

Commentary on Logical Operations in Theory-Building Case Studies**On Systematizing Theory-Building Case Studies****PETER E. NATHAN**^{a,b}^a Department of Psychology, University of Iowa, Iowa City, IA^b Correspondence concerning this article should be addressed to Peter E. Nathan, Department of Psychology, University of Iowa, 11 Seashore Hall E, Iowa City, IA 52242-1407E-mail: peter-nathan@uiowa.edu

ABSTRACT

Bill Stiles wants to enhance the reliability and the utility of theory-building case studies. To do so, he proposes a systematic, replicable approach to clinical case materials that to date has been difficult to use systematically. At the core of his proposal is what he calls “logical operations,” which include some familiar elements of logic: deduction, induction, and abduction. While not without problems in concept and execution, Stiles’ proposal offers those who want to use theory-building case studies in their own work the prospect of an empirically-based analytic tool in a field that has not to this time had one.

Key Words: systematizing case studies; “logical operations”; induction; deduction; abduction; Assimilation model

The thesis of Bill Stiles’ elegant essay, “Logical operations in theory-building case studies” (Stiles, 2009), is that case studies can be effective means of developing and testing theory and practice. Labeling the case studies in which he is most interested “theory-building case studies;” Stiles distinguishes them from “clinical case studies,” which “aim primarily to gain a deeper understanding of a particular case” (Stiles, 2008, p. 9). Stiles also describes three “logical operations” he believes the authors of theory-building case studies utilize to test hypotheses. Some theory-building case studies propose or develop theories of etiology; others describe the signs and symptoms of psychopathologic entities. Since Bill Stiles’ primary focus in his essay is on evaluating the efficacy of treatment, however, that’s what we focus on in this commentary.

The logical operations to which Stiles refers are familiar elements of logic: deduction, designed to create logical consistency and interconnection; induction, by which observations are applied to theory; and abduction, used to create, refine, and elaborate theory. In the ideal theory-building applications on which Stiles focuses, these elements are intended to permit theorists to compare detailed case observations to detailed clinical theories, so that “researchers (can) creatively modify their theories by (abductively) adding to them or altering them so that they correspond to accumulating observations” (Stiles, 2009, p. 9). Stiles has used logical operations himself to develop a theory of psychological change he and his colleagues term the assimilation

model. “It is a developmental account of therapeutic change that describes a regular sequence of stages through which people’s problems pass in successful psychotherapy, along with processes that underlie it (Stiles, 2009, p. 10). We discuss his development of this theory later in this commentary.

I applaud Stiles’ efforts to enhance the reliability and, perhaps, the utility of theory-building case studies by proposing a systematic, replicable approach to clinical material that has proven difficult to use systematically. The intent of what Stiles puts forth is strikingly similar to the intent of those who rejected *DSM-I* and *DSM-II* in favor of a new paradigm for syndromal diagnosis, *DSM-III*. Still, I have some concerns and will voice them here.

MY CONCERNS

When Dan Fishman, editor of *Pragmatic Case Studies in Psychotherapy*, asked me to comment on Bill Stiles’ essay, Dan described it as “a very important, general statement by Bill Stiles about the epistemological potential of the case study method for developing psychological theory, particularly in the arena of psychotherapy theory.” And so it is. Among others, I see my role here to raise some questions about whether in fact Stiles’ essay describes a “general statement” or a specific, more limited one. In comparing theory-building case studies and between-group designs intended to reveal differences in efficacy between treatments, I will also refer to the unique strengths of randomized clinical trials to highlight such differences.

I have three major concerns about Stiles’ arguments, along with some minor ones. The first is with his implication that, when it comes to assessing psychotherapy theory, one has to make a choice between the case study approach and the “group experimental method.” Instead, both methods seem to me to have value and both ought to have a place when what works in psychotherapy is to be evaluated. I also disagree with Stiles’ apparent conviction that any theorist can use “logical operations” to test the validity of his or her theories. I think differently: there are both individual and systemic impediments to applying these elements of logic to make decisions about theory in many case studies. Finally, I disagree strongly with his view that Freud’s case studies, and the many case studies modeled after them that have followed to our time, qualify as “scientific research.” Freud’s case studies, of course, are nonpareils: Remarkably, they meet the standards of great literature; they are literary, humane, and insightful; they serve as unique repositories of the clinical concerns and preoccupations of an earlier era; they reveal a great deal about Freud’s evolving etiologic theory; and they give us insight into the development of Freud’s enormously influential approach to treatment. But with all of that, I do not believe they qualify as scientific research, that Freud used them for that purpose, or that we could do so. In what follows, I will endeavor to explain my reasons for taking these positions.

WHEN DO CASE STUDIES QUALIFY AS SCIENTIFIC RESEARCH?

Toward the end of an earlier essay, “When is a case study scientific research?,” Bill Stiles (2003; reproduced in Stiles [2009] as Appendix A) affirms his conviction that “case studies offer an alternative that can complement hypothesis-testing research” (p. 9). In support of his conviction, Stiles (2003) refers to two of Freud’s best known case studies, Dora (Freud,

1905/1953) and Schreber (Freud, 1911/1958), expressing the belief that they “qualify as scientific research (p. 9).”

As in most of his case studies, Freud had two or three simultaneous goals for Dora and Schreber: (1) Both the Dora and Schreber cases described and delimited the signs and symptoms of what Freud termed, respectively, hysteria and dementia praecox; neither diagnosis is used today; (2) Both the Dora and Schreber cases proposed etiologic theories based on Freud’s developing psychoanalytic theory; neither theory has lasted to our time; (3) The Dora case but not the Schreber case depicted the therapeutic interactions between patient and therapist; in particular, Dora emphasized the role of transference in Freud’s developing psychoanalytic technique.

Daniel Paul Schreber was the president of the panel of judges on the court of appeals at Dresden in the early 1890s when he developed symptoms of the paranoid psychosis which we would today label paranoid schizophrenia. Freud never met Schreber, so he could make no first-hand clinical observations of Schreber’s bizarre delusions of persecution or the ultimately unsuccessful therapeutic efforts Schreber’s psychiatrists attempted. Everything Freud knew about the patient came from his reading of Schreber’s classic *Memoirs of My Nervous Illness* (1903/1955), written some years after his paranoid psychotic break and subsequent nine-year hospitalization. As a result, Freud had no opportunity to subject his observations or therapeutic efforts (there were none by him) to the logical operations Stiles believes move theory along and enable hypotheses to be tested in therapeutic case studies. Freud did draw conclusions about the etiology of Schreber’s psychosis stemming from Schreber’s preoccupation with sexual delusions; they have subsequently been largely discounted.

Dora (Ida Bauer) was an 18-year-old girl who was said to have an unusually close relationship with her domineering father. Dora’s complex of symptoms included depression, suicidal ideation, episodes of amnesia, loss of speech and of voice, and avoidance of social contact. Freud undertook a continuing analysis of Dora’s dreams in the effort to understand her confusing array of symptoms. He concluded that Dora was suffering from hysteria, which we would today consider a mixed mood, dissociative, and somatoform disorder; proposed an hypothesized etiology derived in large part from the analysis of her dreams, which emphasized the role in her illness of a (real or imagined) sexual seduction at the age of 16 and was emphatically rejected by Dora; described the treatment, in which transference played an important role and from which Dora abruptly took leave; and hypothesized the mechanisms by which symptoms were initiated and maintained. Although Freud continued to maintain his belief in the central role of the transference phenomenon in psychoanalytic treatment as well as the value of dream interpretation in assessment through his career, he did modify his treatment methods in part because of his unsuccessful therapeutic interactions with Dora. This case is notable, in the context of this discussion, because Freud repeatedly insisted that his analysis of the case of Dora represented scientific empiricism based on pure observation, even though others have concluded that in fact Freud constructed his theory based on very questionable assumptions. I could not find clear-cut examples of induction, deduction, and abduction in this case study.

Freud described Dora and Schreber’s behavior in great detail in an attempt to reach a reliable diagnosis. He was, of course, at a considerable disadvantage compared to contemporary

clinical researchers; a reliable and useful diagnostic system based on empirical data had not yet been developed (and would not be for another 75 years or so.) Nonetheless, Freud's clinical descriptions were exemplars of the searching, detailed clinical portraits that psychoanalytic clinicians have painted over the years, exemplifying the careful clinical observation that any clinician would do well to emulate.

Freud also utilized the Dora and Schreber cases to develop his etiologic theories, within the overall development of psychoanalytic theory. As he saw these patients, and then as he summarized the relatively few treatment sessions he had with Dora or recounted crucial elements from Schreber's *Memoirs of My Nervous Illness*, we see him weighing nuances of his etiologic theories based on Dora's verbal and nonverbal behaviors and dreams, and Schreber's admittedly subjective description of his feelings, thoughts, and actions. As it turns out, the etiologies Freud ascribed to both Dora and Schreber have subsequently been rejected by most psychoanalytic clinicians.

Most relevant to this discussion was Freud's use of the case of Dora to illuminate benchmarks in the course of psychoanalytic treatment, to analyze Dora's responses to the intensive emotional interchange between patient and therapist and, in all of this, thereby to weigh and assess what was helpful and what was not in her treatment. The result, well beyond the confines of the case of Dora, was Freud's psychoanalytic treatment as we have come to understand it, with its emphasis on the central role of transference in treatment gains, as well as its assumption of a more or less predictable pattern of patient response to therapist forays.

Stiles claims that these cases "qualify as scientific research," in part because they "permeated psychoanalytic theory (that is, the theory was altered by them), and the detailed fit between the theory and the cases helped increase confidence in the theory" (p. 9). These two case studies are arguably among Freud's most influential. But are they examples of "scientific research?" I think not. They are elegantly written, full of clinical insight, obviously the product of a wise and humane clinician, but none of these attributes qualifies them as scientific research. The following quotation from the *APA Dictionary of Psychology* (VandenBos, 2007) helps explain why I take this position.

Scientific method: a group of procedures, guidelines, assumptions, and attitudes required for the organized and systematic collection, interpretation, and verification of data and the discovery of reproducible evidence, enabling laws and principles to be stated or modified. (VandenBos, 2007, p. 818)

In my judgment, the key phrases in this quotation are "organized and systematic" and "reproducible evidence, enabling laws, and principles." Above all, I think the scientific method represents a *systematic* effort to gather data in a *reproducible* manner. I don't believe that Freud's case studies were either reproducible or systematic.

At the same time, if one reviews Freud's numerous case studies sequentially, it is clear that his theories of etiology and of behavior change developed and changed as he tested and discarded some beliefs and retained others that better fit the facts of the cases as he perceived them. This was surely an hypothesis-testing process: Freud was sequentially testing a series of

developing interwoven hypotheses about how a particular complex of signs and symptoms develops and how it might be addressed by therapeutic means—ultimately, by the psychoanalytic method designed to bring the unconscious determinants of symptoms to consciousness and thereby to alleviate the symptoms. But the underlying system by which Freud decided that a theory required change was rarely apparent, although a number of commentators have felt they identified such schemas. And the testing of Freud's evaluative system did not appear to be reproducible, even by Freud. I conclude that the manner in which Freud used his case studies to develop his etiologic theory and his psychoanalytic treatment method did not constitute what behavioral scientists today would consider science. That is not to say, of course, that they were not immensely important to the history of psychiatry. Few other bodies of psychiatric literature can compare to them in influence.

CASE STUDIES AND RANDOMIZED CLINICAL TRIALS

Ideally, in my view, the development of an efficacious therapeutic approach should involve the gradual accumulation of a number of case studies of diagnostically homogeneous patients sufficient to enable repeated testing and continued refinement of the treatment. At that point, however, testing the efficacy of treatments by single case studies ought to give way to the development of an hypothesis testable by a group design utilizing a randomized clinical trial. The hypothesis would predict that the experimental treatment would yield a superior outcome compared to a comparison treatment; both treated groups would comprise patients with the same diagnosis. The virtue of a randomized clinical trial, as against the case study method, is the substantially greater generalizability of its findings. Reasons include the enhanced significance of multiple over individual participants, thereby increasing the power of the treatment comparisons; the random assignment of patients to experimental and comparison treatments, thereby reducing or eliminating the possibility of systematic assignment biases; the use of standardized treatment protocols, including treatment manuals when they are available, to ensure that all therapists learn the same material detailing how the treatment is to be delivered; and the use of fidelity measures to ensure consistent application of the treatment as designed. For those readers who are interested, Jack Gorman and I have described this methodology in greater detail in the Preface to *A Guide to Treatments that Work (3rd Edition)* (Nathan & Gorman, 2007).

Freud's case studies have justifiably become landmarks in the psychiatric literature. Their author's humanity distinguishes them, as does his erudition, breadth of knowledge of classical thought, apposite allusion to literary figures and events, and mastery of the clinical skills of the time. But esteemed as they are, in my judgment, Freud's case studies are not good examples of the process by which Bill Stiles says case studies can be used to test theory or the efficacy of a treatment. Above all, I do not believe they qualify as scientific research. Reasons are many. They include Freud's conviction that his theories did not require inquiry, scientific or not, to demonstrate their validity; in some sense, then, many of Freud's case studies represent theory-demonstration rather than theory-building. It is also the case that most of the patients Freud chose to write about were literally one of a kind, which meant that any systematic effort to capture the most characteristic signs and symptoms of a diagnostic entity, so as to assemble a group of diagnostically homogeneous patients, would be almost impossible to achieve. Finally, because Freud's psychoanalytic method depends so much on the unique interplay between patient and therapist, it would not have been possible to achieve the consistency and

predictability a treatment requires to allow it to be weighed against a comparison treatment. For these reasons, case studies through the years, especially those written from the psychoanalytic perspective, have proven to have limited value in supporting hypothesized etiologic theories, specifying a characteristic syndromal profile for a diagnostic entity, or supporting the efficacy of a treatment. This means, I think, that Stiles' logical operations to test hypotheses in case studies are too often under-utilized for this purpose and hence remain an ideal rather than a reality.

LOGICAL OPERATIONS IN *LIVES IN PROGRESS*

While they are not plentiful, I can cite examples of theory-building case studies which have been used more successfully than Freud's case studies, and psychoanalytic case studies more generally, for the purposes and in the manner Stiles envisions. The examples that come first to mind are the case studies included in the successive editions of Robert W. White's classic text, *Lives in Progress* (1952, 1966, 1972). Offering insights broadly derived from the psychoanalytic, humanistic and personality traditions, White also brings impressive sensitivity to clinical issues as they affect both normal and deviant persons to the presentation and discussion of the several lengthy case studies in each edition of the book. In successive editions, White revisits the actors portrayed in the case studies, updating the trajectory of their lives and modifying his views of their motives, intentions, and self-assessment as additional information about them has come to him.

A basic strength of this approach lies in its close attention to data from the lives in progress of the objects of his scrutiny. With appealing modesty and a very light touch, both of which stand in sharp contrast to Freud, White deduces logically consistent and interconnected relationships in each individual's life as well as, on occasion, between the principals in two or more case studies. While I don't think these instructive observations necessarily qualify as scientific research – White would certainly not have dignified them with that label – they do illuminate important aspects of these lives that otherwise would have gone unnoticed. Does his method enable White to affirm key tenets of psychoanalytic or humanistic theory? On occasion, but so subtle is his hand that one has to look hard for examples.

I cite these books in large part because of the strong impression the first edition made upon me as an undergraduate, when I first read it. Others have written similar books, but it is my sense that White's was one of the first to bring to the college student market actual case studies that illustrated psychopathology at an accessible level; showed how lives over time change (and sometimes, in specific areas, do not) in accord with his theory of behavior change; and suggested how therapy can affect behavior, not always for the better.

Like Freud, White tested his theories of behavior change by means of his case studies. Unlike Freud, White's case studies allow for speculation and doubt as to the validity of an interpretation, in accord with White's *persona* as a modest, truth-seeking psychologist. As a youthful college student with aspirations to become a psychologist, I was struck by the contrast between Freud's certainty that he was right and White's openness to modifying diagnosis, theory of change, and means of bringing it about. The distinction between the two has meaning beyond its impact on a naïve college student: It suggests to me that, for Stiles' logical operations to function to build theory utilizing case studies, the theorist must be open to the possibility that his or her original premises on describing a case could profitably be changed as additional clinical

observations come to his attention. It worked this way for *Lives in Progress*, in which it is not difficult to see how White built theory by using deduction, induction, and abduction as he reviewed changes in the lives of those whose lives he recorded. By contrast, theory-building doesn't work nearly as well in the Dora case or when case studies are used primarily to illustrate or buttress theory rather than to test it.

However, neither Freud nor White took the next step: to determine not only whether their treatment efforts worked but whether they worked better than a robust comparison treatment. Comparisons of this kind are the state-of-the-art in randomized clinical trials of treatments today. I am certain it never occurred to Freud to question the validity of his etiologic theory or the efficacy of his treatment method. He clearly believed that his efforts would not benefit from scrutiny because their worth, in his eyes and in those of most of his supporters, was self-evident. It is also the case that prescribing an RCT in Freud's time would have made no sense: diagnostically homogeneous patient groups would likely have been impossible to assemble; there simply were no comparison psychosocial treatments of any merit; and the specifics of the RCT method itself had not yet been developed.

White was sufficiently modest that he would probably have undertaken an honest effort to compare his treatment to others had one been suggested, but he lived at a time when such a comparison would have been extremely difficult. In the 1950s and 1960s, when the editions of *Lives in Progress* were published, the efficacy of psychoanalysis and psychoanalytic psychotherapy, treatments of choice for several decades to that time for the "neurotic" disorders, had begun to be questioned, following Hans Eysenck's influential attacks (e.g., 1952, 1960) on their efficacy. Although no other equally well-accepted treatments had yet been developed, Joseph Wolpe had begun to publish intriguing findings from his experiments with systematic desensitization (e.g., Wolpe, 1958) and others had begun describing the power of behavior therapy and behavior modification (e.g., Lazarus, 1963; Leitenberg, Agras, Thomson, & Wright, 1968). With what would White have compared his treatment and how would he have done so?

LOGICAL OPERATIONS IN THE DEVELOPMENT OF THE ASSIMILATION MODEL

In his 2003 essay, Stiles describes the process by which he and his colleagues developed the assimilation model of therapeutic change in the following words.

At the core of the assimilation model is an observational strategy: identifying problems and tracking them across sessions, using tape recordings or transcripts. Drawing cases from a variety of therapeutic approaches, we have observed how expressions of a problem differ from time to time, we have inferred a process of change, and we have developed concepts to describe the process (Stiles, 2003, pp. 7-8; reproduced in Stiles [2009] as Appendix A).

In describing the development of the assimilation model, Stiles observes that "although there have been some statistical hypothesis-testing studies addressing the assimilation model, the model has grown mainly from the case studies" (Stiles, 2003, p. 8). True to this observation, in consonance with the theory-building case study model described in the current essay (Stiles, 2009), Stiles and his colleagues (e.g., Stiles et al., 1990, 1991; Stiles Meshot, Anderson, &

Sloan, 1992) detail how and to what extent logical operations played a role in the development of the assimilation model. While it is not possible to glean from these reports precisely when and how logical operations contributed to the final outcome, the assimilation model of therapeutic change, it is nonetheless clear that they did play a significant role in this process.

Do Stiles' efforts to develop the assimilation model qualify as scientific research? Specifically, were they "organized and systematic" and did they yield "reproducible evidence, enabling laws, and principles." It seems to me that they did both. Therefore, according to this standard, they do appear to qualify as scientific research. Of course, Bill Stiles doesn't need me to pass either positive or negative judgment on the worth of what he has done in order to feel that he has made a substantial contribution to the field. Stiles is in the best position to know what he has done, and I trust that he judges his work as positively as do the rest of us.

Still, he could have gone a step further. That step, developing a randomized clinical trial, would have permitted him to compare the efficacy of the assimilation model of psychotherapy with another behavior change method like cognitive behavior therapy or interpersonal psychotherapy. In so doing, Stiles would have been able to know whether his treatment not only worked, but whether it worked as well or better than a robust comparison treatment method. That would have been useful information to have,

A CONCLUDING OBSERVATION

I would like to conclude by returning to an earlier issue – the worth of theory-building case studies as a function of the openness of the theorist to change in his or her theories. This issue plays a central role in determining the extent to which case studies can be used to validate theory and practice. I would hypothesize that theorists often avoid efforts to correct their theories (and, in so doing, neglect to utilize Stiles' logical operations for this purpose) because case studies and other data sources would reveal the theory to be fatally flawed. Instead, by simply ignoring these data, the flawed theory persists. To this end, the development of evidence-based medicine (Patterson, 2002) has reminded us that it is human nature to see what one wishes to see.

When theorists have a substantial stake in a theory, it is not surprising that they emphasize observations, including those from case studies, which lend support to their theory and neglect or deny those that do not. Critics have accused Freud and other psychoanalytic theorists of this behavior; I would generalize these accusations to encompass many others who believe theories should persist. In other words, Stiles' logical operations, which I believe are powerful, are nonetheless at the mercy of human nature: Will theory-building clinicians use them as prescribed to test their theories and take the chance that they will reveal serious flaws in them?

By contrast, when done according to standard protocol, the outcome of a randomized clinical trial is more difficult to influence. While criticisms of randomized clinical trials to test differences between experimental and comparison treatments deserving serious consideration have been made (e.g., Westen & Morrison, 2001; Westen, Novotny, & Thompson-Brenner, 2004), strong support for their indispensability (Crits-Christoph, Wilson, & Hollon, 2005; Weisz, Weersing, & Henggeler) has also been lent.

I urge Bill Stiles to take that additional step!

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