions for discovering the interconnections among conditions, operations and consequences (p. 699): (1) “Given this *operation*, carried out under this condition, what are the consequences? ...”, (2) Given this *consequence*, what operations under what conditions can achieve this consequence? ... [And] (3) Given this *condition*, what operation can achieve this consequence?” (pp. 699–700). Here also Mahrer describes a sequential research process, specifying the discovery-oriented research question, obtaining the relevant data, and again examining the data to obtain a discovery-oriented answer.

This all sounds of course exactly like what any serious student of psychotherapeutic processes does; what makes it research is the systematically specified nature of the process, its accumulation of as many instances as feasible, and its use of a number of independent clinical researchers in the examination of the conditions, operations, and consequences. Thus, what are called two kinds of discovery-oriented qualitative research are described, each with its characteristic methods and procedural steps. This leads to Mahrer’s unnecessary, and, in my terms, counterproductive, concluding implications:

The challenge is that the development of the field of psycho-therapy will benefit more from discovery-oriented than from hypothesis-testing research, and that rigorous and productive discovery-oriented research will unseat hypothesis-testing as the scientific means of inquiry in the field of psychotherapy research.

(p. 701)

A more restrained appraisal of the contribution of this article would be of the value of such a detailed description (with specific descriptive examples) of what are described here as two avenues for the conduct of qualitative therapy research.

The most comprehensive exposition that I have encountered of the values of qualitative research, its goals and procedures, its somewhat varying epistemological frameworks, its critical evolution, including its handling of issues of reliability and validity, its problems, and even its ethical issues, was written as far back as 1995 by Ambert et al., a group of family sociologists dealing with family studies – though easily translatable across to specifically psychoanalytic research. Though here one can see limitations, in psychoanalytically vital issues that are not remarked.

The authors begin, as others have done, with an outline of the goals and procedures of what they call “naturalistic qualitative research”, their focus on observational depth rather than breadth, on ‘how and why’ questions rather than on ‘what’ questions, on a broad spectrum ranging from the micro to the macro, and unswervingly throughout, a commitment to the empirical world, emphasizing within it, meanings, multiplicity of realities, and contextuality. What they call a “final goal of qualitative research is to refine the process of theory emergence through a continual ‘double-fitting’ where researchers generate conceptual images of their settings, and then shape and reshape them according to their ongoing observations, thus enhancing the validity of this developing conceptualization” (Ambert et al., 1995, pp. 880–1).

Expectedly they cite a variety of epistemological postures that frame qualitative studies, centrally the classical Chicago School epistemology, the grounded theory of Anselm Strauss and his collaborators, and also the ethnographic ‘thick descriptions’ of Clifford Geertz. Researchers are encouraged “to identify their own ethnic, class and gendered perspectives and to abandon the illusion that researchers, their informants, and the research setting do not influence each other reciprocally” (p. 882).

The issues of evaluating the relevant literature, and of the evaluation of the research data, are spelled out in detail, with the caveat at the end that: “above all, the richness of the quotes, the clarity of the examples, and the depth of the illustrations in a qualitative study should serve to highlight the most salient features of the data” (p. 884). And issues of validity and reliability, though not as crucial as in quantitative studies, can be encompassed by means of triangulation, use of “thick description” and “representing many layers of diverse realities” (p. 885). (Edelson, of course, has declared, and illustrated, his conviction that intensive psychoanalytic qualitative study [for example, of treatment processes], carried out according to his rigorous specifications, can reach reliability and validity of comparable level to that expected in quantitative [nomothetic] studies.)

This leads to discussion of procedures and ethics, with again a warning: “The potential exists for researchers to become advocates, abandoning the research role altogether” (p. 887). And, of course, there are all the ethical issues which are more salient in research involving intensive interviewing, issues of invasion of privacy, of excess exposure, even of potential betrayal. Obviously, truly informed consent needs to be ongoing and recurring, not a one-time event. And with all this, there are many problems in the evaluating of qualitative research findings. What needs to be kept clearly in mind by the reviewers is that:

There is a difference between a statement that is contradicted by the data and a statement that tries to go beyond the data in order to formulate new research ideas or advance therapy – a problem very specific to qualitative research because of its goals.
(p. 888)

And from the point of view of the research, “One has to distinguish between exploratory research and totally unguided research. The work must have a clear relation to the extant data-based literature” (pp. 888–

9). This is another way of distinguishing between search and research – which then leads to the inevitable concluding questions: “Has something new been learned by the research and what is its significance? Does it contribute to knowledge cumulation? Will it inspire further research?” (p. 890).

Overall the article by Ambert et al., published well over a decade ago, albeit not in a journal customarily read by the general, or the specifically research, psychoanalytic audience, sets forth a most comprehensive exposition of the philosophy of science framework, the goals, the procedures, the pitfalls and problems, and the scientific presentation of qualitative research, and stakes out a persuasive method for its proper evaluation. There are omissions, obvious to the psychoanalytic reader. For example, there is not one word about countertransference issues, in both their potential role as deforming biases when not understood, and as significant avenues to knowledge when properly understood and encompassed. Nonetheless, as a set of guidelines for the conduct of properly qualitative research this article can take its place alongside the many more, and far-longer established, guides for the performance of quantitative research. Altogether, it and the other sources quoted here should help make the qualitative research output, in human science generally, and in psychoanalysis in particular, as scientifically credible as the best of the quantitative research products, and, thereby, equally able to provide incrementally cumulative answers to the myriad research questions that face the still young science of psychoanalysis.

Conclusion

The kind of science that psychoanalysis is (can be), and the kind of research appropriate to it, qualitative and/or quantitative – in all their form variants – have been divisive issues from the very inception of the discipline. I explore in detail the complexity of these issues, definitional and semantic, as well as methodological and substantive, indicating how the various ways of subclassifying research: (1) search *vs.* research, (2) qualitative *vs.* quantitative, (3) idiographic *vs.* nomothetic, (4) general-type *vs.* aggregate-type, (5) clinical *vs.* conceptual *vs.* empirical, (6) context of discovery *vs.* context of justification, all relate to one another, with varying degrees of, but never complete isomorphism across some of these categorizations.

I make two pleas: (1) that we appreciate the porousness of the boundaries between seemingly dichotomized and totally distinct categories, like idiographic (or qualitative or single-case), and nomothetic (or quantitative or multiple-case) studies, with each side of these declared exclusionary dichotomies actually amenable to shaping that encompasses elements of the other side; and (2) that we then transcend these so often specious distinctions in applying research methods, however varying quantitative and/or qualitative, each to the extent that is appropriate separately or in conjunction, across the entire spectrum of research domains in psychoanalysis, clinical, conceptual, and empirical.

I do not discuss in this article the issue of research priorities in psychoanalysis, whether they should be propounded, and, if so, how, nor what will

best convince the outer world, the world of insurance reimbursement, of the values of psychoanalysis, both as theory and as practice, since I feel that these are essentially political and economic, not scientific, matters.

Translations of summary

Welche Art von Forschung in der psychoanalytischen Wissenschaft? Seit den Anfängen der Disziplin wird die Frage, welche Art Wissenschaft die Psychoanalyse ist (sein kann) und welche Art von qualitativer und/oder quantitativer Forschung ihr gemäß ist, kontrovers diskutiert. Der Beitrag ist eine detaillierte Untersuchung der Komplexität dieser Probleme auf definitorischer, semantischer sowie methodologischer und inhaltlicher Ebene. Der Autor plädiert dafür, qualitative (idiographischer) und quantitative (nomothetischer) Forschungsmethoden in dem jeweils angemessenen Umfang innerhalb des gesamten Spektrums der empirischen, klinischen, konzeptuellen, historischen und interdisziplinären Forschungsbereiche der Psychoanalyse anzuwenden.

¿Qué tipo de investigación para la ciencia psicoanalítica? Determinar qué tipo de ciencia es (o puede ser) el psicoanálisis y precisar qué tipo de investigación es más apropiada para ella, cualitativa y/o cuantitativa, han sido cuestiones conflictivas desde los comienzos mismos de la disciplina. El trabajo examina detalladamente estos dilemas en toda su complejidad, en sus aspectos semántico, metodológico y sustantivo. El autor aboga por la aplicación de métodos de investigación cualitativos (idiográficos) y cuantitativos (nomotéticos) en la medida en que sean apropiados, separados o en combinación, en todos los ámbitos de investigación del psicoanálisis: empírico, clínico, conceptual, histórico e interdisciplinario.

Quel genre de recherche pour la science psychanalytique? Le genre de recherche que représente, ou que peut représenter, la psychanalyse, et le genre de recherche qui lui est le plus approprié, qualitative et (ou) quantitative, ont constitué des questions controversées depuis les commencements de cette discipline. L'article explore de façon détaillée la complexité de ces questions, au plan de la définition, aux plans sémantique et méthodologique, ainsi que du point de vue de la substance. Une requête est formulée en faveur aussi bien de la méthode qualitative (« idiographique ») que quantitative (« nomothétique »), chacune dans le périmètre qui lui est approprié, séparément ou conjointement, à travers la totalité du spectre des domaines de recherche en psychanalyse : empirique, clinique, conceptuelle, historique et interdisciplinaire.

Quale tipo di ricerca per la scienza psicoanalitica? Il dibattito su quale tipo di scienza sia (o possa essere) la psicoanalisi, e su quale tipo di ricerca, qualitativa e/o quantitativa, sia ad essa appropriato, è stato al centro di controversie fin dai primi esordi della disciplina. Il lavoro esplora in dettaglio la complessità di tali questioni, dal punto di vista semantico e definizionale, così come metodologico e sostantivo. Viene proposta l'applicazione di metodi di ricerca qualitativi (idiografici) e quantitativi (nomotetici), ognuno dei quali nella giusta misura, usati separatamente o congiuntamente. Tali metodi dovrebbero essere usati nella ricerca di ogni aspetto della disciplina: dall'empirico, al clinico, al concettuale, allo storico, all'interdisciplinario.

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