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BEHAVIOR ANALYSIS AND SOCIAL DYNAMICS: SOME QUESTIONS AND CONCERNS

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I want to thank João Claudio Todorov for the privilege of attending a most stimulating and well organized gathering, and for his outstanding hospitality and powerful intellectual leadership. Of course, many thanks also go his students and colleagues who provided both invaluable service and essential input to the discussion.

Prior to our attending the Brazilian Think Tank on behavior analysis and social processes, we were each asked to submit some responses to questions raised by João Claudio Todorov that formed the basis for each day's discussion. The following is a revised version of what I had submitted based on the discussions in Brazil as well as subsequent considerations. The principal topic was:

INTERACTIONS BETWEEN BEHAVIOR ANALYSIS AND THE SOCIAL SCIENCES

Questions:

(1) *What is the appropriate ontology within which to explore interrelationships between the science of contingency relations and socio-cultural phenomena?*

I'm not sure I understand this question, but assuming the term "ontology" refers to some kind of categorical scheme or fundamental orientation, then behavior analysis defines itself as a *natural science of behavior*. Thus, the field places itself with other natural sciences such as physics, chemistry, meteorology, biology, and the like. Indeed, because behavior is quintessentially a biological property of organisms, behavior analysis can be considered a branch of biology. Clear distinctions between the natural and the so-called social sciences are not obvious and considerable variations abound; however, for some at least, a natural science orientation can serve as a significant barrier to effective interactions with

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those calling themselves social scientists. I'll discuss this issue further in a subsequent section.

The science of behavior analysis has, for the most part, just one big card to play—the concept of *contingency*. This is a very big card because the concept is so pervasive and malleable that it can address a multitude of phenomena (but see below). And, of course, the concept encompasses both shaping (i.e., differential acquisition and extinction) and the primary role of the environment in modulating behavioral change and control through the actions of dynamic relations between contexts, behaviors, and consequences. Thus contingences serve both to change and to maintain behaviors. Most compellingly, they engender orderly patterns of behavior that justify the fundamental assertion that there can be a science of behavior. We discern patterns of behavior in individuals, in the interactions between individuals, in groups of individuals, and even in nations. Terms describing these various patterns range from "personality" to "social practice" to "national character". Aside from sources such as species-related and individual hereditary factors, as behavior analysts, we invoke the operation of contingencies to account for these patterns. But just what contingencies? The number of possible contingencies is literally infinite—equivalent, for example, to the number of possible feedback functions—an infinite set.

The notion of "metacontingency" as "…relations between interlocking behavioral contingencies and their selecting environments" (Glenn & Malott, 2004, p. 100) extends the basic concept to address the more complex dynamical systems inherent in socio-cultural developments and controls. By the way, another way of labeling the sorts of "contingencies of contingencies" captured by the term "metacontingency" might be "hypercontingency." At its simplest, the idea appears close, but not identical to, what the experimentalist calls a "second-order schedule" (e.g., Marr, 1979).

One lesson learned for the study of second-order schedules and, I believe inherent in the behavior analyst's perspective on the pervasive significance of contingencies, is the notion of scale-invariance; that is, we assume, sometimes implicitly, that the principles of behavioral control operate at all levels of behavioral complexity---from pigeon key pecks to human cultural practices. The fact that we gathered in Brazil to discuss the role of behavior analysis in addressing social phenomena is but one manifestation of this assumption.

I see at least three salient issues here that should be addressed carefully in any discussion of the potential contributions of behavior analysis to social phenomena. The first is that the very pervasive and malleable character of the concept of contingency can also seduce one into a false sense of "understanding" simply because one might shoehorn virtually any behavioral phenomenon into the

concept---it may appear to explain everything of interest. Simply because it appears to work in particular situations, one needs to be quite suspicious and sensitive to the limitations of any concept in accounting for events. I get the impression there's a lot of hand-waving going on here.

Second, even if we are basically right in our emphasis on contingencies (and I think we are), we are left with the immensely difficult task of understanding how contingencies actually work to engender the patterns we discern. Our understanding of the mechanisms here is woefully lacking as I'll discuss a bit later under Question #3.

The third issue is that, of necessity, virtually all socio-cultural activity is verbal. Verbal behavior itself is a social phenomenon and is implicit in the peculiar self-organizing dynamics of verbal acquisition and subsequent control---boot-strapping of the highest order. Despite Skinner's earlier discussion of rules and their relations to contingencies and considerable further developments, including such recent research topics as relational frames, I think we still have a quite meager, vague, and fragmentary understanding of verbal behavior. To that extent, we can but have quite limited understanding of virtually any social phenomenon, never mind contribute significantly to its control, until we recognize and address our deep ignorance of this most complex of all behaviors.

(2) *How should behavior analysis interact effectively with disciplines addressing issues of large scale like organizations and culture?*

I mentioned earlier that behavior analysis, in asserting itself as a natural science, faces potential opposition from those who, for what ever reasons, see themselves as social scientists, if scientists at all. This situation has correspondence with what C. P. Snow long ago called the "two cultures," referring to the humanities and the (natural) sciences. As in that classic case, the contempt felt between behavior analysts and "those others" is mutual—if, indeed, "those others" are even aware a field called behavior analysis exists.

Our contempt for much of the social sciences is generally based not on the particular phenomena they investigate, but on their modes of explanation or theoretical stances. We deem them "mentalistic," "dualistic," "cognitive," "circular," "fictional," "incoherent," etc. However valid these labels might be, in an important way, they miss the point. Virtually none of the phenomena investigated by, say, social psychologists are of no interest to behavior analysts, or, indeed, anyone else at all curious about human behavior---compliance, persuasion, prejudice, cooperation, competition, social facilitation, class distinctions, social networks, crowd behavior, stereotyping, etc. The same may be

said of sociologists, anthropologists, economists, public policy experts, political analysts, historians, biographers, novelists, playwrights, and many others. In our attempts to understand, never mind control, social behavior, behavior analysts do not enter into an empty, untrodden field. In fact, we've come quite late and have much to account for.

Thus, I believe behavior analysts who are interested in social/cultural behavior should learn as much as possible about relevant social sciences, in particular the problems addressed and the methods used to address them. For example, I've been impressed by reading the literature on social influence and propaganda (e.g., see Cialdini, 1993; Ellul, 1973; Herzstein, 1986; Pratkanis & Aronson 2001; and Soroweicki, 2004) just how powerful a huge range of techniques are in controlling behavior (we are all victims!). Many of these techniques go back at least to the 19th century, yet, to my knowledge anyway, there is little or no systematic behavior-analytic perspective on this area. Even Guerin (1994) in his excellent and unique book on behavior analysis and the social sciences barely touches on the domain of persuasion. Biglan (1995) does in treating cultural interventions, but most of the techniques he discusses were around long before behavior analysis had anything to say about them.

Our own credibility in interacting effectively with relevant social sciences depends crucially in understanding their contributions, methods, and language. Karl Rove, President Bush's senior advisor and political strategist, may know much more about how to control the behavior of the voting public in the USA than any behavior analyst ever will. He, along with others of his ilk, learned much of their craft from Joseph Goebbels, Hitler's notorious propaganda minister (e.g., Herzstein, 1986). Given enormous resources, this power to influence comes from understanding the experimental as well as historical literature in social dynamics, marketing, propaganda and political persuasion, the use of symbols, motivation, religious practice, class conflict, cultural anthropology, prejudice, etc.

I believe we, as behavior analysts, have at least as much to learn as to teach. The very last thing we should do is say to these other fields, "You don't know what you're talking about; I know how it really works." As behaviorists, we have been singularly unsuccessful in addressing our *own* critics—we, who are putative experts on behavioral control—what, then, is our credibility?

In contrast to most other aspects of social functioning, some behavior analysts have successfully focused on organizations. All organizations operate under a system of contingencies—indeed, that's what makes them organizations. Contingencies may be explicit, as specified by rules, or implicit, but whatever their manifestations, they ultimately control the effectiveness of the organization—compensation and incentive systems, performance evaluation, conditions

for hiring and promotion, management structure and style, data gathering and decision making, labor-management relations, profit sharing, pricing, marketing, quality control, safety, and many, many other operations define the contingencies supporting (or undermining) most any organization.

Organizational-Behavioral Management may have been the most productive cadre addressing this area, but, unfortunately, this is a small group and not growing rapidly enough, if at all. There have also been a few other examples of behavior analysts focusing largely on public safety, child care, and health with some effectiveness. Again, this is a tiny set. Perhaps the most promising area here is behavioral economics. Some of the results of both basic and applied research have already been incorporated into such areas as drug abuse treatment and prevention, but more could be done. To consider a relatively simple example, Pandora's Box need have had only one item in it—the hyperbolic discount function—to represent most all of the major evils confronting human kind. Political and public policy as well as a host of other large and small scale social actions deeply reflect this—let's buy now instead of saving; let's have tax cuts for the wealthy so they'll keep our party in power, never mind the budgetary and social consequences; let's plunder resources now, never mind the inevitable decimation of the environment and ultimate damage to the health of the population; let's go to war now, never mind how we might get out of it, etc., etc. The list is endless. As behavior analysts (and we are not alone), we know something about how to set up conditions to deal with relatively small but immediate consequences versus large, but delayed ones. However, instituting effective public (or personal) policy based on demonstrable scientific principles, as every informed person knows, is virtually impossible. A question we might try to address is why. Now this gets into Question #4; I'll touch on it there.

(3) *How does the concept of metacontingency account for complexity and non-linear dynamics of social systems?*

Focusing for the moment on an organization as a social system, if I understand the concept of metacontingency at all, organizations of any significance and stability are *defined* by their systems, their subsystems, their inputs, their outputs, and their interactions, both within and outside the organization. These processes can be seen as "interlocking contingencies and their selecting environments." The possibility of modifications or perturbations at one or more functional modules or nodes in the system to yield some desired (or undesired) effects depends on adequate understanding of what does what to whom with what consequences. That being said, I don't really see much that's new here

(apart from some terminology) that had not been the major focus of systems design and operations research in such fields as industrial engineering and management science for many, many years. Perhaps I need considerable enlightenment here, but when "metacontingencies" are identified with or comprised of activities labeled "marketing," "human resources," "training," "inventory control," "accounting," "shipping," "sales," etc., I can find this same analysis in any good textbook on business systems. Obviously, each of these subsystems can and should be further broken down in to specific tasks and so on, but a good systems engineer in looking at a detailed organizational chart always asks the questions: What does this identified department do, how does it fit into the rest of the organization, and what happens if I change its operation in some way? Conversely, in designing an organization one has to ask: What are the functions or goals of the organization, what does it take to make these happen, and how would you know? In general, there appear to be strong conceptual ties between formulations of metacontingencies and general systems theory, the latter invoking such concepts as adaptation, hierarchies, differentiation, and homeostasis. For applications of systems analysis to social structures, see, for example, Cortes, Przeworski and Sprague, 1974.

Do metacontingencies *account for* complexity? By some account a large number of vaguely specified and interlocking contingencies *is* complexity—that is, we have a hard time understanding what they are, never mind how they work. As for "complexity" and "non-linear dynamics," these are slippery terms that require some considerable unpacking if they are to have any use other than metaphorical fancy. In the past decade or so, many papers and books at various technical levels have appeared devoted to something called "complexity theory" (e.g., Bak, 1996; Bar-Yam, 1992; Camazine, et al., 2001; Cambel, 1994; Casti, 1989, 1994; Coveney & Highfield, 1995; Jensen, 1998; Kauffman, 1993; Nicolis, & Prigogine, 1989; Waldorp, 1992). as well as a huge number of texts on non-linear dynamics and chaos theory. A small sample of my favorites include: Alligood, Sauer and Yorke, 2000; Baker and Gollub, 1996; Kaplan and Glass, 1995; Moon, 1992; Strogatz, 1994; and Williams, 1997. These are examples of texts are now used routinely in courses in dynamical systems for undergraduate and graduate students in engineering and science. Regrettably, I suspect few, if any behavior analysis students take such courses.

Each of those sources dealing with complexity expend some effort to define what they mean by a "complex system," and there is a good deal of variation in how this is done, in the quantitative methods used to address what they deem complexity, and the examples treated. Typical of criteria are: (1) a number of interacting components or subsystems, that is, the states of these systems are

correlated in some way, (2) nonlinearity, that is combinations of states or inputs are not additive (or subtractive), (3) the behavior of the system is not predictable from separate consideration of its components, but only from understanding the relations among them. This is what is typically meant by *emergence.* (4) There are spatial and temporal scale-invariant properties such that no characteristic event size or time controlling the evolution of the system. This means their stochastic properties will follow power laws. (5) *Self-organization* in which patterns emerge from within the system through mutual interaction of the system's elements. This occurs under certain critical states or conditions, sometimes called *self-organized criticality*. (6) The complex global behavior of the system can sometimes be described by relatively simple, deterministic rules.

As an example, the term "emergence," is certainly commonly identified with social phenomena. This description has been recently applied to individual behavior in areas of stimulus control such as equivalence and relational frames (e.g., Hayes, *et al.*, 2001; Sidman, 2000).

Another and much older area where behavior analysts have found what could be called emergence is in the actions of various schedules of reinforcement. Quite simple descriptive rules can produce highly organized patterns of responding in individual organisms. Two items of interest here: (1) After more than half a century of research in this area, we are still puzzled by exactly how most of these patterns emerge; and (2) We have spent almost no time exploring the simplest interactive contingencies between *just two organisms*, for example, cooperative or competitive arrangements. Don Hake (e.g., Hake & Olvera, 1978) started working in this area nearly 30 years ago, but his most untimely death seemed to bring virtual demise to this research as well. We need an experimental analysis of social behavior pursued with the same vigor now evinced by researchers who publish in the *Journal of the Experimental Analysis of Behavior* (*JEAB)* and the *Journal of Applied Behavior Analysis* (*JABA)*. They largely focus on individual organisms---one at a time. The experimental social psychologists and some animal behavior researchers are way, way out ahead of us.

In the domain of animal behavior, for example, I've seen little actual empirical (not to say experimental or quantitative) activity by behavior analysts in identifying or characterizing group behaviors in dynamical terms so as to achieve the sorts of descriptions and predictions found, for example, in biological systems (Hemelrijk, 2005; Solé & Bascompte, 2006). A recent book, *Self-Organization in Biological System*s by Camazine, *et al*. (2001), contains many fascinating examples, including bee hives, ant colonies, termite mounds, bird flocking, fish schooling, firefly-flash synchrony, etc. Mathematical models of these systems show (as in complexity criterion #6 above) that "complexity" may emerge from

"simplicity" in that a few rules descriptive of relatively local events can produce remarkable organizations in the large. Can we make use of this kind of research?

All the sources on complexity and dynamical systems I mentioned above have in common a *mathematical treatment* of selected examples from physics, chemistry, biology, geology, computing, etc. I emphasize this because even though there are discussions of complexity in other systems (e.g., governments, cultures, etc.), with quite few exceptions, there are no real quantitative treatments. One is left, at best, with metaphor and the value of that is questionable. For example, how might you apply the above criteria *qualitatively* to any significant organization or culture? What would you know if you did? I do not mean by these sorts of questions that the exercise would necessarily be of no value. Certainly many of the sources (e.g., Bar-Yam, 1992) talk about such cases without providing a quantitative account. Often something is gained by looking at old phenomena in new ways, but in the current case, what is to be gained is, to me, uncertain.

If we can't get very far with a qualitative and largely metaphorical perspective on social processes and dynamics, can we begin quantitative approaches of the sort already applied in other domains? A recent review in *Science* (Grimm, *et al*., 2005) discusses *pattern-oriented modeling* (POM) of what the authors call "agent-based complex systems", that is, "…dynamic networks of many interacting agents; examples include ecosystems, financial markets, and cities" (p. 987). POM deals with patterns at different hierarchical levels to develop algorithms that can effectively characterize and make testable predictions in variety of systems from the growth of forests to habitat selection in fish to settlement patterns in tribal groups. The authors claim these methods have considerable generality unlike earlier computational models in, for example, ecology.

(4) *What are some ways that "cultural change" can be brought about by behavior analysts working alone or in collaboration with applied scientists from other domains?*

I've said a bit about this topic in Question #2 above and, as indicated, except for some quite modest successes in very limited (though not insignificant) domains, the problems here are immense. The first question I would ask is: What do we want to change, and why? The entire history of political thought from Plato to Bush has attempted to address this question by defining governmental and cultural systems to achieve particular outcomes, given basic assumptions about the nature of human behavior (e.g., compare Plato with Hobbes with Adam Smith

with Marx with Thoreau with Hitler with Gandhi, etc.). These systems, in turn, typically have built-in methods for control. As behavior analysts, we may see Skinner's Walden Two as a paradigm system to achieve particular outcomes based on an empirical, scientific approach to solving social problems. But Walden Two was based on assumptions of what was appropriate to achieve, like all the rest of political theories. For example, Frazier, the designer, specifies what he deems to be the principles and conditions for the Good Life: Health is better than disease, interesting work is better that idleness, reduce unwanted and unpleasant work to a minimum, exercise talents and abilities, encourage intimate and satisfying personal relations, practice tolerance and affection, get rest and relaxation. Not a bad start for a community, I would say, but would everyone agree with these principles? Many, for example, would want to add much to this list, including, no doubt, some goals that would contradict others.

Forms of control to achieve cultural and other social ends are major components of any government short of anarchy. Our own culture, as reflected in government, for example, depends on the constant introduction and enactment (and repeal) of laws and policies. The differences from a true experimental system like Walden Two are starkly manifest in the rationales for their enactment, and certainly the proper evaluation of their consequences.

In any case, the truly major problems of war, poverty, over-population, disease, genocide, religious and nationalist fanaticism, environmental decimation, resource plundering, etc., etc. are virtually beyond any scientific approaches to address, in part because no political system has ever set up proper contingencies to address such problems in this way. Yes, in the short term we can devise nuclear weapons, go to the moon, produce vaccines and contraceptives, and build faster computers, identify the human genome, and even have some positive influences on health problems like AIDS and smoking. But while the sciences behind these achievements could be applied to address some of the major problems listed above, without the application of a science of behavior to set up conditions for applying effective techniques from whatever source, very little can be accomplished, as Skinner noted more than 50 years ago. In an ironic perversion of this application, the current regime in the USA is the most concertedly anti-science in the modern history of Western culture; and, as indicated previously, it has been quite clever in manipulating the behavior of the voters to support this effort—using a science of behavior to promote its anti-science programs.

(5) *What is a critique of social constructivism and its relation to behavior analysis?*

Speaking of anti-science, the social constructivist movement has been best described by Larry Laudan (1990):

> The displacement of the idea that facts and evidence matter by the idea that everything boils down to subjective interests and perspectives is—second only to American political campaigns—the most prominent and pernicious manifestation of anti-intellectualism in our time (p. x).

There was a time when a few behavior analysts schmoozed with this bizarre crowd for reasons I've discussed elsewhere (Marr, 2003), but happily that time has passed along with the movement itself. Enough said.

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As one who had not hitherto given much thought to behavior analysis and social dynamics, the opportunity to participate in several days' sequestered and passionate discussion of this topic with extraordinarily thoughtful and knowledgeable colleagues was indeed revelatory. I was particularly struck by the variety of backgrounds, interests, and views of the participants, all of whom would describe themselves as behavior analysts. This is a healthy sign for the field.

Human social behavior is, without question, the most complex phenomenon known in nature—more than particle physics or cellular biochemistry or meteorology. Only our mundane engagement with it gives us the illusion of any understanding. The extent to which social processes can yield to the elegantly simplicity of behavior analytic principles remains very much an open question. In addressing questions of the kind I've responded to in this paper, I don't believe any solutions were forthcoming from the Brazil Think Tank, but in wrestling with these questions and many others posed, we certainly gained enlightenment on much we are yet to understand.

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