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CHALLENGES IN CONCLUDING A RESEARCH PROGRAM: SOME REFLECTIONS ON REVIEWER COMMENTS REGARDING "*CYBERRAT, IBSA, AND A 'TURING TEST' TRILOGY*"

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ABSTRACT: Invited reviewer comments by Iverson (2011/2012), by Lewon, Munoz Blanco, and Hayes (2011/2012), and by Phelps (2011/2012) are reflected upon within the context of my own perceptions of selective strengths and weaknesses of my *CyberRat/Turing-Test* monograph (Ray, 2011/2012). Especially noted in my response is the persistent challenge of translating Kantor's (1958) interbehavioral views into actual research methods, my own and many others' contributions to that effort, and the importance of incorporating field/setting conditions into our scientific analyses. While I do not anticipate conducting further programmatic research on such topics, the paucity of current interest in the very important temporal dimensions of such field/setting variables remain nonetheless inviting for further elaboration.
Key words: Interbehavioral Systems Analysis, CyberRat, Turing test, behavior analysis, functional analysis, operations analysis, time series analysis

I am very grateful to the present reviewers (Iversen, 2011/2012; Lewon, Munoz Blanco, & Hayes, 2011/2012; Phelps, 2011/2012) who accepted the challenge of commenting in print on my *CyberRat/Turing-Test* monograph. Of the dozen reviewer invitations that were distributed, most had sound reasons for being declined. But these brave few colleagues accepted, and I fully appreciate the difficulties of their task. In fact, as I emphasize in the monograph's *Author's Note*, I struggled myself with this manuscript over a period of several years. That struggle didn't rise so much from the points I wanted to make as it did from the fact that so many of the points had previously been made elsewhere during my thirty-eight year journey from the first Ray and Brown (1975) publication to this concluding statement on Interbehavioral Systems Analysis (IBSA) as a methodology. I'm not likely to publish much more on the IBSA approach other than what follows here, as I believe I've said about all that I have to say on the topic through one publication or another. And therein lies my problem in writing such a "concluding" monograph.

Throughout the effort of writing it I struggled most with my desire to include too much that had already been said long before the monograph was conceived. In fact, if I were reviewing the monograph as an outsider, I think my own primary criticism would focus on how little of the IBSA methodology that the monograph

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aspires to validate is actually articulated in the monograph itself. Thus it is an easy mistake to read the monograph and believe, as Phelps (2011/2012) states, that "Ray makes the point that interbehavioral psychology never formally developed a methodology" (p. 311). What I really noted was the fact that Kantor himself never developed one (e.g., Moore, 1984). I hope my own research program has offered some small contributions to remedying that shortcoming in Kantor's (1958) original interbehavioral approach.

This point is an easy oversight to make, for not only is Ray and Delprato (1989)—itself a lengthy treatise—virtually required reading to appreciate some of the finer nuances of IBSA as a broadly applicable methodology, but so too is Ray (1992) required reading if the present monograph's reader is to fully appreciate how descriptions must incorporate not just behavior, but also behavioral fields in sufficient detail to allow for believable visual reconstruction through video-incorporated simulation. Phelps (22011/2012) perhaps comes the closest to articulating such an appreciation when he points out that ". . .a lever in a Skinner box is more than just an abutment that can be depressed; a lever is also an affordance for climbing" (p. 312).

I once described how I gained my own insights regarding enabling properties (Ray, 1992), or those properties that Phelps (2011/2012)—and by implication of Phelps's earlier reference, Gibson (1977)—is calling "affordances." To illustrate my discovery I used the example of a simulation based on crude animations of a cartooned monkey in a cage:

> The simulation must begin with not only a selected behavior but also a selected environmental field. The monkey stands, sits, and walks in a specified location. But which needs specification first, the behavior or the location? To answer this, we must determine the mutual enabling functions of each. Walls limit walking (can't go through them) but enable climbing (can climb up them). Open space enables walking, standing, or sitting but disables climbing. Thus we should first specify which environmental field conditions exist, and this will, in turn, access [*current note: this original use of the term "access" could easily have been "affords" as Gibson uses the term*] quite a different array of behavioral categories defining the kinematic matrix. Open floor space will define stand, sit, and walk possibilities. Walls will define stand, sit, and climb.
>
> But how does the monkey ever leave a wall field without walking? He doesn't. Therefore we discover our second specification problem: When walking, animals most often walk in the direction they are facing, but sometimes they turn (change directions) while continuing or initiating a walk. Open spaces imply some probability that this will occur; walls imply a relatively high probability of turning (although the animal may also just stop and stand, sit, or climb). Thus there are actually two determinant kinematic matrices and one new behavior required to simulate our monkey in his cage: (a) an open-space matrix constructed around walk, turn, sit, and stand; and (b) a wall matrix constructed around turn, sit, stand, climb, and walk (if facing away from the wall).
>
> What happened to eating? Here we discover our second environmental enabling/disabling field condition. To eat, an animal must interact with food as a stimulating object. Although he can carry food with him about the cage, he

> typically finds food at singular locations within his environment. Food is thus a more temporally fleeting environmental element than open spaces and walls because animals consume it and environments do not typically offer ad libitum supplies of it. (Ray, 1992, pp. 108-109)

Translate such details to accommodate variations in orientation and location of *CyberRat* in the operant chamber and simple behavior-only categories rapidly expand to far more complex descriptions. Iversen (2011/2012) correctly captures a bit of this issue when he points out that I believe ". . .it is only by enhancing the interbehavioral analysis technique that the model for behavioral transitions can be improved so that video clip selections become more smooth and less 'jumpy'. . ." (p. 306). That, of course, was the point of Video Illustration 5 in the monograph—traditional behavioral categories (or even operant classes) do not capture the nuances of contextual fields that contribute alternative functional implications for interaction.

This latter point is relevant to Phelps's (2011/2012) interpretation of IBSA as elaborating upon the concept of "R-R laws." To the extent that kinematics reflect an "inter-dependency" (or more precisely, an auto-correlation) in behavioral sequences, an emphasis on behavioral kinematics calculated independently of either environmental or organismic setting conditions (cf., Ray & Delprato, 1989) make it easy to overlook the fact that many potent contextual factors contribute, and not all are response-dependent. In fact, kinematics are dynamic in two important ways: 1) they measure via conditional probabilities how behaviors change sequentially; and 2) kinematics themselves change from one context or setting condition to another, as Ray and Brown's (1975) original data from drug (i.e., organismic) and temperature (i.e., environmental) settings, among others, fully illustrate.

And not only is familiarity with prior publications overly assumed, so also is prior use and experience with *CyberRat* itself highly relevant to conclusions one might draw about it. Thus those who have experimented with *CyberRat* most extensively are likely to have a different interpretation of its strengths and weaknesses than those who have fewer hours of experience with it. I take this factor to be a likely source for an interesting contrast between two reviewer comments. First, consider Iversen's statement that "CyberRat cannot be made to do something that the three live rats that form the video data base were not trained to do (i.e., you cannot shape a novel behavior other than lever pressing)" (p. 304). Now contrast this with Phelps's (2011/2012) observation that

> My final comments are recognition for the versatility of the CyberRat system for differential reinforcement and selection of behaviors other than lever pressing. Most students are able to readily shape the behavior of a CyberRat in lever-pressing in a matter of minutes, but when an instructor then tells them to select another behavior. . .and demonstrate control of this behavior, more attention to differential reinforcement of successive approximations is required. (p. 312)

In fact, close observation will reveal that *CyberRat* animals rarely, if ever, turn in specific tight circles in any location, including being near the water dipper. Yet careful selective reinforcement of components of this final "macro" behavior of a complete tight turn in a circle allows one to shape such a behavior out of various clips, even though the original video footage never actually recorded such events! So, unlike the limit implied by Iversen, *CyberRat can* be used to demonstrate at least *some* new types of behavior.

There is also some misunderstanding revolving around the use of multiple animals (Iversen, 2011/2012) in the establishing research for *CyberRat*. Even Skinner could not have made a science based on individual animal analysis of first-exposures to the operant chamber. First-exposures to a given environment can never be repeated in a single organism's lifetime, and how individual animals behave in this circumstance reveals individual differences. It is those differences from animal to animal that become incorporated into calculations like means and standard deviations, and it is exactly those calculations that allow one to simulate realistic differences between animals on an individual basis. Hence *CyberRat* doesn't simulate a "group" effect, but rather it simulates real differences between individual animals based upon differences as empirically evaluated. Likewise, a single animal could have been used to generate all the video clips if we had simply taped enough sessions. It was a time-saving matter that led to the "stand-in" use of different animal actors in *CyberRat*, not an intrinsic simulation requirement. So Iversen's point of differently colored animals, while insightful and correct, isn't really relevant to the "heart" of simulating via video reconstruction. In fact, in *CyberRat* each animal selected for experimental use is an individual—drawn from a population of possible *CyberRat* animals—that is representative of the group sampled, just as real individual rats are representative, but individual, samplings of a population of real rats.

Finally, let me reflect on Lewon, Munoz Blanco, and Hayes' (2011/2012) stated appreciation for the fact that "A descriptive account of an event is necessarily a functional one in that the relations identified by a functional analysis are included in the description, but functional accounts are not necessarily sufficiently descriptive" (p. 317). I wholeheartedly agree with this statement, as well as agreeing with all of what these authors have to say about the positive contributions that have been made by TEAB researchers—both basic and applied.

Lewon, Munoz Blanco, and Hayes (2011/2012) also aptly emphasize important contributions by Skinner (1957) and many others (e.g., Breland & Breland, 1961; Brown & Jenkins, 1968; Falk; 1971; Staddon, 1977; and especially Michael, 1982; and Parrott, 1983) who have contributed to a more descriptive research expansion of TEAB as originally conceived. How these various authors have expanded upon TEAB's primarily functional emphasis is an important recognition that TEAB research remains a dynamic and evolving process. And because some of the evolving contributions cited involve the incorporation of verbal behavior analysis, it is relevant to point out that Kantor also offered a highly descriptive account of language (Kantor, 1977). Thus it is natural that IBSA methods have also been used to probe descriptive phenomenological and verbal behavior. For example, Ray and Delprato (1989), in their section on "*Modes of Description*" (pp. 113-119), offer examples of an IBSA approach to reconstructive simulations that offer verbal reconstructions from behavioral observation codings. As such, *CyberRat* certainly isn't the end of IBSA-inspired simulation possibilities.

While Lewon, Munoz Blanco, and Hayes (2011/2012) are correct in pointing out their many examples of descriptive expansions on TEAB, I will conclude my present comments by emphasizing a point that I feel has been under-explored by almost all who have applied Skinner's functional analysis. Hidden (perhaps in plain view) in Skinner's contributions is his underrated use of time-series analysis vis-à-vis his favoring of cumulative records for data recording. In fact, this TEAB contribution relates, albeit distantly, to the positive recognition in Phelps's (2011/2012) statements concerning ultradian and circadian rhythms in behavior (as well as environments). In my view, those who have contributed much to TEAB haven't done enough, especially in the applied arena, of what I've described in the monograph as operations analysis. Such analyses focus on the temporal characteristics of the interbehavioral stream as individuals interact with environments that, themselves, may be relatively static or dynamic across time. This is especially true of ultradian rhythms (i.e., less than 24-hour periodics) and also of less periodic (i.e., more variable or complex oscillatory functions) environmental schedules.

While there is a massive research literature on schedules of reinforcement that followed Ferster and Skinner's (1957) publication of how schedules of intermittent reinforcement impact temporal patterns—and more importantly, patterns of *change*—in behavioral response rates, little else has come of the use of time-series analysis in TEAB. Yet how we distribute our various activities across our temporally varied, but often highly organized, environments—especially environments that are not contingent or direct products of our personal behaviors (i.e., aren't technically "reinforcers")—is largely ignored by behavior analysts today.

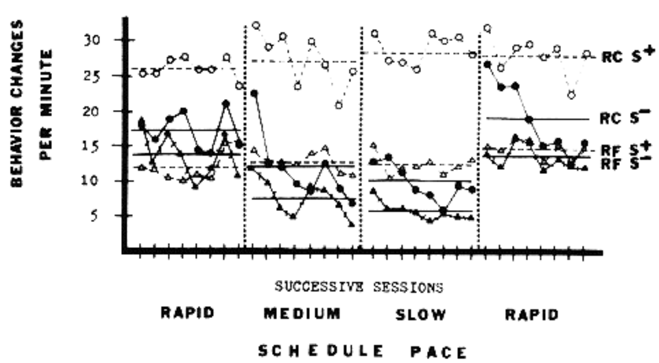
For example, one of my more regretted failures to conduct follow-up research on a sparse bit of published data (Ray, Upson, & Henderson, 1977) relates to experimental scheduling of discriminative stimulus/setting events (as opposed to schedules of reinforcement). That publication illustrated that alternatively paced manipulations of persistent discriminative setting stimuli have rather dramatic implications for "overlooked/hidden" behavioral dynamics such as category-change velocity as well as for behavioral pattern coherence (a term that describes the relative concentration of behavioral sequences into high or low conditional probabilities with few or many alternative patterns of kinematic behavioral change). These data are perhaps worth reproducing here as a case in point for the relevance of temporally-paced environments and temporal aspects of multiple-behavioral integrations (as opposed to singular response categories).

Animals in these experiments were under one of two alternative contingencies of water delivery. One was response-contingent, where each bar-press generated reinforcement delivery (CRF) to the animal, while a yoked-to-response-contingent (non-contingent water delivery) animal in another chamber received the same delivery schedule without any response requirements. We directly observed and recorded all behaviors via an observe/record alternating sampling technique. In addition, a second house light was turned on/off as discriminative stimuli signaling whether water was available or not. The most relevant experimental manipulations were described as follows:

Four sets of eight 15-min experimental sessions involving successive S+ and S- presentations were used. During the first and fourth set, durations were identical to prior discrimination training in which presentations of S + occurred 37 sec apart (mean onset-to-onset) with a range of 15-85 sec (S + or S- mean duration = 18.5 sec; rapid pace). During the second set of conditions (medium pace), S + and S- durations were twice as long as those of rapid pace (mean = 37 sec). A third condition (slow pace) included stimulus durations twice those of medium pace, or four times longer than those of rapid pace (mean = 74 sec) (Ray, Upson, & Henderson, 1977, p. 652).

Two forms of time-series analysis were conducted. One simply evaluated session-to-session changes across multiple days of experimentation as an interrupted time series, but the other evaluated micro-behavioral change rates starting at the onset of S- periods (a period used because it turned out to be the setting that most influenced behavioral flow velocity and/or kinematic patterns) and continuing until the S+ came on. Results of the first analysis are depicted in Figure 1, while Figure 2 depicts the second analysis.

(A)



(B)

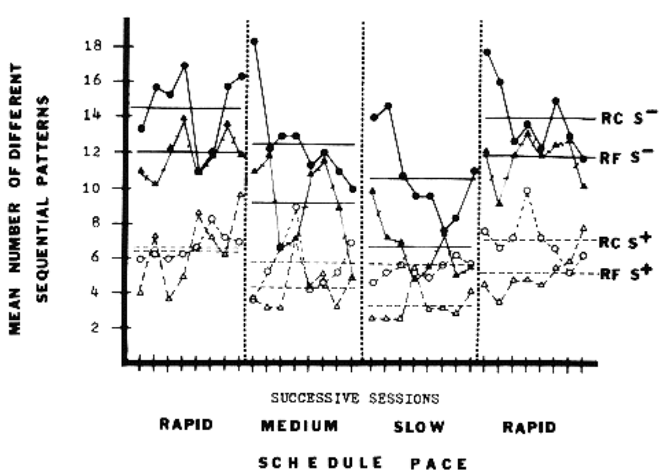


Figure 1. (A) Mean frequency of behavior-to-behavior shifts/min (behavioral flow rate) occurring within each stimulus condition across all successive sessions and schedule pace conditions for response-contingent (RC) and free reinforcement (RF) groups. (B) Mean number of different types of behavior shifts from one category to another (sequential pattern variations) occurring within each stimulus condition across all successive sessions and schedule pace conditions for response-contingent (RC) and free reinforcement (RF) groups (from Ray, Upson, & Henderson, 1977, p. 653).

The critical fact here is that bar-pressing rates were totally unaffected by any of these manipulations. Rather, it was the complex interbehavioral dynamics during S- that showed the effects, both progressively across sessions-within-conditions (Figure 1) and dramatically within sessions but across the S- period (Figure 2).

Note the dramatically high behavior change rate at the beginning of S- (and the much higher rate for rapid-paced conditions vs. slow-paced). Also note the damped-decay functions for both conditions as S- continues. That same publication explored similar analyses of killer whales (*Orcinus orca*) in post-performance "rest" periods and found highly similar damped-decay functions in behavioral flow rates—so similar that the same dynamics model was able to fit the data for both species, albeit with different values in the parameters (see Ray, Upson, & Henderson, 1977).

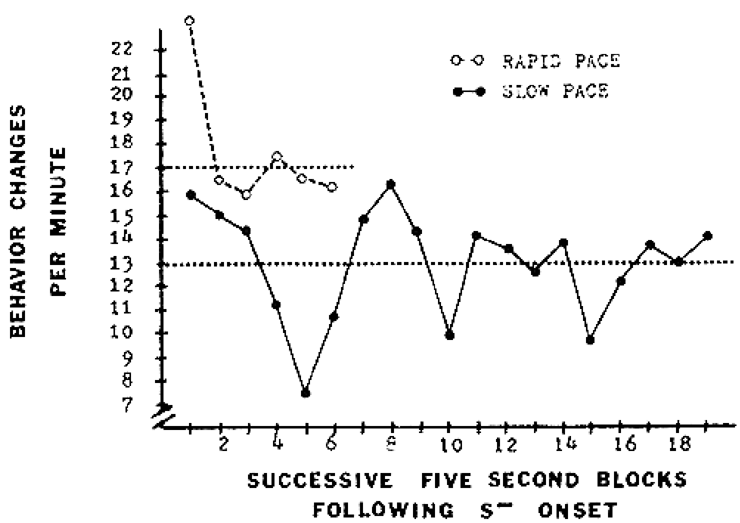


Figure 2. Mean changes in behavioral flow rate during each successive 5-sec block following S- onset and continuing until the mean S+ onset. Data are from the response-contingent (RC) group only and depict changes during the combined final two sessions of rapid S + /S- pace and slow S + /S- pace, respectively (from Ray, Upson, & Henderson, 1977, p. 655).

I continue to wonder how this finding might generalize. For example, would children's hyperactivity (i.e., high rates of change in ongoing behaviors) possibly relate to "setting/task change" scheduling in pre-school and other environments in which applied TEAB researchers often find themselves conducting research or delivering behavioral services? Is there an individually preferred (or, if allowed, self-selected) pace for changing environmentally-defined "behavioral activities" (e.g., shifting from "reading time" to "painting time" to "playground time") that might change our concept of attention deficit disorders? I actually believe there is far more to learn about the operating characteristics of complex streams of interbehavior using operations (time series) analysis *of the entire repertory* observed across multiple settings (cf. Ray, 1983; Upson, Carlson, & Ray, 1981) than perhaps we have found through TEAB's functional analyses—as powerful as functional analysis is. For example, I once reported that killer whales synchronize respiratory behaviors between different individual animals, and that ultradian rhythms might well reflect the coherence of social dynamics and/or loss through death of mates in that species. How humans change rates and/or patterns of interbehavioral activities and settings as an integrated whole during documented bouts of mania or depression in bipolar disorders has, to my knowledge, never undergone a complete time-series investigation, but it certainly seems to be an appropriate topic. Likewise, in a concluding statement in the Ray, Upson, and Henderson (1977) publication we stated:

> Our demonstration that interbehavioral initiation frequencies are independently phased relative to interbehavioral durations has important implications for current behavior theory and research methods. . . .While studies of response fatigue recognize the extremes of these implications, little recognition of the dynamics of normal behavioral durations is to be found in the research literature. We know virtually nothing of the implications of these facts relative to behavior modification when it comes to changing durational parameters. (p. 678)

So perhaps the story of IBSA as a beneficial methodology hasn't been concluded quite yet. There certainly seems to be many contextual dynamics besides simple establishing operations that have been overlooked or simply neglected by TEAB researchers, if not its theorists. Will future TEAB researchers find and attend to such research areas? Time will tell.

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