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ON THE INCONGRUENCE OF THEORY AND PRACTICE IN BEHAVIOR RESEARCH: WHAT CAN AND SHOULD BE DONE ABOUT IT[1]

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There are no such things as applied sciences, only application of science.
Louis Pasteur[3]

On December 10, 1904, at the Nobel Prize ceremony, Professor Count K.A.H. Mörner[4] introduced I.P. Pavlov, recipient of the prize for Medicine, as follows:

> Your Majesty, Your Royal Highnesses, Ladies and Gentlemen, The medical sciences are mutually interdependent. Progress in one field is often closely associated with development in others….It is not always such progress, as immediately are useful and of benefit, which should be considered as especially important; this character can also be attributed to those which are themselves less spectacular but form the basis for others which are then only a further development of it.

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1. Originally this paper was presented at the 8th Biennial Guadalajara Symposium: *Theory, basic and applied research, and technological applications in behavior science: Conceptual and Methodological Issues,* University of Guadalajara, Mexico, February 2004. Subsequently the proceedings of the symposium were published under the same title, in a book edited by Burgos and Ribes (2004). Unfortunately due to a mishap that is very rare in the hands of these editors, a large section of the paper failed to be printed in the book so that there the paper comes a dead stop well before its end. To compensate the loss, the editors have graciously invited the paper to be published in full in this Journal. It appears here in a very slightly modified form. Prior to this invitation I included the missing portions of the paper in an invited address given at the Annual Conference of the Association for Behavior Analysis. This note is to explain the unintended repetition, a practice which I have always disdained.
2. Department of Psychology, Auburn University, AL 36849-5214, USA. E-mail: harzepe@auburn.edu Phone: +334 844-6482, Fax: +334 844-4447.
3. *Il n'existe pas de sciences appliqués mais seulement des applications de la science.*
4. Royal Caroline Institute, Stockholm, Sweden.

> The aim of science is the acquisition of knowledge, the value of which should not be measured by the ease with which it can be brought immediately into practical usefulness…'

Hundred years have passed since then and in that brief time—brief in the big timeline of history—we have moved far from this principle. The principle was tacit from the earliest times when knowledge began to be formalized, to the legacy of the intellectual and scientific giants of Renaissance, and up to about the middle of the twentieth century. Through the millennia we applauded our engineers who built wonderful devices and structures that improved the conditions of our lives, as we did our scientists and thinkers for their marvelous discoveries that at least in part satisfied our never-ending thirst for knowledge. Our departure from all this has not been a happy turn; it has not been towards any bright horizon of continuing progress but rather towards greedy dominance of what the scientist investigates by criteria of what quick profit will result from it.

This is not to neglect, of course, the relation between acquisition of new knowledge and the material value of some of that knowledge, at any given time. The two, as Professor Mörner had noted, interact, as they have done so through the ages. Legend tells us that Archimedes ran out of his bath shouting 'Eureka' when he discovered the relation between the volume of an object and its density. He was delighted because that discovery would enable him to determine whether the crown ordered by King Hiero was made of pure gold or gold adulterated by the jeweler. Archimedes, apart from his mathematical work, also made some 'engines of war' that were used in the war against the Romans. However Plutarch reports that, 'these machines [he] had designed and contrived not as matters of importance, but as mere amusements in geometry' Moreover he did invent the ingenious implement, marvelous in its simplicity, for moving water uphill; namely the Archimedes' screw[5]. None of this has been sufficient, however, to label Archimedes an 'applied …ist' of some sort. He was a great, possibly the greatest, mathematician-scientist of western history. On the other hand, from Renaissance to mid-20th century, the careful observations and measurements of Copernicus, Tycho Brahe, Kepler, and Newton and the conclusions drawn by them and later by Einstein, led to no practical benefit. Benefits of tangible sort from that body of knowledge ensued

5. Permit me a personal note. From childhood I have marveled at the cleverness of this device, and admired the beauty of its simplicity. Paraphrasing Sir Thomas Beecham in a different context—music—it makes me happy and hopeful to think that such a thing could be invented by a human being.

in the form of by-products, and began to emerge only after October 4, 1957 when the Soviet Union launched Sputnik I.

Thus began the age of space exploration, coupled with an almost unnoticed turmoil that changed the conditions under which science, scholarly theorizing, and inventing are done.

The beginning of the age of space exploration marked the rapid acceleration of the dominance of scientific research by those who control the purse strings in western societies[6]. No doubt this is due, in the first place, to the fact that *some* scientific research became very expensive, beyond the reach of the individual scientist or team of scientists, or even individual universities[7]. This development, although quite understandable, produced a number of outcomes that had unfortunate effects—perhaps unforeseeable and even unavoidable effects—on the direction that the sciences have taken from the second half of the 20th century to the present. In democratic and quasi-democratic countries[8] the elected officials facing the daunting task of allocating state and national funds to scientific research found it necessary to require some anticipation of benefit[9] that would accrue from such research. This, it would seem, was to be expected: they needed to demonstrate it to the electorate. The control did not, however, remain confined to extraordinarily expensive research, but rapidly blanketed all academic work. Thus the process reached its peek when political power, in collaboration with equally powerful multi-national commercial companies, acquired control over *all* of the work of the universities: what the academics should be encouraged to investigate, how and by whom the research interests should be determined, and, in sum, what should the development of knowledge include. These conditions have reached a state such that the present practices of our universities would not now permit a young Albert Einstein to be given tenure or be appointed at a rank above assistant professor[10], or an Edward Gibbon who took eleven years to write the *Decline and Fall of the*

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6. I say 'western societies' referring to that particular time. This is not, however, to overlook the fact that since then the practice has rapidly spread across the world.
7. Numerous other factors of this kind are involved here such as the increasing dependence of universities on public moneys under political control. However, considering them here takes us away from the main brief of this paper and will not be discussed.
8. What is described here also took place in non-democratic countries, for example the Soviet Union. There the accounting was to be made only to those in power.
9. Sadly, most western cultures no longer see growth of knowledge as a benefit—'benefit' has come to mean tangible gain: something one can eat, drink, be entertained by, etc.
10. Einstein graduated from the Technical Institute of Zurich in 1902 and worked in the Swiss patent office while working on his *ideas*—not empirical research—on the problems of Physics. He received his Ph.D. from the University of Zurich in 1905, and published three papers, described by Stachel (1998) as 'three papers that changed the face of physics'. On the strength of these papers he was appointed professor at the University of Zurich, and awarded the Nobel Prize in 1921.

Roman Empire to be appointed to a university position let alone be tenured or promoted[11].

It is against this backdrop that attempts to develop new kinds of psychological therapy, focused on the *behavior* of the patient[12] emerged. At first these were generally, and quite descriptively, known as 'behavior therapy'. As they developed, however, there arose fine distinctions in their varied orientations. Thus, there came into existence groupings under the titles 'behavior therapy', 'cognitive behavior therapy', 'cognitive emotional behavior therapy', 'behavior modification', and most recently 'applied behavior analysis'. These distinctions are not, however, substantive. They mainly rest on differences in minor detail concerning the way their practices are explained, and the words they use in those explanations. They do not represent progressive development as might be expected in sciences but continue to exist side by side.

A simple search of those titles on the Internet brings up a would-be serious scholar's nightmare, with numerous companies, institutes, centers, etc. vying for customers, some with music embellishing their web pages, with various claims for the soundness of their theoretical allegiances, and all asserting that they are based on Science—with capital S. For example one claims "Cognitive Behavior Therapy has become the preferred treatment for most emotional and behavior problems[13]"; another claims "Rational Emotional Behavior Therapy works best for individuals desiring a scientific, present-focused, and active treatment[14]"; another explains "Behavior Modification is used to describe a program that relies on *rewarding* positive behavior in order to *increase the frequency of such behavior*[15]" ; and yet another asserts "Applied Behavior Analysis is a treatment based on the *principles of operant conditioning,* termed by the behaviorist, B.F. Skinner. The professional literature has documented ABA therapy as the most effective treatment for individuals with developmental disabilities[16]" (my italics).

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11. In recent decades this trend has taken yet a further large step, with the business/political power complex acquiring control over what shall be taught to university undergraduates, and not infrequently, how. The argument is clear: a state's economy depends on the business productivity, and the productivity depends on employees trained appropriately—that is, according to the needs of the employers—by the universities. The employees thus produced are referred to as 'human resources' equivalent, it would seem, in their value and importance to other indispensable resources such as oil and gas, farm produce enhanced by chemicals and the chemicals themselves.
12. In recent decades 'patient' has been out of favor to name the person to whom therapy is being provided. However, the most commonly favored term, 'client' is even more troublesome, smacking, as it does, of crass commercialism. In this paper the word 'patient' will be used although this, too, does not fit well in some situations.
13. Web page by John W. Bush.
14. Web page under Rational Emotive Behavior Therapy.
15. Web page by Eileen Bailey.
16. By Stacey Enfield in Behavior Analysis Treatment & Training.

This is a picture of intellectual chaos. Such a state is not unusual in the history of Psychology, but it is not characteristic of any science worthy of that name. Differing and competing theories do, of course, exist in the sciences but they are about how empirical evidence are to be interpreted, and they give way to each other when new evidence bearing on them emerges[17]. The claims to scientific foundation by the different behavior therapy groupings[18], if they are to be considered seriously—and so they should be, especially because they affect the lives of countless people, young and old—demand careful consideration. We should note at this point that each of them is successful with *some* of the individuals they treat,[19] especially if they are careful in selecting their subject population in the light of their past effectiveness. The possibility is very remote, close to nil, however, that these groupings are each based on a different 'science'. If there is a science at their base, the differences are likely to arise from different understandings and interpretations of it. Is there such a science, and if so what is it?

Science, of course, is not mere knowledge, no matter how accurate it may be. It is knowledge that has been ascertained by observation or experiment, critically tested, systematized, and brought under general principles. Only knowledge that is ascertained by observation *and* brought under general principles and thus systematized is science. We can accurately observe that there are *n* people in this room, and we can experiment to discover if, for example, background music affects how often the people in the room smile. Neither information thus obtained, by itself, is scientific knowledge. By the same token, the observation that simple praise does not reward John's actions but bacon bits do affect his actions, while accurate, do not constitute scientific knowledge[20]. This would seem self-evident but it is uniformly overlooked by the practitioners of any type of behavior therapy. Confusing the distinction between science and knowledge in general has been a major source of problem concerning the claims that behavior therapy—of each kind on its own right—is based on science.

All varieties of behavior therapy use two main sets of knowledge: (i) classical conditioning and (ii) the effects of rewards; none of them is without these two although they differ in other respects. Classical conditioning entails a body of sound knowledge firmly established in Experimental Psychology and

17. Newtonian and Quantum Mechanics accounts of matter continue to exist side by side although they are incompatible; however they are about two different sets of phenomena, and to date no evidence has become available to bridge the gap between them.
18. I used the term 'grouping' because none of the others, such as 'approach', 'application' etc. accurately describe them, and falsely suggest a basic unity which does not exist.
19. No therapy, whether medical, surgical, psychological, etc. is successful with every person treated.
20. This is an actual example of a boy in an institution.

in Physiology—which was its original home. It is part of scientific knowledge but not a science of its own standing. Knowledge of the effects of rewards is different, however, in that it is simply about a set of phenomena commonly known, prior to any recourse to science.

History bears witness that through the millennia knowledge of rewards (and of the companion phenomena, 'punishments[21]') was a powerful tool in the hands of many, enabling them to influence the fate of individuals at one level and nations at another. Kings, despots, benign leaders, and other characters of history have made astute use of rewards, and kingdoms have risen and fallen as a result. What they knew, however, though enormously powerful, was not science. A simple anecdote will illustrate the point:

Teacher. *What did Isaac Newton discover?*
Several hands go up.
Teacher. *Yes, Brian?*
Brian. *He discovered gravity.*
Teacher. *Before Newton didn't people know that unsupported objects fall?*
Long silence. Then Jill puts up her hand.
Jill. *Well he wrote about it.*
Teacher. *Yes. In fact he did more than just write about it, but we are getting close to the point* [22]

Any claim to scientific knowledge in the use of rewards must rest, therefore, on what science systematically has made of that knowledge. Thus, we have two different, though related, sorts of knowledge concerning rewards: (i) general knowledge of the effects of rewards, and (ii) scientific knowledge of the effects of rewards.

Edward Thorndike (1911) is customarily credited with being the first scientist who did systematically investigate the effects of rewards. He did not, however, discover rewards—just as Newton did not discover gravity—but taking the commonplace knowledge as given, investigated the phenomena of learning and the effects of rewards on learning. In ensuing decades rewards were extensively used in experimental research into learning. That was the era of grand learning theories which produced anv enormous body of literature consisting almost entirely of animal experiments with varieties of mazes (see, for example, Hilgard and Marquis,1940,1961 and Estes, Koch, Mac-

21. For brevity I will not consider punishment separately in this paper. Much of the comments here about reward apply equally to punishment.
22. I have had this sort of conversation, many times, in both graduate and undergraduate classes. It helps classes to start thinking about the distinctions between ordinary knowledge and scientific knowledge.

Corquadale, Meehl, Mueller, Schoenfeld, and Verplanck, 1954). Although the earliest attempts to develop a behavior therapy did take its impetus at least partly from Hull's theory of learning, none of the details discovered by the experimental research reported in that literature had connection with the practices that have developed in the behavior therapy groupings.

Against this background, in current times, the approach that is the most prominent, is applied behavior analysis. It has the claim solely to be based on a clearly identifiable body of scientific literature, the experimental analysis of behavior that more recently has come to be named behavior analysis,[23] presumably in recognition of the fact that all behavioral/scientific work does not, indeed cannot, entail experiments. This scientific literature, pioneered by B. F. Skinner, differs in a number of significant ways from previous theories and related investigations concerned with the effects of rewards. The prior work concentrated on the phenomenon of learning, and for a time the competing and complementing theories of learning were the dominant focus in psychological literature. A major contribution of Skinner was to broaden this focus, so that beginning with the publication of his 'Behavior of Organisms' (my emphasis) in 1938 the subject matter of experimental research became behavior, no longer confined to the narrower range of phenomena entailed in learning[24].

Behavior analysis has at its foundation the following concepts[25]: 'contingency', 'consequence' (of behavior), 'response', 'reinforcer' (and the related concept, 'reinforcement'), and 'discriminative stimulus'. The last three, response, reinforcer, and discriminative stimulus go into 'three-term contingency' which specifies a particular relation between them, and forms, according to the basic theory of behavior analysis, the building block of behavior patterns. The empirical content of these concepts generally have been known in their empirical-scientific form since the experiments of Thorndike, and their more strictly specified forms since the publication of Skinner's first book in 1938. Since applied behavior analysis claims to apply the 'science of behavior analysis', the support for the claim must rest on what developments of behavior analysis have refined and enriched its practices in the ensuing seven plus decades. Although the literature of applied behavior analysis is quite voluminous, it is merely repetitive, reporting endless variations of the

23. The literature of applied behavior analysis and the literature of the experimental analysis of behavior are mainly carried by the twin journals, the *Journal of Applied behavior Analysis* and the *Journal of the Experimental Analysis of Behavior* which are owned and operated by the same organization. All this makes examination of the claim to scientific basis of the application possible with reasonable clarity.
24. Curiously, this major turning point in the history of psychology which is due to Skinner's work, has gone unnoticed by the followers of Skinner.
25. 'Notions' or 'tenets' may equally be fitting here.

applications of these principles, reporting, for example, discoveries of what kinds of things idiosyncratically function to reinforce the specific acts of specific individuals. This information is, indeed, helpful to the practitioner who may be searching to find a reinforcer that will affect the behavior of a particular person, but it does not indicate any advance in the scientific basis or in its application in practice.

The picture is not, however, as bleak as at first it may seem. Through the decades it has gone unnoticed that applied behavior analysis has outpaced behavior analysis, highlighting the necessity that the science of behavior analysis[26] must benefit from the reported observations in applied behavior analysis. What is now needed—urgently needed—is not supposed applications of behavior analysis in applied behavior analysis, but the application of applied behavior analysis to develop behavior analysis. The reason for this assertion is simple: applied behavior analysis has served to bring behavior analysis into contact with natural contingencies, and shown up what could not be captured in experimental work with artificial contingencies and their long established but seldom questioned component concepts.

Laboratory experiments in all sciences entail preparations. In such preparations components of the phenomena being investigated are removed from their natural habitat, and for experimental purposes or theoretical reasons they are modified, often into forms that do not occur in nature. The science then tests, with equal rigor, to see if the principles discovered through the laboratory research hold for natural phenomena. It is this second, all-important, phase that has been missing in behavior analysis, notwithstanding the guesses and wishful extensions that have merely rested on the first-phase experimental research. Best examples of the latter are Skinner's influential book 'Science and Human Behavior' (1953), its predecessor 'Walden two' (1948) and sequelae, e.g. 'Beyond freedom and dignity' (1971). These are the reflections of a master thinker musing on the possibilities of the line of experimentation he has initiated. They are not scientific reports.

In the case of behavior analysis the mismatch between scientific research and application of findings to natural conditions is primarily due to the fact that the units studied in experimental research do not exist in nature. In the standard Skinner box the 'response' studied is not 'behavior' in the established sense of the word but it is digitized behavior. Behavior is a highly varied, indefinite set of analogue phenomena. No organism naturally repeats the same

26. I use this term here for convenience but not with the implication that behavior analysis is a science of its own standing. It is, in fact, a part of the confederation of scientific endeavors collectively labeled 'psychology'. Claims to the contrary, and their inevitable result of ignoring developments in other parts of psychology have unnecessarily handicapped behavior analysis, depriving it of much progress that might otherwise have occurred

minute act for hours on end. By the same token, the experimental stimulus studied is almost always a discrete event with clear onset and offset while under natural conditions most environmental events that enter into some relationship with behavior do not have discrete borders. Moreover, in nature none of the biologically vital events claimed to be reinforcers—eating, drinking, sleeping, sexual interaction—occurs in minute snippets. In contrast with experimental research applied behavior analysis had to deal with the mainly analogue events that have no clear borders. As even a cursory examination of the literature of applied behavior analysis will show, the natural conditions have compelled the applied worker to apply the terms of behavior analysis to the phenomena he/she has to deal with, thus inadvertently creating a false impression that a science is beings applied. Thus, we hear of the person who is trying to give up smoking being treated as follows: 'he is reinforced when he goes half an hour without smoking, then for 40 minutes, and so on;' we hear of the person being given non-contingent reinforcement; and almost always we find that the behavior said to be reinforced is not at all like a response but rather a complex and varied pattern of behavior such as getting dressed, purchasing items from the institution store without assistance, conversing with a neighbor, etc., etc.

In all of this the concepts of 'consequence of behavior', and 'contingency' play important parts and contingency is thought to be clearly distinguished from contiguity. Yet here, too, is we find the concepts oversimplified. What follows an action may be contingent or contiguous, but the dichotomy is not a clear one. For example, a brick falling on one's head when walking in a street is a contiguous consequence[27]. This is, of course, so simple an example that at first sight it seems hardly to merit attention. The simplicity hides, however, shades or degrees of relations that may arise between even the simplest of acts and their consequences. Had there been warning notices about walking there, had the individual personally been warned about walking there, had he previously been fined for walking there, and so on, we would be inclined to assign increasingly greater responsibility to that individual. The basic relation between the specific act and its simple consequence would have remained unaltered, but in each case the factors that go into the occurrence of the effect would be different. Common phrases such as negligence, carelessness, stubborn defiance, and the like may qualify the act and go into explanations of why the incident occurred[28]. They also qualify the complex of factors that go

27. Psychological literature on rewards, reinforcers, etc. often confuses the causation of an event and the effect of that event upon behavior that may be occurring at the time. Of course, the person's walking there does not make the brick fall, but its *falling on the person's head* is a consequence of his happening to be walking there.
28. In natural conditions no event has an identifiable single property, causal or otherwise. By 'natural conditions' I mean all those other than experimental conditions.

into the consequence: in one case the individual may receive compensation and in another, reprobation.

On the other hand acts do, of course, cause consequences and there are, indeed, contingent relations, where the occurrence of an act is a sufficient condition for the occurrence of a contingent event. It is not, however, the necessary condition and it depends on whether the necessary conditions also prevail at the time. Through the 20th century the contingent action-consequence relations have been the most studied in experimental psychology, and since the1940s they came to be termed, a là Skinner, response-reinforcer relations, mostly investigated in experiments with animals[29]. That research has produced a substantial experimental data on the behavioral effects of reinforcement and the complexities involved in the reinforcement process. However, this body of evidence, although meticulously precise in its experimental method and data analysis, has remained confined to the carefully prescribed situations of the experimental investigation.

The present discussion points to the inescapable conclusion that the theory of behavior analysis, i.e. reinforcement theory, is unnecessarily narrow as it stands, and is in need of revision. Such a revision requires new concepts, and reintroduction of some concepts that were eliminated in the early course of the development of the experimental analysis of behavior. Amongst those that need to be revived is the concept of reward. Rewards have identifiable effects on an individual whether presented contingently or not, and the effects when presented contingently are likely to be different from the effects when presented non-contingently. No doubt distinguishing the two conditions and many others that may not be apparent now, is a task for empirical study.

The need for revision of the reinforcement theory has clearly been shown by the literature of applied behavior analysis. That evidence also shows the obvious: natural contingencies will affect behavior regardless of the theory to which the practitioner may subscribe. Now the task is to investigate those natural contingencies. To sum, it is time for applied behavior analysis to be applied in the scientific development of behavior analysis.

29. Strictly speaking 'behavior-analytic' or Skinnerian research distinguishes reinforcement, a technical term, from reward. Only a specific response, such as a bar press is reinforced and only that response is affected. On the other hand, in general usage it is the individual who is rewarded. One of the limitations of reinforcement research is its neglect of the far more heterogeneous effects of rewards as opposed to narrowly prescribed effects of reinforcement.

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