

THE INTERBEHAVIORIST

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THE INTERBEHAVIORIST

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Interbehavioral Psychology
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THE INTERBEHAVIORIST publishes news, information, discussion, journal and book notes, book reviews, comments, and brief articles pertaining to interbehavioral psychology – a contextualistic, integrated-field approach to the natural science of behavior.

The newsletter also publishes professional communications that fall between informal correspondence and colloquia, and formal archival publication. As such, the newsletter supplements contemporary journals dedicated to basic and applied research, to the history and philosophy of the behavioral sciences, and to professional issues in the field. The newsletter strongly encourages submission of notes about current professional activities of its subscribers, news and observations about interbehavioral psychology and related perspectives, comments on journal articles and books of interest, more extended book reviews, and brief articles. All submissions should be sent in duplicate hard copy and a single computer disk copy (any major word processor; any Mac or IBM disk format) to the editor and should conform to the style described in the Publication Manual of the American Psychological Association (3rd edition).

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Call for News

THE INTERBEHAVIORIST publishes news about subscribers' activities and information about others' activities that may be of interest to readers. If you have published an article, chapter, or book with an interbehavioral orientation, or have read one published by someone else, particularly if the source is obscure, please let us know about it.

The Agora

Interbehavioral Sessions at the Association for Behavior Analysis San Francisco Hyatt Regency May 24-28, 1992

(Editor's note: We have included sessions known or thought to have an interbehavioral orientation. We apologize for any mischaracterizations and omissions.)

Monday, May 25

4:00-5:20pm PACIFIC O

The concept of context in the description of behavior

Chair: Edward K. Morris (*University of Kansas*)

The concept of context in the conceptual analysis of behavior. Edward K. Morris (*University of Kansas*)

The concept of context in the experimental analysis of behavior. Bryan D. Midgley (*University of Kansas*)

The concept of context in applied behavior analysis. Pete Peterson (*University of Kansas*)

Summing up: The generic nature of the concept of context. Edward K. Morris (*University of Kansas*)

8:30-10:30pm GOLDEN GATE

ABA EXPO

Interbehaviorists in ABA

(Please stop by and say hello)

Tuesday, May 26

11:00-11:30am PACIFIC K

Some thoughts on complex human behavior and its relation to conditional discrimination procedures. Emilio Ribes-Inesta (*University of Guadalajara, Mexico*)

11:00-12:50pm PACIFIC O

Behaviorism and naturalism

Chair: Noel W. Smith (*State University of New York-Plattsburgh*)

Discussant: Donna M. Cone (*University of Rhode Island*)

Naturalism and psychological science. Paul T. Mountjoy (*Western Michigan University*)

Behaviorism re-defined: Subjectivity re-examined.

Parker E. Lichtenstein (*Denison University*)

Scientific status of mental and spiritual powers from the standpoint of the new science and liberal religion. Dennis J. Delprato (*Eastern Michigan University*)

Toward a naturalistic psychology. Noel W. Smith (*State University of New York-Plattsburgh*) &

Lance L. Smith (*University of California-Berkeley*)

12:30-2:00pm GOLDEN GATE

Poster session

#31 Comparative analysis of game manipulations in the Oregon and Arizona lotteries: Ticket sales as a function of game adjustments.

Charles A. Lyons (*Eastern Oregon State College*)

& Patrick M. Ghezzi (*University of Arizona*)

1:00-1:50pm PACIFIC A

Interbehaviorists in ABA Special Interest Group Meeting

Chairs: Linda J. Hayes & Debra W. Fredericks (*University of Nevada*)

Purpose: To function as an opportunity for interbehaviorists to discuss issues of common interest as well as help one another solve problems peculiar to the interbehavioral perspective in psychology and philosophy.

Agenda:

1. Election of officers;
2. Convention report and plans;
3. The Interbehaviorist report;
4. Student issues;
5. interbehavioral news and notes;
6. Discussion.

Wednesday, May 27

11:00-12:50pm PACIFIC O

Are private events private?

Chair: Mark A. Swain (*University of Nevada*)

Discussant: Elliot Bonem (*Eastern Michigan University*)

Constructs and events in theory formulation: Implications for psychology. Kelly G. Wilson (*University of Nevada*)

Privacy and the distinction between psychological and non-psychological events. Dennis J. Delprato (*Eastern Michigan University*)
 The privacy construct as a stumbling point in psychology. Noel W. Smith (*State University of New York-Plattsburgh*)
 In praise of "subtle events". Linda J. Hayes (*University of Nevada*)

1:00-2:50pm PACIFIC K

Mechanism and contextualism compared
 Chair: Peter Harzem (*Auburn University*)
 Discussants: A. Charles Catania (*University of Maryland-Baltimore County*) & Gary Novak (*California State University-Stanislaus*)
 What it would be like to be a mechanist. Edward K. Morris (*University of Kansas*)
 The straw machine as tar baby. M. Jackson Marr (*Georgia Institute of Technology*)

1:00-2:50pm SEACLIFF

Current technologies in interbehavioral data collection
 Chairs: Tom Sharpe (*University of Nebraska*) & James J. Fox (*East Tennessee State University*)
 Discussant: Donald M. Baer (*University of Kansas*)
 Field systems technological applications: Teacher education strategies and tactics. Tom Sharpe (*University of Nebraska*) & Andrew Hawkins (*West Virginia University*)
 An approach to measuring interbehavioral fields within early educational environments. Judith J. Carta & Jane B. Atwater (*Juniper Gardens Children's Project/University of Kansas*)
 Computerized data acquisition and analysis of multi-dimensional behavior. Duane C. Lord, Mark A. Swain & Linda J. Hayes (*University of Nevada*)

Thursday, May 28

10:30-12:00pm GOLDEN GATE

Poster Session

#29 A peer-mediated intervention for enhancing conversations between normally developing and educably retarded children. Patrick M. Ghezzi, Sidney W. Bijou, Elias Robles (*University of Arizona*)

11:00-12:50pm PACIFIC G

Are all data useful? Behavior analytic, medical and psychotherapeutic perspectives.
 Chair: Ramona Houmanfar (*University of Nevada*)
 Discussant: John D. Cone (*United States International University*)
 A functional analysis of how data can insubstantiate intuition. Ramona Houmanfar & Linda J. Hayes (*University of Nevada*)
 Behavioral data and pharmacology: Real concerns regarding illusory beliefs. Debra W. Fredericks, Linda J. Hayes, Dee Ann M. Radcliffe & Jacqueline E. Collins (*University of Nevada*)
 Clients and clinicians: Whose behavior needs changing anyway? Niloofar Afari & Kelly Wilson (*University of Nevada*)

News and Notes

Sidney W. Bijou and Patrick Ghezzi have co-authored a useful text entitled *Outline of J. R. Kantor's Psychological Linguistics*, which is being published by Principia Press and will be available in time for the fall semester.

Marion White McPherson, of the Archives of the History of Psychology, has authored an article that may be of interest to interbehaviorists entitled *Is psychology the science of behavior?* This article appears in the February 1992 issue of the *American Psychologist*.

Context Press has just published two new books of interest: Hayes, S. C. & Hayes, L. J. (Eds.), *Understanding Verbal Relations*, and Leigland, S. (Ed.), *Radical Behaviorism: Willard Day on Psychology and Philosophy*. The Context Press address is listed on Page 2.

That Little Extra

The Interbehaviorist thanks the following persons who contributed a little extra when renewing their subscriptions for 1992:

James Herrick
 Charles Lyons
 Ed Morris

Article

Field Theory and the Legitimacy of Causal Constructions

Steven C. Hayes
University of Nevada

The present paper argues that from a field theoretical viewpoint 1. causality has no ontological status, and 2. causal talk has a place in basic and applied science. The contradiction between these two points is removed when the general anti-ontological nature of field theory is explored.

Causality in the World

In a field theoretical account of psychology, causality cannot be said to exist in the world. That is because, within this perspective, all events are to be seen as situated in a context, and no contextual event that participates in a field can be said to be irrelevant, by definition. When the totality of the field is described, the nature of each participant is defined in terms of all others. Thus, for example, the statement "the spark caused the explosion" assumes combustible material, oxygen, temperature, and so on. When all of these are included in the analysis one can only say that there was an explosion that included the participation of sparks, combustible material, oxygen, temperature, and so on. None of these participants *caused* the whole event, rather the working together of all these participants *is* the event.

If it is true that causality cannot be said to exist in the world, it is also incorrect to say "causality does *not* exist in the world" and to mean this as an ontological statement of what does and does not exist. Let me explain.

The Realness of Events

From a thoroughgoing field theoretical viewpoint, while (as a matter of postulation) the world is real, there can be no grounds upon which to assert the reality of any specific event in the world. Quite simply, no event can be said to exist, if we mean this ontologically. This is because any statement about anything is itself an action that is situated in a context. The action of analysis – of

identifying events – cannot step outside of the field in which that action occurs.

Any attempt to claim reality status for the referents of statements is an attempt to appeal either to some other event outside of the present field or to a special status for a current participant. Suppose I say "I am now referring to something that is real." What is the role of the "realness" of the referent in the statement. The "realness" of the event could be *a priori* and separable from the participants in the statement. For example, if I say "Buicks exist" I could defend the truth of the statement by asserting that there truly are cars called Buicks. I could take an observer to a Buick dealer and point to the cars. But in this defense, the issue of realness is being argued outside of the field that was directly involved in the particular claim made by a particular individual. This method of argument violates a field theoretical perspective, and in the same way as does claiming there are causal events in the world. "Realness" is not a thing that has causal properties over behavior, or that can be viewed independently of behavior. The question of "realness" must bear on the participants in the field that included the statement "Buicks exist." We are not speaking of some other field – we are speaking of this one.

Speaking of this field presents another problem. The "realness" of events could involve an appeal to some special property of a participant. Perhaps "realness" is a quality of particular events more than others and this quality has some special role in certain statements. From a field theoretical view, any claim that gives special status to some participants over others in a field should be viewed as illegitimate, because all participants participate. No participants can be any more real than any other.

Analysis as Construction

An act of analysis is an act of verbal construction. Construction is not the same as discovery. The metaphor of discovery assumes that the event is already there before "the cover is taken off" and it is "dis-covered." But we have no reason to suppose that events pre-exist as events, independently of our history with regard to them. This is not to say that construction occurs in a vacuum. From a thoroughgoing field theoretical perspective, an act of analysis is occurring in and with the one world – the "real" world. The realness of the world is a matter of postulation, not an empirical matter.

When we make reality claims for talk about events we are claiming that the event was already there as an event and our talk corresponds with it. But the context in which there is an event at all is also historical and situational. For example, when I show an observer the Buicks in the car lot, this at least assumes a history with visual stimulation or visual categorization, if not a more specific history with automobiles. When we make reality claims for statements by saying that "the event was already there, waiting to be discovered" we ignore *these* contextual participants. When they are included we have no independent access to any event.

What I am asserting is that no ontological claims of any kind are legitimate within a thoroughgoing form of field theoretical psychology because such claims always reduce themselves to some form of correspondence in which particular participants in an interaction are illegitimately given special status – causal or otherwise (Hayes, L. J., in press). We cannot step outside of the world and view our acts of analysis as direct avenues to anything. They begin and remain as situated actions. Thus, there can be no legitimate claim to a reality basis for particular forms of talk.

Talk as Doing

Some may view these assertions as solopcistic or mystical. Those used to justifying talk on the basis of its "basis in reality" might ask "Given this view, why do we speak?" It is because some verbal actions are more useful than others, quite apart from our analysis of the reality basis of these actions. On the basis of experience it is evident that there are ways that the world works and that

our talk about the world makes a difference. That fact that the world works as it does, however, is not an avenue to "reality" conceived of as preorganized events awaiting discovery. Instead, the working of the world is simply a context for speaking.

If speaking is always ultimately pragmatic, then we must examine any truth claim not in terms of its reality basis but in terms of its utility (Hayes, S. C., Hayes, L. J., & Reese, 1986). I return now to the statement "the spark caused the explosion." The word "cause" in this statement is invalid – from a field theoretical viewpoint – if it is taken to be a statement about the existence of events in the world. But so – I am claiming – are the words "spark" or "explosion." The world is not divided up *a priori* into two classes of events called sparks and non-sparks, and if it were there would be no way for us to know that. The presence of "sparkness" in a field is a reflection of our unique histories with regard to the abstraction of aspects of a totality. If we could see only infrared radiation, "sparks" might be in the same class as "friction" – in this case there might be only one event, not two. If we could see only gravity there might be no event at all. Whether we are dealing with no events, one event, two events, or an infinity of events is not a matter of the world but of our interaction with it. For us to claim "oh no, sparks and friction are really separate" or "oh no, sparks really do exist" – as if it is the real world we are speaking of when we speak – is to impose our history upon the world when it is only through that history that events can be abstracted as events at all.

Those who field theorists who think that scientists must speak ontologically are lead inexorably into silence. Instead of "the spark caused the explosion" we would have to say nothing at all (Hayes, L. J., in press). This would be absurd and would also be inconsistent with a field theoretical account because we would then be giving the view "scientists must speak ontologically" some reality status such that it would dominate over all other aspects of a situation. It is evident to any experienced human that speaking makes a difference. A person who claims otherwise might as well claim that it does not hurt to be hit by a 2 by 4. If such a

claim is made in front of me, I will be quite happy to test the claim via a vigorous whack to the head. There is a real world (by postulation); there is a way that things work (by experience); some talk is more useful than others (by experience). None of this is an avenue to the reality of events qua events.

Utility

What is meant by "talk is useful"? Usefulness is not a thing. It makes sense only relative to the desirability of given outcomes. What is useful with regard to one goal is useless with regard to another. Thus, if all talk is only pragmatic the best we can do is to state – naked and in the wind, so to speak – what we are trying to do and to get on about doing it. We cannot justify our goals and we cannot evaluate them except in a limited sense (Hayes, S. C. & Brownstein, 1986). We can only declare them.

Measured against some goals, causal talk can be quite legitimate. Causal talk is useful when the purpose of talk about events is change in these events. Causal talk always assumes some participants and focuses on others. When I say "the spark caused the explosion" I am saying "avoid sparks if you wish to avoid explosions." Such talk is not making an ontological claim, or at least it should not be. This particular causal construction (the cause was a spark) would only be useful when combustible material, oxygen, temperature, and so on do not vary enough in the current context to provide useful guidance to a listener with regard to the avoidance of explosions. Conversely, if I am welding combustible metal in a vacuum and there is an explosion, I might say "the loss of vacuum caused the explosion." With regard to avoiding explosions, sparks become context when welding, joining other contextual features such as heat or combustible material. In terms of the goals of causal constructions these contextual features are no longer an issue. Instead the causal issue now focuses on the ways that oxygen can participate in the situation.

Stripped of its ontological claim, causal talk is especially useful only when control of phenomena is a goal of analysis. There can be no basis, however, to argue that causal talk is relevant only to applied science and not to basic science (Kantor,

1961). This would be true only if basic science *cannot* have the goal of control of the phenomena of interest. Such an assertion would be dogmatic, however, because there are no non-dogmatic grounds to argue that some goals are more legitimate than others. Basic science *need not* have control as a goal, and when it does not, causal talk will not be helpful. But it *can* have such a goal. It is *perfectly legitimate* to have such a goal. And when it does causal talk can be helpful.

The careful avoidance of causal talk can be useful if control is not a goal and one wishes to take a descriptive approach. Part of the value of field theoretical perspectives lie in the way that they focus our attention on features of a total phenomenon that are often missed in normal discourse. Even then, however, we must not let ontology creep in through the descriptive door. There are no "events" to describe independently of our histories with regard to the abstraction of events, and the pragmatic impact of such abstraction can only be seen in transitions from one field to another field.

Conclusion

I have argued that any attack on causal talk is inconsistent with field theory unless the attack simply specifies the pragmatic weakness of causal talk as measured against a particular valued outcome. Indeed, making room for causal talk is helpful to field theoretical perspectives because it can help inoculate adherents more generally against unwarranted reality claims.

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Article

Consciousness Reconsidered

Mark A. Swain
University of Nevada

A problem exists in behavior analysis. It is not a new problem and has proven itself to be a vexatious thorn which nobody has removed. This problem has created rifts between psychologists that stand staunchly impervious to attempts at resolve. The problem is consciousness. In the following pages I would like to clarify my philosophical positioning on the topic of consciousness as it relates to the scientific study of behavior. This topic has been chosen because it seems to be present in the most deep-rooted discords that exist between behaviorism and traditional psychologies.

Behavioral Science and Traditional Psychology

To most psychologists, behaviorism is a hopeless post-positivistic and crude analysis of motor movement - rat psychology, lever presses and food-pellets. However, for those intimately involved there is a richness and beauty which far exceeds these common descriptions of our science. Yet, we seem to compartmentalize ourselves into a tiny group, studying what interests us and publishing in "behavioral journals" which "behaviorists" read - while at the same time reserve little room in our intellectual harbor for so-called "mentalism". Is it possible that we too tend to limit our understanding of other psychological fields as they limit ours. Perhaps a conceptual reconstruction of consciousness is in order.

Denial of Consciousness

Watson (1925, p. 3) criticized introspection and claimed that "consciousness is neither a definable nor a usable concept; that it is merely another word for "soul" of more ancient times." Zuriff (1985, p. 15) also recapitulates that the behaviorist movement for the most part rejects the notion of consciousness "as some non-physical substance" which is a left over "residue from earlier unscientific mystical belief in spirit or soul." Perhaps the rejection of consciousness is a wise scientific decision for which credit should be due. Then again, where are the prizes and accolades

from the rest of psychology and the scientific community? Why are the masses not impressed with our rejection of consciousness? Perhaps Klein, (1984) echoes some of this sentiment when he makes the claim that Watson failed to recognize a "dividing line between man and brute." Ironically, these were Watson's actual words, he indeed, saw no dividing line. Watson was emphatic that his reactions against consciousness was a reaction against the longstanding religious tradition of dualism. It is still true that behavioristic reaction against consciousness is in part their reaction against dualism (e.g., Kantor, 1924) however, some see this as an over reaction and therefore dismiss behaviorism and behavioristic over-reactivity (Lycan, 1987). If we are being dismissed perhaps we should investigate. The notion that behaviorists are "right" and everybody else is "wrong" (although romantic) is a dogma that we need to address.

Both Stephenson, (1953) and Zuriff, (1985) question the tactics by which consciousness is rejected by the behavioral movement. Stephenson laments that the rejection of consciousness has also resulted in the neglect of studying "personal subjectivity". Stephenson (1953, p. 87) went on to say ... "[behaviorists] take pride in ridding themselves of subjectivity: a corpse would be more welcome at a wedding than anything subjective in a conditioning experiment" Zuriff (1985, p. 15) contends similarly, that behaviorists tend to question the ontological status of consciousness, while at the same time, address methodological problems associated with it's study, namely, that it cannot be studied "objectively." If the ontological existence of consciousness is questioned or denied, why then are methodological concerns at issue? Perhaps there is confusion as to what is meant by the word "consciousness." This is my position.

The Meaning of Consciousness

Although some contend that consciousness exists ontologically as a non-physical entity (Underwood and Stevens, 1981, p. vii) my radically naturalistic

positioning holds that consciousness, conscious, conscience, knowing, awareness, experience, and subjectivity are all interbehavior. Therefore, investigative efforts may (and should) be applied to their description. As Kendon Smith (1969) wrote, "the history of science attests plainly to the fact that any investigative effort gets along better if it knows exactly what it is talking about" – therefore, we continue with just this task.

The Etymology of Consciousness

The following definitional talk comes from the Oxford English Dictionary (unless otherwise noted). Consciousness refers to "joint or mutual knowledge" not to an entity. Conscience is derived from the Latin *conscire* "con- together + scire to know; thus *conscire* ... to know along with another...[or]... to know with oneself only." St. Thomas Aquinas also refers to this definition of conscience in his 13th century essays on "truth" and he further refers to conscience as "the application of knowledge to something" – he also specifically refers to conscience as an act "conscience cannot denote any special habit or power, but designates the act itself." Knowledge or know is derived from the Latin *cognoscere* meaning to "know by the senses" - the definition refers to "perceive (a thing or person) as identical with one perceived before...to recognize... or distinguish (one thing) from (another)." This definition of "know" straddles much of what behaviorists mean when they say "matching to sample." Therefore, knowing implies some act of discrimination. Consciousness must therefore refer (in part) to discriminative behavior. The dictionary also implies that conscious and awareness are synonyms. The definition of awareness then refers one back to consciousness in the following manner – "The quality or state of being aware; consciousness." The word consciousness also is related by nomenclature to conscious experience. Experience is derived from the Latin *experientia* – which translates "to try, put to the test – thus one is not surprized to find the following definition of experience: "The action of putting to the test." Therefore it is submitted that "to put into experience" is to "put into action."

This etymological description implies that the word consciousness, and it's cognates conscience, know, awareness, and experience all are defined by some sort of action or behavior. Why then is there a long tradition upheld by behaviorists of referring to these words in italics as if they were impure or implied something unscientific? Skinner, (1953) and Kantor,

(1924) were both adamant that these constructs were troublesome because they were too often reified by traditional philosophies and then transmuted into entities which somehow transcended events. Skinner used the term "explanatory fictions" to refer to inner constructs which are used to explain behavior. Kantor (1969) was clear when he wrote "...all psychological activities are directly observable and can be described without reference to consciousness and introspection." Ryle (1949, p. 13) stated that "as the faces of coins are either heads or tails...so, it is supposed, some existing is physical existing, other existing is mental existing." The problem of consciousness is that it has been traditionally condemned to a non-physical mental existence.

Evolutional Continuity

Naturalists who adhere to a monistic philosophy have argued that there is no reason to assume that there exists both a physical realm and a non-physical realm (e.g., Kantor, 1924, 1926; Ryle, 1949). Kantor (1959, p. 43) developed what he termed a Schema of Evolutional Continuity which was designed to reveal, outwardly, that only physical things and events exist. The first evolution encompasses the development of the planets, stars, chemical elements, chemical compounds and the various chemical processes and was thus referred to as inorganic evolution. The second evolution entails phylogenetic evolution which comprises the evolution of organism-environment adjustments and adaptations along with the evolution of species, genera phyla and the further development of the planets. The third evolution (Ontogenetic Evolution) consists of the "embryological development of individual organisms". The fourth evolution (Interbehavior History) involves the "evolution of acts, traits as responses to objects, conditions, and institutions and the development of stimulus and response functions." Kantor, (1959) used this continuum to argue that all interbehavior is continuous with all other interbehavior and all sciences are related by this continuity. Kantor (1959, p. 42) went on to say "There is no point, in other words, where transpatial processes suddenly appear, – for example 'consciousness'. If anything actual is meant by the term 'consciousness' it must be an interbehavioral field". Globus (1976, p. 290) made a similar argument when he stated "...there is no place to arbitrarily draw a line (or even a range) in a hierarchy of systems increasing in complexity, above which we can say that mind occurs and below which it does not." Ryle (1949, p. 199) similarly

commented: "Nothing but confusion is achieved by labeling worlds after particular avocations." This is precisely the problem with the traditional conceptualization of consciousness, it has been defined as "unobservable and intangible, hence known only through its manifestations" (Kantor, 1924, p. 32).

The Diphaneity of Epiphenomenalism

The traditional separation of consciousness from worldly events has been associated with Descartes. Stephenson (1980, p. 885) suggested "...over the centuries, until the time of Descartes, conscious was a matter of being conscious with... [meaning shared knowledge] ... Descartes changed this in his separation of mind and matter by putting what had hitherto been completely public (but unmentioned) ... into each and everyone's own unshareable mind, as consciousness, unsharable with anyone." The 1900th century epiphenomenalistic view of consciousness, although more naturalistic, accommodated non-physical consciousness by viewing it as a by-product of brain metabolism. This "by-product" consciousness although the product of a physical system was postulated as having no reciprocal effect on its physical originator. As Huxley (taken from James, 1890, p. 135) remarked, "...consciousness would appear to be related to... body simply as a collateral product of it's working, ... without any power of modifying that working ... there is no proof that any state of consciousness is the cause of change,... the brain ... is the immediate cause." This theory, known as the Conscious Automation-Theory, clearly allows for the non-physical consciousness, however, impugns any scientific investigation towards its understanding because of its superfluous, non-causal, existence.

James (1890, p. 147) questioned the epiphenomenalists portrayal of consciousness, believing that consciousness must serve some biological function. The Jamesian perspective heavily inspired by Darwinian evolutionary thinking postulated consciousness as an agent that navigated complex adjustmental interactions. This regulative function of consciousness was thought to be necessary in order to provide direction to the brain, which was, according to James, biologically unstable. James (1890, p. 144) refers to the instability of the brain in the following passage, "...let consciousness only be what it seems to itself, and it will help an unstable brain to compass its proper ends." James argued that the cerebral hemispheres were so intricate and complex that they could

not possibly operate as to keep Man alive without some selecting agency. As James (1890, p. 144) stated: "The brain is an instrument of possibilities, but of no certainties. But the consciousness, with its own ends present to it, and knowing also well which possibilities lead thereto and which away, will, if endowed with causal efficacy, reinforce the favorable possibilities and repress the unfavorable or indifferent ones."

Although, James agreed that consciousness was an epiphenomenon of brain activity, (like that of Conscious Automation Theory) he vehemently opposed the view that it did not function in a causal role. However, James (1890) denied that there exists some pure conscious "stuff" detached from all physical coordinates. According to James, (1890) "I believe that 'consciousness,' when once it has evaporated to this estate of pure diphaneity, is on the point of disappearing... It is the name of a nonentity, and has no place among first principles." James appears to have sat on the fence, not willing to give up dualism yet not willing to attribute all psychological processes to it. Kantor (1924, p. 32) viewed this type of position as "...a last refuge and sanctuary into which the psychologist plans to escape from responsibility... of interpreting his facts and making them into definite materials of natural science." Today, behavioral psychology still is reliant on the biology of the organism to account for certain behavioral phenomena such as reinforcement "sensitivities" which supposedly are passed phylogenetically to aid in species survival. Consciousness and private events have also been given to biology. It seems that psychology (including behavior analysis) has a prolonged history of giving its unexplained to biological science.

Biological Tradition

As Skinner (1989, p. 60) mentioned, "a central issue in early behaviorism was the existence of consciousness" - this he explains was a product of cultural bias towards the preoccupation with internal explanatory agents. Skinner (1953, p. 258) also commented on dualistic culture when he described the "distinguished men" and their efforts to describe the structure of the universe: "The scientists humbly admits that he is describing only half of the universe, and he defers to another world ... a world of mind and consciousness ... for which another mode of inquiry is assumed to be required. Such a point of view is by no means inevitable, but it is part of the cultural heritage from which science has emerged." However, Skinner (1953, 1989) does not explain consciousness,

private events, or mental events entirely by cultural tradition, instead he urges that they are in fact biological. According to Skinner: (1953, p. 281) "The modern counterpart of the study of mental events in a world of consciousness is the study of the action of receptors and the afferent and central nervous system ... [and] ... "I preferred radical behaviorism, which accepted the existence of private states, but as states of the body, the study of which should be left to the physiologist (Skinner, 1989, p. 110)." Behaviorism has been sharply criticized for its reliance on physiology to explain its so-called private events (e.g., Kantor, 1947). Ryle, (1949, p. 12) explained the situation as: "They [episodes of private occurrences] can be inspected neither by introspection nor by laboratory experiment. They are theoretical shuttlecocks which are forever being bandied from the physiologist back to the psychologist and from the psychologist back to the physiologist. I suggest that Ryle is correct in his humorous, yet, scathing assessment. Behavior analysis has, like James, sacrificed its own to biology.

Event-Bounded Consciousness

Science and Meta-Physics

It is not appropriate to assert that science does not deal with meta-physical content. Scientist-philosophers have been grappling with meta-physical queries for at least two thousand years. There is a difference in explaining things and events meta-physically, and explaining things and events which are referred to as, meta-physical. I submit that consciousness has long been referred to as meta-physical, explained meta-physically, but is not necessarily, meta-physical.

For the duration of this paper, I will describe the categorical mistake associated with consciousness, provide a naturalistic conception of consciousness, and then address the issue of psychological subjectivity.

Category Mistakes

For those who reject dualism (or safely assume it does not impact scientific lawfulness) a mistake related to the first is suspected. Ryle (1949, p. 16) depicted a scene where a visitor is given a complete tour of the Oxford campus. During this tour, the visitor sees the beautiful buildings, meets the people, goes to the libraries, views the museums, goes to the sporting fields, etc. Yet when the tour is completed, asks, "But where is the University?" According to Ryle, (1949, p. 16) the visitor has erroneously assumed that the University is another element in the set of elements which comprise the university. This

he refers to as a "category mistake." I suspect the same occurs when the behavior analyst studies the behavior of an organism in all its delicate elements of antecedent control, response topographies, operant development, consequential control, schedule performance, discriminative control, elicitation, conditioned stimuli, habituation, potentiation, stimulus gradients, generalization, differential reinforcement, reflexes, conditional reflexes, etc., yet, then is asked, "But where is consciousness?" Hopefully, the behavior analyst will say, "everything I study is what we refer to when we use the word consciousness. There is no separate category called consciousness. You see, I cannot pick it up and show it to you. That would be a silly notion!" In the words of Karl Lashley, (1954, p. 425) "There is no one criterion of consciousness."

Coordinates of Consciousness

If one were to map a brain using a Cartesian coordinate system, the brain would be extended with definite parameters. However, if one wished to map consciousness on the same coordinate system, consciousness, would be unextended. To explain, extended implies that something must be "here" and not "there", therefore it must have parameters, if not, it is spatially unextended. Unextended therefore implies that the "object" in question is either "no where" or "everywhere" (Globus, 1976, p. 278). Consciousness I submit is unextended, in both definitional senses. If one speaks of consciousness as a meta-physical entity (a super naturalistic choice determiner) then it is unextended in the sense that it is "no where" – simply a fictional substance, like phlogiston. However, if one speaks of consciousness as complex interbehavior, then it is "everywhere."

Criterion of Consciousness

In order for something to be (or act as) conscious, it must be physically extended, it must be somewhere. Therefore, it is possible that there exists a continuum of consciousness which parallels Kantor's Schema of Evolutional Continuity. The rock sitting on a hill satisfies the first criterion of consciousness. If one wishes to recognize the rocks "thereness" and specify that the first criterion is satisfied, so be it. However, psychologically we mean more when we speak of consciousness. We must also have a living creature. A living creature is defined by "an object which will absorb substances from some environment and synthesize from them a replication of each of the substances which comprise the object" (Smith 1958). Thirdly, in order to psychologically be spoken of in

terms of consciousness the living organism must demonstrate cognoscere - "to know by the senses" - to discriminate. Therefore, if something is (1) physically extended, (2) a living object and (3) can discriminate, it can be spoken of in terms of consciousness. In this fashion, consciousness need not be denied.

This conception does not violate any behavioral or scientific principles. Behavior is what behaviorists proudly study - and yes, it is conscious behavior. We do not study dead non-conscious bodies rolling down hills or falling to the ground. We do, however, study situated activities of living organisms. Therefore, the behaviorists hope for a continuity between the organic and inorganic sciences is recognized and supported. Consciousness does not represent anything non-material that differs radically from that of which other sciences investigate. Consciousness is not an entity, it does not guide, it does not interpret, or cause. It just is.

Scientific Subjectivity

The area of subjectivity seems to have little credit in the behavioral movement (Zuriff, 1985). Perhaps, subjective reports remind us too much of the traditional introspection offered as a means of accessing some inner world of consciousness or mentality. The psychological concept of inner and outer is a distinction that has outlived its usefulness. The notion that some behaviors are outward (or public) and some are inward (thus private) is another verbal bifurcation of nature which has not yielded much in the way of understanding. The traditional dualistic "inner and outer" distinction is elementary, yet, non-sense, as Ryle (1949, p. 12) explains, the "antithesis of outer and inner is of course meant to be construed as a metaphor, since minds, not being in space, could not be described as being spatially inside anything else, or as having things going on spatially inside themselves." If the distinction between traditional "outer and inner" is rejected by behaviorists why then is the distinction between public and private so important to behavioral psychology? Especially, in light that it has been decided that private events are events for the physiologist to study! Perhaps, it is important to take a lesson from Bretando, who recommended that psychology stand on its own, and explain actions on psychological grounds.

There is little to doubt that people have personal reactions to things and events. However, introspective self report has been largely avoided as a means to study this type of behavior. Both Stephenson, (1953)

and Zuriff, (1985) agree that the behavioral avoidance of self report is largely pragmatic. Talk involving one's personal views and opinions seems to be "loose", "sloppy", "variable", and is unscientific, not objective. As Stephenson, (1953, p. 88) stated, "According to logical analysis 'subjectivity', when it is not confused with 'mind,' is merely an indication of undependability, variability, and absence of attainable 'constant relations.' The scientist is engaged in separating what is dependable from what is undependable from what is unstable or variable, and 'subjectivity' is the name we give to the latter, the untamed horses of the scientific ranch." When one reads this quote by Stephenson it is difficult not to think of T. S. Kuhn's work on scientific evolution in which he describes "normal science" and the strict adherence to proper paradigm pursuits. According to Kuhn, (1970, p. 37) "...one of the things a scientific community acquires with a paradigm is a criterion for choosing problems...To an extent these are the only problems that the community will admit as scientific or encourage its members to undertake."

Stephenson (1953, p. 91) argued that it is not entirely correct to reject introspection: "The argument that, because introspection is unreliable, we must therefore give up the internal framework is a cliché of the American Behaviorists. The logical position, however, is merely that introspection proved unprofitable, undependable, and generally unreliable; therefore, either the method was inadequate, or its objectives chimerical, or both." Stephenson (1953) saw no need to completely abandon the study of subjective personal opinion. He spoke of subjectivity in terms of consiring, in the communicational sense - simply, someone speaking from their own subjective point of view. Ryle (1949) spoke of the first personal pronoun, "I" as being unique, "I...always indicates me. 'You', 'she' and 'they' indicate different people at different times. 'I' is like my shadow; I can never get away from it...there is no mystery about this consistency, but I mention it because it seems to endow 'I' with a mystifying uniqueness." As such, subjectivity need not imply anything mysterious. The q-technique that Stephenson (1953) offers in which only "concrete behavior" is studied without resorting to the explanatory "brain" or "physiological correlates" of behavior is both naturalistic and consistent with rational scientific tenets. Perhaps most behaviorists are not familiar with Stephenson's (1953) *The Study of Behavior* because the study of subjectiv-

ity, even if approached naturalistically, with objective and measurable behavior, it is simply not supported within our "normal science" of behaviorism.

Conclusion

These arguments in no way imply that behaviorists should abandon the positions by which they have achieved sound scientific descriptions of behavior. The retreat back to earlier epochs of spiritualisms and other fictional inner agents would be a horrible mistake. Skinner's (1961, p. 334) claim that "behavior is an acceptable subject matter in it's own right" still rings very true. However, we need to better address "personal" issues such as consciousness and subjectivity without dogmatically reminding the public that these issues are illegitimate or "the action of receptors and of the afferent and central nervous system" (Skinner, 1953). I firmly believe that behavior analysis is capable of addressing these issues while standing on its own. The analysis of conditioned seeing, conditioned hearing, and other implicit phenomena is just a beginning.

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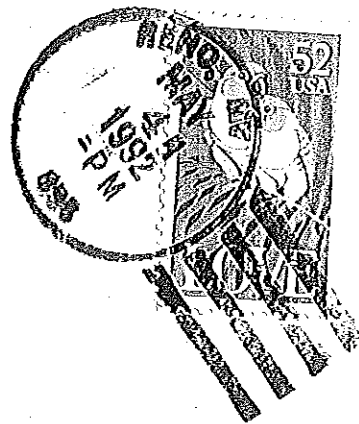
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