

SYSTEMS, METACONTINGENCIES, AND CULTURAL ANALYSIS: ARE WE THERE YET?

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In their paper, Glenn and Malott (2004), both of whom have made important contributions to the science of behavior and its applications, have taken on a very complex and critically important subject, dealing with organizational change from a cultural analytic perspective. The article raises a great many interesting and important questions, which clearly need to be examined if behavior and cultural analysis is to move toward practical application at the cultural level. Only three of those questions will be touched on in this commentary, specifically:

- 1) Why is there so far no substantial empirical work that emerges directly from metacontingent theory?
- 2) Is the integrated systems/cultural analytic approach as presented by Glenn and Malott adequately parsimonious?
- 3) What units of analysis and selective mechanisms are of most use in understanding cultural level selection?

Why No Empirical Work?

Cultural analytic theorizing focused on metacontingencies became quite active by the late 1980s. Sigrid Glenn's seminal paper on the subject was published in 1991, and had a very significant effect on the field. From that point forward for a number of years, practically all discussions of cultural analysis cited that work. To the best of my knowledge, however, no basic experimental research has ever been published in this area, and applied experimental work involving cultural entities (organizations, communities) has generally relied on much simpler analyses. The approaches taken have typically consisted of either intentional shifts in a small number of postcedents of instances of desirable and undesirable cultural practices, or the use of simple feedback mechanisms (e.g., Biglan, 1995; Mattaini & Thyer, 1996; Metzler, Biglan, Rusby, & Spague, 2001). The experimental organizational behavior management and cultural intervention literature, and most of the conceptual work in those areas as well, seems to focus on very simple interventions.

Why are there no detailed metacontingent analyses leading to specific experimental manipulations? There appear to be several possibilities. Perhaps the conceptual work that has been done is not yet advanced enough to provide actual, on-the-ground guidance for designing and conducting experiments. Perhaps it is just too soon, and investigators have

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just not yet begun to conduct experimental work or even observational studies, although certainly in the standard scientific paradigm work in an emerging field would be expected to cycle among conceptual frameworks, observation and experiment. Perhaps new experimental procedures will be required to capture such complexity. Perhaps those involved in the experimental analysis of cultural level phenomena believe they already have the tools they need to understand, predict and control the variables of interest. Or perhaps much of the complex cultural analytic work currently available is flawed in ways that make its application problematic. Examining the other two questions raised above may help to resolve which of these explanations are most significant.

The Problem of Parsimony

Throughout its development behavior analysis has maintained an austere elegance achieved by few other scientific disciplines. Conceptual work in cultural analysis, however, has perhaps naturally been more complicated, and agreement on the boundaries of the domain and appropriate units of analysis has not yet been reached (Mattaini, 1996). Cultural analysis has also so far been more welcoming of conceptual frameworks with roots in non-behavior analytic disciplines than is the case in most of the rest of behavior analysis. During the late 1980s and early 1990s, the cultural analysis literature referred extensively to the cultural materialist work of Marvin Harris (1979, 1989), for example, which because of its materialist and etic approach seemed more congruent with behavior analytic thinking than most other models for understanding culture. Concepts from Harris—like *superstructure* or *permaclonic systems*—were commonly found in the cultural analytic literature of the time. Harris’s work no doubt inspired behavior analysts to explore new avenues, but the extent to which another layer of hypothetical constructs was necessary and useful was even then somewhat controversial, and few of those constructs continue to be discussed in depth in cultural analysis.

It is possible that something similar will happen with concepts drawn from systems theory, but perhaps not. Over time, such constructs may prove to be unnecessary complications, but as discussed below, they may actually help structure advances in cultural analysis. Malott, Malott and Suarez (2003) have outlined procedures for behavioral systems analysis, which is useful for understanding hierarchical processes in organizations, for example. Emerging systemic research also appears increasingly congruent with advancing cultural analysis as a subdiscipline. If so, however, discussions of systems constructs and dynamics in the paper “Complexity and Selection” may be too limited and idiosyncratic. For the past century, a wide range of systemic perspectives have been applied to organizations and other complex cultural systems, from General Systems Theory (Morgan, 1998; von Bertalanffy, 1967) to the science of autopoietic systems (Hudson, 2000), but that work is not referenced or incorporated in depth in the current paper.

In particular, recent advances in understanding self-organizing, autopoietic (“self-making”) systems may have a great deal to contribute to the understanding of cultural processes. Scientists studying autopoietic systems have not yet tested their framework in

social systems to the extent they have with physical and biological systems—this may in fact be work for cultural analysis. Consider two examples of how autopoiesis may be useful for cultural work. First, autopoietic systems structure their own boundaries. While many earlier versions of systems theory (including that briefly sketched by Glenn and Malott, 2004) suggested that systemic boundaries are to some extent arbitrarily constructed by the observer, this no longer appears to be accurate; boundaries are real, and are structured by internal processes. Second, autopoietic theory explains how transactional processes structured into one entity (e.g., an organizational culture that reinforces acts of innovation) may differ from those structured into another (the culture of a competing organization that values compliance with tradition). As a result, how each responds to highly similar stimuli from an impinging environment may be dramatically different; further, the aggregate outcomes achieved are likely to be different as well. Autopoietic concepts may be more useful for organizing efforts to predict and control such effects than alternative current perspectives.

But if systems constructs may be useful, what of metacontingencies? Is the concept of the metacontingency itself necessary for a parsimonious understanding of cultural phenomena? Is the metacontingency a meaningful and measurable construct? These admittedly provocative but largely unexplored questions need to be asked now, since a generally accepted affirmative answer may take the field in one different direction, while a negative may take it in another. Which road is taken may have profound implications for the sub-discipline of cultural analysis for a long time to come. Consideration of the most useful units of analysis and selective mechanisms for cultural work provides guidance here.

Units of Analysis and Selective Mechanisms

Skinner was somewhat inconsistent in his very brief discussions of cultural level phenomenon, and that inconsistency has left a number of intriguing and important questions unanswered. At the biological level, certain relatively stable sets of genes survive better in particular environments than do others—they are selected by the environment. There have been and are disagreements in the field (e.g., whether what is selected is the gene or the species, or the extent to which random processes contribute to species change), but the principle is straightforward and well established. Likewise, at the behavioral level, all else being equal, those actions that lead to improved outcomes tend to persist; they are selected by the contingencies of reinforcement currently in place (which sometimes can change very rapidly).

Skinner indicated that similar selective processes occur at the cultural level (1981), but here things become somewhat murkier. Skinner sometimes discussed the *cultural practice* (a shared repertoire often, but not always, maintained by cultural contingencies because of its contribution to survival of the group) as what is selected, analogous to stable sets of genes or response classes. In other places he might be read as describing cultural entities as what is selected, with the survival of the group as the relevant outcome. He was clearer about two things, however: the definition of culture (“the

contingencies of social reinforcement maintained by a group,” Skinner, 1987, p. 74), and that “no new behavioral processes” are involved at the cultural level (p. 74)—behavioral contingencies explain cultural phenomena. Overall, his consideration of cultural matters must be viewed as sketchy and very preliminary, although some of his observations in this area are critically important.

All of the applied cultural work of which I am aware has focused on changing interlocking sets of cultural practices, including repeated scenes (or *enactments*, Glenn, 1991, from Harris) in which those practices participate in interlocking contingencies of reinforcement. Biglan (1995), for example, begins his analyses, and his extensive current program of research, by identifying cultural practices to be increased and/or decreased (e.g., teen smoking in a community). Supporting and opposing practices likely to interlock with those are then identified, like sales of tobacco products to minors or increases in cigarette taxes. Supporting and opposing practices for those may also need to be identified, like publishing in the newspaper the names of tobacco outlets that do, and those that refuse to, sell to minors. Biglan’s team then proceeds to try to strengthen the entire network of practices that appear consistent with the desired outcome, often in not just one, but a group of somewhat similar communities.

In our violence prevention and nonviolent action work, we begin by identifying enactments or scenes that the available data suggest are likely to be consistent with positive outcomes (like certain forms of written recognition from teachers when a child makes incremental progress or does something prosocial) (Mattaini with the PEACE POWER Working Group, 2001). We then attempt to determine what interlocking practices would ensure that the teacher receives appropriate reinforcing postcedents from peers and administrators for doing so, and what practices would ensure that teacher peers and administrators in turn experience supporting postcedent events for doing so. This analysis continues out into the contingency network, until an apparently stable network of interlocking practices like that shown in Figure 1 can be identified.

Note that certain events external to the school culture itself figure in these contingencies (for example, actions by members of the school board and the trainer, which are embedded in their own sets of contingencies and can also be diagrammed if desired). Aggregate outcomes of sets of interlocking practices, including impacts on the community from increasing rates of prosocial or decreasing rates of violent and coercive acts, serve as active variables if and only if they figure effectively in the set of interlocking contingencies. In our applied work, we clarify apparently potentially useful points for intervention within this web—and then we try it. Different school cultures, not surprisingly, produce different results with similar external inputs (see discussion of autopoietic systems above), so monitoring and continuing experimental manipulation are essential. Each of the participating contingencies can be observationally examined: did a planned postcedent change occur, and did it have the expected outcome? The answers then guide next experimental steps.

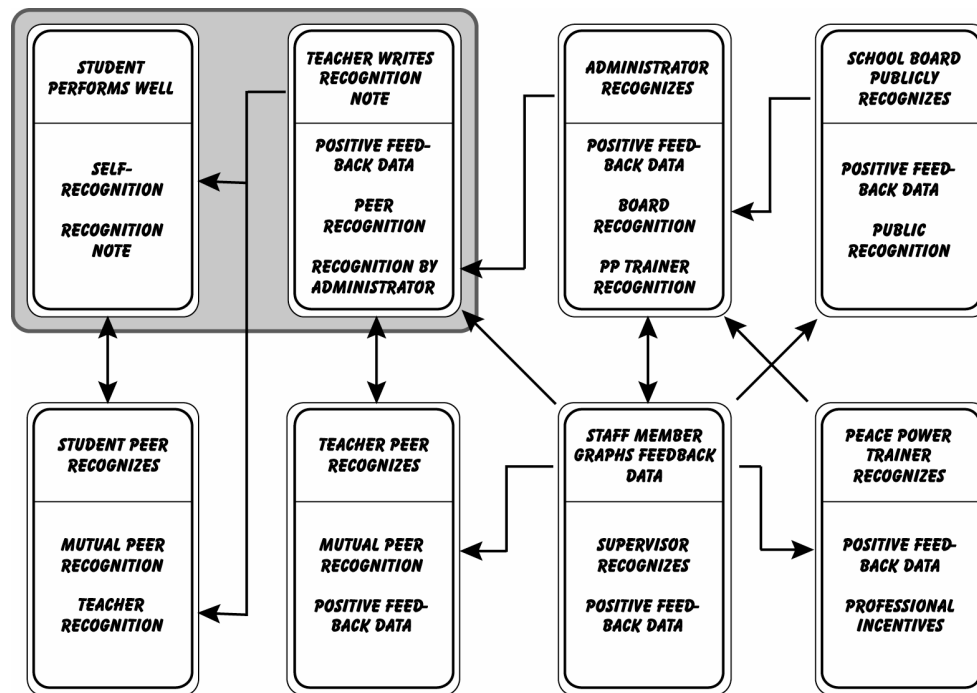


Figure 1. An interlocking set of cultural practices within, and impacting on, a school culture in a program designed to increase the collective incidence of prosocial behavior, and decrease that of violent and coercive actions. Note that this diagram has been simplified for presentation, depicting a single scene, only certain salient postcedents, and omitting discriminative stimuli and establishing operations altogether. Analytic project diagrams include further precision and detail. © 2002, PEACE POWER Working Group, used with permission.

A single event may have multiple effects that resonate and reverberate throughout the system (figuring in contingencies of reinforcement of different kinds for many actors—what Ulman, 2004, calls the macrocontingency). An understanding of macrocontingent relations is a useful addition. However, neither Biglan's (1995) analysis nor the kind of work exemplified in Figure 1 relies on an explicit analysis of metacontingencies, although an understanding of interlocking behavioral contingencies clearly is central to each. (Biglan might disagree with this interpretation, but my reading of his projects suggests that his analytic focus is primarily on interlocking practices.) This is not to say these projects could not be analyzed in terms of metacontingencies, only that so far this has not been the primary approach used. Unanswered questions that can only be addressed at a "higher level" of organization do not seem to arise in these projects, so long as interlocks are clearly emphasized.

An advantage of a parsimonious approach emphasizing such interlocking practices is that what is selected (cultural practices in context), and what selects those practices (the culture—the interlocking contingencies of reinforcement maintained by the group,

which are realized in other practices) are clear (Mattaini, 1996). Those networks of practices can be analytically diagrammed, tested for effects, and ultimately intentionally manipulated. Such clarity of analysis is more difficult with the metacontingency. What is selected by the metacontingency? Not the organization, although the metacontingency literature sometimes seems to suggest this. Selection requires many instances (genotypes carried by individual members of a species; many single acts that are members of multiple functional response classes). Every member of sexually reproducing species necessarily dies, every action ends, and no cultural entity survives forever—individual members of those classes are not selected. Only classes themselves are selected. An organization is a singularity, so it cannot be selected. Perhaps in the metacontingency it is the entire set of interlocking behavioral contingencies that is selected. This generally appears to be the perspective taken by Glenn and Malott (2004). In one place, they discuss metacontingencies themselves as being selected, but this clearly cannot be, since metacontingencies are described as the mechanism of selection, and they cannot serve as both what is selected and the selecting mechanism. But if entire sets of interlocking practices are the unit selected, it appears difficult to achieve much precision in the analysis, or to derive much guidance for action. If aggregate outcomes of one set of interlocking practices are not adequate, perhaps another set would produce better ones, but which one? Metacontingent analysis as described by Glenn and Malott remains quite abstract, and the advantages of devoting attention to such analysis are not apparent. Acting to achieve cultural change appears rather to require abandoning consideration of metacontingencies, and turning instead to the analysis of interlocking practices.

This, to me, is the heart of the matter. Theoretical frameworks in science ultimately are not a matter of ontology; whether or not metacontingencies exist is not the question. Rather, the barely emerging sub-discipline of cultural analysis needs to struggle with which explanatory systems are most parsimonious and consistent with current knowledge, which lead to testable hypotheses, and which prove most useful for advancing the field. There would be a satisfying symmetry if selective processes at a cultural level were directly analogous to those at the biological and behavioral levels, if for example genes were directly analogous to networks of interlocking cultural processes, and contingencies to metacontingencies, so all selective mechanisms could be neatly displayed in a two-way table. Elegance of presentation does not necessarily lead to explanatory parsimony, however. So far, a convincing case for the metacontingency as the essential conceptual framework for cultural analysis has not been persuasively made. This may or may not remain true in the future, and those developing that framework should certainly pursue and refine their work. For the moment, however, it appears critical that premature conceptual closure in cultural analysis be avoided, and that multiple possible frameworks for such analysis—so long as they are consistent with the underlying science of behavior—be explored.

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