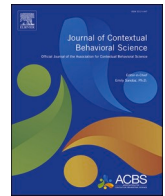ELSEVIER

Since January 2020 Elsevier has created a COVID-19 resource centre with free information in English and Mandarin on the novel coronavirus COVID-19. The COVID-19 resource centre is hosted on Elsevier Connect, the company's public news and information website.

Review Articles

Selection by consequence: A response to Hayes and Fryling (2019)

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ABSTRACT

The paper by Hayes and Fryling (2019) seeks to inform readers that the Kantorian system of Interbehaviorism has been misunderstood and misrepresented by contextual behavior scientists. Furthermore, these authors suggest that much is to be gained by embracing the system developed by Kantor, most importantly that being large scale system building efforts. We disagree with this position, and find the Kantorian system to be of questionable relevancy and at risk of potential extinction within the behavioral community. We also have concerns that perhaps the insights provided by Hayes and Fryling will fail in recruitment of additional members to the Interbehavioral cause. Although the overarching theme of Interbehaviorism is present within emerging dynamical approaches to behavior science, adopting the vernacular of Kantor may be unnecessary to continue his tradition. We recommend allowing empirical selection to run its course in determining the eventual fate of Kantor's Interbehaviorism.

The religious community of the Shakers was founded in the mid 1700s in England and began to assemble in the United States a few decades later. This group had beliefs and practices that even today would be considered admirable, including equality for men and women, pacifism, and advocating for social justice. At the height of the Shaker movement there were over 6000 members living across multiple states and in various sized communities. As of 2017, only two Shakers remain (Blakemore, 2017). Even with a seemingly rational platform which the Shakers rested upon, one rather obscure practice has led to their soon to be extinction. The Shakers did not believe in sex. Rather they practiced celibacy and hoped to gain members through recruitment alone. Perhaps this striking restriction of such a primary reinforcer was enough to ensure cultural practices were not passed on through kinship, or maybe it was the overall emphasis on simplicity of life in a culture that was evolving as a more complex system. Regardless the mechanism, the Shaker movement today essentially is nonexistent, and their offspring are nowhere to be found. A similar empirical selectionism may be facing Kantor's Interbehaviorism, yet for different reasons. Like the Shakers, Kantor's small Interbehavioral community has failed to replicate despite what might appear as an admirable perspective on behavioral science. The underlying theory, though both promising and nuanced, may not be sufficient without empirical replication showing that the various words and symbols in fact lead to socially important behavior change. On the surface, Interbehaviorism appears both rational and progressive much

like the Shakers. However, much like the Shakers, the essence of Interbehaviorism may be still present in contemporary dynamical models of behavior that evolved alongside Kantor's (perhaps, without explicit consideration of Interbehaviorism), representing an extinction of his vernacular more so than an extinction of his ideas. These emerging approaches are developing along with empirical data that aid in their selection by behavior scientists. We contend that we must allow selection to take its course and let empiricism, not discourse, be the decider of what is "true" within a purely scientific account of behavior.

The paper by Hayes and Fryling (2019) is the most recent installation calling for behavior analysts and contextual behavior scientists to more carefully understand the value of Interbehaviorism in modern science of behavior. Kantor's impact has been minimal to date and the authors describe here and elsewhere (e.g., Hayes & Fryling, 2015) that the marginalized respect, relevance, and referencing of Kantor within behavioral psychology comes from a misunderstanding of the Interbehavioral system. The authors are compelling in their portrayal of Kantor's analysis and contextualism, yet history has not been kind to Kantor's legacy. The "misunderstanding argument" has been reported elsewhere (e.g., Hayes & Fryling, 2015), and could be interpreted as an example of a complexity bias in which superiority of scientific theory is somehow better if complex than simple. From an evolutionary perspective, complexity only remains if complexity aids in survival beyond simplicity – and when it comes to scientific theorizing,

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simplicity is everything (i.e., we seek the simplest explanations that fit the data best). The term erisology (Nerst, 2016; Singal, 2019) refers to the quest for understanding why after considerable argument the opposing positions are no closer to a resolution than previously (Lllano, 2019; Singal, 2019). In the context of Kantor versus the rest of behavioral science, what still has yet to be demonstrated from the Kantor's side in our opinion to move closer to resolution, is an objective verification of utility. Is adoption of Interbehaviorism worth the scientific cost of greater model complexity and learning an austere vernacular? Whether that utility is at the pragmatic applied level, or even at the philosophical discourse level, we are concerned that the present narrative on the superiority of Kantor (also see Hayes & Fryling, 2015, 2019) still lacks a persuasive argument for adoption.

An alternative position that we espouse here is that Kantor has laid out a scientific system of psychology that is neither more comprehensive nor more practical than others such as Watson, Skinner, or Pavlov, but rather he provides the reader with an introspective and conceptual narrative on how the psychological world *may* work. As more contemporary models have evolved within behavior science, for potentially this very reason, the overwhelming majority have evolved as empirical extensions of the work of Watson, Skinner, and Pavlov – and not of Kantor (see Smith, 2006 for exceptions). What Kantor did accomplish was the development of a comprehensive field theory that is retained in these more contemporary accounts, but the degree to which contemporary behavior scientists have considered the work of Kantor is questionable. Perhaps the disagreement alone serves a role in science, as it is only through descent and critique that advances in science even occur (i.e., variation preceding selection). Thus, we have no interest in recommending Interbehaviorism be "deleted" or removed from scientific discourse. Empirical selectionism will run its course regardless of our interests or those proposed in the original paper. Instead, our commentary focuses on a discussion of three key challenges that Kantor's Interbehaviorism possesses, and the reasons why they might have not and continue to risk not make a lasting impact within behavior science. Within each, we turn directly to the paper by Hayes and Fryling (2019) and the strengths and limitations found within.

1. Interbehaviorism challenge 1: interbehaviorism is not necessarily novel

Kantor's Interbehavioral Field (IBF) Theory (Delprato & Smith, 2009) represents an extension of field theories put forward by Einstein and Infeld (1938) and later Dewey & Bentley, 1925 to the field of psychology. Rather than analyzing the mechanical events that control behavior, the IBF is made up of interacting components that together produce psychological events. Three major components of the IBF are the contact medium, setting and setting events, and the interbehavioral history. Although these components are described as occurring within the IBF, the IBF cannot be reduced to these individual parts due to complex interactions at lower levels of analysis. This is not unlike the three-body problem facing physicists such as Newton and Laplace and eventually "solved" by Poincaré at the end of the nineteenth century (Chenciner, 2015). The three-body problem occurs when attempting to predict the orbital behavior of three planetary bodies. Although prediction of two bodies can be easily achieved, the addition of the third body produces sufficient complexity at lower levels such that making predictions given knowledge of the component parts of the system becomes impossible. Poincaré proposed, however, that prediction may be forthcoming by treating the movement of the three bodies as one singular higher-order event that paved the way for research on dynamical systems and chaos. Unlike dynamical systems theories that develop quantitative and testable models of the dynamic and chaotic evolution of complex systems, Kantor's Interbehaviorism provided only a narrative description of the IBF. Irreducibility does not and should not equate to non-testability if attempting to remain within scientific discourse (Popper, 1963). To the contrary, because of the chaotic interaction of

events at lower levels of analysis within physics models, analysis or testability of those lower level events is immensely challenging except for within highly controlled contexts where variability is artificially constrained. To achieve testability requires focusing on higher-order patterns of the system, and in so doing, generating testable hypotheses about the evolution of that system.

Irreducibility was already a major aspect of Einstein's theory of relativity when Kantor developed his IBF explanation for psychological events. This approach represented at-best a metaphorical extension of other field theories that were "in the air" at that time. Because of the irreducibility problem, higher-order evolving patterns in systems are examined and used to make predictions, however no clear higher-order patterns are quantitatively described within Kantor's work in such a way as could be empirically tested. Indeed, as noted by Hayes and Fryling (2019), Kantor's account is too complex for such an experimental analysis, but what does that leave in terms of empirical strategy to vet this or that theory scientifically? Other behavior scientists and developmental psychologists, however, took the work being done on dynamical systems and applied it directly to behavioral events in order to deal with system complexity. Thalen (1985) developed several studies to show how infant motor movement develops dynamically, resulting in gross individual differences that are self-organizing and highly susceptible to small differences at initial conditions. Kelso (1995) has since extended this work by looking at the dynamical self-organization of behavioral and neural pattern generation, showing multistability and bifurication. In both lines of research, interdependent mechanisms allow for testable predictions and have been used to describe the considerable individual differences observed across people. In more recent applications of dynamical systems modelling, this can be achieved by using artificial intelligence and running simulations until stable predictions are achieved (Bruzzo & Vimal, 2007). Using Artificial Intelligence in Medical Epidemiology (AIME), researchers were able to predict the outbreak of the COVID-19 virus with up to 88 percent accuracy in Malaysia and Brazil (Allen, 2020, p. 2020, Medical Expo). Undoubtedly, a virus outbreak is maintained and propagated by very many lower-level events that interact in complex ways. And, predicting who will be infected and at what time is likely impossible. However, AIME like other AI systems are designed to detect complex higher-order patterns by making predictions and learning from errors, not unlike operant learning. In so doing, the AI system becomes increasingly more accurate. Self-evolving models such as AIME extend directly from dynamical systems theory (Devaney, 2018). Kantor's work and the emergence of dynamical systems theories were occurring simultaneously, however the testability of dynamical systems compared against an unclear link to empirical testing for Kantor's IBF may explain why behavior scientists have avoided the IBF as a possible explanation for behavior, yet new theories are incorporating the work of dynamical systems modelling.

In our own work, we have examined the apparent self-organization of verbal relational behavior from an RFT perspective consistent with Relational Density Theory (RDT; Belisle & Dixon, 2020, in press). RDT provides quantitative predictions about the self-organization of relational frames, allowing for the model to be refined in light of new empirical evidence. RDT attempts to explain higher order evolving properties of relational networks quantitatively in order to make predictions that can be falsified empirically and ultimately to refine the model. The first prediction extends from Behavioral Momentum Theory (BMT, REF): $\Delta R = -X/Rm$, or, a change in relational behavior is equal to force (a systematic change in the environment) over the mass of the relational class. Relational mass is a combined function of the strength of relations within a class, or relational density (Rp) and the total space or nodes contained within the class, or volume (Rv): $Rm = Rp * Rv$. Belisle and Dixon (2020) provided a first empirical demonstration of relational resistance in a basic experimental arrangement by developing classes that differed in relational density and relational volume, and showed that classes with greater density were more resistant when volume was held constant. In similar study, Belisle and Clayton (under review)

showed the emergence of relational classes in geometric space using a multidimensional scaling procedure modelled by Clayton and Hayes almost twenty years prior. By graphing relations in this way, researchers can see the total space assumed by a class, the strength of individual relations (i.e., distance between stimuli), and the distance between classes which may provide a quantitative description of coherence (i.e., pre-experimental relatedness of two or more classes). Within RDT, relational gravity proposes that classes with greater mass (Rm) and with smaller distances between them (Rd, relational coherence) are more likely to merge, expressed by the equation: $F = (Rm1 * Rm2)/Rd$. Indeed, results showed a successful class merger for a "coherence" group where the targeted merger were classes that were pre-experimentally close compared against a "non-coherence" group where the merger targeted classes that were pre-experimentally distal. We provide RDT not to suggest superiority to Kantor's IBF, as indeed relational behavior comprises only a small part of the totality of the IBF. Rather, RDT was developed from existing, testable ideas such as BMT and dynamical systems theories, and thus it should be concerning that other novel approaches designed to deal with the modern challenges of complexity may remain uninfluenced by Kantor. Yet we must disclose that we have read Kantor's work prior to RDT development, leading to the possibility that maybe at an implicit level, there could have been a slight influence by Kantor, as our history now included this content. Indeed, the first author of the present paper has engaged in considerable discussion of Kantor and was the graduate advisor of the second author, both of whom developed RDT. The degree to which this discussion contributed or was needed for the development of a dynamical model cannot be ascertained nor assumed; however, Kantor's assumptions persist within the surviving vernacular of other scientific approaches (e.g., density, volume, gravity, etc.).

Kantor's Interbehaviorism is often thought as an alternative to the radical behavioral approach. Even Skinner (1988) himself commented *"why have they (interbehaviorists) not built a nest of their own to lay their eggs in?"* We believe that interbehaviorism really should not be considered an entirely unique idea or approach within the broader scientific community, honestly may not have even been that novel within the radical behaviorist community. Although the initial writings of Kantor historically precede those of Skinner, the repeated beckoning for the radical behaviorists to pay attention to Kantor came well after both scholars had become contemporaries. As noted elsewhere by Skinner, "… any unit of operant behavior is to a certain extent artificial. Behavior is the coherent, continuous activity of an integral organism. Although it may be analyzed into parts … we need to recognize its continuous nature in order to solve certain common problems" (Skinner, 1953, p. 116). The seemingly discrete units of the three-term-contingency may therefore be best viewed as a tool to isolate specific mechanisms for targeted intervention. By doing so, the behavior scientist can make testable predictions about how manipulation of any single part of the complex and continuous environmental event will influence behavior. Perhaps this is why Hayes and Fryling (2019) claim that the methods used for experimental work by the Interbehaviorist are the same ones used by the radical behaviorist. If so, then what is the difference? If nothing, then what is the utility of adopting the more complex model and vernacular with a potential loss in empirical testability? As we describe below, the lack of testability in Kantor's Interbehavioral approach creates the real risk of problems within a scientific account of human behavior.

Specific Reference to Stimulus Function in Hayes and Fryling. In their current paper the authors make the claim that perhaps Kantor's most important contribution to the field of behavioral psychology is that of the notion of stimulus function. Kantor discusses, and these authors recollect, how stimulus objects as physical things may acquire interbehavioral functions given interaction between the organism and these objects. Once such interaction has occurred, the objects themselves become more complex, as they now possess additional psychological functions. Take for example a rock that is initially nothing more than a stimulus object in the world. Upon approaching by a person, the

interbehavioral event between the rock and the organism occurs, which renders the rock as now containing stimulus function. In this case the rock was needed to keep a breeze from blowing closed an office door. This rock, is now functionally altering the world, and may be thought of possessing a stimulus function. With additional interaction, this same person using the rock to block the door closure may engage in additional interbehavior with the rock, as it is grey like their father, who was considered the "rock" of the family. Now, upon glancing down to see the rock, memories of childhood start to occur. While a provocative interpretation, the same sequence of events can be explained through a Skinnerian system using stimulus discrimination and perceptual behavior (seeing in the absence of the thing seen), or placed into layman terms to aid in ease of discourse. In the latter dialogue, the rock reminds them of their father because it is grey and strong, just like their dad. Parsimony of explanation is perhaps not a universally embraced value within philosophy, yet when seeking explanations that can eventually be put to the empirical test, parsimony tends to matter.

Arguably, "stimulus function" was the point of emphasis of work based on respondent conditioning of Pavlov and Watson. Generally, "functional" approaches in science describe how one event causes (elicits or evokes) another event. Within the basic respondent model, a conditioned or unconditioned stimulus elicits a conditioned or unconditioned response, and as noted by Watson (1924), higher-order conditioning across the lifespan of an individual is likely immensely complex. Indeed, the "Activity Stream" proposed by Watson highly resembled an evolving dynamical system. After conception, conditioned emotions and behaviors are developing and play a considerable role in the development of future conditioned emotions and behaviors. For example, a mother that provides nutrients and warmth becomes a conditioned stimulus eliciting appetitive emotional affect in the baby. Should this occur very early in life (a bifurcation within the complex system), stimuli subsequently paired with the mother are more likely to obtain those same eliciting functions. If, however, nutrients and warmth are not provided early, those same functions may fail to transfer leading to complications later. Small changes at initial conditions influence the evolution of the system considerably, just like in a dynamical system. This model was entirely based on the eliciting effects of stimulus function.

Skinner extended this account primarily by introducing the idea of the operant and therefore behavior function - focusing on changes in the environment produced by behavior. However, a critical aspect of this theory is that reinforcement alters the evocative potential of stimuli in the environment - reinforcement alters stimulus function. At the most basic level is the three-term contingency, however Skinner too discussed the potential complexity and interaction of many environmental and behavioral events that contribute to stimulus control. For example, when examining private verbal behavior, the speaker and listener are contained within the same skin, where response evoking stimuli are produced by the person themselves (Skinner, 1957). Recombination also provided a rudimentary example of relational framing, describing how patterns of verbal behavior may combine to produce novel verbal utterances without direct reinforcement. The ideas at the roots of relational framing was mentioned by Skinner within Verbal Behavior (1957) under the auspice of association, and this model of responding to stimulus functions and not just objects was eventually matured upon by Relational Frame Theory with immense success (Hayes, Barnes-Holmes, & Roche, 2001). We do not contend that the idea of "stimulus function" is not immensely important within our field; however, others were developing this idea in absence of the Interbehavioral version. Maybe it is because these other approaches to stimulus function were scientifically testable, that they led to relatively more robust lines of empirical research and have since been adopted by behavior analysts (i.e., empirical selection). Yet it is also possible that any of these above researchers were in fact somewhat influenced by Kantor's work as we were (see Hayes, Barnes-Holmes, and Roche, 2001 as an example). Even Skinner himself interacted with Kantor during their concurrent faculty

appointments at Indiana University, thus it would be a fair assumption that some interaction between the two yielded at least a "participating factor" in Skinner's entirety of context from which he wrote subsequent work. Still, we do not know the contribution of this interaction and it may not matter over 50 years after this mutual appointment.

2. Interbehaviorism challenge 2: interbehaviorism is not a scientific approach and therefore cannot be empirically "selected"

Hayes & Fryling, 2019 suggest on at least two occasions that Kantor's work is consistent with a natural science approach: "*Consistent throughout all of this work was Kantor's thorough commitment to the development of natural sciences, and to the removal of non-scientific sources of influence throughout the entire scientific enterprise (e.g., especially dualistic sources of influence)*" (p. X); and "*a natural science of behavior, the most fundamental distinction worth considering for this purpose pertains to the admission or exclusion of references to super-natural entities and powers in such speculations (i.e., the distinction between dualistic and natural science perspectives)*" (p. X). We agree that the inclusion of only natural (i.e., physical) phenomena is a necessary assumption of a natural science account as is consistent with scientific physicalism (see Belisle, 2020), in which imaginary causalities are unnecessary. However, this assumption is also true for a natural philosophy of behavior. One must then distinguish between a philosophical approach to knowledge creation and a scientific approach. The former (philosophy) is rooted in rationalism, where the soundness of a conclusion or idea is founded on the seeming coherence (validity) between true premises. Indeed, Interbehaviorism provides an elegant set of coherent claims that, when taken together, appear to be descriptive of very many psychological events that behavior analysts are interested in understanding (or, more importantly, influencing). Rationalism is limited and the practice of philosophy remains quite limited. Science generally and psychology more specifically require some semblance of a threshold to surpass. Even a layman's definition of "science" as noted in the Merriam-Webster dictionary states "*knowledge about the natural world that is based on facts learned through experiments and observations*" (Staff, 2004). Specifically, science is rooted in empiricism as it exists in opposition to rationalism, or knowledge creation through systematic observation and experimentation. The National Science Foundation, an independent federal agency, mission is "*to promote progress of science, to advance the national health, prosperity, and welfare …*" (NSF, 2020). The National Institute of Health, one of the largest funding mechanisms for pursuing research funding within the United States notes its mission statement is to "*seek fundamental knowledge about the nature and behavior of living systems and the application of that knowledge to enhance health, lengthen life, and reduce illness and disability*" (NIH, 2020). The discipline of psychology is defined as "*the scientific study of the human mind and its functions, especially those affecting behavior in a given context.*" (Staff, 2014). Regardless of which definition of science one embraces a general theme is echoed – engaging in action to advance understanding. And with respect to psychology, if resting upon a scientific system as related to human beings, it appears that at a bare minimum the discipline requires action on behalf of a scientist. While it has been previously argued that Kantor's system could be a catalyst for engaging in action (Hayes & Fryling, 2019), the interbehavioral system itself appears not. We believe that if we use data as a metric of such a catalyst, the data are essentially nonexistent, albeit excluding any data obtained via unmeasurable influence.

Even those behavioral scientists well versed in Kantor's system have not utilized the uniqueness of his work to more effectively explain the scientific psychological discoveries that have been made without reference to Kantor, nor have they discovered new psychological phenomena as a derivative from Kantor's system. Rather we continue to be left in that same position as noted by Nerst – no closer to agreement than before. Perhaps Kantor's work fits better within the domain of philosophy than science, as this is specifically noted by Hayes and Fryling

(2019). However these authors use the terms "Interbehaviorism" and "interbehavioral psychology", which we would assume at least the latter describes the attempted engagement in the practice of science. The dichotomy between the two disciplines has been noted by Snow (1963) as two distinct forces competing for impact within a scientific revolution. The Hayes and Fryling paper, and its initial bone of contention (Hayes, Barnes-Holmes, & Roche, 2001) mirror this battle as if one is trying to claim superiority over the other. The solution given by Hayes and Fryling is for the functional contextualists to develop larger scale systems, as if somehow doing so will lead them closer to the totality of an organized analysis as purported by Kantor. However, we believe such a drift towards a conceptual lamenting at the expense of "*learning through observation and experimentation*" would be a step backwards on the pragmatic world view of which functional contextualism, or any contextualism rests upon. Again, regardless of our beliefs, scientists are already making their selections and those models are largely absent of Kantor's vernacular and assumptions.

Behavior analysts are not at fault for not understanding Kantor's work. The present paper frequently appeals to the behavioral community that has failed to decipher the true value of Kantor. The authors trace the inaccuracies back to the original writings of Hayes and colleagues in 1993 through what they consider the biggest misrepresentation of Kantor, being in Hayes, Barnes-Holmes, and Roche in 2001. The one discussion this paper and others by L. Hayes (e.g., Hayes & Fredricks, 1999), do however fail to provide a conceptual scientific explanation as to why most of the field has failed to understand Kantor's work. Are readers under too extensive response effort to comprehend the analysis, are scientists choosing the smaller reinforcers of experimental control over the lager reinforcers of whole system discovery, or is stimulus control poorly evoked in the writings of Kantor? We have suggested above the increasing the complexity of a model and introducing an austere vernacular comes at a cost to the scientist – greater complexity will be "selected in" only if the cost is worth the pay-off (i.e., utility in solving new problems). Technically speaking, when placed on a concurrent schedule of reinforcement when both alternatives (radical vs interbehavioral) result in the same outcome, responses will tend to be biased to the schedule which requires less responses requirements in order to obtain the consequence. Within empirical selectionism, the onus is on Interbehavioral researchers to show this greater utility. That is, to solve applied problems not solvable using radical behavioral models. This work is not cited by Hayes and Fryling or, to our knowledge, elsewhere. Without this work, empirical selection cannot occur because there is nothing to select. Again the fact that Hayes and Fryling's own admission that the interbehaviorist still utilizes the same methods for experimentation as the more common behaviorist, directly makes the point of the response effort of the interbehavioral system may be set aside whenever science actually has to be engaged in. It is the lack of utility on the applied end of this spectrum which has potentially limited the amount of introspection on the conceptual end that most behavioral scientists have found themselves engaging in. The authors attempt to define Kantor as a pure philosopher, when his writings then are to be embraced by scientists and technicians, appears to contain the very dualistic characteristics they wish to divorce from by espousing a purely naturalistic account.

One potential criticism of our paper here is that we too have failed to truly understand or grasp the nuances and complexity of Kantor's account. Indeed, this would mirror similar criticisms levied against the behavior analytic community in general. If it is the case that *we* have failed to grasp the nuance and complexity of Kantor's account, this may indicate the very problem at the root of Interbehaviorism. Within a pragmatic science, the ultimate truth criterion is whether a theory or model allows for greater successful working by the scientist (Ivancic & Belisle, 2019), and perhaps we have failed this test of engaging in workability. If the behavior scientist cannot adequately understand or grasp the model following reading, writing, and attempting interpretation of the work, the model may lack sufficient parsimony and elegance

to allow for utility. Therefore, we propose that the burden of understanding the "complex" model proposed by Kantor should not be placed on the misunderstanding or underachievement of behavior analysts, but rather should be a matter of concern for Kantor and his contemporaries that improved parsimony and utility might need to be improved upon in order for Interbehaviorism to be translated into to solve real-world challenges.

3. Interbehaviorism challenge 3: extinction May be the road to survival of Kantor's ideas

The term "contextualism" defined by Pepper (1942) is one of four world hypotheses or categorizations as to how schools of thought within philosophy are divided. Very much like Kantor, Pepper's work was of questionable impact within his home discipline of contemporary philosophy, and the attention given from behavior analysts tends to focus on two world hypotheses – mechanism and contextualism. The former hypothesis explains the world as an intricate series of moving parts, when assembled together and with the knowledge of how the collective works, can lead to knowledge about the world. The latter hypothesis emphasizes a broader interactive relationship amongst elements of a context rather than individual pieces which make up the context. Prior writings have often placed radical behaviorism within the mechanism, and post-skinnerian work such as contextual behavior science within contextualism (Hayes et al., 1988) although some debate about this division has occurred (Leigland, 1993). The papers by S.C. Hayes et al. and L. Hayes and Fryling (2019) split hairs even further as to where exactly to place Kantor's interbehavioral system. We agree with Hayes and Fryling (2019) that Kantor should not be typecast as a descriptive contextualist, and furthermore we believe that any behavioral science should not attempt characterization within the Pepper categories of world views, as this book poorly connects with the historical or contemporary discipline of philosophy. Rather we believe that both S. C. Hayes and L. Hayes and Fryling (2019) should attempt to align behavioral science with the well-established conceptual system of pragmatism (James & Burkhardt, 1975). Doing so we believe will pragmatically position our discipline within broader circles of intellectual discourse, and concurrently focus all towards a more unified goal of science, that being pragmatic utility.

Pragmatism extends well beyond behavior science but, as noted by Ivancic and Belisle (2019), and is uniquely situated within a radical behavioral approach to understanding the behavior of behavior scientists. A philosophy, theory, or model are verbal behaviors engaged in by scientists to solve real-world challenges. This is echoed within Model Dependent Realism in that there is no absolute truth (Hawking, 2010), rather there are approaches that work to solve problems and approaches that work better to solve the same problems (Belisle, 2020). Interestingly, as noted by Hayes and Fryling (2019), "Also included [in Kantor's account] are propositions pertaining to the fact that scientific enterprises, as aspects of larger cultural complexes, are evolutional. This is to say scientific domains are cumulative and corrigible." (p. X). Kantor appreciated the evolutional of the verbal behavior of scientists, where philosophies, theories, and models build on prior work. However, like with natural selection in the evolution of species, it is not simply that changes build on prior changes; rather, genetic mutations that encourage the survival potential (and, therefore, reproducibility potential) of the species retain and non-adaptive mutations are selected out. Skinner extended this account at the level of the behavior of single organisms, whereby behaviors that contact reinforcement are selected for, and those that fail to produce desired consequences are selected out. A selectionist account of scientific theory or model development may be best provided by Kuhn (1962) in *The Structure of Scientific Revolutions*. New theories or models are proposed when anomalies or challenges arise that current models cannot account for or solve. Should new approaches prove more successful than old ones, those approaches are more likely to be selected for. That is, scientists are more likely to adopt

new strategies because those strategies allow the scientist to solve new problems. This can operate at the level of the behavior of a single scientist or operate at a cultural level as more traditional scientists retire or die and new scientists who are more likely to adopt newer material enter scientific disciplines. In either case, those theories or models that work (i.e., successful working) are retained and those that work less well or not at all are selected out. That is, selection by consequence.

This account is entirely within a pragmatic approach to science and is consistent with Skinner's radical behaviorism (Ivancic & Belisle, 2019). The ultimate test of this or that model is whether the model has been used to solve new challenges – does it survive the test of time, or does it die in favor of new models? Selectionism is a cruel arbitrator over disagreements, and in the case of Kantor's Interbehaviorism, time has not been favorable. However, more contemporary models that incorporate dynamical systems such as developed in the work of Thelen and Kelso, retain much of the essence of Kantor's ideas. These approaches make no mention of the contact medium, setting and setting events, and the interbehavioral history. They do mention bifurication, self-organization, and susceptibility to initial conditions. Discussion of Kantor's work may have participated in our synthesis of RFT and dynamical systems within RDT, perhaps implicitly. The specifics of Interbehavioral system appear to be increasingly selected out of the behavioral account, but the general message retains within more complex models that extend empirically from the work of the likes of Watson, Skinner, Pavlov, and others. Functional contextualist models provide an exemplar of this empirical progression (RFT as a basic model and ACT as an applied treatment model, see Belisle, 2020) as an opposite case example. Even though potentially faulty rules adopted by many behavior analysts led to initial hesitation or outright refusal to allow RFT and ACT models into the field, other fields working to improve the human condition (clinical psychology, social work, education) quickly adopted these models with great success. Empirical demonstrations of stimulus relations that supported the ideas of RFT grew at an exponential rate in the 2000s (O'Connor et al., 2017) eclipsing Skinner's verbal behavior approach, and ACT is being touted as a transdiagnostic solution to many psychological or behavioral disorders. And, despite initial hesitation, as noted by Belisle et al. (2020), research utilizing RFT models with children has grown exponentially within major applied behavior analytic journals, including children with and without disabilities. This work incorporates dynamics and successfully moves from theory to impact. Can better outcomes be achieved using Interbehavioral models? Time will tell and selection will be the arbiter of the end of this story. We contend that limited empirical work completed so far appears to favor extinction as the eventual outcome. Just as the Shakers needed sex to reproduce, so too do theories need empirical data to ensure generational survival.

Successful working has not yet achieved within Interbehaviorism. The original article proposes, "scientific domains are completely free from absolutes, ultimates, and universals" (Hayes & Fryling, 2019). Is this statement itself an absolute? Irony aside, science is first and foremost predicated on specific assumptions that are held absolutely. For example, in a physical or natural science, physicalism operates as a base assumption. That is, only physical events can cause other physical events. Pavlov, Watson, Skinner, Kantor, and contemporaries represent an extension of scientific physicalism within the context of developing a natural science of human behavior. At a basic level, theories or models that violate the assumptions of physicalism can and should be dismissed from the outset, which runs in contradiction to more dualistic or mentalistic accounts more pervasive in other branches of psychology. Pragmatism is another assumption that, though not held by all scientists, is an assumption held up by those self-proclaimed as functional contextualists or as pragmatists more generally. This assumption extends upon physicalism so that not all physically adherent theories are held equal simply because they exclude non-physical explanations. Being a "natural" science or philosophy of behavior is insufficient. The theory or model has to actually work to solve problems, and new theories must demonstrably work better than older more established theories to be

adopted by the larger scientific and applied community. Kuhn, in Scientific Revolutions (1962), extends this idea by proposing that the adoption of more successful theories or models is not an intentional choice of scientists, rather consumers of science will select technologies produced from the more successful models. Over time, the successful scientist will gravitate towards approaches that allow him or her to work more successfully within their world and eventually to abandon old models in favor of newer models. If we accept pragmatism as our second absolute assumption, the non-adoption of Kantor's models might been seen as proof that these models are not superior to anything that existed prior or that has been since developed.

Kantor did not invent the "bias" of the observer in the observed. However Hayes and Fryling express this point as a core feature of the interbehavioral system that the rest of science fails to detect. "*Moreover, while it is the case that Interbehaviorists would agree that no one escapes the effects of his or her personal and cultural histories, this does not mean that scientific knowledge is personal and ephemeral.*" Pp. xx, "*The empirical methods employed by interbehaviorists are the same as those used in the sciences more generally Pp. xx,*" and "*The practices of the investigative subdomain of behavior science, known as the experimental analysis of behavior, are among the empirical methods employed by Interbehavioral Psychologists …*". *Pp XX.* We assume the authors are referring here to the methods used by other radical behavior analysts, and if true, it appears that the interbehaviorist needs to shape shift into radical behaviorist in order to accomplish verification of phenomena. We believe statements such as these, and the conclusions that result indicate a concerning shortcoming of Interbehaviorism.

Summary

We applaud Hayes and Fryling (2019) for their recent well-written and convincing contribution to scientific discourse around the role of interbehaviorism for the behavior scientist. But what is interbehaviorism? We believe that it is an unprovable and untestable, narrative about how the psychological world might work. And when if someone tries to test interbehaviorism, they destroy its assumption of disrupting a field of interaction. We believe that you can't have it both ways - say you are a science, but not be placed under the truth criterion of science. And you can't say you are a philosophy of science but say nothing about philosophy. What is left is a body of work that has inspired some to place elements of the system into the account of human behavior (field theory), but the totality that Kantor hoped for has never been actualized, and we have concerns that it never will. A 100 plus years have passed since Kantor's original writings, and science has for better or worse, moved on. Like Brother Arnold and Sister June, the last two remaining Shakers (Blakemore, 2017), eventually even the most inspirational movements that fail to gain adopters will come to an end. Only time will tell as to if Kantor's contributions will eventually follow the fate of the Shakers, but as we look at the general lack of data before us, the odds are pretty high that they will.

Declaration of competing interest

Both authors declare they have no conflicts of interest.

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