

Commentary on Logical Operations in Theory-Building Case Studies

The Logic of Case-Study Methodology

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ABSTRACT

The question of how we deploy logic in developing and justifying scientific theories of any sort is an important one. In this commentary, I consider William Stiles' (2009) arguments about the logical operations that are deployed especially in theory-building case-study research in psychotherapy. My commentary contains three sections, defined by three sets of distinctions: (a) the distinction between description and explanation in theory building, (b) the distinction between theory building and theory justification, and (c) the distinction between nomothetic and idiographic approaches to theory building in case-study methodology.

Key words: descriptive theory; explanatory theory; causal explanation; theory justification; objectivist epistemology; perspectival epistemology; relativism; theoretical generalization

The question of how we deploy logic in developing and justifying scientific theories of any sort is an important one, and so I am pleased to have this opportunity to comment on William Stiles' (2009) article. That Stiles addresses deduction, induction, and abduction in the context of "theory-building case studies" in psychotherapy is no small undertaking; he is therefore to be commended on that ambitious basis alone. In this commentary, I present my understanding of his central arguments, and I raise questions about some of those arguments. I structure my response by way of three sections that comprise this commentary: (a) the distinction between description and explanation in theory building, (b) the distinction between theory building and theory justification, and (c) the distinction between nomothetic and idiographic approaches to theory building in case-study methodology.

DESCRIPTION VS. EXPLANATION IN THEORY BUILDING

Stiles begins by comparing "clinical case studies" with "theory-building case studies" (p. 1). Clinical case studies, he says, "aim primarily to gain a deeper understanding of a particular case" (p. 9), and "may use multiple theories to aid understanding of the cases; the theories may point to phenomena that would not otherwise be noticed and suggest meanings that would not otherwise emerge" (p. 9). About theory-building case studies, he says,

Theory-building case studies, however, aim to build a single coherent theoretical account, one that is general, precise, and realistic. Although applying the theory to the case takes a first step in understanding the case, the point of the research is to apply case observations to the theory, to evaluate and improve it. (p. 10)

This sounds reasonable to me (see Held, 2006a, 2006b), but questions emerge nonetheless.

My first question is, What does Stiles here mean by the term “theory”? In a section aptly entitled “What Is a Theory?” (pp. 10-11), he begins by stating that “scientific theories are *descriptions* [italics added] of aspects of the world, such as how psychotherapy works, stated in words or numbers or diagrams or other signs” (p. 10). But there and elsewhere he states that theories are not just descriptions; they are also what I take to be *explanations*: “Theories knit observations together, suggesting how observation of one thing may indicate that other things have taken place or will take place” (p. 10). Later he invokes explanation more explicitly, in saying, “Labeling this process [of modifying theory] as abduction does not explain *how* researchers creatively construct new theoretical tenets” (p.19).

Stiles thus seems to be saying that theories tell us not only *that* something is the case (a description of something), but also *how* that something works—for example, how (or why) one entity is *related* to other entities. The latter may include *explaining* the basis for the relationship between the observed events, beyond *describing* their co-occurrence. I am of course here speaking of the notion of “causal explanation,” though nowhere in Stiles’ article could I find discussion of how any such *causal* theory or account is either “built” from case studies or evaluated in case-study research.¹

Compounding the definitional question (regarding the meaning of the term “theory”), Stiles uses the word “how” both in his discussion of *description* of psychotherapy’s workings and in his discussion of *explanation* of psychotherapy’s workings (pp. 10, 19). Thus, although he appears at first to draw some sort of distinction between description and explanation, the nature of that distinction remains unclear. For example, later on Stiles says that abduction “describes the operation of constructing a new hypothesis or of explaining new observations within a theory” (p. 10). He also at times uses the term “theoretical explanation” (p. 14), or at least “explanation” (p. 19), in a purely descriptive and/or logical sense:

To be useful, an abduction must be germane, in the sense that its terms must characterize the new observation in a way that logically connects it within the structure of the theory (this is what we mean by *explanation*). . . . The new abduction must be logically consistent with the rest of the theory. (p. 19)

He then defines the term “explain” as a matter of logical consistency with descriptions: “The altered theory must continue to explain (to be logically consistent with the descriptions of) the observations previously explained by the theory” (p. 19). And he also uses the term “theoretical description” (p. 8) without distinguishing it from the term “theoretical explanation” (p. 14).

¹ See Fishman, (2006a, 2006b), Held (2006a, 2006b), and Miller (2006a, 2006b), for a “dialogue” in these pages about the role of causality in case-study research, including the roles of induction and deduction in “theory-building case studies.”

I find this definitional aspect of Stiles' article somewhat dizzying, despite the fact that I myself, in quoting psychologists Paul Rozin (2001) and Michael Katzko (2002), have championed the descriptive function of both natural and social science research in these pages. I cannot imagine that Stiles would disagree with either of the following quotations of those two psychologists, even in the context of the "theory-building case-study" research in psychotherapy that he advocates:

Natural science enterprises often start with a domain of interest and curiosity, usually some real-world phenomenon. A first step is often verification that the phenomenon actually occurs. This may often be followed by an attempt to explore the generality of the phenomenon. A more disciplined description or exploration of the phenomenon often then ensues, with an attempt to discover laws or invariances. Such ventures are often not theory motivated, but rather are motivated by an attempt to be precise about the world, with the idea in mind that future theories will have to have something to explain. (Rozin, 2001, p. 5, as cited in Held, 2006a, p. 6)

One alternative to the causal-law interpretation [of scientific method] is to view theory as a framework for descriptive generalization. The data are the particulars: They exemplify principles to be captured in a general theory. . . . To get the most out of the data, an effective strategy searches for similarities among diverse situations and similarities in the meanings of the descriptions of those situations. . . . There is nothing unscientific about descriptive generalization. (Katzko, 2002, pp. 266, 268, as cited in Held, 2006a, pp. 6-7)

In short, it is not clear to me how Stiles distinguishes the terms "description" and "explanation," as they apply to theory (e.g., "theoretical description," p. 16, vs. "theoretical explanation," p. 14). Nor can I see how description itself, or how either (a) a characterization of a "new observation in a way that logically connects it within the structure of the theory" (p. 19) or (b) "an altered theory [that is] logically consistent with the descriptions of . . . the observations previously explained by the theory" (p. 19), constitutes an *explanation* (p. 19), at least not insofar as that term is typically used.

Moreover, at least in my experience, the term "theory" is often used to convey that which explains (not infrequently in causal terms) the observed inter-relations that obtain among a set of phenomena—that is, the workings or the "how" of something, over and above its descriptive "what." As I myself put it, "Scientific laws [or regularities] typically consist in a theory about the underlying, nondirectly observable cause of some reliably observed, empirical relationship" (Held, 2006a, p. 11). Hence the common use of the term "theoretical explanation"—which Stiles (p. 6) himself uses—to stand in for "causal explanation" (and vice versa). For example, Ronald Miller (2004), who advocates descriptive "understanding" (p. 34) over "causal analysis" (p. 141) in the human sciences (in addition to advocating case-study methodology in these pages [Miller, 2006a, 2006b]), nonetheless seemed to equate the terms "explanatory theory" and "causal explanation" (2004, pp. 33-34, as cited in Held, 2006a, pp. 11-12). Perhaps Stiles means to speak of *descriptive theory* as opposed to *explanatory theory*, but in that case it would be helpful to know just how he distinguishes the two, and in what sense (or when) a *description* constitutes a *theory*. Put differently, does any kind of "description" or "characterization" of an observation (in language) constitute a theory (see my note 3)?

THEORY BUILDING VS. THEORY JUSTIFICATION

Although Stiles' article is entitled "Logical Operations in Theory-Building Case Studies," he rightly addresses three distinct albeit related endeavors: building theories, altering theories, and evaluating/justifying theories. Regarding theory evaluation and justification, he appears especially keen to "move case studies up in the hierarchy of evidence, at least for studying psychotherapy and other context-dependent, complex human phenomena. [This argument] suggests that case studies are appropriate in the context of justification, not just in the context of discovery" (p. 12). I return to this important assertion in due course.

Just here I focus on a different claim made by Stiles about the epistemic matter of theory evaluation and justification, namely, that the act of *comparison* is the basis for theory evaluation, or what he aptly calls "quality control." Here are a few quotations of Stiles that make this point:

(a) Scientific quality control on theory is accomplished by comparing theoretical statements with observations. In case studies, detailed case observations are compared to detailed clinical theories . . . , incorporating unique as well as common aspects of cases. (p. 20)

(b) Scientific research provides quality control on theory by comparing these theoretical descriptions with observations. (p. 10)

(c) Statistical hypothesis testing and case study are alternative strategies for empirical scientific research on psychotherapy. Both provide quality control on theory by comparing theories with observations. . . . [But] whereas a hypothesis-testing study addresses only one theory-derived statement or a few statements, a case study addresses many different theoretical statements in the same study. (p.11)

(d) In contrast to the statistical hypothesis-testing strategy, the theory-building case study strategy compares many theoretically-based statements with correspondingly many observations[,] . . . by describing case observations in theoretical terms. In essence, a case study asks how well the theory describes details of the case. (p. 11)

(e) Both hypothesis-testing research and case studies . . . use induction, turning observations into descriptions and comparing these with theory to assess the theory's plausibility. (p. 20)

For the most part, these quotations express the standard epistemic view that in order to evaluate and justify a theory, we must determine whether our observations conform in some way to the claims made by the theory—that is, we must compare "theoretical statements" (p. 17) or claims to observations. I turn to some of the differences between hypothesis-testing and case-study research that Stiles explicates in the next section. Here I again draw attention to Stiles' use of the term "theory," especially in quotation (d) above. He says there that "a case study asks how well the theory describes details of the case"; he does not (there) ask how well the theory explains details of the case. Does this mean that in evaluating and justifying theory, we do not seek evidence for a theory's explanatory properties? Second, by saying in quotation (e) that hypothesis-testing research and case studies function by "turning observations into descriptions and comparing these with theory to assess the theory's plausibility," he may be raising a familiar epistemic problem. He refers to that problem more explicitly in this next quotation:

Although scientists themselves can compare theories with their *perceptions* of events, others can only compare theoretical statements with scientists' *descriptions* of events. These descriptions of events are *not given* by those events but require acts of *creation* by the observer [all italics added]. (p. 17)

What does Stiles mean in saying that “descriptions of events are not given by those events but require acts of creation by the observer”? This could be interpreted to express some form of constructivist or social constructionist epistemology, in which knowers make “reality” (in language) rather than discover reality (see Held, 1995, 2007).² If this is indeed what Stiles means—and again I remain unsure about his intended meaning—then he may perhaps be advancing some form of anti-objectivist or antirealist epistemology, in which our experience of the (empirical) world cannot in principle give us knowledge of an *independent* reality—that is, knowledge of a reality that is independent of the knower, including her “beliefs,” “psychological and cultural expectations,” “values,” and “frame of reference” (Stiles, pp. 14, 17). The following quotation seems to me to support Stiles' adoption of this kind of epistemology, at least to some extent:

As I understand it, objects, events, and qualities of the world (the things to which signs refer) cannot be known directly³ but only through the way people see, hear, feel, smell, or taste them or through their perceived effects on sensing instruments. Our experience of the world is thus as much a product of our own biological, psychological, and cultural makeup as of the objects and events that impinge on us. Because we cannot know the world directly, we construct and share descriptions and theories⁴ about it based on our experience of it. (p. 17)

Note in this last quotation that because our *experience* of the world is a product of cultural (as well as biological and psychological “makeup”), Stiles' epistemology seems not only to some degree anti-objectivist, but also to some degree relativist: that is, cultural context at the collective level (if not biological and psychological makeup at the individual level) presumably plays a role in the reality we experience or “construct” in language/description (rather than discover), in which case “truth” may be perspectival or relative to each knower's cultural (if not individual) context (see Held, 2007). This conclusion is reinforced by this next quotation:

[Induction] has to contend with the capacity of human perception to be shaped by the perceiver's frame of reference—a capacity that often favors confirmation of observers' preferred theories. (p. 17)

² Of course, *descriptions* of events or *theories* about events—it is still not clear whether for Stiles theories are synonymous with descriptions—are indeed constructed by us in language/symbols. But that does not mean that the *event* that is being observed or described is *itself* constructed in the act of observing, describing, or knowing it. See Held (1995, 2007) for elaboration regarding this radical constructionist thesis.

³ Indirect or mediated knowing, in which we require theory to access a reality that is not directly observable (e.g., the physics of the very large or very small, or causality in general), does not preclude an objectivist epistemology, in which the truth of a claim about the world does not depend on anyone's beliefs about its truth. See Held (1995, 2007) for elaboration of the distinction between direct and indirect/mediated knowing, as well as the oft-missed distinction between the use of *theory* and the use of *language* in the knowing process.

⁴ Note that here Stiles seems to speak of descriptions *and* theories as if they are different kinds of entities, or at least are not synonymous.

Elsewhere, in an interesting discussion of how listeners' (including scientists') "personal or cultural" (p. 14) (vs. logical) expectations affect their understandings of events (in this case women's reactions to rape), especially whether they see their expectations as logical or reasonable, Stiles states that "the expectation [of one of the lab members] seemed coherent in the sense of narrative coherence (McAdams, 2006), which depends partly on listeners' psychological and cultural expectations and on their values and beliefs about what is normal or expectable" (p. 14). This again invokes the epistemic relativism of the prior quotations, as perhaps does this next quotation, in which both our theories and our observations are not known objectively, that is, independently of the experience of them that is allegedly colored by our "frame of reference" (p. 17)—which consists in our "psychological and cultural expectations" (p. 14) as well as our "values and beliefs" (p. 14):

When the observations fail to match the theory (or, more precisely, when researchers' *experience* of the observations fails to correspond to their *experience* of the theory), even after methodological checks, researchers may creatively (abductively) modify the theory by adding to it or altering it so that it does match [all italics added]. (p. 20)

To recap: Stiles seems in these and other assertions to adopt some form of constructivist, constructionist, or anti-objectivist epistemology, in which our "experience of the world" (p. 17) or our "experience of the observations" (p. 20), is at least to some nontrivial extent constructed or created by us—that is, it is "as much a [relativistic] product of our own biological, psychological, and cultural makeup as of the objects and events that impinge on us" (p. 17). We do not, it seems, discover/know an independently existing reality so much as construct in language, description, and/or theory the "reality" we experience (see my note 2). If this is indeed what Stiles means to say, then this is as much a problem for "hypothesis-testing" science as for "case-study" science, because our observations would then be so significantly "permeated" by theory (among other factors) that there could never be sufficient independence between them to allow a fair/unbiased/objective test or evaluation of the theory. Recognizing that no one has yet to make a compelling case for a *complete* independence of theory and observation, philosopher Susan Haack (2003), in her *Defending Science—Within Reason*, had this to say in support of her own "innocent realism":

In every perceptual event . . . there is something received, something resistant to one's will and independent of one's expectations. In every judgment . . . there is something in some degree interpretive, and hence fallible. There is no sharply distinguishable category of statements the meaning of which is exhausted by experience. . . . The meaning of any statement depends on the links between its words and others as well as on the links with the world learned directly by ostension. . . . To deny that there is a sharply distinguishable category of interpretation-free observation statements is not to say that "it's a glass of water" or "it's gone green" or "lo, a raven" are really theoretical statements after all. (p. 128, as cited in Held, 2007, p. 232)

A year earlier Haack (2002) said,

The innocent realist is a contrite fallibilist. . . . The tension between independence [of the entity to be known] and accessibility [on the part of the knower] . . . may be altogether

superable if our understanding of independence is modest enough and our understanding of accessibility is fallibilist enough. (p. 88, as cited in Held, 2007, p. 131)

It is hard to know whether Stiles would agree with Haack; he sounds to me as if he finds considerably less independence between theory and observation than does Haack and other objectivists. After all, he says that our biological, psychological, cultural, value-laden, belief-laden, expectation-laden, framework conditions/contexts affect the nature of our observations, which I interpret to mean he believes that our perceptions of the world (i.e., our/researchers' "experience of the observations," p. 20) are always relative to or contingent upon these conditions, thereby making them perspectival or relativistic. But unlike Haack, Stiles does not speak expressly of theory permeating observation; rather, he states the converse in a section heading entitled "Observations Permeate Theories" (p. 18).

To be sure, there is nothing controversial in this section heading, especially if we interpret it to mean that we derive a (theoretical) generality inductively by way of a set of individual observations. And from those generalities we deduce consequences that we can then test in observation. Put succinctly, observations *should* permeate scientific theories! But again, if observation is profoundly and unavoidably "permeated" by psychological and cultural expectations, beliefs, and values (including theory, which, in its broadest sense, at least contains a set of to-be-tested beliefs or expectations in the form of explicit claims about the empirical world), then there can be no independent test or evaluation of theory in observation. In that case, we indeed make "reality" relative to our expectations, beliefs, and values (including our theories) rather than discover an independent reality, in both our scientific and our everyday knowing activities. We can then have only relativistic or perspectival "justification" of theory rather than justification simpliciter. Put differently, we can then have only perspectival "truth" rather than truth.⁵

To put this in the context of theory evaluation and justification (in distinction to the "context of discovery," Stiles, p. 12) in case-study research: if, as Stiles suggests, the therapist/researcher holds a theory or generality that helps her understand and describe a particular case or set of cases—"applying the theory to the case takes a first step in understanding the case" (p. 10)—then how can she or anyone test that *same* theory or generality by "comparing" (pp. 9-11, 16-17, 20) it to her observations of that (or any) case, which observations may be so permeated by theory that they do not have sufficient independence of the theory to serve as a fair/unbiased/objective test of that theory? I am of course suggesting that such evaluation of theory is then not in principle possible, although this conclusion depends on just what Stiles means by the term "theory," and in all fairness I remain unclear about his definition of this term.

In any case, let us now leave the matter of theory's definition and evaluation/justification aside, and instead turn our attention to the aforementioned notion of generality itself, as it pertains to theory (in general) and to theory-building case-study research (in particular).

⁵ Although the truth of any empirical matter itself is absolute, our *judgments* about the truth of empirical matters, which are all that we can have, are necessarily fallibilist. Hence philosopher Harvey Siegel's (1997, p. 6) "fallibilist but absolutist conception of truth." See Held (2007, pp. 275-281) for elaboration.

A NOMOTHETIC (GENERALITY) VS. IDIOGRAPHIC (PARTICULARITY) EMPHASIS

It seems evident that a driving force in the development in psychotherapy research of a case-study methodology, in distinction to traditional hypothesis-testing methodology, is the desire to preserve the unique particularity that contributes to the very essence of the therapeutic enterprise, at least as it is practiced. Science typically concerns itself with arriving at generalities or “generals” (as Haack, 2003, p. 131, following Peirce, aptly called them), which by definition subsume more than one instantiation of a kind. But the practice of psychotherapy requires keen attention to the unique particularities that therapists must attend to in each clinical encounter, as they strive to understand and so respond optimally to just *this* individual’s suffering (Miller, 2004). Stiles himself thus rightly underscores the importance in case-study psychotherapy research of attending to unique particularity for the purpose of building theory/generalities, beginning with a subheading entitled “Unique Features of Cases Can Inform Theory” (p. 17):

Psychotherapy theories are meant to encompass more than is ever encountered in a single case. Each case includes details not shared with other cases, and a good clinical theory helps practitioners understand the variations as well as the common features. Conversely, unexpected distinctive features of cases can show where theories need to grow. Unlike statistical hypothesis testing, where rare or unique features may be regarded as error, case studies allow researchers to incorporate them into research and theory. . . . I have drawn this point from Rosenwald’s (1988) theory of multiple case study research, where he argued that an adequate understanding of social phenomena must encompass its varied manifestations in different people. (p. 17)

And recall that Stiles gave his appropriate nod to the importance of generality in theory-building right off the bat: “Theory-building case studies . . . aim to build a single coherent theoretical account, one that is general, precise, and realistic” (p. 10).

The generality/particularity distinction can of course be recast in terms of the tension between nomothetic and idiographic approaches to (psychotherapeutic) theory, research, and practice (Held, 2006a, p. 2). The tension is an important one: the thorny problems that inhere in the Boulder (scientist-practitioner) Model since its inception have persisted, as evidenced by, for example, the creation of the Vail (professional/Psy. D.) Model and more recently by the development of alternative approaches to clinical science seen in *Pragmatic Case Studies in Psychotherapy!* Still, the founding editors of this journal themselves have professed the necessity of deriving generalities (inductively) from the particularities of individual cases, if bona fide disciplinary psychotherapeutic/clinical *knowledge* is to be gained (Held, 2006a, pp. 7-9, 14). For example, Daniel Fishman (1999, as cited in Held, 2006a, pp. 7-8) seeks “guiding conceptions” (pp. 12, 236) “not as general laws, but as conceptual themes and related practical guidelines for future action” (p. 230) that would improve “the overall practice of therapy with that type of patient” (p. 226). Ronald Miller (2004, as cited in Held, 2006a, p. 8) searches for “heuristics” or “rules of thumb” (p. 130)

that permit a kind of case law to be established in psychology on how various cases are to be most effectively understood and handled. These “laws” would not be regarded as fixed and

universal but, as in the legal system, as providing guidance and instruction to professionals tackling new cases. (p. 210)

Stiles seems to appreciate the role of generality in (clinical) knowledge, both in terms of (a) the possibility of having (general) “principles,” and in terms of (b) his own compelling “assimilation model,” in which he (inductively) derived a set of stages of psychological change that, it is suggested, apply to all of us. About (a), Stiles, in distinguishing physical from psychological existence and knowledge, nonetheless said,

Psychological phenomena of interest to psychotherapists are typically developmental and historical, and they seem less subject to the sorts of (relatively) simple generalizations that describe physical phenomena. Physical theories deal with events and relations that seem somehow closer to basic units. Nevertheless, psychologists have principles that can be applied in lived contexts and can help people understand unique developmental and historical progressions. (p. 14)

And about (b) Stiles stated,

I have used case studies extensively in building a theory of psychological change that I and my collaborators have called the assimilation model (Stiles, 2002). It is a developmental account of therapeutic change (Leiman, 2004) that describes a regular sequence of stages through which people’s problems pass in successful psychotherapy, along with processes that underlie it.⁶ A core strategy has been tracking problematic topics across psychotherapy sessions in intensive case studies, gradually elaborating a theoretical description of the observed change processes. (p. 10)

Elsewhere Stiles (2003; reprinted as Appendix A in Stiles, 2009) put it this way: “On their way to becoming resources in successful therapy, problematic experiences appear to pass through a sequence of stages or levels of assimilation” (p. 8; see Table 1, p. 11).

Of note is that Stiles (2009) expressly calls his “assimilation model” a “theoretical description.” This makes good sense, since it is a “stage theory,” in which the stages constitute theoretical constructs derived from the observation of a set of particular instances of the relevant phenomena—in this case, case studies. And though he does not say so explicitly, I assume Stiles believes that the use of his assimilation model by *any* therapist may cause better outcomes in therapeutic practice, regardless of the therapist’s (or client’s) “frame of reference” (p. 17) (see Held, 2006a, pp. 11-12). If I understand Stiles correctly, then it would appear that he does not put forth his own theory (or model) perspectively—that is, relative to his or anyone’s “frame of reference” (p. 17), by way of the psychological, cultural, value-laden, belief-laden, expectation-laden conditions/contexts (pp. 14, 17) that for him seem necessarily to qualify or color all empirical observations and thus knowledge claims about the world. The same can be said about his views regarding “empathy” and “mechanical theory” in psychotherapy, which are not put forth perspectively:

⁶ What remains unclear to me is whether the “processes that underlie” the stages are put forth by Stiles to *explain how* (by what causal process?) one stage leads to the next.

Through case studies, then, the richness of empathy with another human being can be registered, and inhuman or mechanical theories can be challenged. The experiences investigators share with participants in their research can also be shared with readers and brought to bear on theory. Through case studies, investigators can build theories that accommodate rich and empathic observations of people. (p. 18)

To frame my assertion as a question, Is the assimilation model of therapeutic change true *about* only *some* people, and true *for* only *some* inquirers (see Held, 2006a, p. 17)? Nothing Stiles says suggests that he would accept this ontological (in the former case) and epistemic (in the latter case) perspectival or relativistic qualification, though it would be interesting and helpful to know just where he stands on the matter.

CONCLUSION

Whether Stiles' or anyone's approach to case-study methodology is superior to or complementary to traditional hypothesis-testing methodology in generating therapeutic/clinical knowledge remains to be seen. However, from my reading of Stiles, Fishman, and Miller, I believe it holds considerable promise (Held, 2006a, 2006b). Nonetheless, I also believe that, like all methodologies, it could surely benefit from clear explication of its own conceptual and philosophical underpinnings, including the logical operations discussed by Stiles in the creation, alteration, evaluation/justification, and application of psychotherapeutic theories. I am therefore pleased to see that William Stiles has turned his attention to just that significant undertaking.

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