

Psychology is Not the Science of the Organism, and Physiology Will Not Solve the Problem of Privacy: Commentary on Jacobs et al. (2016) ¹

Andrés García-Penagos ^{2, 3}

Department of Psychology
University of Tennessee. Knoxville.

Abstract

Jacobs, Isenhower and Hayes (2016) advocate for the use of recent theoretical and methodological proposals in the field of ecological psychology to overcome long-standing problems in the conceptualization of private events in radical behaviorism, and in the experimental and applied analysis of behavior. They conclude that the notion of patterns of tensegrity at the level of the cell, tissue, or muscle, spreading to higher levels according to the principles of multifractal geometry allows for a better answer to the Skinnerian question “What is inside the skin?” Unfortunately, it is not in the understanding of such bodily events that the problem with privacy lies, but in the abundant conceptual difficulties it brings about, an issue that will not be solved by appealing to (mostly) hypothetical physiological mechanisms. Furthermore, Jacobs et al., overestimate the implications of such analysis for the issue of privacy, and in so doing, have failed in demonstrating how their multiscale approach provides an advantage over other approaches to privacy, particularly the Skinnerian approach.

Key words: *private events, bodily events, reductionism, eliminative materialism,*

Resumen

Jacobs, Isenhower y Hayes (2016) sugieren el uso de propuestas teóricas y metodológicas recientes en el área de la psicología ecológica para superar problemas de larga data en la conceptualización de los eventos privados en el conductismo radical, así como en el análisis experimental y aplicado del comportamiento. Concluyen que la noción de patrones de tensegridad al nivel celular, histológico o muscular que se transmiten a niveles superiores de complejidad de acuerdo con los principios de la geometría multifractal, permite dar una mejor respuesta a la pregunta formulada por Skinner respecto a “¿Qué hay dentro de la piel?” Desafortunadamente, no es en la comprensión de tales eventos corporales donde yace el problema con la privacidad, sino en las abundantes dificultades conceptuales que tal noción introduce, un problema que no será resuelto apelando a mecanismos fisiológicos en su mayor parte hipotéticos. Más aún, Jacobs y cols., sobreestiman las implicaciones de tal análisis respecto al problema de la privacidad y de tal forma no logran demostrar cómo su aproximación multiescalar ofrece ventaja alguna sobre otras perspectivas acerca de la privacidad, particularmente la Skinneriana.

Palabras clave: *eventos privados, eventos corporales, reduccionismo, materialismo eliminativo*

There is a lot in the recent state of radical behaviorism that makes one think of Sisyphus of the Greek myth. In this particular case, I think, we can blame B. F. Skinner for founding the bases of what seems to be a never-ending debate, an instance of what the ancient philosophers would call “aporia.” The

¹ Reference to this article on the web is: <http://conductual.com/content/psychology-not-science-organism-and-physiology-will-not-solve-problem-privacy>

² Correspondence concerning this article should be addressed to Andrés García-Penagos, Department of Psychology, University of Tennessee, Knoxville, TN, 37996-0900, USA. E-mail: agarcia9@tennessee.edu; agarciapenagos@gmail.com

³ Acknowledgement: I thank John C. Malone for enlightening conversations and advice about the topics in this commentary.

issue of concern is that of private events and their role in the explanation, prediction, and control of (human?) behavior, or more precisely, whether radical behaviorism, and by way of it the experimental and applied analysis of behavior, can or should ignore those events that are central to other conceptual and methodological approaches to the subject matter of psychology.

This seemingly trivial issue was given centrality in Skinner's view: "What is inside the skin, and how do we know about it? The answer is, I believe, *the heart of radical behaviorism*" (Skinner, 1974, p. 233; italics mine). By doing so, the endless struggle of behaviorists with privacy started, the tortuous attempts to define those events to only see the boulder going downhill again where someone else will start the climb again, with identical results. And here we have yet another approach to privacy that promises to deal with the problems and move in the direction of an empirical answer, a laudable goal indeed, but one that I fear is doomed to fail as have been many others before, for reasons that I will explain throughout this comment.

The main concern for Jacobs, Isenhower, and Hayes (2016) is that Skinner's views still haven't provided "a coherent system within which to analyze and interpret private events" (p. 6), a fair argument, of course, but a worthy one? That is, do we need such a system? The issue as I see it requires conceptual clarification from the start, with the simplest and maybe most important question being: What *are* private events? As I will attempt to show next, the fact that the authors are unclear about this most basic of questions is a bad omen for the success of their new (?) alternative.

The Problem of Privacy, from Skinner to Jacobs et al.

To be fair to the authors, their definition problem is not an individual malady but actually an epidemic. Skinner himself was after all ambiguous in his usage of the concept (Zuriff, 1979), including all sorts of different terms under the umbrella of privacy, from toothaches to autoclitics, to subvocal speech. That the authors haven't bothered to define to which of these, if any, does their alternative apply, introduces right from the start an ambiguity that only worsens as one reads through the piece. Are the authors referring exclusively to the control of behavior by *bodily* events? One would assume so, until one reads later that: "The precision with which participants can report on length, shape, and width is not a matter of happenstance, guesswork, or covert hypothesizing. In other words, *this ability is not by means of some private event*" (p. 12; italics added). So what are the authors actually arguing against?

Jacobs et al. argument begins with a description of what the authors see as the problem of privacy, arriving at the conclusion that it is a pseudo-problem (following probably the arguments espoused by Hayes & Fryling, 2009, which are apparently to be accepted as final conclusions). Now, there are many problems with Skinner's account (see e.g., Baum, 2011; Ribes-Iñesta, 2003), to be sure, but I have some difficulty with the reasons *why* they see privacy as problematic. Their main argument is puzzling: by proposing a distinction between public and private events, Skinner proposed a dichotomy, the "remnants" of which are still with us (p. 7). Leaving aside the issue of how a conceptual argument leaves remnants, and what those remnants are, here is the formulation of the dichotomy: "*no* phenomena can be accessible while inaccessible. If an event is public, it is not necessarily inaccessible, but if an event is private, it is necessarily inaccessible" (p. 5; italics in the original). Skinner would be surprised of knowing those are the conclusions from his papers.

Obscurely, the authors argue that "[d]ichotomies pose both ontological and epistemological problems for coherent and parsimonious theoretical accounts of behavior." This is news for me, indeed! There are some troublesome dichotomies, of course, but I can think of plenty which do not pose the dramatic ontological and epistemological problems the authors argue: the S^D-S^A dichotomy, or that of

reinforcement/punishment, or liberal/conservative or thousands of others in both technical and lay language. Maybe what the authors are afraid of is not dichotomies, but *dualism*, which is a completely different issue, and I think such confusion is unforgivable.⁴

More importantly, however, is that Jacobs et al., seem to assume that inaccessibility is the only problem in Skinner's approach to private events. This is, in fact, the lesser of its problems. Some of the events Skinner included in his taxonomy of private events are physiological events inside the skin, and these evidently are *accessible* in principle, by technological measures, even if inaccessible now, as Skinner himself would expect (and even, and uncontroversially, Baum, 2011; see also the discussion of Privacy A and B in Rachlin, 2003). In this respect, we can only agree: inaccessibility is indeed a pseudo-problem, easily circumvented by technological advances. Jacobs et al., are not alone in believing this is the only problem with Skinner's views, it seems, as many other authors (e.g., Palmer, 2009; 2011) in the aporetic debate of privacy emphasize this point over all others.

What I would call the *deep* problem of privacy has never been one of accessibility. Skinner wouldn't disagree that those physiological events are in principle accessible, and charging him with arguing for the opposite is clearly a straw-man argument. Further, it is important to point out that Skinner after all wasn't that interested in studying private events *per se*, but how they (seem to) control behavior. In his writings he showed, to the best of my knowledge, no interest in defining them or in how exactly do they manage to "control" behavior beyond calling them stimuli or responses. His interest was mostly on the issue of "intentional idioms" and their translation to functional terms, on the premise that most of them are just fictional explanations. That is, his interest was *not* in the mechanisms of private stimulation or response, but in the sort of verbal behavior that he called self-descriptive behavior.

In my opinion, a good argument can be made that Skinner was not interested in whether said private events were accessible to a researcher in a lab, but whether they're accessible to a community which will reinforce or punish the individual's behavior: "Although the private world is defined anatomically as 'within the skin,' the boundaries are the limits beyond which the reinforcing community cannot maintain effective contingencies" (Skinner, 1969, p. 230, footnote 2). In this sense, even if those events are detected by say, an optical fMRI, the verbal community might not consider this information useful insofar as the community won't be responding to the physiological event in the same way that the person experiencing it is: "The way in which I observe [private events] 'introspectively' is not the way neurologists would observe them if they could, and until they can observe them as they would like to do, their neurology will remain 'only indirectly or inferentially knowable.' I know about presumably the same events directly and without inference, though in an almost certainly limited and faulty way." (Skinner, 1988, in Catania & Harnad, 1988, p. 313).

In typical Skinnerian fashion, his arguments were ambiguous. The quotes above might be taken to mean that ultimately for him what is important is not the nature of the private event, but how self-descriptive behavior enables some sort of social coordination, an argument that seems close to Wittgenstein's famous "beetle in the box" thought experiment. But it can also be taken to imply some version of solipsism, in the sense of being events that *only I* can fully observe (know?): The dentist might see my damaged tooth and infer from my behavior that I'm in pain, but ultimately cannot *observe* my pain the way I observe it. This is where most critics find a problem with the Skinnerian position, as it seems to

⁴ It seems that the authors' problems with dichotomies come from a misunderstanding of the following quote from Hayes & Fryling (2009): "We have argued that the problem of privacy in the analysis of behavior is a pseudo-problem having its source in an *illegitimate* dichotomization of psychological events on the basis of which side of the organism's skin they are held to be taking place" (p. 54; italics added). Notice that in this quote what is seen as problematic is the illegitimate use of them, not dichotomies *per se*.

be evidence of ontological or epistemological dualistic implications that are antagonistic to the tenets of radical behaviorism (Baum, 2011; Harzem, 2000; Ribes-Inesta, 2003).

Furthermore, Skinner's incoherent use of the adjective "private," including in his definition not only physiological events inside the skin, but all sort of other events had the very unfortunate consequence that we have had countless and pointless debates on "privacy" as if all sides were discussing about the same phenomena, as if they all had the same referent. It doesn't help that Skinner often referred to these events as private stimuli and responses, again as if these were actual objects or events in the world. As they say, if all you have is a hammer, everything looks like a nail, but I digress.

Jacobs et al., have fallen prey to this very trap by suggesting that when the problem of accessibility is circumvented privacy no longer poses a problem. Their argument in this case has the following form: Since Skinner's analysis of private events applied to the events of interoception and proprioception, and ecological psychologists have proposed a better analysis of proprioception, we can conclude that their analysis applies to, and does a better job of, *all* of privacy, providing a coherent system to analyze and interpret those events. But is that really the case?

Will Dynamic Touch Solve the Problems with Privacy?

A central part of the argument of Jacobs et al., is their description of research by ecological psychologists on so-called dynamic touch. Jacobs et al., consider that this research is "an exemplar of a multiscaled analysis that overcomes the pseudo-problem of privacy" (p. 11). I certainly am unsure of how a pseudo-problem can be overcome by research instead of conceptual analysis, but again, maybe I'm just misinterpreting their terms. Maybe what they mean is that such research illustrates the point that an analysis of certain private events can be made, demonstrating the futility of the appeal to accessibility. And who would disagree? Would any serious radical behaviorist, of a Skinnerian or other persuasion, believe that there are no biological components to any instance of behavior? As Rachlin (2003) put it, "Of course there are all sorts of events going on inside my head, neural events, hormonal events, physiological events. I can even talk to myself, possibly picture things to myself and possibly introspect about those words and pictures" (p. 187).

But again, Jacobs et al. suggest that the interesting research on dynamic touch is superior insofar as it provides "evidence that there are bodily processes and events to which the organism is sensitive" (p. 12). I guess I've been going to the restroom all these years for the wrong reasons! It is news to me that we needed that evidence. And they continue to clarify it, "This is to say that participant behavior is not only under the control of the inertia tensor, but is also under the control of bodily events" (p. 12). And again, who would disagree? Do we know of *any* behavioral event which is different? That is, is there any behavioral event that is not under the simultaneous control of environmental and bodily events? Even cognitive psychologists think of representations as having a biological basis. If the authors are suggesting that Skinner thought differently they're clearly misrepresenting his views.

But, why do the authors think this research presents an advantage over previous analyses of privacy? It seems that what throws them off in such belief is that in the experiments on dynamic touch participants are blindfolded and yet are very accurate in estimating the length, shape, and width of objects and, lo and behold, those estimates are often of a verbal nature. So, should we assume that what we're seeing here is a report of the private event? Is the participant in such experiments reporting on the action of his tensegrity system or just reacting, behaving towards the mass, length and ultimately the "inertia tensor" of an object? The answer is of course that the participant's estimate is a function of both, but no *a report* of them.

Let me explain myself with a little analogy. Imagine that in a different experiment, probably the simplest of all, we ask a participant to label the color of a key that we can transilluminate. Most adult humans will excel easily at this simple task, saying “blue” (or its equivalent) to a key illuminated in the appropriate wavelength. This experiment is almost identical to those in the dynamic touch experiment, except involving color instead of shape, length and the like. Now, when the participant says “blue,” should we assume that this is a report of private event? Should we assume that the participant is reporting about the increased activity in her cones around the fovea, the activity of parvo and magno ganglion cells and about the role of the LGN in the thalamus, and the increased activity in the different areas of the primary visual cortex? Do we gain anything by calling this report a private event? How is this different from the case where a blindfolded participant describes an object that she is wielding as being L-shaped?

Interesting as this research is (and I *do* think is), and supportive as it is of the notion that perception does not require representational processes, it seems to me that this has little to say about privacy, and even less to say about the issue of self-descriptive behavior. The authors, I think, are aware of the issue as we read that: “If a unique pattern [of coordination of body parts specific to certain circumstances] is discriminable [...] it *might* play an important role in accounting for the self-descriptive response” (p. 14; italics added). This view is hardly different from that of Skinner, only that instead of talking of private stimuli and responses, we’re talking of patterns of muscular activity that are somehow specific to a circumstance. Couldn’t the orthodox Skinnerian just say that what Skinner meant by a private stimulus is such a pattern?

Jacobs et al., argue that this research provides evidence for what ecological psychologists call proexteroception and exproprioception. These two terms, however, are not hypotheses, but instances of taxonomical categorization, that is, labels or categories proposed to classify events, and as such they do not *require* evidence. What dynamic touch provides evidence for is that haptic perception is a function of the physical properties of the wielded object by way of the muscular changes and deformations produced by such wielding. How do the authors jump from this clear and straight conclusion to hypothesize that “humans self-describe and speak of ‘feelings’ in the presence of bodily states that are lawfully constrained under certain circumstances” (pp. 14-15) is beyond me.

Two questions come to me from this loose and bold assertion. First, what is self-description? Is this behavior unique to humans? Second, and most importantly, since participants’ estimates of length and others in the dynamic touch are equally controlled by bodily states that are lawfully constrained under particular circumstances, are then those estimates instances of “feelings”? Are all so-called private events feelings? And if not, how are “feelings” different from those other phenomena? And what are those bodily states? And what are the “certain circumstances”? One would imagine that the answer to these important questions would be the goal of the following sections in their paper --after all, they’re proposing a new approach to privacy--, but that is not the case, which suggests they expect the reader to accept their crude hypothesis without further consideration, and figure out the details by her or himself. The concerned reader should not fret, because in his help the authors have provided us with a discussion of how the lawful constraining of bodily events might occur, and it might only require to learn a little bit (or maybe more than that) of cell biology, functional histology, fractal geometry and biotensegrity.

Twas Brillig, ECM, MCS, MFT and the Tensegrity Icosahedron

Although the particulars about “feelings” are not described at all in the article, the authors seem to suggest that knowledge about bodily processes, or more precisely their “lawful constraining” to particular circumstances, might give a clue. It is not clear, then, whether their analysis of tensegrity and multifractality are just an exemplar of the action of bodily processes in general applicable only to issues of proprioception, or whether it underlies the action of *all* bodily processes. It is also obscure by extension

whether the authors are merely illustrating that some research can adequately address phenomena that are said to be private in an empirically rigorous manner (as I think it does) or whether the notion of multifractal tensegrity is or should be central to any treatment of so-called privacy, although they seem to favor the latter.

In considering Jacobs et al., adoption of this theoretical framework I will follow mostly the proposals of Turvey & Fonseca (2014), which seem to be the ones guiding their conceptualization. It is important to point out from the start that Turvey and Fonseca are mostly interested in a systematization of the *physiological mechanisms* underlying proprioception, and *not* in the issue of self-descriptive behavior that is central to the debate of privacy. The proposal of Turvey and Fonseca is essentially an extension of the work of Donald E. Ingber (e.g., Ingber, 2003) and others suggesting that the structures of cells, tissues, organs, and even organisms can be mathematically modeled according to the principles of tensegrity.

Turvey and Fonseca's views are also rooted in J. J. Gibson's ecological approach to perception (see Reed, 1988, for a comprehensive review). A fundamental tenet of the ecological approach is that the environment is rich in information, and as a consequence the perceiver is active in trying to extract such information from stimulation by exploring, looking, listening, tasting, smelling. In this manner, perception consists of such behavioral adjustments and does not require or imply any sort of elaboration in the form of cognitive processes. By proposing the tensegrity hypothesis, Turvey and Fonseca were trying to do for "haptic"⁵ perception what Gibson had done for visual perception.

In visual perception Gibson's approach emphasized the dynamic properties of the *optical array* as a rich source of information that can be extracted by an organism. But, what is the equivalent source of information for haptic perception, particularly when we seem to be so accurate in reaching and grasping for things, as discussed in the previous section? Turvey and Fonseca's reply was that the tensegrity-like properties of the "muscular, connective tissue, skeletal" (MCS) system might play a similar role, a "tension array" or a "deformation array" (Turvey & Fonseca, 2014, pp. 169-170), that is, that the different states of tension or distension connecting the parts of the MCS, and the changes in these states, provide information about the position of the body and its parts, and about changes in this position, and that this information is extracted by the haptic perceptual system, so that the individual behaviorally adjusts accordingly.

The novelty in Turvey and Fonseca's (2014) analysis lies in incorporating the issue of multifractality, not as Jacobs et al., argue "due to the diversity of bodily components from nanoscale to macroscale" (p. 13), but because of their ultimate interest in motor behavior. Their question is, if the information for proprioception comes ultimately from changes in the extracellular matrix (ECM) affecting the tensegrity of cells, how is it that information at the cellular level is transformed, so to speak, into motor behavior at the level of the whole organism? Unlike the reasons for adopting tensegrity as a model, coming from a large tradition in cell biology and physiology, their adoption of multifractality as the answer to this question is less clear.

The interest in fractals, multifractals and the like comes from the so-called "complexity sciences" (Kelty-Stephen & Dixon, 2012), that originated mainly in the possibility of describing and understanding turbulence as a physical phenomenon by using power-law relationships, and in the use of the related area of fractal geometry which is argued allows for a mathematical description of phenomena of increasing

⁵ Turvey & Fonseca (2014) used the adjective "haptic," to refer to nonvisual perception occurring when one holds an object for a particular goal, say a hammer, a baseball bat, or a pen.

complexification.⁶ In particular, the issue of multifractality refers to the use of a number of power-law relationships that are supposed to deal with “nested structure across multiple scales” to paraphrase Kelty-Stephen & Dixon. To Kelty-Stephen and Dixon, multifractality is a framework that enables the analysis of phenomena that seem chaotic or unpredictable, that is, turbulent, including “cognitive” phenomena, hypothesizing even that their analysis might allow understanding of representations from a physical viewpoint.

The point I want to emphasize is that whether the body is a multifractal tensegrity system or not is *not* a fact, but a *working hypothesis*, one that requires that “the body in all of its parts, at all of its scales, is constituted by the same kind of system under a common scale-independent thermodynamics” (Turvey & Fonseca, 2014, p. 152), in particular in what refers to multifractality. Their conclusion, as described by Jacobs et al., is that it is the differing deformation *patterns* of the multifractal tensegrity (that is, of the cell, tissue, muscle, and body as a whole) that provide information about particular body states. As a hypothesis, multifractal tensegrity as an accurate description of “what is inside the skin,” is ultimately an empirical matter, but as Jacobs et al., point out it puts the spotlight on the complex issue of how physiological events occurring at the cellular level might be functionally related to events at higher levels of organization. The issue of course is how this is relevant to the analysis of privacy.

I think the highlight of the argument of Jacobs et al., lies in bringing attention to the fact that the use of concepts like private stimuli or private responses to describe or characterize physiological events as related to behavior is a very bad practice. They replace these concepts, however, with another that although arguably better is just as vague, that of patterns of cascade effects in the MCS system. The point is not whether such patterns occur or not, or whether the body can reorganize –reach a new equilibrium–, as a result of a (mechanical) change in the tensegrity at either the ECM, cell, tissue, or muscle level. It well might, for all we know. The point is that ultimately some of these events can be “reported” and some others cannot.

The question is, then, if indeed organisms “come to be sensitive to these unique patterns and their relation to stimulatory changes originating in the environment” (p. 15), what does it mean to be *sensitive*? Does it mean anything other than saying that organisms alter their behavior partly as a function of those patterns? And if that is all it means, what then of self-descriptive behavior? Is the self-descriptive behavior a necessary result of those patterns? I can report, perhaps, that an object I’m holding at dark is my smartphone or an empty glass. But I cannot report increases in say, insulin after dinner, or cannot report about the action of the muscles at my thigh when I press the brakes of my car in driving. Both of these events might well “obey” the principles of tensegrity, and truly enough my body as a whole can be altered by both events, in the sense that the action of both might provide some sort of negative feedback loop that will “tell” my body that I shouldn’t eat more or that I can release the brakes now. They probably are patterns of MCS action that can be related to particular stimulatory changes in the environment (e.g., dinner time). And yet, they escape verbal description.

So, even though the multifractal tensegrity provides a more elegant, if sesquipedalian, account of the action and interaction of physiological events at different scales, at least in what regards privacy we are exactly in the same point. What advantages does this analysis offer over one that simplifies said patterns into private stimuli and responses? As I said before, couldn’t a Skinnerian say that those patterns of activity *are* the discriminative stimuli that Skinner proposed? Should the role of the psychologist interested

⁶ The most particular characteristic of fractal objects is that their big complexity can be seen independently of the scale at which it is observed, such that when a part of a fractal object is enlarged it is observed that the enlarged part is *similar in structure* to larger parts of the object or to the whole object itself (this is what is meant by the adjective “self-similar”). This *scale-invariance* is described mathematically by a power law, which includes a particular scaling exponent.

in so-called privacy be the identification of those *unique* patterns? Is this what the authors mean by “a coherent system within which to analyze and interpret private events”?

Psychology is Not the Science of the Organism

It is easy to understand now why Jacobs et al., had some problem with the fact that Skinner didn't attempt “to specify the physical goings on within the organism's body” (p. 3). For them, it seems, unless behavior analysts understand the body they cannot fully understand behavior. I'm confused, though, on whether the understanding of the body as a multifractal tensegrity is necessary to understand *all* behavior, or whether it is only necessary to understand private events. Maybe the answer to my previous question about what the authors mean by privacy is that private events are bodily events, and maybe by extension all physiological, bodily events are private events. Whatever the case, they are ultimately correct: no understanding of behavior is truly complete without understanding its physiological bases. No understanding of human behavior is also truly complete without understanding social institutions, politics, economics, and culture. To completely understand aggression, for instance, we need to understand it at the level of neurotransmitters, at the level of neural networks, at the level of the central nervous system structures or at the level of the activity of the peripheral nervous system and the endocrine system; we need to further explore it at the ecological level and in terms of increased fitness; we can also explore it at the ontogenetic level in terms of development or in terms of a history of reinforcement and punishment; further, we can also explore it at the level of groups, and institutions, as a result of economic and political strife, and also as a cultural phenomenon.

As should be obvious, such complexity implies that the different scales are qualitatively different even if interdependent, analyzed at different spatio-temporal scales, and with different methods. So, why should we expect that the scientists and researchers working at a particular scale should explore those of other scales? Who has the time, the resources, and the academic background and expertise to address such complex phenomena in more than one scale? Why should the physiologist interested in the properties of synaptic communication be concerned with understanding aggression as a result of economical or political alienation? Why should the applied behavior analyst hoping to reduce the frequency of aggressive behaviors of a client be concerned with cortisol levels or increased activity of the amygdala, particularly when he cannot observe any of them? So, likewise, why should a behavior analyst interested in why someone says “I feel like dancing” be concerned about whether the cell is a tensegrity icosahedron? And isn't this precisely what Jacobs et al. propose when they suggest their approach will lead to an “empirically driven account” of private events? That *we* should start analyzing patterns of multifractal tensegrity (which, by the way, are to the best of my knowledge unobservable) as they correspond to environmental events?

At least Skinner left the task to the “physiologist of the future” (1974, p. 236). Further, this is what Skinner, *forty* years ago, had to say about this issue: “To agree that what one feels or introspectively observes are conditions of one's own body is a step in the right direction [...] [*b]ut what is felt or introspectively observed is not an important part of the physiology which fills the temporal gap in a historical analysis. [...] self-knowledge arose much later in the history of the species, as the product of social contingencies arranged by the verbal community”* (Skinner, 1974, pp. 238-239; italics in the original). I think that his comments are quite relevant to the proposal by Jacobs et al.

You Will Know Them by Their Fruits

It is quite telling of the futility of the whole enterprise that the authors do not provide *any* evidence of how their multiscale approach would deal with any of the problems traditionally posed by privacy in radical behaviorism, not even Skinner's famous “toothache” argument. The closest they get to

provide such evidence of the usefulness of their approach is in an obscure paragraph that starts with the ear as a tensegrity system and then mysteriously ends up being about Skinner's translations of intentional idioms to "Behaviorese," concluding that their emphasis in lawfully constrained patterns "*might* give new meaning to Skinner's (1974) behavioral translation of self-descriptive statements concerning 'feelings.'" (p. 16; italics are mine). What such 'new meaning' is, I suppose, is left to the reader to figure out, although maybe what they mean is stated in the next paragraph: "We extend Skinner's (1974) interpretation by contending that statements concerning feelings are indicative of environmental contingencies *and* lawfully constrained bodily states" (p. 16; italics in the original). How exactly is this an extension?

Further, if we take the example they use, where Skinner argues that in some cases saying "I feel like playing cards" can actually mean "I feel as I often feel when about to play cards," their new translation would come up as something like this: "The multifractal tensegrity to which my body is accommodating now is like the multifractal tensegrity to which my body accommodated when about to play cards." Hardly a superior translation, I would say. It's not surprising that when they argue that "[the] statement, 'I feel like going to a concert' can be translated into empirically derived statements concerning the coordination of the whole body—from ear to cell—under circumstances related to concert-going," (p. 17) they forget to provide even a single example of what such statements—which mysteriously seem to include the ear—would look like.

Nonetheless, the authors are hopeful that the specification of these unique patterns—which if we're to believe them are now being developed at the level of cellular behavior—will eventually be shown to spread to higher levels according to the principles of fractal geometry, and then, *alas* only then, we will know something about what is it that we feel or introspectively observe, "that is, the observer's own body" (p. 17). I guess everything that the physiologists have been telling us for the last few centuries is wrong. In any case, this highly speculative hypothesis is supposed to "demonstrate a methodological path toward eliminating privacy from behavior analysis" (p. 17). It seems to me, though, that all that their approach points out to is a sort of *eliminative materialism*, that is, the reducing intentional idioms to physiological states. In light of their emphasis in the MCS maybe we should call it *peripheral*, but hardly a new proposal if one knows anything about the rise and demise of logical positivism, or about the recent ideas in the embodied cognition field. What is, however, the *methodological path* that they suggest? They actually haven't offered one, unless by that they mean: "Wait until the ecological psychologists figure it out," or maybe that when asked we should appeal to the existence of some hypothetical tensegrity spreading multifractally from our cells.

I hope it should be clear that I have nothing against Turvey and Fonseca's (2014) theoretical proposal, and much less against their interesting research. I do think that they point out to interesting phenomena and that they are able to deal, conceptually at least, with complex phenomena without appealing to representations and some other forms of hypothetical constructs. I also concur that understanding the physiological bases of behavior is important. I think, however, that Jacobs et al. adoption of this model is somewhat premature and that the implications it might have for the issue of privacy need to be worked out in a more accurate manner than a "might." In between, the authors have done little to solve the jigsaw puzzle of privacy, and might even have misplaced a few of the pieces. I think the abundant conceptual errors, the misinterpretations, and the misplaced trust in an as-to-yet hypothetical system are nothing but evidence of how futile and ultimately counterproductive the debate of privacy is, how little it has advanced our knowledge of behavior in general, and how it might lead one to do not only bad psychology but bad physiology, points that have been elegantly discussed by Harzem (2000), Rachlin (2003), and Baum (2011) among others.

References

- Baum, W. (2011). Behaviorism, private events, and the molar view of behavior. *The Behavior Analyst, 34*, 185–200.
- Catania, A. C., & Harnad, S. (Eds.). (1988). *The selection of behavior: The operant behaviorism of B. F. Skinner*. New York: Cambridge University Press.
- Harzem, P. (2000). Towards a new behaviorism. *European Journal of Behavior Analysis, 1* (1), 51-60.
- Hayes, L. J., & Fryling, M. J. (2009). Overcoming the pseudo-problem of private events in the analysis of behavior. *Behavior and Philosophy, 37*, 39-57.
- Ingber, D. E. (2003). Tensegrity I. Cell structure and hierarchical systems biology. *Journal of Cell Science, 116*, 1157-1173. doi: <http://dx.doi.org/10.1242/jcs.00359>
- Kelty-Stephen, D. G., & Dixon, J. A. (2012). When physics is not “just physics”: Complexity science invites new measurement frames for exploring the physics of cognitive and biological development. *Critical Reviews in Biomedical Engineering, 40*, 471-483.
- Palmer, D. C. (2009). The role of private events in the interpretation of complex behavior. *Behavior and Philosophy, 37*, 3–19.
- Palmer, D. C. (2011). Consideration of private events is required in a comprehensive science of behavior. *The Behavior Analyst, 34*, 201–207.
- Rachlin, H. (2003). Privacy. In K. A. Lattal & P. N. Chase (Eds.), *Behavior theory and philosophy* (pp. 187-201). NY: Kluwer Academic/Plenum press.
- Reed, E. S. (1988). *James J. Gibson and the psychology of perception*. New Haven, CT: Yale University Press.
- Ribes-Iñesta, E. (2003). What is defined in operational definitions? The case of operant psychology. *Behavior and Philosophy, 31*, 111-126.
- Skinner, B.F. (1969). *Contingencies of reinforcement: A theoretical analysis*. New York: Appleton-Century-Crofts.
- Skinner, B. F. (1974). *About behaviorism*. NY: Vintage Books.
- Turvey, M. T., & Fonseca, S. T. (2014). The medium of haptic perception: A tensegrity hypothesis. *Journal of Motor Behavior, 46* (3), 143-187. doi: <http://dx.doi.org/10.1080/00222895.2013.798252>
- Zuriff, G. E. (1979). Ten inner causes. *Behaviorism, 7* (1), 1-8.