Killeen and Jacobs (2016) Are Not Wrong

Michael Davison¹

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Of course I like the Killeen and Jacobs (2016) paper! I like it because it underlines and reinforces what we have all known forever, that behavior is controlled by organismic dispositions as well as environmental dispositions, and that “reinforcement” is the local jigsaw fit of these two sets of dispositions. Organismic disposition affects perception (mirage) and sensation (via behavior) and instrumental behavior and consuming behavior—and all these, of course, affect organismic disposition, and each other.

Having said this, the catchy title does not withstand any deep analysis, and any decent behavior analyst would want to rephrase this as “Snow is called white, coal is called black, and food is called a reinforcer”, etc., these being defined by our current verbal community—which is, as Killeen and Jacobs (2016) say, what you can do with them (emit verbal behavior). The title is a red herring, because I doubt anyone has said, or would want to say, that a *primary* quality of food is reinforcingness. Nothing much follows from this, and snow remains white, coal black, and food a reinforcer because it is their secondary qualities (what you can do with them) that are important – as Killeen and Jacobs remind us at length. The important aspect of this paper is to reassert that it is organismic disposition that makes food act as a reinforcer, in exactly the same way as dispositions and contexts will determine the apparent color of snow and coal. So, if you like the idea of “reinforcement” (I do not), there is no reason not to continue to use the term “reinforcer” as a shorthand for the juxtaposition of “a behavior, an appropriate food and a hungry organism,” with maybe a few other caveats. A more accurate statement is, “Coal is black, snow is white, food is a reinforcer, and all are true when other conditions are right” – but this does not make for an attention-getting title. “Reinforcer” suffices for much of what we (experimental, applied, practical behavior analysts) do, in which many critical conditions are assumed and not explicitly reported. It gets the point across. Killeen and Jacobs’ organismic state-affordance

✉ Michael Davison
m.davison@auckland.ac.nz

¹ School of Psychology, The University of Auckland, Private Bag 92019, Auckland 1142, New Zealand

concordance ideas, rather than the 3-term contingency, is a step in the right direction for the development of a science of behavior. But is it a giant step?

The aspect that I like less is that the conceptualization that Killeen and Jacobs (2016) are offering, despite nomenclature changes that do help understanding, remains a “reinforcement” model—the RC-OC concordance is the glue that holds it all together, and stamps it in. Do we really need a model in which learning, writ large, is stamped in by a local O-E dispositional concordance? I think not.

In its reconsideration of organismic state, Killeen and Jacobs, (2016) approach seems to be saying much the same as Cowie and Davison (2016), though the terms are somewhat different—we focused on what an organism will do in the future, rather than what it was doing right now. We suggested that stimulus input from *all* sources, be they interoceptive, proprioceptive, exteroceptive changed the state of the brain so as to predict what proprioceptive stimulation would move the organism closer to an input state that solved a conditional organismic need or Phylogenetically Important Event (PIE, Baum, 2012). In this very dynamic way, animals move through time and space and arrive at destinations (which Killeen and Jacobs called conditional affordances, which currently have value). I guess that Killeen and Jacobs, nominally, want to call the concatenation of phylogenetic, ontogenetic, and current internal inputs, “State”, and there must be a similar environmental state which represents everything since the Big Bang to now (think gravity through to the party going on next door). This sounds to me like Baum’s (1973) E-Rules and O-Rules¹ though, but because of the changing nature of both myself and my environment, “rule” might have connotations that are too fixed for my purpose—so an O-Rule for me now (but not for you now) might be, *I’ve learned that shouting at the rowdy people next door only causes trouble*. Rules are meant to be broken. The nice thing is that fundamental environmental and organismic dispositions continue unaffected by any behavior organisms emit (the laws of Physics, Chemistry, and hence Physiology). But local rules or local contingencies are mutable over time and are influenced by our own behavior and the behavior of other organisms. It is the local contingencies that are dynamical and complex, and can only be very poorly predicted. In none of the fundamental sciences can we predict the movement of a leaf on a rose bush, the avalanche down a hillside, or what I’m going to do next. It is the set of local contingencies, in conjunction with the fundamental laws, that we need in order to *do* something about the behavior of an environment or an organism. This is where we need the sheaves of equivalent organismic states, which will provide us with a statistical, probabilistic, sets of possible outcomes from sets of possible interventions, given that we also have similar sheaves for organisms (including the experimenter) that may interact over time with the organism in current focus. This approach certainly can, and does, work in a rough-and-ready way, but to me it seems that the level of analysis is rather too coarse for a science of behavior.

The stimulus-input approach that Cowie and Davison (2016) proposed can be exemplified by a consideration of what controls “organismic state.” For example, at first blush, an organismic state of, say, hunger might be reasonably predictable from a recent history of abstinence, but this history is summarized at least in part by the current state of the gut – and the gut is, of course, an external input to the organism (it’s just a

¹ Baum’s explanation: “E-rules... determine feedback to the organism” and “O-rules... determine output from the organism” (p. 138).

tube of skin through the organism). The input to the organism provided as stimuli by the alimentary flora and fauna, and from constriction of the stomach, is no different from a bunch of photons or a touch or an insult to the skin. But the organismic state of “hunger” can also be changed by an exteroceptive stimulus (bacon cooking!). Gentling along this track, thirst is sensed in a similar way, but from the internal environment of the bloodstream. What I am suggesting is that all such sensations as inputs provide controlling percepts, and hunger is a potential controlling stimulus just like any other impinging stimulus. Indeed, Temple (1973, Experiment 4), in his PhD thesis, did an explicit experiment looking at stimulus generalization gradients across pre-feeding amount, which provided quite reliable gradients. Well, we thought they did, but our journal of choice didn’t, so this research remains unpublished. I’m suggesting that all organismic dispositions, from hunger to road rage, are simply stimulus inputs of one sort or another. To what, I hear you cry: Presumably to the brain.

Nominalism

Being nominally aphasic, I have always had trouble with categorization and nominalism, preferring to eschew these for continuity and change. I don’t like to define a change between categories by a particular point of difference in amount or location. A stimulus is a discriminative stimulus only if it signals that current conditions differ from conditions in the past or the future, otherwise it is just an irrelevant stimulus. And all sorts of sensed stimuli, be they exteroceptive, proprioceptive, or interoceptive, can act as discriminative stimuli. I recognize the need to cast the discussion in the terms used by Killeen and Jacobs (discriminative stimulus, State, Affordance, and so on) at least initially—it is the language of our science—but we must recognize that a state is only an operational state to the extent that it signals something. As Killeen and Jacobs say, though, such nominal symbols as O (organism), S_d (discriminative stimulus), R_i (behavior), and R_c (consequence) in their extended 4-term contingency do a helluva lot of work, and make the constituents dark, and I think they also bleed into each other. Killeen and Jacobs do shine further appropriate light on these with their vector-algebra approach in the Appendix to their paper, and show how each of these is co-dependent with all the other terms—thus does their paper lead the reader through from a simple, easily understood 3-term contingency to a totally interactive 4-term contingency (their Equation 5). They state “An algebra such as this tracks behavior as it moves through space and time...” and, in a sense it does, but I don’t think that they sufficiently bring out the dynamical nature of the interactions (perhaps transactions is a better term). There is an infinitesimal calculus going on here in which, potentially, very small changes any of the terms to the right of Equation 5 changes everything now and in the future. It’s a complex dynamical system that we are dealing with (Davison, 1998), and every infinitesimal R_i modifies all terms on the right of Equation 5 – it changes exteroceptive stimulation S_d and potentially R_c , so the overall trajectory $\mathbf{B}$ will or may change. Equation 5 accommodates this and, in a sort of static sense, provides some appropriate general predictions. Limitations and sensitivities of the transactions are supplied by psychophysics, physiology, and ethology.

But why four terms? Is each of these a discrete thing? Why divide up the input to the system into these nominal categories? For many years (starting with Davison & Tustin,

1978) I have been trying to develop ways of dealing with reinforcers and discriminative stimuli in the same terms, and we have recently attempted to bring elapsed time and location into this grouping (Cowie, Davison, & Elliffe, 2014). I see no reason not to deal with all organismic input—all of the O-related categories—in the same terms. All of these (both dimensionally and temporally) are continuous stimulus inputs and are dealt with in parallel to determine **B**, the behavioral trajectory. The Killeen and Jacobs approach seems to give organismic state O special status in determining the behavioral trajectory (Appendix Equations 3 to 5), which to me seems unwarranted—nothing in these terms is a more special category than any other. Nominal states, like a pain in the arse² and a rumbling in the gut, are just inputs to the general system that keeps us alive and kicking. The system allows us to go beyond an almost mechanically labile responding to each whisp and whim of stimulation, and allows us to learn what goes with, and leads to, what in the environment and ourselves and allows us to set out on a path that will, with patience and luck, transiently terminate in a particular combination of inputs. Instrumental responses have stimulus properties; consuming responses have stimulus properties; drugs in the bloodstream have stimulus properties; the knife in my back has stimulus properties; none of these are different in kind from common or garden exteroceptive stimuli – all are input through sensors, and we will only get into trouble if we divide these up into this, that, and the other, category. Arguably, we got ourselves into this trouble many years ago, and a lot of the toing and froing over the years has been about how properly to categorize various different events as reinforcers or not-reinforcers, and as reinforcers or stimuli.

Further, latent and incidental learning (e.g., Tolman, Ritchie, & Kalish, 1946) occurs without a motivation operation or a consummation operation (as discussed *inter alia* by Cowie & Davison, 2016), implying that the system naturally collects potential affordances and the instrumental behavior—stimulus conditions—that lead to and from these. The system collects fuzzy maps of conditional transitional probabilities among stimuli so that behavior in space-time can be planned towards a transient end – be it the way to the local sushi shop or to where the dog may have had an accident in the house. Knowledge, and its use in planning, arises from these conditional stimulus transitions without the need for “reinforcement”. This is not a mental map nor a cognitive map; nor are Google maps, which are conditional transitions stored *in machina*—as in animals, too. This is predictive processing (Clark, 2016)—learned conditional stimulus relations guide behavior, and errors in prediction change guidance. Could it be that learning proceeds without the need of reinforcers and reinforcement and potential affordances, and simply consists of stored conditional stimulus relations—but that what and when behavior occurs depends critically on known affordances and current Os, with consummation as the result—all, I stress, as stimulus inputs? This view provides a sort of Nekar cube of the current view, which stresses the importance of reinforcement for learning and the modification of ongoing behavior, but not for unmodified ongoing behavior, which appears to “just occur”.

I am seeing ongoing conditional stimulus change as the warp and weft of psychological spacetime. I am not wanting to suggest that affordances distort this spacetime to provide gradients that attract animals, because this simply would not accommodate planned behavior trajectories such as the umweg and commitment and the use of new

² “Ass” to some readers.

materials for tools (Auersperg, Borasinski, Laumer, & Kacelnik, 2016) and prospecting for an egg-laying site (Scardamaglia, Fiorini, Kacelnik, & Reboresda, 2017). Rather, like simply looking where we are going, we also look forward in time, and are guided through spacetime by what we have learned about all current conditional relations between stimuli. What the current organismic state, and indeed *any* current input, does is to modify the current and future conditionals so as to produce a possible trajectory. Gravitational attraction caused by the mass of affordances gives behavior change that is too linear—unless interim states have conditional value that bend the trajectory. But that returns us to conditional reinforcement, and stimuli gaining value by association with (perhaps delayed) reinforcers. Of course, conditional reinforcement has been a great magical help to behavior analysis, providing an aether to transmit primary reinforcers to behavior.

To my mind, behavior understood under this proposed prospective system is properly molar and extended. In Killeen and Jacobs' terms, an $[O, S_d]$ now modifies an animal's journey through space and time (R_i) that will likely be accompanied by $[R_c]$ at some point. The more often this journey occurs, whether followed by $[O, R_c]$ or not, the longer and stronger the conditional string (perhaps better, skein) becomes, and the behavior becomes habitual and starts to have the properties of a fixed-action pattern or a reaction chain. It allows an animal to choose now between an R_i that results in a sooner small amount of R_c , or an R_i that results in a later larger R_c ; and to choose whether to take the less valuable R_c or have a later choice between the less valuable and greater value R_c .

Is the approach that I am describing Tolmanian (Tolman, 1951)? I admit that it sounds like it at first blush, especially the notion that animals carry with them a learned spatio-temporal map, and have this map (but not a representation in the usual sense) available. But I do not wish to use processes such as purpose, expectation, belief and spatial *representation* as explanatory processes. Rather, I see these (specifically, the behaviors that control such pieces of verbal behavior) as something in need of explanation, and which may be explicable by the approach taken here. I also do not think it would be useful to categorize this, or any approach, or indeed anything, on the basis of a small number of similarities, and thus have it rejected out-of-hand on the basis of all existing arguments against that category.

State

I'm having a little trouble with Killeen and Jacobs' sheaves of O. Firstly, I guess, this is O_{now} state because for them it summates history and phylogeny and current input from within the skin. It is, as Killeen and Jacobs say, unknowable in detail, but they suggest it may be roughly classified into sheaves – and the way this might be done should be of interest to behavior analysts. As we have argued (Davison, Sheldon, & Lobb, 1980), the usual way of measuring current disposition in humans is the *personality test*, which is something quite universally reviled by behavior analysts. But it does provide some measure of current disposition – even, arguably, the Rorschach can do this, answering “which behaviors are currently higher probability”. O_{now} is an actor in behavior that is very brief, very temporally local, and

manipulable—but I can report the behaviors and situations that led me to this state. And here is the nub of the question: Animals remember—are controlled by—previous events, be they behaviors, environmental stimuli, and presumably previous proprioceptive and interoceptive stimuli. The undoubted fact that behavior can be controlled by the past as well as the present surely means that O_{now} contains within it recent O states (the context of O_{now}), and I would make the same argument for all current stimuli – all occur in the context of previous stimulation, and so the meaning of current stimuli is indeed time-contextual and conditional. Because of this, stimulus conditions now have a temporal extent from the past, through now, into the future. Cowie and Davison (2016) suggested that organism learn such conditionals without recourse to reinforcement, simply as a natural process. As Clark (2016) suggested, when such conditionals are available, the system predictively processes, matching prediction with current input, and working to minimize the error between the two. The animal moves forward in time and space, following what it knows until it falls into error, at which point it has available other tactics to move on. The learning of stimulus conditionals allows animals to “know” where goods are even when O says it’s not important now, and no R_i or R_c occurs. That’s a map. Learning by observing seems odd and difficult for BAs without allowing reinforcement-free learning of conditional-stimulus transitions.

Equivalent state, while perhaps a categorical hindrance to the science of behavior, remains useful and important to the practical application of the science, to the technology. In the same way as the chemical makeup of a piece of structural steel, the state of the steel, is essential to know when constructing a bridge to withstand an earthquake, the equivalent state of the organism is important to know when trying to determine how a particular intervention will change the behavior of this organism. We do tend, I think, to conflate the fundamental science with the applied science—I’d own that fundamental science does need to be simplified and categorized if it is to be applied, because these are two very different levels of analysis—yet they feed off each other. The structural failure of a bridge could well require the fundamental science at a lower level to be reinvestigated. Killeen and Jacobs’ paper is perhaps focused more on the technology of behavior analysis than on the fundamental science, though their Appendix certainly moves in the direction of fundamental science.

Under the approach that I have outlined, not a lot changes in the application of our proposed approach to Applied Behavior Analysis practice. To change a child’s behavior, we simultaneously and sequentially arrange some setting and some progressively modifiable contingency and some organismic state and some consummation, and the child learns the conditional stimulus conditions that is accompanied by state-appropriate consummations. But, assuming some “theory of mind,” maybe we can let other children watch the process, or the terminal part of the process, and they will show substantial savings on their own learning. We learn good skills and good social behaviors (and bad skills and bad social behaviors) by watching and reading and listening. These don’t have to be explicitly shaped, they are learned from parents and peers and videos and so on.³

³ I just wish that Hollywood and its ilk did not keep showing us that extreme violence wards off all Armageddons and that sexual violence was the way to another person’s heart.

Conclusion

I think the conclusion that I am coming to is this: For ABA research and practice, and for common parlance, nominal stimulus and setting and behavior class and functionally determined reinforcement suffice, at least with more focus on organismic disposition than we have had. This traditional approach works and is widely understandable (it controls the right sorts of behaviors). The current toolbox of technology may be incomplete, and will need enhancement with ongoing results from the science of behavior – as it has been enhanced already by the discovery of equivalence classes (Sidman, 1994). I'm thinking here particularly of observational (e.g., Bullock & Neuringer, 1977) and latent learning. Input from the science of behavior must continue, but it will continue in the same nominal way as now—science findings will need to be categorized and named to so be made acceptable and usable on a day-to-day basis. But the fundamental science of behavior needs to proceed in a different way, and is only hamstrung by the commonplace nominalisms and categories that are useful in applications. Finding out how the behaving organism works as it transacts with its ongoing environment requires sophisticated behavioral measures in all sorts of environments, and equally sophisticated physiological and electrophysiological and chemical and neurochemical measurement. There is nothing wrong with intervening variables between environment and behavior if these are grounded in research rather than in common parlance. Behavior Analysis science and practice are not separable endeavors. The science provides the nutrients of an ever more effective technology of behavior analysis.

Anyone for embodied behaviourism?

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Compliance with Ethical Standards

Conflict of Interest The author declares that he has no conflict of interest.

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