Behavior and Social Issues, 15, 68-80 (2006).

WILL CULTURAL ANALYSIS BECOME A SCIENCE?

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ABSTRACT: In this paper, the author argues that the concept of the metacontingency is not yet well enough developed to be very scientifically useful, although developers like Glenn and Malott (2004) should be encouraged to pursue more thorough development. By contrast, systems theory which Malott and Glenn (2006) also emphasize, may have real and immediate utility in the analysis of cultural systems. In fact, systems theory suggests certain inherent limitations to the metacontingency construct as it is most commonly conceptualized, although alternative models may prove more intellectually coherent and useful. This paper argues that modest work emphasizing observational studies and relatively simple experimental efforts to influence networks of interlocking contingencies have greater promise at this point in the development of a natural science of cultural analysis than do continuing efforts to refine abstract conceptual schemes without grounding in data.
KEYWORDS: Applied cultural analysis, metacontingencies, interlocking cultural practices

With the other authors included in this special section, I wish to express my deep appreciation to our hosts in Campinas, Brazil, for their assistance in making possible the Think-tank on Metacontingencies and Cultural Analysis in August, 2005.[2] This concentrated time together allowed those of us participating to sharpen questions that appear critical to advancing a natural science of cultural analysis. In my response, I wish to comment on three major themes of the think-tank and the papers leading up to it: (a) the concept of the metacontingency; (b) the place of systems theory in a science of cultural analysis; and (c) the emergence of a science of applied cultural analysis.

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[2] Special thanks are due to Joao Claudio Todorov, as well as to the Associação Brasileira de Psicoterapia e Medicina Comportamental (ABPMC, ABA-Brazil) and Instituto Brasilense de Análise do Comportamento (IBAC), with the collaboration of the Universidade Católica de Goiás and the Association for Behavior Analysis International (ABA).

METACONTINGENCIES

The discussions in Campinas clarified my thinking regarding the metacontingency as a central concept for cultural analysis; I must confess that I now find myself more doubtful about the intellectual coherence and scientific utility of the usual metacontingency model than previously. Clearly the complex of behavioral events occurring within a cultural entity (say, a business, or a church) is dynamically responsive over time to the totality of the impinging environment. That assertion, however, does not take us very far analytically or scientifically. The metacontingency does not seem to help us to understand the dynamics of selection present. The analysis of metacontingencies attempts to capture many interwoven transactions as a dynamic whole, but so far it is not evidence that we have identified measurable or manipulable units that can capture "everything at once"—particularly as everything at once changes over time. For scientific explanation, prediction, and control, the field needs to focus, in my view, at the level of interlocking contingencies, which have accessible and observable analytic elements (antecedents, practices, consequences, motivative operations, and so forth) that can be observed and manipulated (Mattaini, 2004; see also Salzinger, 2004). So far, no one appears to have specified or manipulated the elements of a metacontingency, if in fact such elements exist.

A further problem, as discussed below, is that the proponents of the metacontingency construct rely in part on systems theory (Glenn & Malott, 2004; Malott, 2003), and one of the most important emerging concepts in contemporary systems theory is that of self-organization (*autopoiesis)* (Hudson, 2000). In self-organizing systems (all living systems and some others), many of the events that occur within the system are determined by other events within the system itself, and *not* by events outside the system boundary. Given a self-organizing system, the impacts of the external "receiving system demand"—often described as the active selecting mechanism in the metacontingency—or of the environment in general provide at best a very partial explanation of the events occurring within the focal system. The internal dynamics of an autopoietic system, rather than external "demands," determine much of what happens inside. It is important to avoid confusion here; a self-organizing system is not a closed system—it is in dynamic contact with other events and conditions across its boundary, but clarifying those events does not necessary lead to prediction or control of what happens within the focal system.

I elaborate on self-organization in more detail later in this paper. The point here is that systems theory itself may fatally wound the metacontingency … but we shall see. The developers of the concept continue to work hard to refine and

elaborate it (Sigrid Glenn and her students are currently attempting initial experimental work, which clearly calls for clarity of constructs and units), and they may yet produce a set of robust scientific constructs that lead to active experimentation and deeper theory. At this point, however, I do not believe this has been achieved. (Ulman's [1998, 2004] *macrocontingency*, which has received little attention, may have something to offer to our science. Ulman and others define the macrocontingency differently than do Malott and Glenn in their recent work, however. Further work related to macrocontingency analysis may be worthwhile.) Some have suggested that certain existing empirical work (e.g., Todorov, 2005) offers initial exemplars of metacontingency analysis, but all existing empirical work of which I am aware—including Todorov's—can be more simply understood in terms of interlocking contingencies, which (unlike the metacontingency) also offer mechanisms for manipulation of observable variables. The conceptual problems faced by the metacontingency are serious, and the construct still appears too abstract to guide rigorous scientific work. Should it remain at the current level of abstraction, it appears likely to prove at least a distraction, and at worst a serious impediment to advancing science.

On the other hand, an alternative—and in my opinion brilliant and conceptually coherent—model of the metacontingency as elaborated by Houmanfar and Rodrigues (2006) in this issue may prove both more elegant and more useful. Skinner (1981/1987) seems to have had an emergent framework like metacontingencies in mind when he defined the third level of selection, indicating that, "A culture evolves when practices originating [in reinforcement of novel behaviors emitted by individuals] contribute to the success of the practicing group in solving its problems" (p. 54). The definition of the "success of the practicing group" is a conceptual problem here, as is the selective mechanism—some sort of "group reinforcement" rather than reinforcement of the behavior of the members of the group, perhaps? We currently know nothing about such a mechanism, and claiming that it is "emergent" without being able even to describe—much less explain—it in observable terms does not seem very useful. On the other hand, the analysis of interlocking contingencies is progressively developing, evoking empirical work, and appears to be demonstrably useful today for advancing a natural science of culture and cultural practices.

SYSTEMS THEORY IN A NATURAL SCIENCE OF CULTURAL ANALYSIS

Where Glenn and Malott (2004, see also Malott & Glenn, 2006) may make a very valuable immediate contribution is in bringing our attention to the potential importance of systems theory for understanding cultural systems. Behavior analysts are appropriately (if not always adequately) skeptical about importing

bodies of theory from outside the discipline into our work. The example of anthropologist Marvin Harris's cultural materialism (1979, 2001) comes to mind. Given its epistemic overlaps with behavior analysis, cultural materialism was widely discussed in behavior analysis from the mid-1980s through the mid-1990s. It is not clear, however, that substantive scientific advances or empirical work emerged from those discussions, and little is currently heard about cultural materialism. Harris's work does have much to teach behavior analysis, but there has not been much advantage, at least yet, to importing his theoretical structure in its entirety into behavior analytic science.

The same may prove true of systems theory, but perhaps not. Systems theory has successfully captured the realities of dynamic systems of many kinds, proving useful for both prediction and control in many cases, and clarifying why those may not be possible in others (Mattaini, in press). I want to highlight a few central concepts from systems theory here, and briefly note their consistency with and possible utility for a natural science of cultural analysis, and in particular with an applied science of culture.

Behavior analysts have sometimes conceptualized their work with organized groups of persons (especially organizations) in terms like *goal-directed systems design* (Malott, Trojan Suarez, & Malott, 2003) or behavioral systems analysis, but of course they are not the first to make sense of complexity in systems terms. Throughout the early and mid-20th century, a variety of thinkers and scientists were moving toward systems thinking and systems theories. Among these, the work of von Bertalanffy (e.g., 1968) was seminal. Von Bertalanffy's aim was breathtakingly ambitious: the "Unity of Science" (p. 86). In *General System Theory* (1968), he stated:

> The fact that certain principles apply to systems in general, irrespective of the nature of the systems and of the entities concerned, explains that corresponding conceptions and laws appear independently in different fields of science, causing the remarkable parallelism in their modern development. … Reality, in the modern conception appears as a tremendous hierarchical order of organized entities, leading, in a superposition of many levels, from physical and chemical to biological and sociological systems. Unity of Science is granted, not by a utopian reduction of all sciences to physics and chemistry, but by the structural uniformities of the different levels of reality. (pp. 86-7)

Note that von Bertalanffy was not suggesting in his General System Theory (GST, later usually referred to as General *Systems* Theory) some sort of metaphysical project, but rather a deeply scientific perspective for attempting to model the complexities of dynamic realities. Such modeling appears to be

precisely what is needed to advance a natural science of cultural analysis. Perhaps something like the pattern-oriented modeling briefly noted in Marr's (2006) contribution to this issue will be the direction this work will take.

Uri Bronfenbrenner (2005) attempted to capture the complexities of intersystemic dynamics by elaborating *microsystemic* factors (transactions between individuals and their immediately impinging systems including family, school, work, and peers), *mesosystemic* interactions (transactions among the impinging microsystems), *exosystemic* factors (influences from extended family, mass media, and other systems with which the individual is not in immediate contact, but which indirectly influence individuals and microsystems), and the *macrosystem* (larger sociocultural forces). He also discussed what he called the *chronosystem*, which roughly encompassed the dimension of time and socio-historical conditions. In this work, Bronfenbrenner provides a skeleton for elaborating the levels of analysis that may be required to adequately understand the dynamics of cultural systems and their impact on individual behavior. Understanding contingencies and their interlocks at each of these system levels sets behavior analytic science an adequately demanding challenge for the foreseeable future.

Contemporary systems theory has advanced beyond these formulations in ways that are particularly consistent with a natural science of behavior. Two advances are especially important to note here:

(1) an understanding emerging from modern physics that networks of transactional relationships, rather than objects, should be regarded as the basic elements of reality; and

(2) an appreciation of the central place of self-organization in those transactional networks.

Reality as transactional

Contemporary research and theory in physics and biology suggest that reality is best thought of not as a collection of objects, but rather as an "inseparable web of relationships" (Capra, 1996, p. 37). The relational components of this web are *patterns of transactional events*; objects (including organisms) are emergent products of those patterns, and have reality only in networks of relationships. This approach is highly consistent with contemporary behavior analytic science, in which not persons, but acts, are the primary subject for study. For example, Lee (1988) suggests that a person can best be understood as "an accumulation of acts grounded in the behavior (i.e., movements and postures) of a single organism" (p.

93). (B. F. Skinner used similar language in his work.) As Lee uses the term, acts are *transdermal events* constituted of transactions between the organism and the environment—what she calls "things done." A scientific understanding of human behavior "resides in the interpenetration of the organism with the contingencies of the culture" (Lee, 1988, p. 114). In addition, for Lee, conduct is organic; any particular action "is embedded in a network of overlapping and interlocking actions" (Lee, p. 57). Cultures clearly emerge from ongoing patterns of such transactional, interlocking practices.

Self-organization

Recent systems work has also expanded our understanding of the structure and boundaries of living, self-organizing networks (Hudson, 2000). As noted above, such autopoietic networks are literally "self-making"—they dynamically organize and construct themselves without guidance from outside. The dynamic patterns of transactions that constitute an autopoietic system are organized *by the network itself*, and construct their own boundaries; the system boundary of such a network is a natural result of how it functions. If cultural systems are autopoietic in this way (and they appear to be, although there is not universal agreement here), they can only be understood through analysis of the interlocking dynamics within the system itself, *in conjunction with* the processes by which the focal system couples with its larger environment. Let me provide a simple practical example. Imagine two families (each a system), both living in a high-risk, impoverished urban neighborhood (itself a relatively porous system). When an additional environmental stress occurs, say the drive-by shooting of a neighborhood child, one family may be overwhelmed by the stress, while the other (more "resilient") family may make short-term plans to reduce the risk to family members, and long-term plans to move to somewhere safer. Knowing external events and conditions does not allow us to predict how the family will react; the families' own internal cultures are critical variables (and are emergent from other variables, current and historical).

Analyses limited to the demands of or transactions with environmental contextual factors are therefore not adequate for understanding (or changing) what happens internally. By contrast, the observational and experimental modeling of interlocking contingencies appears likely to advance understanding both of the internal dynamics of cultural systems and of the impacts of contextual factors. For example, external impacts, if they affect behavior within the system, must figure in contingencies related to that behavior; examining this in the context of the other active contingencies (those from within the system) should help in clarifying why behavior does or does not change when those external stimuli do.

Only analysis of multiple interlocking contingencies (or perhaps better, interlocking practices; see Houmanfar and Rodrigues, 2006) can offer an explanation at this level. (See also Sandaker [2006], for discussion of complexity theory, another stream of thought from contemporary systems theory that may prove useful to behavior and cultural analysis.)

APPLIED CULTURAL ANALYSIS: OBSERVATION AND EXPERIMENTATION NEEDED

Conceptual analysis has a distinguished history in the science of behavior, going back to some of B. F. Skinner's earliest work (e.g., 1953). Such analysis has been important in refining the scientific questions that have brought us to this point, and in evoking important hypotheses. But no science advances very far without rigorous observations (which allow predictions to be tested) or experimentation—or both. I have argued elsewhere and reiterate here that we are at the point in the science of cultural analysis where we must move beyond purely conceptual work, for two reasons. First, without testing, there is no empirically-grounded way to answer the most important scientific questions, or to determine which of multiple theoretical formulations is most likely to lead to further advances. There is a real risk that without strong, progressive programs of empirical research, those within behavior analysis with interests in cultural level phenomena will become increasingly marginalized as innumerate romantics, and a true science of cultural analysis—applied or not—will not develop within our lifetimes. Second, many of those within the behavior analytic community who are interested in the analysis of cultural level phenomena became interested precisely because they wanted to have an impact on important social issues—but that requires an applied science, and within behavior analysis, applied science requires experimentation.

This is very complex work, and natural cultural systems seldom welcome experimental intervention except under exceptional circumstances. One exception has been organizational behavior management; considerable applied work there clearly has involved changes in cultural practices (behaviors transmitted within and across generations) and cultures ("contingencies of social reinforcement maintained by a group," Skinner, 1987, p. 74). Community-focused and prevention-oriented work also usually involves shifts in cultural practices and cultures (Biglan, 1995).

Accumulated experience in these areas suggests that maintenance of gains made in such work is very challenging—as systems theory would predict. A phenomenon observed in all living systems is homeostasis, the process by which systems tend to right themselves when perturbed, returning to baseline conditions.

Behavior science can help us understand how this happens by analyzing the networks of interlocking practices present. Newly-established cultural changes often diminish over time, in part many of the contingencies associated with the previous *status quo* remain in place. As experimental contingencies are faded, as they almost universally are in applied work, the salience of the original contingencies reemerges. Many of the best-established community prevention programs, for example, are seldom maintained even in the sites where the research has been done, much less propagate widely to other non-research sites (Embry, 2004).

Observational studies

So, where to begin, given these challenges? One step that is likely to be essential is to embrace observational studies of interlocking practices within cultural entities. I include here studies analyzing secondary data observed by others, with appropriate attention to their limitations. For example, one study we have conducted (Mattaini, 2006) uses an unsupervised neural network methodology to identify clusters of interlocking relationships among (a) classes of actors, (b) antecedents, (c) postcedents, and (d) contextual factors, associated with (e) violent incidents in an alternative school. In this setting violence and threats were regular events, undoubtedly maintained by interlocking practices among staff and students as well as contextual factors. Recognizing and taking steps toward understanding the cultural interlocks present may help plan potentially effective interventions within the networks of interlocking practices present. In that study, we identified sixteen clusters of violent and coercive incidents, including four clusters involving violence among students:

- Escalation from verbal exchanges (nearly always in the presence of peers);
- Sequelae of "running wild," usually on the bus;
- Sequelae of attention seeking behavior across genders; and
- Events related to particular types of motivative operations earlier in the day.

Each cluster involved common patterns of setting events, antecedents and postcedents. (Patterns related to verbal abuse of, and physical confrontations with, staff also emerged.) Such studies could help us learn to model cultural interlocks within small cultural systems, an important scientific advance.

Experimental efforts

A second essential scientific direction is to "try it" (Bates, 1990/1950)—to intervene experimentally and see what happens. As a field we so far have very little idea how to change a metacontingency experimentally, but we do have some understanding of how to analyze and shape cultural practices, and to some extent interlocking practices. Given our own experience, and the results reported by Embry (2004), I have less and less confidence in our ability to make major concurrent changes in many interlocking practices within a cultural entity. Embry's focus on attempting to embed *evidence-based kernels*—simple practices known to have some power to change other behaviors, rather than complex "programs" is almost certainly a critical next direction in an applied prevention science, and we have come to emphasize this in our recent PEACE POWER prevention work (www.peacepower.info), particularly over the past year.

Such kernels are clearly most likely to take hold and be maintained if, once in place, they quickly and predictably produce effective natural reinforcement—what Malott, Trojan Suarez, and Malott (2003) term a *behavior trap*. We have come to believe designing around potential behavior traps to be more important than has generally been recognized in community intervention. Consistent with Lutzker's ecobehavioral work (1998), developing such community programming requires the analyst to identify antecedent strategies—including marketing—that evoke initial behavior, which then may, if we have adequately analyzed the interlocking contingencies present, be maintained by natural contingencies. In some cases, we may be able to move out perhaps a few levels more distally into the network of interlocking contingencies, but given our current level of knowledge, probably not much further. It may sometimes be possible to permanently embed new reinforcers within a system, but seldom at all levels at once—somewhere in the network, a natural behavior trap probably must be present. In discussing a drug-free life, Miller and Carroll (2006) indicate that, "If [it] is not fun and rewarding, it is unlikely to be stable" (p. 307), and something like this is true in every area of our work, including establishing and maintaining new practices within a cultural entity. The changes have to "work" for someone within the existing matrix of contingencies shaping and maintaining their behavior. This is, admittedly, more modest work than the redesign of metacontingencies—but it is tremendously challenging in its own right, and it is work that can sometimes be done with the science we currently have at hand.

For example, Figure 1 summarizes data for three schools (one alternative high school [diamond data points] and two elementary schools) in which we have worked to embed simple cultural practices. Efforts were made to collect data

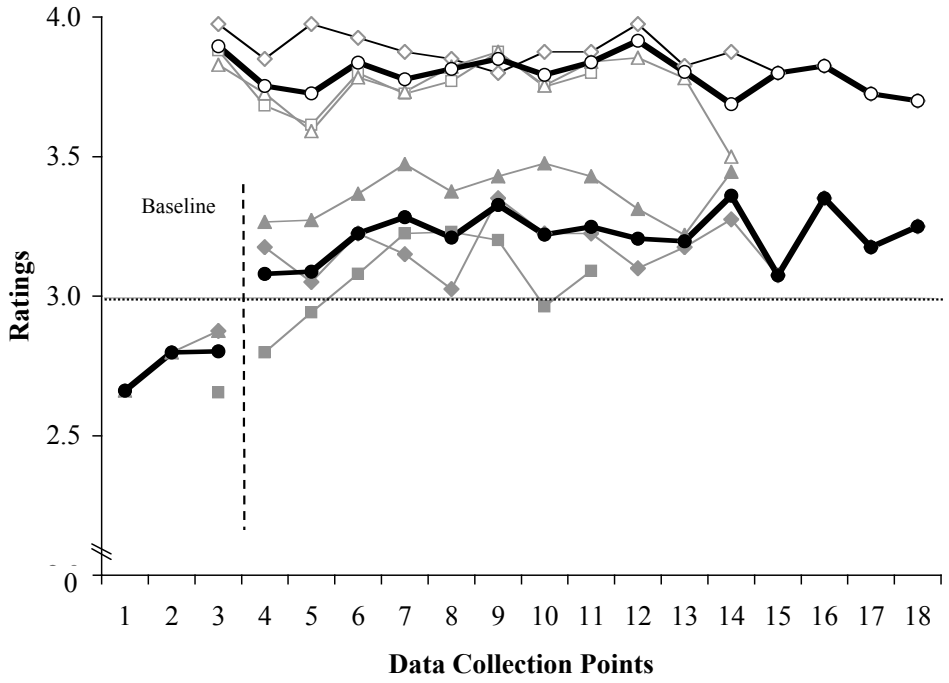

Figure 1. Summary data from three school projects designed to increase the incidence and prevalence of four high-level response classes with promising evidence bases. Heavy lines represent averages across schools.

monthly, over up to two years. Timing of data collection points is variable among programs; they are therefore shown here on an ordinal rather than interval scale. Number of data points available for each is also variable. The data series with open markers reflect average teacher ratings of the importance of the four high-level behavior classes on a scale ranging from 1 ("not important") to 4 ("very important) for each school; heavy lines depict averages across schools. (The four target behavior classes involve increases in reinforcement for prosocial and academic behavior, action to reduce aversives, opportunities for participation and involvement, and effective conflict resolution.) Ratings of importance are high after initial training, and remain generally stable.

The data series with closed markers reflect teacher reports of the actual incidence of the high-level response classes on a scale of 1 ("seldom or never") to 4 ("very often"). For heuristic purposes, a level of 3 ("often") has been established by project staff as a benchmark which we aim to consistently exceed. The three projects, all from small cities, each show discontinuities in level from

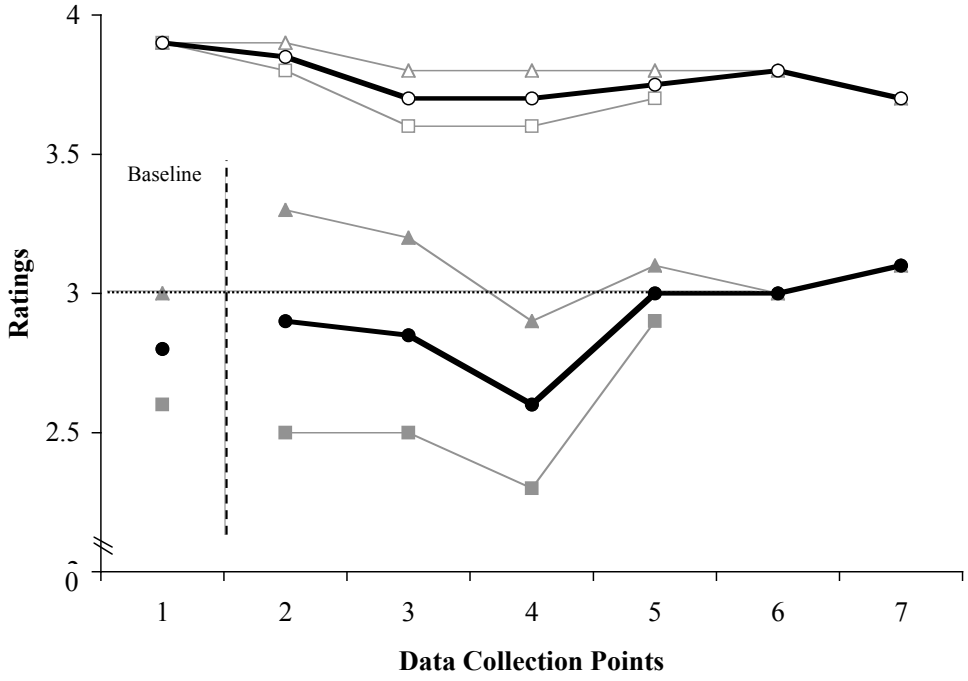

Figure 2. Summary data from two inner-city elementary school projects designed to increase the incidence and prevalence of four high-level response classes with promising evidence bases. Heavy lines represent averages across the two schools.

baseline to intervention, suggesting cultural change. Single-point or increasing trends in baseline limit confidence in the data. Only at one elementary school were three baseline data points available—the first two retrospective, and there is an obvious trend in that data. Only one baseline point was available for the other schools. The measures are also far from the most rigorous. Despite these and other limitations, the data are of interest when compared with those in Figure 2.

The two projects in inner-city elementary schools shown in Figure 2 reflect similar high ratings of importance of the targeting response classes (strong social validity), but no evident pattern of improvement subsequent to the initiation of consultation. One began with the highest level of targeted practices among all the schools, and remained at about the same level. The second began at the lowest level among all the schools, and did not show a change in level for several months after the initiation of the project. The data depicted in the figures likely reflect social validity, acceptability, and changes in verbal behavior as much as substantive changes in overt behavior within the culture. But they are a start, and

are encouraging enough that we continue to pursue this work, with increasing emphasis on simple evidence-based kernels and potential behavior traps, particularly in inner-city schools. Given the importance of a "differential option" for the poor and most vulnerable (Farmer, 2003), it is important to continue efforts to find workable strategies for these schools, although it is also proving useful to do some developmental work in somewhat easier settings to refine our procedures.

CONCLUSION

Multiple relatively small-scale efforts to change simple cultural practices (including programs of research targeting technologies to maintain and disseminate promising approaches), appear be among the most promising directions in the immediate future for an applied science of cultural analysis. Expanded emphasis on observational studies including those drawing on the documented observations of others is a second important direction. The results of such studies are likely to help us refine our conceptual frameworks and move our science forward in a disciplined manner. It is certainly important to pursue multiple lines of theory and investigation in this young sub-discipline. The most promising approaches, however, are likely to be consistent with those typically taken by the other natural sciences, relying on observational and experimental studies to help refine conceptual frameworks and test the hypotheses those frameworks suggest in a recursive developmental process.

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