THE SEPARATE WORLDS OF OPERANT AND NON-OPERANT PSYCHOLOGY¹

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Encapsulated schools of thought have occurred in all sciences at some stage in their development. They appear most frequently during periods where the fundamental assumptions of the science are in question. Manifesto papers, acrimonious controversy, mutual rejection, and isolation of other schools' strategies are hallmarks of such episodes. The existence of schools cannot be explained by relegating them to the limbo of "aberration" in ongoing scientific activity. Historical and psychological analysis indicates that similar variables are operating in both school formation and more "normal" science (Krantz, 1969; 1971).

Accurate description of such periods is made difficult by the absence of some necessary historical information; more complete data can be obtained by examining a contemporary illustration. Operant conditioning was chosen as an example of the "school" phenomenon. Through open-ended taped interviews with 35 key researchers² (60% of them held editorial positions on one or both operant journals) a partial reconstruction of operant conditioning's development was made (see Krantz, 1971). In the present analysis, parts of this history³ are used to clarify some of the implications arising from this course of development.

The choice of operant conditioning was suggested by such comments as:

"Sometimes I am concerned by the fact that disciples of operant conditioning have formed a tight little club known as Division 25 (Experimental Analysis of Behavior). Here and in their *Journal of the Experimental Analysis of Behavior* they talk and write to each other, guaranteeing positive reinforcement on a rich, fixed or variable, interval schedule." (Harlow, 1969)

"To many outsiders an operant conditioner is a hardnosed experimentalist who spends endless hours in the enthusiastic analysis of cumulative records from one or two subjects, attacks anything that sounds even mildly theoretical or physiological, ridicules anyone who has ever used statistics of the R. A. Fisher variety and ignores the work of any psychologist who does not publish in JEAB. Not since J. B. Watson's time has any band of behaviorists seemed so assertive in its likes and dislikes and so convinced that its techniques and experimental approach will not only change psychology but in the process reshape the world." (Hearst, 1967)

Thus, the operant conditioning movement is viewed as an insular group of scientists who share a common systematic affiliation and who reject other approaches.⁴ Breakdown of communication, lack of utilization of findings, and particularly, the failure to cite, are the indices used to support this characterization.

MUTUAL ISOLATION: CITATION DATA

"To obtain a quantitative measure of the relationship between operant and non-operant psychology the mean percentage of self-citations per article in the *Journal of the Experimental Analysis of Behavior* (JEAB)—the oldest operant journal, summed across articles

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²The unreferenced quotes used in this paper are taken from the interviews. Anonymity has been maintained because interview participation was contingent upon this condition.

³A few paragraphs from the Krantz (1971) paper have been quoted in the present analysis with the permission of the *Journal of the History of the Behavioral Sciences*.

⁴See Krantz (1971) for a discussion of the adequacy of this characterization from "insider's" and "outsider's" perspective.

Fig. 1. Mean percent of self-citation of JEAB, JVLVB, and JEP across years.

per year, was computed.⁵ The obtained curve, shown with the self-citation of the *Journal of Verbal Learning and Verbal Behavior* (JVLVB), to control partially for JEAB being a relatively new journal, and with the self-citation of the *Journal of Experimental Psychology* (JEP), one of JEAB's and JVLVB's source journals and the highest self-citing journal in psychology (Xhinghesse and Osgood, 1967) is shown in Fig. 1. Clearly, the slope of JEAB's self-citation curve is greater than that of JVLVB, both compared to the slightly decreasing rate of JEP's self-citation.⁶ It would not be over-interpreting this data to say that JEAB, by increasingly citing itself, is isolating itself from other approaches."

"Reciprocally, JEAB's findings are being isolated by other approaches in psychology. In two recent learning texts, Deese and Hulse (1967) and Hall (1966), JEAB citations constitute 0.012 and 0.006 of all citations. In contrast, JVLVB articles are cited 0.026 and 0.018

Fig. 2. Mean percent of reciprocal citation of JCPP to JEAB and JEP to JVLVB across years.

in these two books. Although text citations mirror to some extent the accepted "paradigm" of a field (Kuhn, 1962), they often lag behind the changes occurring in a field. A more current index is the degree to which JEAB's major source journal, *Journal of Comparative and Physiological Psychology* (JCPP), cites JEAB.⁷ As shown in Fig. 2, JEAB's publications are not being incorporated into its source journal, JCPP, to the same extent as JVLVB's publications are in its source journal, JEP.⁸ In summary, the citation data indicates that there is a mutual isolation of operant research, by JEAB, in its high self-citation and by other approaches in their failure to cite operant findings."

⁷Respondents expect that the new journal, *Learning and Motivation*, will replace JCPP as the primary source for JEAB with high reciprocity of citation between them. Editorial changes in JCPP have led some authors interested in JEAB findings to publish in this new journal.

⁸As shown in the table below, a 2 by 4 analysis of variance indicates a significant difference in the incorporation rate of JVLVB vs. JEAB citation by their respective source journals. This effect is interactive with years with the major divergences appearing in 1968 ($t = 5.70$, $df = 597$, $p < 0.01$) and 1969 ($t = 2.92$, $df = 527$, $p < 0.01$).

Source	MSS	df	F	p
Journals	1293.38	1	17.84	<0.01
Years	192.56	3	2.66	<0.01
Journ. × Yrs.	534.81	3	7.38	<0.01
Within	72.51	2001		

⁵Given the differences in the number of JEAB and JVLVB articles available for potential citation during the journals' publication histories, a conditional probability index of citations could have been employed for Fig. 1 and 2. However, the determinations of an appropriate available pool of citable papers necessary for such a weighting involved so many arbitrary decisions and assumptions that the present more direct measure appeared the least biased.

⁶Comparison of the JEAB and JVLVB self-citation slopes, for comparable publication histories (JEAB, 1958-1965; JVLVB, 1962-1969) yielded a $t = 4.68$, $df = 7$, $p < 0.01$.

THE INCOMMENSURABILITY QUESTION⁹

There have been numerous interpretations¹⁰ of why this mutual isolation exists. One view, that operant and non-operant psychology are to some degree incommensurate, is of crucial importance. If this interpretation is accepted, then the failure to cite can be explained and justified on strictly logical grounds. "If we are really talking about different things," the extreme form of this position would maintain, "then there is no reason to cite each other."

"To illustrate this interpretation let us examine the view of one respondent who wrote a summary chapter on a central topic in the learning field. When asked why there was no citation of the extensive non-operant literature on a closely related topic, he indicated that he was aware of this body of findings but found it both virtually useless, because it covered only a very narrow aspect of the problem, and uninterpretable, because its design strategy led to confounded results. The underlying argument behind the lack of interpretability of such studies is stated, in its most extreme form, in Sidman's *Tactics of Scientific Research* (1960). He argues that traditional experimental designs, employing discrete trial data summarized across trials and subjects, destroys, confounds, or omits the significant data of moment to moment rate changes in a single organism's behavior." This issue is not solely a procedural one: it is interconnected with the more fundamental problem concerning the locus of variability. Between-groups designs assume that variability is intrinsic to the organism, thus the need for its statistical control; the within-groups design assumes an imposed variability, thus the need for experimental, environmental, not statistical, control. The relationship between these two design strategies is seen by Sidman as being incommensurate:

"Where irreversibility is met, there is no individual curve that can answer the questions one may put to the group curve, or *vice versa*. The student should not be deceived into concluding that the

group type of experiment in any way provides a more adequately controlled or more generalizable substitute for individual data. . . . For the two types of data represent, in a very real sense, two different subject matters."

This divergence over method was of crucial importance, historically, in the founding of JEAB in 1958. "Most operant conditioners, in submitting their papers to JEP and JCPP, found a non-receptive editorial policy centered around the employment of single subjects measured across time. Since both JEP and JCPP, the highest status outlets for animal research, were not accepting their articles, operant conditioners published their material in less visible sources, particularly in the *American Journal of Psychology and Science*, along with such lower status outlets as the *Proceedings of the New York Academy of Science* and *Psychological Record*."

"From the perspective of the operant conditioner the journals' evaluations were unfair and totally irrelevant to their stated concerns. Particularly, as one respondent pointed out, operant conditioning methodology is quite close to that of psychophysics; in this area the use of such designs was quite acceptable to the journals, particularly JEP. The rejection of the operant conditioner's animal studies, using a similar psychophysical design, was based, in part, on the modal nature of animal research where large sample, between-groups designs were the norm, rather than upon what the operant conditioner considered reasonable 'scientific' grounds. A unique archival source with editorial standards appropriate to the literature of operant conditioning was felt necessary if not imperative."

The disparity between design strategies remains largely unchanged, as shown in Table 1. Non-operant researchers employ between-groups designs; operant psychologists use within-organism designs.

Table 1
Design Strategies Employed in JCPP, JEAB, JABA

Journal	Year	No. of articles	% Within	% Between	% Mixed
JCPP	1967	183	6.0	88.5	5.5
JCPP	1969	287	6.6	88.5	4.9
JEAB	1967	57	91.2	1.8	7.0
JEAB	1969	104	87.5	3.8	8.7
JABA	1969	32	87.5	9.4	3.1

⁹The philosophical bases of this issue are treated in Feyerabend (1962, 1970) and Kuhn (1962, 1970a).

¹⁰See Krantz (1971) for a discussion of the differing interpretations.

Does this continued divergence represent a state of logical incommensurability, as suggested by Sidman, or an insularity in examining what are the empirical consequences of such differences? Conflicting findings based in different procedures are not uncommon in psychological research. Nor are the necessary transition experiments performed to localize the sources of difference. Without such analyses, any judgment on the incommensurability issue would be incomplete.

Another form of possible incommensurability appears in the disparity between the concepts used by operant and non-operant researchers. It has been the tendency of JEAB authors to use terms and abbreviations, which can be understood mainly by other operant conditioners. Such terms as F15, yoking, fading, or tandem schedule, are infrequently used outside of the operant literature.

Although the tendency to innovate new terms for the range of phenomena studied by a new approach is a common development in science (Hagstrom, 1965) the potential integration of divergent strategies of research is partially a function of the availability of coordinating definitions between established terms and a new language. In short, the use of a new language without necessary linkages can lead to a mutual isolation of the innovating approach.

An even more problematic situation arises when there is apparent overlap between terms. For example, the non-operant term "secondary reinforcement" and the operant term "conditioned reinforcement" refer, in common, to the situation where a previously neutral event acquires reinforcing properties. Are these terms mutually translatable? Gollub (1970) noted that each term is based in a different hypothesis and that

"... attempts to prove the exclusive validity of one over the other of these formulations have been generally unconvincing. Experiments have established an empirical edifice for each account which, although not fully explaining all the data presented by the other formulation, is too well constructed to permit empirical disproof. A fair assessment of the status quo acknowledges two different, although sometimes overlapping, procedures for establishing conditioned reinforcement."

The confrontation of the two hypotheses is made difficult by differences in their design

strategies. In comparing two recent books in this area, one edited by Wike (1966) (in the non-operant framework) and the other by Hendry (1969) (in the operant framework), Gollub (1970) notes virtually no overlap between the designs employed in these two volumes.

But the lack of relationship between these two concepts has deeper roots than the design question, as suggested by the following statement:

"Proponents of an experimental analysis of behavior usually avoid such questions as 'why does a reinforcer reinforce?'. Instead, they discover and identify reinforcers by their effects. . . . The compulsion to explain reinforcement in terms of some ultimate biological cause, whether unconditioned or conditioned, is not a cross for the experimental analyst to bear." (Gollub, 1970)

This "cross" is one that the non-operant psychologists desires to bear; it relates to his fundamental assumption that effective explanation can occur only when the behavior is referred to some state within the organism. (See Krantz, 1965 for comparison of the operant and non-operant views of explanation.) The dismissal of such explanatory approaches is stated in the comment of one respondent:

"The reason I can't use other studies and data is that the philosophical underpinnings, interpretations, methodologies, are different. I simply can't integrate them in what I want to do. . . . The problems they're interested in solving are not my problems. They're about hypothetical variables. The philosophical problem is completely different. We believe that we are completely deterministic in our point of view and other people say that they are deterministic with modifications, let's say intervening variables. . . . They're not very commensurate. We have to reanalyze the problems, look at the data, look at the phenomena. I mean reanalyze again and do the research again from the behavioral analysis point of view."

In such a view, translation of terms would be impossible: it would destroy those very conceptions of behavior, and their assumptive base, that makes each theory unique. Language translators of poetry and literature have long been aware of this problem—"a rose by any other name is not necessarily a rose." But where the linguist's decisions are, in the final analysis, guided by taste, the scientist has one further recourse—a defining experiment.

If secondary and conditioned reinforcement mean the same thing, at least in some situations, they should make similar predictions. If they mean different things by being embedded in totally incommensurate systems, they will not only predict different things but no common grounds for such tests could be found.

Such defining comparisons have seldom occurred in this area of research because there is virtually no utilization of other's findings: the Wike (1966) book makes little use of operant findings; the Kelleher and Gollub (1962) review of this area has few non-operant citations. And as Thomas (1970) noted in his review of the Hendry (1969) book:

"Another feature of the present work is its provincialism, with a substantial proportion of the citations having appeared in one source, the *Journal of the Experimental Analysis of Behavior*. I hope that the day will come when operant principles and procedures are so integrated into the mainstream of experimental psychology that good work can reach the wide audience it deserves regardless of the framework within which it was generated."

If psychologists are to engage in science and not solely in linguistic analysis, empirical tests of the incommensurability question must be provided. Retaining the same terms which may refer to different aspects of reality (for example, "stimulus"—see Gibson, 1960) leads to confusion and controversy often without awareness as to the source of disagreement (Feyerabend, 1962); innovating new terms, without coordinating links leads to lack of understanding, insularity, and isolation; premature and inadequate translation can lead to the destruction of important aspects of the terms and the theoretical system; critical empirical tests can at least provide one reasoned basis for determining the meaning of terms and relationships.

Granting that such comparisons can be productive, should they be given a high priority in future research? More specifically, is the advancement of knowledge better served by exploring the isolated concept's generality or by coordinating and determining the overlap between concepts? This question can be viewed on another level: is the advancement of knowledge better served by scientists stressing their differences by working in encapsulated schools or by scientists joining forces

in exploring the points of communality between their divergent approaches? (See the following section for an analysis of the conceptual and practical problems related to this question.) Some of the value decisions required to answer these questions are suggested in the following comment:

"If you are arguing that we should make such correlations, that is a more difficult point. There may be some advantage to busily plying your own trade as if the rest of the world didn't exist. The rest of the world may not like it, but you may get quite a distance and get more new information 'by burying your head in the sand' approach. That could be argued. Historically, the operant movement has moved along and discovered new phenomena, in part, because of its willingness to isolate itself from the rest of psychology for a period. It could be a healthy thing. The operant psychologist, when many people were bogged down with theoretical issues that weren't getting resolved, went ahead and got empirical functional relationships."

The mutual isolation data suggests that the totalness of this historical stage is still present in the operant community. Opinions differ on the future directions to be taken. In the interview sample, most respondents, although intuitively aware of the isolation findings, expressed little concern. They saw little reason to modify the direction and nature of their work. For a smaller number of respondents, the isolation data was viewed as a failure of the operant conditioner, either out of laziness, lack of concern, or conceptual blindness, to see and show the systematic relevance of his work to psychology. These respondents believe that there are broad systematic questions to which all psychologists independent of approach can, and more strongly, should, address themselves. JEAB's recent editorial directions of requiring more extensive introductions to their articles (an approach in contrast to the current strategy of journal publication where introduction and discussion sections are reduced for the purpose of saving space), of requesting that reprints be sent to all cited individuals in a JEAB author's paper, and of the appearance of the cumulative citation index, which compiles all citations that have appeared in JEAB publications, all indicate the desire to relate operant research to other investigations and improve communication and incorporation.

CONCEPTUAL AND PRACTICAL IMPERIALISM¹¹

Whereas the citation data indicates isolation, another aspect of operant conditioners' behavior indicates a concern with other fields and approaches in the form of, what might be called, conceptual imperialism. Such imperialistic behavior involves working in areas not traditionally dealt with by operant conditioning—areas that often employ quite different systematic approaches. Conceptual imperialism states that the operant strategy can deal successfully not only with its own domain of problems but handle, as well, if not better, many diverse issues in psychology.

There are many ways that such a conceptual imperialism can be transmitted—this is the domain of the practical imperialist. Some operant conditioners have opted to state their conceptual imperialism in a brusque "isolationist" form. Here, the message is that in providing a "better" approach there is no reason to take into account those strategies that are being displaced; "why cite the wrong approaches when the right one is here?"

To maintain militantly such a position is to make the philosophically naive and interpersonally insensitive assumption that one can change the science without the consent of the scientist. To act in contradiction to this truism, that scientific ideas are produced, evaluated, and changed by scientists, is to invite conflict, lack of success and very likely, rejection and other's isolation of the imperialist's position. To illustrate this point, let us examine the development of operant conditioning in the post-World War II period.

"Following World War II, a program utilizing Skinner's *The Behavior of Organisms* (1938) as its source was developed by Keller and Schoenfeld at Columbia University. At the core of the Columbia program was the Introductory Psychology course where all of the diverse topics in psychology were treated from the perspective of Skinner's behavioristic approach. With supportive laboratory experience, this approach was in marked contrast to traditional introductory courses which contained a potpourri of topics, each embodying dissimilar theoretical approaches. A casual

comparison of the course's text, *Principles of Psychology* (1950) by Keller and Schoenfeld, to such introductory books of the period as Munn or Boring, strongly indicates the difference in approach."

"The Columbia course was so successful that enrollments exceeded departmental facilities. Students from the natural sciences and engineering were now attracted as contrasted to the largely humanistically oriented students of the past. Soon most of the Columbia College undergraduate curriculum was revised in line with an operant conditioning approach. A relatively small graduate program in operant conditioning then evolved with many of its students coming directly from the undergraduate program."

"The description of the Columbia program (Keller and Schoenfeld, 1949) came under attack by Wendt (1949). His comments can be viewed as being symptomatic of a growing response to the operant movement. He indicated that: 'The requirement by a department of adherence to any system of thought in its courses, be it Wundtian, structuralism, neo-Thomistic humanism or Skinnerism, eventually results in an inbred department and its isolation from the rest of science and from other disciplines.' He then went on to describe the developmental stages of an academic cult from a small number of enthusiasts in the basic courses, to the hiring of similarly minded faculty people, the lack of support and eventual dropping out of dissenters finally leading to the eventual isolation of the cult. He concluded: 'I recognize that the Columbia system is enthusiastically received by the students. But all cults have that advantage which rests on the fact, well-known to students of propaganda, that simplification introduced into confusion has high acceptance value. The worth of an educational policy must rather be evaluated by its long run contributions to the pursuit of truth. . . . Systems have their function when eagerly developed by individuals, but their administrative imposition on a college can be only harmful in the end.'"

"The fact that Wendt found it necessary to be critical at all is interpreted by respondents working at that time as: 'It means we were having an effect. People don't bother to criticize things that aren't influential.' Nor was Wendt alone in his criticism. The antagonism was seldom as extreme as one

¹¹I am indebted to Donald Baer for suggesting this distinction.

respondent experienced it, namely, 'You'd go down to Harvard Square and people would stop you and say, 'Oh, you're one of those Skinnerian types,' but it clearly did exist. What contributed to this conflict was the often militant posture of some operant conditioners in the late '40s and early '50s. As one respondent characterized the situation: 'We were getting the tough guys [referring to the natural science oriented students]. As they began to be good, they began to question their teachers. There was a sense of power. There is a little bit of arrogance. We could do things they couldn't do.' The potential threat of being effective was heightened by 'you'd be really telling a guy that spent ten years of his life on a problem like latent learning, that you wasted your time. How do you expect him to respond? The essential fact about an aversive stimulus is that it reinforces any performance that removes it. And there is nothing more aversive than a threat of withdrawal of reinforcement when it concerns your bread and butter and your status.' As one respondent characterized the early '50s: 'It took guts to be an operant conditioner then. Everyone was belligerent.' "

The post-World War II situation is not an isolated case; similar histories could be written with the clusters of operant conditioners at the University of Washington (in the early '60's) Arizona State University (affectionately known as Fort Skinner in the desert, in the mid '60s) and the present department at Western Michigan University. The clustering of scientists around an approach exists to some degree in many graduate departments, but the extensiveness of the operant conditioners' clustering has been shown in very recent history, only in the case of Clark Hull at Yale.

Such clusterings have a number of important effects: they state an implicit (and often explicit) rejection of all alternative approaches. Communication and collaboration between operant conditioners is facilitated by a commonness of approach and language. They also provide an environment that is supportive. As one respondent characterized operant conditioners in the '50s: "We never had any acceptance so we banded together." Such social support can also lead to the following outcome:

"To the extent that rude behavior and the imperialist behavior are chained, social reinforce-

ment of the first by sympathetic audiences probably will maintain the other as well. That, I suggest, is the story of Western Michigan and Arizona State; self-maintenance of rude conceptual imperialism, at the expense of actually succeeding in practical imperialism or conceptual assimilation by others. Self-maintenance, because such people are witty and charismatic and tend to assemble and maintain sympathetic audiences for just that purpose. A case of *foile-a-deux* reinforcement style. . . ."

For the student, such clusters provide a potential danger far more subtle than the form of systematic option denial suggested by Wendt (1949). Graduate education can be viewed as the main socializing force in communicating the norms of being a scientist and in training for particular orientations and strategies within a scientific field. Recent discussions (Kuhn, 1962; 1970b; Polanyi, 1966) indicate that the learning of a scientific approach occurs not only through a verbalizable, didactic strategy but also through ostension via relevant "doing". Such resultant knowledge is tacit, implicit, and not immediately accessible to awareness. The failure to teach other systematic options, or the consistent devaluation of other approaches, coupled with ostension experience in only one systematic option, can lead to a non-critical acquisition of a scientific approach and perpetuation of a modelled style of conceptual and practical imperialism in evaluating others' research and strategies.

Such styles of education and scientific behavior are fed by the very hostility they provoke. One respondent, when asked why operant conditioning has attracted militant advocates, commented:

"To keep hanging in when you have 90 percent of the people around saying 'don't rock the boat', pretty soon you have a selection factor. . . . if you can still hang in there and push that powerful strategy against all the forces . . . that say don't move against tradition . . . you get a pretty aggressive group hanging in there at the top pushing."

A less-strident, "softer" sell of an approach can produce the desired changes in others' scientific behavior. Whether such a less abrupt mode of imperialism will produce the extensiveness and speed of change desired is an open question. The option seems to be of adopting a rude form, where success or failure may be total or to employ a "softer", more assimilative approach where some degree of conversion is likely.

DISCUSSION

The operant conditioner, however he chooses to communicate his imperialism, can provide strong evidence for the approach's success. "There is little question that since the founding of JEAB the movement has undergone marked growth. Perhaps the greatest testimony to this growth is the continued flourishing of JEAB. Moving from a state of concern as to whether the first year's issues would appear and financing largely by the contributors, JEAB now has a constant flow of manuscripts and a large number of subscribers. Where the number of active researchers was approximately two dozen when JEAB was founded, by 1967 there were 342 unique authors with 125 (36.6%) contributing two or more articles to JEAB."

"The most striking growth index of the movement is the increase in membership in Division 25 of APA. Where the greatest percentage gain in members was 37% between 1966 and 1968 for all other APA divisions, Division 25 increased by 110%, having a rank of sixteenth out of 29 divisions. With the appearance of a recent handbook of operant research (Honig, 1966), the continuing flow of introductory and reading texts from an operant point of view, the exponential sale of *Walden II*, teaching machines, and the omnipresent "Skinner" box in most psychology departments ("the Skinner box for rats and pigeons, has displaced the maze as a favorite apparatus among American students of animal behavior"—Hilgard and Bower, 1966), there is little question that operant conditioning is a vital movement within and outside psychology."

"As the operant movement has grown, there have been gradual differentiations with more specific research interests developing a sizable number of practioners, particularly in such areas as applied behavioral analysis (as evidenced by the appearance of JABA in 1968¹²), and in part, behavioral pharmacology. Now effective communication can occur not only within operant psychology but within an operant specialty." Such differentiations could increase the insularity between operant and

non-operant psychology and isolation between segments of operant research.

"With this success, the movement's cohesiveness has decreased with its internal differentiations and the increased blurring of its boundaries. When the group was small and the movement young, the sharing in the effectiveness of the approach and in a common history provided the source of identity. Now the approach has lost some of its newness in that it has been tried, been proven successful to the point that a growing literature has evolved, technological innovations have been made in laboratory and environmental settings, and, at least on the instrumentation level, it has been incorporated in a number of areas within and outside of psychology. While professional identity within the movement has been partially assimilated with other approaches in psychology, it is still the case that the research of many operant conditioners, however they identify themselves, remains distinct and unincorporated within psychology."

But the social growth of an approach does not directly demonstrate its "truth". At its worst, such growth may simply indicate a poorly evaluated "fad"; at its best, such growth suggests that the system provides relatively the most adequate available approach.

Yet the proselytizing behavior of some imperialists suggests that the operant approach is indeed considered to be the one "true" approach. To directly demonstrate the "truth" of one system over another requires a confrontation of their predictions against empirical reality. However, this is no simple matter as discussed earlier and as further illustrated in the following example.

While all psychotherapies aim at curing the patient, a claim of "truth" by one approach implies the prediction that its procedures will provide the best, most effective (or efficient) cure. Consider whether an existential therapist and an operant therapist could agree on what constitutes "cure". From the existential perspective, cure would be obtained when the patient feels that he is free to make choices and to confront the realities of existence; from the operant perspective the absence of non-functional behavior is the sole necessary index. The operant therapist cannot accept the existentialist criterion because it refers to, and is indexed by, internal states of the individual.

¹²See Krantz (1971) for a discussion of JABA's founding and other internal changes within the operant movement.

Nor are the external events it refers to capable of physicalistic determination. From the operant perspective, such states and events are viewed as unnecessary and invalid speculations. The existentialist considers the operant criterion as weak because it deals with only one somewhat superficial aspect of man's existence. The most significant point of conflict arises from the existentialist's view that a patient, following cure, may continue to maintain his previous non-functional behavior as long as it does not trouble him or restrict his personal freedom. But for the operant therapist, the maintenance of such behavior, by definition, means that the patient has not been cured. One cannot resolve this conflict by invoking the pragmatic criterion of "which therapy works better" because each side can rightly claim success in that each produces effective "cure" *by their own criterion*.

Thus, the possibility of empirical discrimination of "truth" can be obviated by the very assumptions of the systems under examination. Conflicting viewpoints often dictate different criteria for such comparisons; unless the disputants can agree to accept a common criterion, in some respects non-theory embedded, there is no scientific way to resolve the issue. (See Krantz, 1969, for a detailed analysis of this problem.)

The main source of this problem lies in the differing assumptive bases from which the differential criteria and predictions are derived. The assumptive propositions, while providing the necessary bedrock from which testable propositions are developed, are not capable of being directly verified. They are not propositions of the form, "the world is . . ." but rather "let us assume that . . .". They may take the form of statements of assumption, imperative, tautology, or directive but not a synthetic structure. For example, among the beliefs of operant conditioning would be such propositions as: "Inferences or statements about consciousness are not valuable in a science of psychology", or "The preferred design strategy for psychology should be the study of changes in behavior within a single organism measured across time". These propositions can be accepted only on faith as the "givens" of the system; their value (not their truth) is demonstrated, on the simplest level, by their internal consistency with other propositions in the system. More importantly, the significance

of such beliefs is demonstrated by the power of the total system in generating valid empirical generalizations. In addition, the degrees to which such beliefs become the shared, accepted property of the relevant scientific community further increases the credibility for their maintenance. Yet none of the above sources of support established the "truth" of the beliefs—they simply supply increasing support for their continued maintenance.

Since the belief propositions are not stated in an empirical form, empirical data cannot show them to be true or false; the editing of the external world serves only to determine the truth or falsity of the system's derived empirical propositions; such findings, in turn, give credence to the maintenance of the beliefs. Although a strong commitment on the part of the scientist to such beliefs may lead him to phrase the propositions as "It is true that . . ." or "The correct approach is . . ." such changes represent a perhaps psychologically necessary personal, and often a shared communal, attachment rather than a logical shift in the nature of the propositions. If it should reach the state of affairs that such faith propositions become so generally accepted by significant segments of the scientific community, their form may increasingly tend to appear more than simply as statements of faith and beliefs but rather take the character of "truisms".

Thus, beliefs, while they provide in normal functioning the necessary "blindness" to interpret and understand a fluctuating, complex universe, often can become malfunctional. Recent research on the nature of cognition and attitude change indicates that beliefs to which an individual maintains a strong commitment are extremely resistant to change. In science, such belief maintenance can lead to single-mindedness and inflexibility, particularly where there exists incompatibility between different belief systems and thus differing focusses on the world.

In the case of the mutual isolation of operant and non-operant psychology, there are differences in belief that differentiate these approaches. To what extent these differences are of a revolutionary character in that they involve incommensurability between the conflicting systems is at this point unclear.

We have come full circle in a very literal sense. If the case should be that the systems

are incommensurate, then each approach can dictate a valid set of criteria and non-overlapping predictions. Both can be true and isolation and non-utilization of each other's findings becomes logically justifiable. But there is also a price to pay for such incommensurability; conceptual imperialism becomes illogical and scientifically unjustifiable because there can be no differentiating demonstration of superiority (see Heese, 1970). If, on the other hand, the differing systems are to some degree commensurate and, as one respondent suggested, "you're willing to do the necessary scholarship to find out what other people are finding out", then the mutual isolation can be reduced and empirical tests of "truth" can be made.

There is another possibility to be considered; if the fruits of conceptual imperialism (through conquest or assimilation) can produce a consensus within psychology then truth is established by fiat. However, the history of science provides an important object lesson—well accepted consensual "truths" of one historical period are often viewed, after major shifts in perspective or revolution, as less compelling asides (see Kuhn, 1962, for a detailed analysis of this viewpoint). All scientists can agree on the reliability of "facts"; the significance of these facts is at the root of the competition and isolation between competing approaches.

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