An Analogue of Science

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AN ANALOGUE OF SCIENCE

by

Michael Albert Minervini

A Dissertation
Submitted to the
Faculty of The Graduate College
in partial fulfillment of the
requirements for the
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There is no science of human behavior, not even an incipient one. The popular conception of applied behavior analysis as a genuine principle-driven technology is mostly an illusion. Two sorts of evidence support this conclusion. The first is an ever widening split between the field's basic and applied realms. The second, thus far unacknowledged, is that when the concepts of operant and respondent conditioning are extended to human behavior, they are often rendered as no more than metaphors. These metaphors are not confined to casual discourse or even to interpretation. In fact, they are the prevailing form of extension in large segments of the applied domain. Hence, the real value of the experimental analysis of behavior for clinical application is heuristic in nature. Metaphorically extended principles yield techniques, and techniques, however valuable they may be, should not be confused with technology.

In addition to questioning the conventional wisdom which holds that a new science is in the offing, the ambitious promise behind that science also warrants a renewed and vigorous skepticism. Parts of the scientific promise have been grossly oversold, especially the part which assumes that psychological discoveries will be required to save humanity from itself. It may simply be a mistake to accept the very plausible notion that a science of behavior will provide solutions to the big problems in human engineering.

None of this is necessarily bad news for the practitioner. The marriage of scientific method and human behavior can still offer many practical, although highly
circumscribed, contributions. Some of these may provisionally qualify as technology, but most will fall into the broad category of technique. It is suggested that both direct and heuristically based interventions be pursued, especially where their payoff is likely to be the greatest. Nevertheless, it is important to remember that the discipline's aspiration to scientific status can best be evaluated when we begin to recognize that the two endeavors are not all of a piece.
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An analogue of science

Minervini, Michael Albert, Ph.D.
Western Michigan University, 1988

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ACKNOWLEDGEMENTS

In spite of the arguments put forth in the pages which follow, I remain committed to a position that might be described as strong environmentalism. It is fitting, therefore, that the debts acknowledged in this space are recognized as both manifold and deep. Foremost among them is the indispensable contribution of my parents, Albert and Jean Minervini. Their continuous support of my academic career has persisted for many years now, often in the face of relentless advice to the contrary (offered by the standard assortment of well-intentioned friends, relatives, and neighbors). Indeed, for the past several years, during which time writing has been my sole occupation, I have had no source of income to speak of, save for that provided by my parents.

The third principal contributor to this dissertation is Jack Michael, the chairman of my doctoral committee. Jack has been my friend and mentor for over 15 years, so the full extent of his impact is impossible to gauge. However, it is no exaggeration to say that I have never developed a professional thought or utterance which has not felt his influence. Furthermore, it was Jack who suggested (possibly to his regret) that I use the university dissertation requirement as a vehicle to convert my themes on metaphor and science into a coherent, transmissible package.

A far less tangible, but no less significant contribution to this endeavor falls within the domain of personal and emotional support. John Cooney and Kathleen Madigan gave me strength and hope when I was sure I would fail. They have counseled me, encouraged me, and applauded even my remotest approximations of progress. A great deal of the credit for the successful completion of this paper belongs to them. And then, inevitably, there is the crucial role played by my brother
Tony. Tony remained at home and kept our family intact while I was left to pursue my own ends. I shall always be grateful to him for that.

These are some of the people who have figured most prominently in what must have seemed like an interminable project. Each contributor has had a direct effect on the material which follows. More important, it should be obvious that my indebtedness to them goes well beyond the borders of the present effort.

I am also thankful for the assistance I received on the mechanics of this process. Because I cannot type, every word in this rather lengthy document had to be individually processed by someone else. This task was shared by Shery Chamberlain and Jack Michael. Ms. Chamberlain performed the bulk of the keyboard entry, usually on short notice and always with good humor. Dr. Michael provided the necessary software and spent many hours personally configuring and formatting various drafts of the manuscript. I know of no advisor who has ever taken on so much tedium with so little complaint. My parents, Shery, Jack, and my brother Matthew all helped with the proofreading. Cathy Raymore made painless some essential last minute typing.

Finally, none of this would have been possible if not for the extreme flexibility of Rollin Douma of The Graduate College. I required so many extensions of the deadline to complete my work that Dr. Douma could have long ago, and with complete justification, made the administrative decision to terminate my graduate program. He chose not to do so, and I can only hope that the resulting product proves worthy of his generosity.

Michael Albert Minervini
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PART I: PSYCHOLOGY AS SCIENCE

The Scientific Promise

In 1948, three of the entries in the Letters-to-the-Editor section of the Physical Review were submitted by members of a research team at Bell Laboratories. All three were related, but the first bore the most portentous title: "The Transistor, a Semiconductor Triode" (Bardeen & Brattain, 1948). Early demonstrations of the device were probably taken as little more than a curiosity by all but a select community. Even these experts could not have known the scope of the revolution that they were witnessing, a revolution whose impacts are now so universal that they scarcely warrant documentation. Remarkably, scientific achievements of this magnitude are not uncommon; in fact, their pace actually seems to be accelerating. Ours is an age characterized by bursts of progress in the physical sciences so striking that the resultant technology is recognized as miraculous by specialist and layman alike. In less than a single lifetime we have seen the advent of space travel, gene splicing, artificial organs, and the prospect of limitless energy. The modern world turns on (and may end by) the products of science.

This state of affairs has left the nonscientist in an awkward position. On the one hand, only trained professionals can even attempt to keep up with current work in relatively narrow fields of inquiry, fields which themselves continue to differentiate. Yet, the implications of this highly specialized knowledge for the general populace are far from abstract. No one, at least in so-called civilized societies, is untouched by the scientific enterprise. We have learned to respect science even if we cannot understand it.
The same cannot be said of contemporary psychology. The thoughtful citizen is hardly in awe of behavioral science, such as it is, as compared to the established natural sciences. Certainly this is not for want of esoteric material. Psychologists have amassed a formidable collection of scholarly books and archival journals, replete with impenetrable jargon, formulae, and schematic diagrams, all sufficiently opaque to the uninitiated. But real progress, especially as evidenced by the flood of technology in the sister sciences, is still a promise. Even the psychological laboratory, with all of its impressive trappings, has failed to achieve widespread legitimacy. As Skinner (1972) has pointed out:

Psychology in general, and experimental psychology in particular, is still a long way from providing a conception of human behavior which is as readily accepted by those who deal with men as the views of physics are accepted by those who deal with the physical world. And psychologists themselves are not doing much about it. (p. 318)

This may simply be a reflection of the discipline's youth. Perhaps it is inappropriate to expect too much too soon. It is, after all, still possible that truly impressive advances lie in the field's future (a theme to which we shall later return). However, whether or not a mature science of human behavior is ultimately possible (or necessary, or even desirable), history may well judge twentieth century psychology not as science, but rather as an aspirant to some of its methods.

Part of the problem is that this aspiration to the scientific program has been expressed so differently among the variety of endeavors that are now called psychology. Today's eclectic finds no shortage of orientations from which to choose, each with a unique mission and, perhaps more disturbing, with an apparently unique set of organizing principles as well. Of course, most psychologists would agree that the goal of their particular enterprise is the betterment of the human condition, and further, that this is attainable through an enhanced understanding of human nature, as that topic is most broadly conceived. But a closer look at several of
the current paradigms reveals deep differences in the working conception of what human nature is. Indeed, there is virtually no agreement on what fundamental questions need be posed in order to better understand it. If there is a commonality of effort here, it is difficult to discern; a fact, incidentally, which does not always escape the introductory student.

A lack of consensus in so difficult a field is obviously a matter for interpretation; it may be viewed as either vibrant diversity or hopeless confusion. In either case, it provides a background against which one brand of psychology becomes notable both for its explicit allegiance to naturalism as well as for the development of what appears to be a viable technology. That movement was originally called the experimental analysis of behavior. B. F. Skinner, father of the approach, outlined his version of a scientific psychology and its contribution to humanity in what has since become a landmark work. Near the very beginning of his *Science and Human Behavior* (1953), in a section entitled, "Science as a Corrective," Skinner gave the first generation of behavioral psychologists their credo: "The methods of science have been enormously successful wherever they have been tried. Let us then apply them to human affairs" (p. 5). The publisher must have recognized the statement as provocative, if not prophetic, for it appears as the lead quote on the back cover of the paperback edition.

More than thirty years have passed since the publication of *Science and Human Behavior*, and although advocates of the behavior analytic (Skinnerian) position are still a minority, their rising influence is undeniable (e.g., Hoon & Lindsley, 1974; Wyatt, Hawkins, & Davis, 1986). But are we on our way to a genuine science of human behavior, and with it the enormous success promised in Skinner's hopeful forecast? During the last decade, a controversial literature has arisen from within the ranks of behavioral psychologists. A number of their recent commentaries have
expressed dissatisfaction not with the amount but rather with the nature of growth in
this young field (Minervini, 1986). Of central concern is the fact that as the applied
sector (now called applied behavior analysis) has grown, it has seemed to detach itself
from its basic research roots, assuming a life of its own. Some (e.g., Deitz, 1978;
Michael, 1980b), viewing this separation as harmful to the discipline as a whole, have
called for a reunification of the basic and applied areas. Others (e.g., Baer, 1981;
Malott, 1981) see the progressive independence of applied behavior analysis as a
natural step in the development of a maturing, healthy technology. Several details of
this split will be addressed later, but one thing is becoming clear: Something has
gone amiss in the realization of Skinner's vision. The popular characterization of
behavior analysis (currently the collective title for behavioral efforts in theory,
research, and application) as an emerging natural science, parallel in its development
to the early stages of the established physical sciences, now warrants reexamination.
That is not to say that behavior analysts have become indistinguishable from the
jumbled mainstream that is psychology. The behaviorist method and general
orientation still constitute a unique and rigorous approach to the challenging subject
matter of human activity. What requires scrutiny, however, is the extent to which
behavioral psychology's scientific aspirations have been fulfilled.

In order to evaluate the success of the behavior analytic movement, one must first
appreciate the depth and scope of its ambition. It is no exaggeration to say that
Skinner and his followers believe that nothing less than a comprehensive science of
human behavior is necessary to save the world, that this science will flow from what
is now a small band of revolutionaries, and that the time for humanity to change its
present direction is fast running out. A secondary theme is that traditional psychology
and the prescientific doctrines of western democracy are seen as inertial forces which
must be overcome in order for the revolution to proceed. Rather than dilute the force
of their position by attempting to paraphrase it, a few representative quotations will allow the field's proponents to speak for themselves.

First, a qualification is in order. In the behavior analytic framework, when one addresses the big problems in human engineering, a particular type of behavior is at issue. Skinner has circumscribed and labelled this domain with the term operant, and has distinguished it (on the basis of its susceptibility to consequent stimulation) from what is called respondent or reflexive behavior. As is true for a number of his greatest contributions, one frequently hears that this juxtaposition was a nondiscovery, a trivial demarcation known even to the psychology of the ancients. Despite these monotonous allegations however, most of us owe our easy familiarity with the operant-respondent distinction to the fact that Skinner (1953) made it explicit. In doing so, he opened the door for laboratory extrapolation to a much broader range of human action than had heretofore been possible:

Reflexes, conditioned or otherwise, are mainly concerned with the internal physiology of the organism. We are most often interested, however, in behavior which has some effect upon the surrounding world. Such behavior raises most of the practical problems in human affairs and is also of particular theoretical interest because of its special characteristics. (p. 59)

So fundamental is the concept of operant behavior that the assertion of its central role in daily life has become an axiom for committed Skinnerians. Hineline (1980) has put it this way:

Most radical behaviorists follow B. F. Skinner's lead in arguing that the human behavior of educational, social and political importance is mostly behavior controlled by its consequences — that is, operant behavior. Further, their research and their analyses of the human condition draw heavily upon operant principles. (p. 70)

And that is the link to the theme of crisis and world salvation, for it is operant behavior that has brought us to the edge of the precipice. Accordingly, only by understanding the determinants of operant behavior can we hope to save ourselves. This notion has long been a mainstay in Skinner's writings but lately it has become
preeminent. Indeed, many of his recent works (e.g., Skinner, 1978a) begin with a (somewhat routine) doomsday prophecy of the following sort:

We are continually reminded that, for all its past triumphs, mankind may be headed straight for disaster. Unless something is done, and soon, there will be too many people in the world, and they will ever more rapidly exhaust its resources and pollute its air, land, and water, until in one last violent struggle for what is left, some madman will release a stockpile of nuclear missiles. (p. 17)

Unfortunately, there is little the established sciences can do to help us out of this plight. In fact, they often contribute to our problems by amplifying civilization’s most destructive products and tendencies (Skinner, 1971):

In trying to solve the terrifying problems that face us in the world today, we naturally turn to the things we do best. We play from strength, and our strength is science and technology. To contain a population explosion we look for better methods of birth control. Threatened by a nuclear holocaust, we build bigger deterrent forces and anti-ballistic-missile systems. We try to stave off world famine with new foods and better ways of growing them. Improved sanitation and medicine will, we hope, control disease, better housing and transportation will solve the problems of the ghettos, and new ways of reducing or disposing of waste will stop the pollution of the environment. We can point to remarkable achievements in all these fields, and it is not surprising that we should try to extend them. But things grow steadily worse and it is disheartening to find that technology itself is increasingly at fault. (p. 1)

Where, then, is a solution to be found? Perhaps the one science that could make all the difference is the one we have yet to develop:

What we need is a technology of behavior. We could solve our problems quickly enough if we could adjust the growth of the world’s population as precisely as we adjust the course of a spaceship, or improve agriculture and industry with some of the confidence with which we accelerate high-energy particles, or move toward a peaceful world with something like the steady progress with which physics has approached absolute zero (even though both remain presumably out of reach). But a behavioral technology comparable in power and precision to physical and biological technology is lacking, and those who do not find the very possibility ridiculous are more likely to be frightened by it than reassured. That is how far we are from “understanding human issues” in the sense in which physics and biology understand their fields, and how far we are from preventing the catastrophe toward which the world seems to be inexorably moving. (p. 3)

Of course, Skinner’s portrayals rarely end on such a pessimistic note. His litany of ills merely sets up contrast for the brighter world that is the promise of a new,
remedial science. After all, as the following statements indicate, the behavioral world view is an essentially hopeful one:

A scientific view of man offers exciting possibilities. We have not yet seen what man can make of man. (Skinner, 1971, p. 206)

...an effective psychology would eventually develop a central conception of human behavior which not only would be fundamentally "right" in the sense of enabling us to understand behavior, whatever that might mean, but would generate powerful techniques having important applications in every field of human affairs. (Skinner, 1972, p. 314)

We may shortly be designing the world in which men will henceforth live. But how is it to be designed, and to what end? These are difficult questions, to which nothing short of an effective science of man will provide the answers. The methods of science no longer need verbal defense; one cannot throw a moon around the earth with dialectic. Applied to human behavior, the same methods promise even more thrilling achievements. (Skinner, 1972, p. 330)

More important, perhaps, than the potential for thrilling achievements, is Skinner's assessment of the real progress already made by behavioral practitioners. Even with a somewhat primitive science they have apparently logged many successes in a variety of disciplines:

By turning from man qua man to the external conditions of which man's behavior is a function, it has been possible to design better practices in the care of psychotics and retardates, in child care, in education (in both contingency management in the classroom and the design of instructional material), in incentive systems in industry, and in penal institutions. In these and many other areas we can now more effectively work for the good of the individual, for the greatest good of the greatest number, and for the good of the culture or of mankind as a whole. These are certainly humanistic concerns, and no one who calls himself a humanist can afford to neglect them. Men and women have never faced a greater threat to the future of their species. There is much to be done and done quickly, and nothing less than the active prosecution of a science of behavior will suffice. (Skinner, 1978c, p. 55)

This is an impressive list, yet it does not fully convey the extent of the behavioral promise. In some areas, such as education, Skinner cannot resist superlatives in his description of the prospects for improvement:

The principles of an experimental analysis are now being extended to the field of verbal behavior, and it is inconceivable that the results will not be used to improve instructional procedures. And with fabulous results. Enough has already been done to justify the prediction that what is now learned by the
average college student will someday be learned in half the time with half the effort. (Skinner, 1972, p. 317)

With so much at stake, and with such promising early results, one might suppose that a psychological consensus, a unified conception of human behavior, was finally at hand. But that is hardly the case. For more than a quarter of a century Skinner has acknowledged that his supporters are but a "happy few" among the majority of psychologists. Of the rest he has asked, "Why are they not currently developing the pure science of human behavior from which such tremendous technological advances would certainly flow?" (Skinner, 1972, p. 318). The answer has punctuated nearly every one of Skinner's major works; a series of digressions has led us astray: "The problem, I submit, is digression. We have been drawn off the straight and narrow path, and the word diversion serves me well by suggesting not only digression but dalliance" (Skinner, 1978d, p. 69). These digressions have taken the form of various flights from the experimental laboratory (see Skinner, 1972, pp. 314-330), each of which has yielded a common result. Preoccupations with the inner life, with individual autonomy, and with theory testing, have all served to divert our attention from the environment as the ultimate cause of behavior.

A principal force underlying these diversions is the mentalism that has so firmly embedded itself in the casual discourse of human action as well as in the formal statements of traditional psychology. Because it is so deeply entrenched in our culture, mentalism, it is said, remains the chief impediment to the development and implementation of a science of behavior. Even for trained Skinnerians it stands ever ready to swallow any unsuspecting behaviorist who might backslide into its open jaws. That is precisely the fate of many "fledgling" behaviorists according to Branch and Malagodi (1980), who have titled a recent paper, "Where Have All the Behaviorists Gone?" This is no mere whimsy on the part of the authors; they see mentalism as a real threat to the spread of behaviorism and hence to the world's
survival. Indeed, their characterization of humanity's crisis and its relation to mental­ism is remarkably similar to Skinner's. After having taught an academic course on the subject, they write:

Over the course of the term we and our students became increasingly convinced that many of today's world problems to a considerable extent are due to the western world's preoccupation with mentalistic "explanations" of both troublesome and potential problem-solving behaviors....We are overpopulating the planet, exhausting our resources with geometric acceleration, engaging in excessive agressive [sic] behavior, and spending as yet unacquired money at a frightening pace. All of these problems, of course, are behavioral and appeals to the good will of men as well as attempts to change their minds are working no better now than they have for the past 2500 years....by divesting ourselves of mentalistic interpretations we may draw attention to manipulable aspects of the environment that will possibly allow us to deal more effectively with the serious problems we face. (p. 31)

It would be a mistake to think of these authors' complete commitment to the scientific promise as an isolated phenomenon. In his presidential address to the Association for Behavior Analysis, Michael (1980b) described his own conversion to the behavioral orientation. Although speedier than most, it does convey a sense of enthusiasm that is probably representative of many in the behavioral community:

I was teaching an introductory course and while looking for some reasonable lecture material stumbled onto Science and Human Behavior. In the span of a few hours I was converted to Skinner's point of view. Science and Human Behavior was not only the most convincing and detailed analysis of human behavior that I had seen, but it also implied a world view that I found very attractive. The book left me with the strong impression that the most important thing I could do with my life was contribute to the further development of the science of behavior and promote behaviorism as the principal basis for dealing with human problems on an individual, but more importantly, on a broad cultural level. There was a kind of desperate temporal aspect to this enthusiasm also, in that we had to move quickly because there might not be much time left. I still have this sense of time running out. (p. 5)

In a democracy of theories, such unrestrained zeal for one version of psychology is bound to inspire attack. Having grasped the extent of the behaviorist's ambition, one might easily conclude, as have many of Skinner's critics, that his program is too broad to be very deep--that a behavioral account is necessarily simplistic. But Skinner has never been one to avoid close analysis:
Such a plan cannot be carried out at a superficial level. The engineer who builds a bridge successfully has more than a casual impression of the nature of his materials, and the time has come when we must admit that we cannot solve the important problems in human affairs with a general "philosophy of human behavior." The present analysis requires considerable attention to detail. (Skinner, 1953, p. 42)

Furthermore, although optimistic about the possibility of understanding human activity, he recognizes the serious challenges that it presents:

Behavior is a difficult subject matter, not because it is inaccessible, but because it is extremely complex. Since it is a process, rather than a thing, it cannot easily be held still for observation. It is changing, fluid, and evanescent, and for this reason it makes great technical demands upon the ingenuity and energy of the scientist. But there is nothing especially insoluble about the problems which arise from this fact. (Skinner, 1953, p. 15)

There is, then, despite its strong claims, a good deal of balance to the Skinnerian framework. Those who are students of his position know it to be richly textured and far from simple minded. At the same time, however, we have seen that caution does not prevent a bold statement when one is called for. Consider, for example, the following passage in which Skinner (1972) questions the life's work of no less a figure than Albert Schweitzer:

We may agree that the world would be a better place if more men would concern themselves with personal and political problems. But we must not forget that the remedial step is necessarily a short-term measure and that it is not the only step leading to the same goal. The lively prosecution of a science of behavior, applied to the broad problem of cultural design, could have more sweeping consequences. If such a promising alternative is actually feasible, anyone who is capable of making a long-term contribution may wisely resist the effect of other consequences which, no matter how important they may be to him personally, are irrelevant to the scientific process and confine him to short-term remedial action. A classical example from another field is Albert Schweitzer. Here is a brilliant man who, for reasons we need not examine, dedicated his life to helping his fellow men--one by one. He has earned the gratitude of thousands, but we must not forget what he might have done instead. If he had worked as energetically for as many years in a laboratory of tropical medicine, he would almost certainly have made discoveries which in the long run would help--not thousands--but literally billions of people. We do not know enough about Schweitzer to say why he took the short-term course. Could he not resist the blandishments of gratitude? Was he freeing himself from feelings of guilt? Whatever his reasons, his story warns us of the danger of a cultural design which does not harness some personal reinforcement in the interests of pure science. The young psychologist who wants above all to help
his fellow men should be made to see the tremendous potential consequences of even a small contribution to the scientific understanding of human behavior. It is possibly this understanding alone, with the improved cultural patterns which will flow from it, which will eventually alleviate the anxieties and miseries of mankind. (p. 322)

For most people that would constitute an act of hubris, but for Skinner it is merely an act of consistency. Taken in context, it is not an unusual opinion for one who believes that basic science is about to offer, "the behavioral equivalents of compass, gunpowder, and moveable type" (Skinner, 1978b, p. 96). Thus, it should be clear that the scientific promise of behaviorism is far from a casual one.

In the foregoing sample, we have reviewed some of the fundamental assumptions of the behavioral movement, expressed in the language of its advocates. The spirit of these assumptions is perhaps best summarized by Michael's (1980b) description of the thrust of his early teaching activities: "The message of all this verbal behavior was essentially that we were in a lot of trouble and behaviorism was the only way out" (p. 5). This sentiment depends in turn upon another implicit assumption, namely that a science of human behavior would be very much like the established physical sciences of chemistry, physics, and biology. Before proceeding, it would be useful to examine some of the things that we have come to take for granted about those sciences.

Of particular importance to the point of this paper is the fact that the natural sciences on which we all rely are principle-based. These principles are real; their meanings are well agreed upon and our language about them is tightly controlled. Indeed, this standardization appears to be indispensable to those who work in a given scientific specialty. Additionally, certain fundamental principles, such as valence, mass, and cell, are common to both the basic and applied branches of their respective sciences, specialization notwithstanding. Furthermore, when technology takes advantage of a scientific discovery, the basic concepts of the laboratory are used to
transmit the contribution. Such concepts, therefore, possess more than mere heuristic value for the applied field.

A modern conventional wisdom states that this transfer from research to application is seldom immediate. We have learned to accept a reasonable lag between the two endeavors. Nevertheless, our faith in pure research is sustained by the fact that a certain amount of it generally results in an eventual payoff for society. Of course, none of this is meant to imply that the influence flows in only one direction. One expects, in a discipline truly based on science, a healthy degree of reciprocity between its basic and applied realms. Michael (1985) has called this arrangement the "normal" relation and argues that it is the appropriate one for behavioral psychology. Naturally, exchanges in such a relation presuppose that researchers and technicians speak the same language; it is in this sense that a science's core principles are said to be fundamental.

The normal relation is important because it may well be requisite to another of the common characteristics of science: Science makes progress. Elsewhere (Minervini, 1986) I have described the special types of progress which are linked to a field's vitality, that is, to its scientific health, "...a notion which carries with it several related themes, among them cumulation, both in the explanatory power of an ever-refined set of fundamental relations, and in the increasing practical control of the subject matter described by these general laws" (p. 8). Other forms of progress may be more conspicuous--a field's organizational growth or political influence, for example--but to be counted among the natural sciences, progress at the most basic level is essential.

The preceding is by no means intended as a complete list of the defining features of science. It does, however, indicate that we demand a great deal from the scientific enterprise. The products of science are nearly always better than the best craft, so much so that a differential of effectiveness between the two, if one could be
measured, might serve as a useful index of when a given field has achieved scientific status. A large effectiveness differential is most apparent in areas like transportation, where travel time between locations dropped dramatically with the advent of flight and again with jet propulsion. The same is true of crop yields in agriculture and information transfer in communication. In each case, the substantial gap between what was and what is now possible is attributable, not primarily to improvements in craft, but rather to the intervention of science.

Surely, differentials of this scale, projected to the field of human affairs, have fueled the promise of a behavioral science, at least for those who view psychology as more than a patchwork of disconnected cures. Add to this general orientation the strong belief that civilization itself hangs in the balance, and it is easy to understand the compelling sense of mission shared by Skinner and the behavior analysts.

A Personal Malaise

I have both subscribed to and argued for the scientific promise (as rendered by Skinner) for nearly twenty years. In the last few years, however, for reasons which will become clear in arguments made throughout this paper, I have become uneasy about the scientific foundations of the behavioral movement. In fact, my purpose here is to challenge the fundamental notion that the behavior analytic program is what it seems to many to be thus far, the successful realization of Skinner's ambitious vision. More specifically, I am now quite skeptical regarding propositions of the following sort: that we actually have, or are even moving toward, a full-fledged science of human behavior; that our practical efforts (called applied behavior analysis) constitute a genuine technology, that is, a body of procedures derived from the experimental laboratory; and finally, that any of this is necessary to save the world.
What follows, therefore, is (to borrow one of my favorite Skinnerian words) an act of apostasy. In particular, I am troubled by the way in which the field has grown, more precisely, by how it has failed to grow, especially in the last decade or so. To oversimplify things a bit, it now seems to me that the vigorous lateral spread of behavior analysis has been accompanied by little in the way of vertical growth. That is to say that while many more laboratories now have Skinner boxes, and while many more clinicians now talk about reinforcement, there are few cases where a discovery in the former has led to increasing the sophistication of the latter. Curiously, this situation has not deterred the practitioner. To be sure, applied behavior analysis has long been distinguished by a high degree of creativity; however, that may be just the problem. The early applied work impressed me for a reason that I now view as a weakness. Many of the most exciting studies consisted of creative leaps that were obviously behavioral in a general philosophical sense, but were not merely extensions of the basic science. Of course, familiar processes were almost always invoked, and indeed they usually seemed somehow relevant. Yet, in retrospect, these rationales now appear to have been based on a good deal of loose talk, and that is a central theme of the present paper.

This should not be interpreted as another in the continuing series of proclamations on the death of behaviorism (e.g., Koch, 1964). Rather, it is a deflation of the prospects for a science of human behavior and, by implication, of the host of attendant promises covered in the previous section. Furthermore, my particular apostasy should not be confused with either heresy or defection. I have no original system or personal reconstruction to offer as a solution to the problems with behavior analysis that are about to be described. Nor will the proponents of competing orientations find any source of comfort in these pages. My allegiance has not shifted. It has simply dissipated. With few exceptions, the alternatives to a
behavioral account still seem to me as foolish as ever. The reader will also note the conspicuous absence of suggestions for a general rapprochement of behaviorism with its theoretical adversaries (e.g., Catania, 1973; Mahoney, 1977). I remain unconvinced that diametrically opposed views on the fundamental conception of the human organism can be resolved peaceably or rendered simply as a matter of turf.

As this is a familial criticism, my intended audience is within the behavioral family. At most, I hope to encourage a reevaluation, a change in the way we think and speak about our profession and its subject matter. However, I am aware that a call to humility is not likely to be well received in a period of expansion and apparent growth, especially by members of what is still an unpopular position. So, in the spirit of humility, I have a secondary, more tractable goal. If a single apostate cannot convince the faithful that the behavior analytic promise has been oversold, perhaps I can at least contribute to the refinement of some of our language practices. It was, after all, an examination of behavioral language that first shook my own conviction.
PART II: A RECONSIDERATION

In the balance of this paper, I will attempt to establish that the popular conception of behavior analysis as a science is incorrect: that our science is not a science at all, not even an incipient one. So thorough a reconsideration must first consider how members of the behavior analytic community currently view their discipline. After touching on the conventional wisdom regarding our scientific status, we shall examine two trends which serve to undermine it. One of these, the aforementioned weakness in what is ostensibly a precise technical language, will form a substantial part of the present argument. Next, should the reader be persuaded that a general reconsideration is in order, we shall turn our attention to its form and implications. (Of course, there are familiar positions within the behavioral folklore which might be invoked as defenses against any threat of revision, but it will be argued that they are insufficient to justify the prevailing attitude.) Finally, some prescriptions for a less ambitious behavior analytic program will be offered. These may be acceptable even to those who do not agree with the general orientation proposed herein.

The Conventional Wisdom

Behavior analysts have long thought of themselves, to a certain extent at least, as mavericks among the staid institutions of what we are fond of calling traditional psychology. It would be incautious, therefore, to foist any sort of monolithic characterization upon so diverse a population. Nevertheless, a review of The Behavior Analyst—one of the principal forums for discussion of the field's development—suggests a nearly uniform perception that all is reasonably well with respect to the movement's natural science status. That is not to say that problems are
seldom acknowledged. To be sure, a large percentage of this particular journal's articles are devoted to just that subject. Most of these, however, concern strategies for the behavioral camp's survival in a hostile environment. Indeed, a solid scientific base is often implied as a fait accompli. The real question seems to be how best to proceed, especially in light of the danger posed by certain outside forces.

Much of the advice on the future of behavior analysis has appeared within the last decade and has usually, but not always, been published in behavioral organs. Suggestions for change are in no short supply and have covered a wide variety of topics, ranging from renaming the field (Epstein, 1984) to merging it with microeconomics and decision theory (Miller, 1983). A recent sample of this literature includes the following themes: strengthening the theoretical commitment of behaviorists, and focusing their political power on the acquisition of resources (Branch & Malagodi, 1980); separating our discipline even further from mainstream psychology (Fraley & Vargas, 1986); and, moving it into new arenas, such as the national marketplace (Pennypacker, 1986), and large-scale cultural analysis (Malagodi, 1986).

None of this is very damaging to the scientific posture. For one thing, many of these critiques are directed toward external threats. There are, however, occasional hints that our difficulties, if not fundamental, are at least internal. As we shall see in the next section, a number of recent commentaries have expressed concern about the now familiar split between the field's basic and applied realms, and about the subsequent dilution of applied behavior analysis (e.g., Deitz, 1978; Michael, 1980b). Thus, it is clear that some within the community do acknowledge important challenges to its growth. Even so, the problems that they present are, almost without exception, portrayed as remediable. Perhaps for that reason, the advice offered by
these authors, although not widely heeded, has often been favorably received in the behavioral literature (Epling & Pierce, 1983; Pierce & Epling, 1980).

By contrast, wholesale attacks on the operant paradigm, even from a former ally (e.g., Schwartz, Schuldenfrei, & Lacey, 1978) have usually produced retaliation in kind (Branch & Malagodi, 1980). Behaviorism has long been an unpopular doctrine. Skinner's work, in particular, has spawned a fraternity of critics--let us call them the transenders--who insist that they have both absorbed and gone beyond his position. Skinner's sympathizers, as might be expected, react unfavorably to such assaults. They discount them on the grounds that what is being passed as transcendence is merely ignorance of the complexities of a subtle world view. The point is an old one, but it frequently holds true.

Less easy to dismiss are some of the conspicuous disagreements that sometimes crop up from within the ranks. A good example is the issue of whether research surrounding the so-called matching law (Herrnstein, 1961) could have substantial applied value. Several authors (Epling & Pierce, 1983; McDowell, 1982) have argued that the practical benefits of these findings are there for the taking, that they need only be recognized and exploited. Ironically, others not only fail to see applied significance in this line of investigation, they actually cite it as a misguided trend in the basic area (Cullen, 1981; Michael, 1980b). It would be tempting to rationalize these opposing views as a minor skirmish. After all, research on matching is relatively new, and controversy at the forefront of discovery need not be disturbing. It may even be healthy. But the nature of certain types of controversy ought to make us uneasy. For one thing, a dispute may have deeper implications than are immediately apparent. In this case, for example, the molar orientation on which matching accounts typically rely is not just an addendum to reinforcement theory; it is a reformulation of a primary principle in behavior analysis. If it proves true, what can
be said of the thousands of reinforcement contingencies implemented under the old formulation? Have we been doing the right things for the wrong reasons? No one expects consensus in a young discipline, but diametrically opposed positions on what should be elemental topics are not uncommon in the behavioral family. It makes one wonder about just how much of the field's content, beyond its methodology, is well agreed upon.

Of course, the impact of sporadic disunity may be softened by pointing to the remarkable achievements of applied behavior analysis. The impressive gains made in this area seem to lend support to the entire behavioral establishment. There are, however, aspects of even these vaunted technical successes which are not reassuring. Recall the point made in the previous section regarding excessive creativity in the extrapolation of laboratory principles. The innovative character of many contemporary behavior-change procedures strains the meaning of technology as that term is used in the physical sciences. This point is not generally appreciated in the behavioral literature. Indeed, it is rare for a committed behaviorist to acknowledge that a large segment of our applied effort functions quite independently of the concepts developed and elaborated by basic research. Michael (1985) offers a notable exception. In commenting on the "systems" approach in behavior modification, he writes:

> Those working from this perspective may use the terminology of the conditioning laboratory if it proves convenient, but they seem to have little real compulsion to relate their procedures or results to basic conditioning concepts and principles. From their perspective the substantive aspect of the experimental analysis of behaviour is little more than one of several possible sources of ideas for manipulating independent variables, along with "common sense" or other theoretical orientations. (p. 160)

Although he is critical of this general orientation, Michael views it as something of an anomaly, an unfortunate departure from the genuinely scientific mainstream of behavioral technology. In fact, his criticism appears in a paper devoted to reaffirming
the close relation between fundamental research and application, the very relation (i.e., the "normal" one) that we have come to expect of a true natural science. Thus, as with most in-house commentaries, the scientific status of applied behavior analysis is left intact.

Closer to a broad indictment of the field is Ribes' (1977) claim that a real technology of behavior does not yet exist. He begins with the proposition that, "There is no adequate parametric research on human and social behavior; that which we do have lacks an adequate base in theory" (p. 419). From this, the following, somewhat provocative conclusion is derived:

What is widely called behavior technology is just a set of initial procedures that seem to work in very contrived situations. They consist largely of extrapolations from simple experimental paradigms or models, such as extinction or reinforcement. Further, the design of procedures based on parametric research data, as in other fields like physics and engineering, has not been realized. (p. 420)

Considering that this comes from the pen of an advocate, it is an extremely unfavorable assessment of behaviorism's practical contributions. Nevertheless, it should serve to remind even the most optimistic among us that what we now call technology is far from a straightforward affair. For a variety of reasons, applied behavior analysts cannot proclaim their allegiance to the laboratory with nearly the conviction of, say, practicing physicians. A veteran medical researcher's strong statement in a recent Science article says it best: "...every one of the great advances in modern medicine and surgery has come through research" (Comroe, 1978, p. 932). No psychologist can say the same.

How should the preceding collection be interpreted? The themes which have emerged from it--outside attacks, internal discord, a questionable source of innovation--may well constitute serious fissures in what is generally accepted as a solid framework. But these isolated pockets of bad news are exceptions to the rule.
They are not representative of the behavior-analytic self-image, and, except for Ribes, their authors stop short of saying that the applied field has been miscast as a true technology (but see London, 1972, for a behavior therapist's view).

Furthermore, lest one dwell too long on the possibility of a fundamental flaw, it must be remembered that the voices of caution are periodically offset by a louder and larger chorus of dedicated supporters. Baer (1981), for example, has denied that there is any bad news to report regarding the field's development. Others have not only endorsed the growth and "differentiation" in behavior analysis (Fawcett, 1985), they have actually predicted that it will ultimately serve as the foundation for a number of established social agencies, such as law and education (Fraley, 1981). Skinner (1983) has even suggested that the experimental analysis of behavior may rescue psychology.

In sum, then, there is a broad consensus among behavior analysts, at least with respect to their discipline's natural science status, and its tone is overwhelmingly positive. (Some of the assessments are almost self-congratulatory.) The enduring conventional wisdom, notwithstanding some general reservations and recommendations, seems to be that we are making slow but steady progress toward a mature science of human behavior. This, in turn, supports the widespread belief that the field's ascendancy is just on the horizon.

Yet, in the face of all this enthusiasm, there are rumblings. Several of the most prominent have just been reviewed, albeit briefly. We shall now turn to a closer examination of two phenomena within the behavioral movement that are especially distressing. They have received separate treatment in the literature, but may have a common origin. These two rumblings are perhaps the most telling in the sense that either, if substantiated, could force a reconsideration, even a retraction, of the behavior analyst's claim to scientific status.
Some Rumblings: The Split

Although still a relatively young discipline, behavior analysis is evidently old enough to reflect upon itself. During the last decade a number of commentaries with precisely that purpose have emanated from within the behavioral community. What unites this collection of articles, apart from the fact that many of their authors are prominent members of the field, is a common interest in what could be described as the health of applied behavior analysis. The principal health concern in this literature is over a conspicuous split that has developed between applied behavior analysis and the experimental analysis of behavior. The split, important in its own right, is essential to the present argument in that it provides one of two routes which should lead one to question whether so-called behavioral technology is truly the offspring of a natural science. As a detailed treatment of the split has been presented in a previous paper (Minervini, 1986), I shall use this section to abstract only those points relevant to the broader issue of genuine scientific status.

Skinner's opinions usually provide a convenient springboard for any evaluation of the movement for which he is primarily responsible. His proposal that the experimental analysis of behavior may yet rescue mainstream psychology (Skinner, 1983) suggests that the father of behavior analysis is not inclined to dwell on matters of internal discord. In fact, criticism of the external, nonbehavioral majority has become a Skinnerian trademark:

If only psychologists, we read again and again, could rid themselves of preoccupations—with the inner life, with individual autonomy, with the testing of artificial theories—science could at last yield a true behavioral technology, and with it the means to forestall the oncoming cataclysm. (Minervini, 1986, p. 107)
Ironically, a number of Skinner’s strongest advocates do not always share this single-minded emphasis. They have actually raised the possibility that our field may not be able to rescue itself, let alone anyone else:

Many contemporary behaviorists have recently started to focus on the details of their own revolution, not on various flights from it, nor even on the stubborn recalcitrance of traditional psychology. And from the resulting, lively dialogue has emerged a discontent among our own ranks: Separation within behavior analysis has become an issue. (Minervini, 1986, p. 107)

The separation in question has occurred along two related dimensions, each of which has been carefully documented:

The fundamental split is between the experimental and applied analyses of behavior...Here the principal worry is that the applied area, which presumably employs basic operant principles, has taken on a life of its own and seems to have lost interest in its laboratory ancestor. This trend has in turn spawned (or perhaps simply deepened) a second division: Within applied behavior analysis the pursuit of cures is now readily distinguished from the search for understanding. (Minervini, 1986, p. 108)

The primary applied-basic split has been quantified by several types of citation analyses (Pierce & Epling, 1980; Poling, Picker, Grossett, Hall-Johnson, & Holbrook, 1981) which together indicate that applied researchers rarely cite basic experimental work in their publications. Documentation of the second division, the separation (within applied behavior analysis) of cure-help research from that which emphasizes understanding, has been somewhat more subjective. Nevertheless, even here the results are unambiguous: As applied behavior analysis has grown it has become an almost exclusively technical enterprise, retaining little of its former interest in analytic or conceptual problems (Hayes, Rincover, & Solnick, 1980; Pierce & Epling, 1980).

Although the facts of this divorce are generally uncontested, opinions have been sharply divided as to whether it is a good or bad thing. The substance of the debate is nicely captured in a handful of landmark papers whose authors may be classified as
advocates of either cure or understanding. I have previously summarized the distance between them as follows:

Those who are primarily interested in cures generally accept separation as a natural outcome and want to move on to practical affairs. Those who promote scientific understanding seek to reestablish the ties between basic and applied research, thereby restoring the field to what they see as an original state of integration. (Minervini, 1986, p. 108)

Azrin (1977) began this dialogue a decade ago when he pitted the goals of understanding and cure against one another and urged that the business of applied behavior analysis ought to be about cures. This was a position born of frustration and impatience. While working as a clinician he had searched the literature for concrete solutions to a number of behavioral problems. There, he discovered no shortage of analog and demonstration studies, theoretical analyses, case histories, and the like. But actual treatment evaluations, those meeting the minimal criteria of objective specification of procedure and outcome (and incorporating a comparison with at least a no-treatment control condition), were almost impossible to find. Applied researchers had failed to squarely confront the practical aspects of their subject matter. They had, according to Azrin, designed studies with "implications rather than applications" (p. 141). He blames much of this on an inadequate methodological strategy, that is, on the very practices intended to advance scientific understanding. His methodology, on the other hand, emphasizes dimensions appropriate to outcome research. These include: degree and duration of improvement, speed and cost of treatment, percentage of patients benefitted, and social acceptability of the procedures used. Azrin's overriding philosophy is that research carries with it a "promissory note" of societal benefit. Delivery on that note is already long overdue. Applied psychologists, he believes, must now begin to make good on their promise by designing studies that are uncompromising in their outcome orientation.
That was the opening salvo in a conflict which has yet to be resolved. A number of replies from the advocates of understanding were soon forthcoming. The first, by Deitz (1978), was the most direct rejoinder to Azrin’s paper, but several others in the same general spirit would follow within the next few years (Birnbrauer, 1979; Michael, 1980b; Pierce & Epling, 1980). Their common theme is that Azrin’s advice is already being followed too well. Although these authors are careful not to degrade the value of applied research, per se, they all warn that the loss of a strong analytic tradition would be devastating to the field’s progress. Deitz’s message, for example, is that shifting the researcher’s emphasis to the production of cures would be premature, and that it might actually cut off the source of future cures. His argument presupposes an evolutionary process analogous to the one that took place in medicine, a process in which the growth of a powerful technology depended upon a commitment to scientific understanding (see Deitz, 1982). Indeed, that logic is the cornerstone of all those who are wary of prepackaged, unanalyzed cures.

The positions expressed by Azrin and Deitz share an interesting strategic similarity. Both authors are unhappy with the current state of affairs and both employ warnings of what will happen if their advice is not followed. Stirring up concern seems to be the order of the day:

One must now decide whether to worry more about the infinite postponement of beneficial discoveries (the risk associated with circuitous research), or about the dismal prospect of never having a true scientific technology (the possible price of seeking only immediate cures). What is to be done? (Minervini, 1986, p. 110)

Yet these two poles do not represent the full range of opinion on this complex topic. A third alternative, forcefully articulated by Baer (1978, 1981), is that there is nothing to worry about. He bases his optimism on the premise that the apparent differences between basic and applied research are illusory, that is, that the two endeavors are simply points on a continuum. For him, even the most molar, unanalyzed treatment
packages constitute valid examples of basic science, so long as they prove to be functionally related to behavior. The logic here is that gross evaluations of independent variables should actually be thought of as generality tests of familiar basic concepts such as reinforcement, punishment, and the like, whether or not the researcher conceives of the study in those terms. Thus, according to Baer (1978), "...no research that solves a social problem is less than basic, even if it is applied as well" (p. 16). And if the so-called divorce is actually superficial, it follows that there is nothing to worry about.

As for the clinician's isolation from the body of published experimental work, Baer (1981) sees it as a blessing in disguise. The applied area's ignorance of the experimental literature has itself, he says, supplied the makings of an important, albeit crude experiment. Put simply, it asks the question: Can applied behavior analysis, severed from its foundations in the operant laboratory, "...get away with it for long" (p. 91)? Deitz, of course, would be unwilling to risk the field's survival on the outcome of that experiment, but it is, in any event, already well under way. For Baer, that is just an additional source of good news. In fact, the only development he regards as bad news is that the advocates of understanding have seen fit to label certain trends in our profession (e.g., see Michael, 1980b) as bad news.

Except for the comments by Baer, this is a debate full of prescription--more cures, improved understanding, better training--each camp's position has its own set of practical implications. Much of this advice, especially that which is not mutually exclusive, should probably be pursued. But it is noteworthy that the essential separation between laboratory and clinic has also been treated prescriptively, and that may constitute a misinterpretation. Consider again the argument that the applied-basic split is something to be repaired. As I have suggested elsewhere, "...the prescriptions for reintegration make sense only if the current situation is the result of
accidental, or at least arbitrary circumstances which can indeed be rectified" (Minervini, 1986, p. 113). Those circumstances warrant a closer look. Which of them has yielded the impression that the split can be fixed, and is such a proposition justified?

When the advocates of understanding call for reintegration, their logic proceeds from the assumption that the applied area has, or once had, firm roots in the laboratory. The threat, they believe, is overspecialization. Likewise, when the advocates of cure applaud the independence of applied behavior analysis, they too accept specialization as the underlying mechanism of action. Neither group questions the applied analyst's scientific foundations; neither disputes the notion that technological specialization is the proper account of the split. They simply disagree about how much is appropriate at this stage of the field's development. If that is an accurate characterization of the split, it is easy to see why some writers are so unconcerned about it (Baer, 1978, 1981; Malott, 1981; Poling et al., 1981); after all, a healthy young science should specialize as it matures:

Less analysis and more cure, less well trained people, the repetitious use of just a few basic principles—these could all simply indicate specialization of function, and, as Baer has pointed out, how could that be bad news? (Minervini, 1986, p. 114)

It couldn't. On the other hand, if the correct interpretation of the split were not accidental, but inevitable isolation, the entire behavior analytic enterprise would be cast in quite a different light, and that might be bad news indeed.

The split may not be so much a matter of the clinical investigator's distance from a scientific base as it is about the actual existence of that base. We have long supposed that applied behavior analysis is unique among the indistinct mass of therapies in psychology because it alone can lay claim to practices founded on
laboratory-derived principles. I now suggest, for reasons that will become clear in subsequent sections, a suspension of that widely held belief.

A number of puzzles surrounding the split are nicely resolved if we concede the possibility that our applied efforts have too hastily been assigned the label of technology. First of all, the debate between the advocates of cure and understanding is not really about specialization. If it were, Azrin and Deitz would be negotiating a question of emphasis, a few degrees of movement in an essentially sound framework. But their respective complaints are too serious for that to be the case. To complicate matters further, each proponent seems to be representing a minority position. Could both be correct despite their directly opposing points of view?

It may be that each author justifiably laments a scarcity of the type of applied research which he promotes, while at the same time overestimating the strength of his opposition. Azrin is probably right when he says that very few studies have squarely addressed the dimensions appropriate to outcome research (speed, cost, and the like). But when he assumes that the majority of studies (which he criticizes as being indirect) constitute good examples of understanding-type research, it is clear that his standards for that label are more flexible than Deitz's. The same holds true for the opposing pole. Little of the work which Deitz categorizes as technology would satisfy someone with a thorough commitment to outcome-oriented research. Indeed, it may well be the case that very little of what has been produced in our field could meet the standards set by a strong advocate of either position. (Minervini, 1986, p. 36)

Although these are legitimate grievances, it does not follow that technological specialization (too much or too little, depending on the advocate) is the culprit, even though the arguments are often stated in those terms. An alternative account is that writers on all sides of the issue have conceived of applied behavior analysis as a much more scientific enterprise than it actually is. The corpus of behavioral fact and principle could simply be insufficient for substantial yields in either understanding or cure. From this perspective separation is the inevitable result, not of specialization, but of the basic science's inability to complete the bridge from laboratory to clinic.
The split, in other words, ought not to be viewed as an accident, but rather as a symptom of the absence of a genuine scientific base.

Contrary to what Baer suggests, it is not the case that behavioral principles are obviously at work (and merely unidentified) in even molar independent variables (such as a feedback program in industry). In fact, the opposite is true. Many researchers who actively invoke familiar principles in their studies often do so gratuitously. Neither is it the case, as the advocates of understanding suggest, that the applied area suffers from an excessive emphasis on technology. It suffers from the absence of technology:

If we had a true scientific technology no one would object that there is too much of it, or that it has become too specialized, or that it employs too few basic principles. The truth is that much of the applied area makes no use of fundamental discoveries (such as they are) except as heuristics...It is the failure to develop even a primitive (but solid) technology that has people worried. If the laboratory had been producing information of substantial clinical relevance (even allowing for a quarter-century lag time), our exhortations to stick to science would be superfluous. Equally unnecessary would be the various attempts to immunize young behavior analysts against the dangers of eclecticism... What could compete with the attraction of an actual technology? Behavioral practitioners are committed to observable results. It is unlikely that a series of accidents could have caused them to stray so far from a potential wellspring of innovation. (Minervini, 1986, p. 117)

Furthermore, to insist that integration be forced on the applied researcher is a position that is increasingly difficult to defend; it simply should not be a matter of prescription in a science born in the twentieth century. The fact that close contact with the laboratory has not occurred naturally will likely prove more significant than has generally been recognized thus far.

And what of the creativity that has become a trademark of applied behavior analysis? This too is rendered understandable if the assumed scientific conception is abandoned. I have earlier suggested that a certain type of applied innovation might, paradoxically, constitute a weakness. It should now be clear that the weakness is with our technological base. When the behavioral practitioner confronts a problem in
a clinical setting which serves, say, an adult outpatient population, detailed guidance is often a "seat-of-the-pants" operation (London, 1972). This has been true since the outset. How else could Skinner (1948) have designed his fictional (Walden Two) community—surely one of the most ambitious projects ever proposed in behavioral engineering—as early as 1948? The usual answer, of course, is that he extrapolated from laboratory discoveries. But at that time there was very little material to extrapolate from. The first journal devoted to basic operant research (the Journal of the Experimental Analysis of Behavior, or JEAB) was more than a decade away, and it would take twice that long to launch the field's principal applied organ (the Journal of Applied Behavior Analysis, or JABA). Was Skinner's extension too creative to be grounded in an incipient science? One wonders if the substance of his utopia would be appreciably different if it were written today. To what extent could it profit from thirty years of basic research? Do they read JEAB in Walden Two?

In the inaugural issue of the Journal of Applied Behavior Analysis, Baer, Wolf, and Risley (1968) described the guidelines that they hoped would shape this newly emerged endeavor. Their paper gave no hint of the split that would so sharply divide the experimental and applied analyses of behavior. In fact, there were few gloomy hints to be found anywhere in the behavioral community during that era. Laboratory discoveries had apparently just spawned a new applied science and its associated technology. The usual connections were presumed to exist and it was expected that they would continue. By 1981, however, separation had not only become a familiar theme, it had even been quantified. One group of authors who measured the split summarized their results as follows: "The gulf between applied behavior analysis and some areas of the experimental analysis of behavior is wide and unbreachable" (Poling et al., 1981, p. 102). Indeed, a review of each of the three studies which
attempted to document this phenomenon leads to the same sort of inescapable conclusion:

For good or ill, the once unified behavioral family is now a house divided, and there appears to be little chance of its reconstitution. Of course, one must acknowledge that the bearing of all this on the field's scientific health is still open to interpretation. But, this much remains certain: The apple has fallen far from the tree, and in ways that would have been difficult to imagine at the time of Baer, Wolf, and Risley's classic publication. (Minervini, 1986, p. 86)

That condition alone, if not sufficient to provoke outright apostasy, should at least shake the faith of a true believer. The split could readily have been taken as prima facie evidence that our field merely resembles a natural science. Curiously, it has not had nearly that effect. Perhaps it is the type of evidence which is not as compelling as it might be because it can be explained away. Recall Baer's point that the divorce is an illusion, that the basic science's concepts are relevant in applied work whether or not they are explicitly labelled. Besides, what about the thousands of more obvious cases in which fundamental principles are invoked and seem clearly to apply? Although we may not have achieved a full-blown science of human behavior, it certainly appears as though the beginnings of one are discernable. Have not Skinner's discoveries provided a core of essential information which has repeatedly survived extrapolation to natural settings? Or are the assumed connections between science and practice more tenuous than we have come to believe? Such questions, as any Skinnerian knows, point directly to an examination of the scientist's verbal behavior about behavior. A close look at our technical vocabulary may reveal something about whether the scientific basis of applied behavior analysis is real or imagined. We must, therefore, now take up the subject of behavioral language, for it constitutes a second challenge to the natural science conception of the field.
Strong assertions about complex topics are usually risky. Nevertheless, it seems safe to list among the requirements of science that its technical language should reflect the highest levels of integrity and precision. While we may grant that the knowledge base underlying concepts like mass or mitosis or valence is constantly shifting, it does not follow that those terms are open to broad interpretation. What is known may change but that fact is seldom called upon to excuse lapses in the everyday use of a technical term. Not that lapses are common—in general, the scientist's language is quite precise. This is not, of course, the result of democratic policy making or strict methods of enforcement. Rather, it is simply a feature of any endeavor which depends on cumulative information that a certain amount of standardization is indispensable. It is difficult to imagine how the established natural sciences could have become so, were it not for their unusual degree of linguistic uniformity.

Over the years, the behavioral community has engendered its share of terminological conventions that on close analysis have proven faulty. A number of these clear-cut abuses of our special vocabulary have been recognized as such and addressed in the literature. Let us consider a few representative cases. Crossman (1983) offers a useful example of just this sort of critique in the area of reinforcement schedules. His complaint is with the popular notion that the common slot machine yields payoffs according to a variable-ratio schedule. By dismantling one of these devices, he was able to show that each pull of the handle does not increase the probability of a payoff (thereby violating a characteristic of all ratio schedules). Instead, the actual arrangement is that of a constant-probability schedule. Yet for years, scores of teachers (myself included) have cited the slot machine as a real world analog of one of Skinner's four basic schedules. The illustration, Crossman points out, even appears in several introductory textbooks on behavioral psychology.
The simple fixed-interval schedule is another technical concept whose direct relevance to human affairs has been grossly overstated. Here again, however, criticism of sloppy usage has eventually been forthcoming. Michael (1980b) has recently debunked the longstanding misconception that students who cram for upcoming examinations are exemplifying fixed-interval (FI) performance. He argues that this widespread error, along with a host of similar instances of what he calls "superficial nonsense," is one of several harmful effects brought about by an increasing technical shift in the applied field. As a corrective measure, Michael recommends meeting the challenge of complex human behavior with equally complex behavioral analyses.

A possible attempt at such complexity comes from Poppen (1982), who provides quite a detailed treatment of the fixed-interval scallop in human affairs. Poppen's elaboration consists of describing about a dozen variables (other than the schedule itself) which have been shown to influence human FI performance in the laboratory (the point being that the FI label is often applied too quickly to cases of everyday behavior in natural settings). Although the results of this tactic are somewhat predictable, its overall effect is nonetheless salutary. It is, perhaps, only a small revelation to discover that factors like instructions, competing contingencies, and interval duration can modulate the shape of the prototypical scallop. Even so, that kind of information does serve to caution against the simplistic attribution of any accelerated pattern of behavior to standard fixed-interval reinforcement. Thus, Poppen's message concerns the precision of verbal practices within a scientific community, and, like Michael's and Crossman's, it is directed at those who would see schedules where they are not.

Reinforcement schedules are not the only concepts prone to slippage when behavioral language deteriorates. Common descriptions of applied procedures, such
as response cost, are also not immune to the effects of inconsistent usage (see Luce, Christian, Lipsker, & Hall, 1981). Nor are the defining features of fundamental principles, as they too have been shown to require periodic examination and adjustment (e.g., see Michael's 1980a comments on the discriminative stimulus). Even a term as familiar and elemental as generalization can become the subject of controversy.

For many years, applied behavior analysts have subscribed to the same relatively narrow definition of generalization as their laboratory-based counterparts. A decade ago, however, that tradition was challenged when Stokes and Baer (1977) proposed a new, much more "pragmatic" interpretation of the term, one which deliberately violated standard usage. For them, "generalization may be claimed when no extratraining manipulations are needed for extratraining changes; or may be claimed when some extra manipulations are necessary, but their cost or extent is clearly less than that of the direct intervention" (Stokes & Baer, 1977, p. 350). By considering together any and all beneficial effects not directly due to treatment, this approach obviously makes relevant many behavioral processes other than simple stimulus (or response) generalization.

According to Johnston (1979), those other processes belong to a broader domain, which he calls generality, and should not be blended with the specific, stimulus-bound phenomenon whose niche has already been established. Regarding the issue of transfer to nontraining settings, he says:

In fact, there does not appear to be a distinct phenomena, effect, or process to describe, and there is a danger in any summary term that disguises the actual principles at work. It is simply that the behavior modifier’s job is not finished until the subject is behaving appropriately in all of the desired settings. (p. 2)

Johnston's paper, it should be noted, is more than a treatise on language. Having drawn the distinction between generalization and generality, he goes on to list some
categories into which generality might be subdivided, and to comment on the style of research needed to address them. Nevertheless, his position on generalization is compelling in its own right, and, as an argument for tightly defined basic concepts, it is well suited to the present theme.

The final entry in this abbreviated series aims to clarify one of the most frequently used terms in the radical behaviorist's vocabulary. Lattal and Poling (1981) make a number of claims (only a few of which will be mentioned here) concerning the word contingency and its variants. They suggest that contingency suffers from multiple meanings, and that these should be eliminated by restricting the term to its generic usage. In other words, contingency would merely serve as a global referent to the familiar relation between antecedent stimulus, response, and consequence; it would convey no information about dependent relations among the three components. Specifically, nothing would be implied about the response-consequence relation. As a substitute for using contingency (or the adjective contingent) to describe this relation, the authors recommend the unambiguous phrases response-dependent and response-independent. This is, I think, only a partially satisfactory solution. Although Lattal and Poling argue persuasively that response-independent is a superior alternative to the somewhat ambiguous term non-contingent, they fail to make the case that anyone has ever been confused by a reference to (for example) contingent food. Indeed, nearly all behavior analysts use contingent as a synonym for dependent. It is unlikely that so strong a convention will be overcome by the force of logical symmetry. Contingent might turn out to be an awkward but survivable complement to response-independent.

A similar difficulty plagues another of Lattal and Poling's main themes. Considerable attention is devoted to what they see as the problem of "functional," as opposed to procedural, descriptions of response-event relations. Functional
definitions emphasize process, or changes in the dependent variable, and Lattal and Poling warn of the drawbacks associated with this interpretation of contingency. But where are the offending cases? The authors offer little in the way of evidence that this sort of infraction is as pervasive or troublesome as they portray it to be. In fact, considering its history in the operant literature, it is not at all clear that contingency could even be rendered as a behavioral process. Its linkage with the procedure of consequation is overwhelming. Hence, the threat of multiple meanings seems to have again been overstated.

These shortcomings are unfortunate since they obscure many of the good points that Lattal and Poling do make. They provide, for example, useful criticisms of expressions like free reinforcement and superstitious behavior. And, they correctly state that when a response-consequence relation is labelled as a contingency, the question of temporal contiguity between the response and its consequence is not addressed. (Of course, this is a deficiency which is shared by the phrase response-dependent.) In spite of its weaknesses, therefore, their paper must be credited with several contributions to the present collection.

The preceding is not intended as a complete survey of every type of linguistic rumbling in our field. The examples chosen, however, do represent a characteristic reaction of behavioral scholars to what they perceive as a problem within their own professional specialty. Note that all of the violations cited are characterized as mistakes, and that most of these are attributed to various forms of superficiality. It would be reasonable, then, to recommend more analysis as a solution (as is the case with the articles by Crossman, Michael, and Poppen). Alternatively, one might try to either establish a more precise convention (as Lattal and Poling did) or to argue for the retention of an existing standard (recall Johnston's position on generalization). But there is an additional significance to this advice which must not be overlooked: Each
author calls for only a piecemeal revision in verbal practices. The majority of these commentaries are not presented as symptomatic of a more widespread deterioration in behavioral language. Moreover, none question the fundamental sufficiency of the conceptual framework which they attempt to shore up. Indeed, the overall effect of exposing oneself to an occasional round of acknowledged verbal abuse is (for the advocate at least) to be left with the scent of healthy self-criticism in the air. Having thus been apprised of a manageable number of pitfalls, the informed behavior analyst may feel free to proceed with the elaboration of what now seems to be an even sounder and more thoroughly vindicated program of human natural science. If that is a legitimate posture, then I have wrongly titled this as a literature of rumblings—proportional remediation might have been more appropriate. On the other hand, should the concerns just sampled reflect a much larger set of deeper and unacknowledged difficulties, their implications would be worth considering.

Metaphorical Extension in the Analysis of Human Behavior

A Question of Principles

In the first section of this paper, I attempted to identify some of the elements that qualify an enterprise as genuine science. The pivotal requirement was that the discipline in question be principle-based. Real principles (the ones that endure) are not easy to come by. They must be more than inventions of convenience. We expect them, for example, to be internally consistent, and to be verifiable. Moreover, we assume that, once discovered, their meanings will be relatively inflexible, refinements notwithstanding. This is where the language practices of the natural scientist become crucial. Concepts like mitosis and meiosis are compelling because of their practical
implications, but those implications could hardly be grasped if the two terms were employed interchangeably.

Because many of the acid tests for a principle's validity cannot be carried out in a vacuum, it is important that the precision of a technical vocabulary be extended, intact, beyond the laboratory. For psychologists, this means observing strict standards of linguistic uniformity whenever a concept with roots in basic research is said to be used, especially with respect to human behavior.

Such use also constitutes relevance testing, and it may take a variety of forms (Verplanck, 1955). Perhaps the commonest strategy, variously called extrapolation by analogy (Verplanck, 1955) or simply interpretation (see e.g., Skinner, 1974), is to link (verbally) principles and observed phenomena on the basis of plausibility alone. This method is particularly vulnerable in that it is not supported by experimental test. Nevertheless, interpretation is a legitimate and useful occupation, and it is sometimes the only avenue open to one who works on a discipline's frontier (Schnaitter, 1978).

A more aggressive alternative to interpretation involves the direct application of a basic principle (in either an experimental or a therapeutic setting) to some aspect of human behavior. Baron and Perone (1982) summarize this approach as follows:

A different way of testing the generality of animal-based principles is to apply procedures suggested by the principles to the solution of practical problems. Thus, operant conditioning principles may serve as the basis of programs designed to modify the behavior of pupils in the classroom or patients in the psychiatric hospital. The success of the program then is taken as support for the validity and generality of the principles in question. (p. 145)

Reliable successes of this type can certainly increase the practitioner's confidence in his or her clinical procedures. As we shall see, however, the question of whether any support should accrue to a given scientific principle is a tricky business.

Although verbal interpretation and technical application are not the only ways in which to use (and hence test) a fundamental principle, they are probably two of the
most exploited activities in psychology. Behavior analysis is no exception in this regard. We shall, therefore, consider the precision with which behavioral terms are rendered in both contexts. Such an investigation, with an eye toward deviations from standard usage, may reveal what our language practices have to say about the field's standing among the established sciences; for if principles are gratuitously invoked on a routine basis, the entire scientific conception would become suspect. Thus, the questions for the present chapter are these: What of our basic concepts? How precisely are they employed in interpretation, and, when used in therapy, do they tightly link laboratory and clinic? Have we, in applied behavior analysis, the product of a new natural science expressed in technology?

Some may contend that the true touchstone of an ostensible science lies in its accomplishments, not in the purity of its theoretical vocabulary: that what we do has primacy over how we talk about what we do (e.g., London, 1972). Unfortunately, that is a debate with no clear resolution in sight; in any event, it is beyond the scope of this paper. I shall be content to address only those who believe that behavioral theory does in fact drive a good deal of actual practice, and that its terms cannot, therefore, be considered superfluous.

A second objection might be phrased as follows: Even if linguistic integrity is an index of scientific health, why include interpretive accounts in the evaluation? Is not interpretation by its very nature an act of license? To some extent it is, but that fact does not excuse loose talk. Speculation must follow the rules of fact and logic, so it can usually be judged as good or bad. (Several of the critiques reviewed in the previous chapter were about the quality of interpretations.) Furthermore, extrapolation constitutes so much of the behavioral culture (e.g., Skinner, 1953, 1974) that to exclude it from the proposed assessment seems unjustifiable.
Having said that, however, it does not follow that an interpretive violation is as serious a matter as terminological errors which occur in situations where principles are actually being manipulated. A sloppy verbal extrapolation can sometimes be forgiven in the spirit of the exercise. On the other hand, should the technical language of the practitioner prove to be systematically flawed, the implications would be devastating. Consider, for example, an applied behavior analyst who employs behavioral contracts with juvenile offenders, and who claims to derive this procedure from the operant paradigm. If he cannot justify his talk of responses, reinforcements, and contingencies, the apparent theoretical base for a common practice becomes an illusion. Moreover, unlike the mistakes covered in the preceding chapter, this sort of verbal abuse would be ubiquitous. And, if a professed exception (i.e., slippage in technical language) turns out to be the rule, it would force (as the main heading of this section suggests) a general reconsideration of the field’s scientific health.

Revision on this scale is no mere abstraction; it threatens the soundness of some rarely questioned working assumptions. An enduring conventional wisdom in applied behavior analysis states that its techniques have their origins in the early discoveries of the operant laboratory. Although not an obviously controversial position, it has by no means gone unchallenged. One particularly uncharitable depiction of the "Evolution of Behavior Modification" comes from London (1972), who argues that theory is virtually irrelevant as a source of clinical innovation:

The early growth of behavior modification as a professional specialty was largely polemical and political, not theoretical, and most of its scientific hoopla evolved to serve the polemical needs of the people who made it up—not all of it, however, and not only polemical needs.

The study of learning for behavior therapists, in fact, was always more for the purpose of metaphor, paradigm, and analogy than for strict guidance about how to operate or about what it all means. Whatever value theory may have for dictating laboratory procedures, therapeutic operations have been essentially seat-of-the-pants affairs, and still are, because they address...
immediate practical problems that require solutions in fact, not in principle. (p. 914)

The impact of London's comment is softened somewhat by the fact that its context is behavior therapy rather than applied behavior analysis. It is unlikely, for example, that the editorial board of the Journal of Applied Behavior Analysis would be willing to categorize the bulk of their enterprise as an essentially "seat-of-the-pants" affair. Nevertheless, the theme he raises is close to one that will be echoed throughout this paper: To what extent are theoretical concepts actually at work in applied settings?

London is concerned with the distance between conditioning theory and clinical practice. (We shall extend this concern to include the distance between theory and interpretive verbal extrapolation as well.) He measures that distance with the terms "metaphor, paradigm, and analogy." Later it becomes clear that the two principal categories are metaphor and paradigm. Neither of these, London argues, carries the force of a scientific law; yet together, they capture most of the procedures used in behavior modification, procedures which he characterizes as follows:

A few are paradigms or models, that is, ideas that seem to explain a group of facts pretty precisely; most are metaphors or analogies, that is, ideas that look like they might fit a group of facts, where there is no clear evidence that they really do.

In general, the treatment methods derived from speculations about conditioning studies of animals are metaphorical or analogous, especially desensitization and implosion, or flooding. In general, the treatment methods that fall under the heading of education or training or, in jargon, of instrumental learning are paradigmatic or exemplary, including modeling, shaping, and possibly aversion techniques. (P. 917)

That is somewhat illuminating in the sense that metaphor is apparently a looser business than paradigm, but the concrete basis for the distinction is still not obvious. Perhaps it is best expressed in London's rationale for the separate classification of desensitization and aversion therapy:
Desensitization is only a metaphor or analogy to conditioning: first, because it involves a specific cognitive process (the use of language) that classical conditioning does not; second, because it involves a mechanism of sequential imagination that can only be guessed at from conditioning studies; and third, because it is subject to a variety of successful variations that could not be predicted from the situations from which the metaphor was derived in the first place....Aversion treatment is more paradigmatic than metaphoric because its critical stimulus is not cognitive; that is, it hurts the patient physically. Giving a person an electric shock is much more like shocking a dog than telling scary stories to a person is like blowing air up a rat's behind; ergo, it is more paradigmatic than metaphoric. (pp. 917-918)

What is the relevance of this perspective for the applied behavior analyst? It is worth noting that London's observations regarding basic principles have been available for some time. His paper was published in 1972 (at a period when behavior modification and behavior therapy were often blended) but it has not become a landmark work in the 15 years since its first appearance, at least in what has emerged as the literature of behavior analysis. This may be because the field was then too young for serious reflection and self-criticism, much of which has appeared within the last decade. Or it may simply be that the thrust of London's commentary was perceived to be about (as he says in his title), "The End of Ideology in Behavior Modification," a seemingly political topic. In any case, his remarks on the metaphorical nature of most behavior-change techniques are provocative. They provide a useful approximation to the analysis of language that is about to be presented here. Although London did not specifically examine abuses of technical terminology, his position on the indirect role of basic research leads to the same general conclusion, namely that the behavioral community has seriously overestimated the scientific basis for its clinical practices.

The difference between London's argument and the one that follows can be summarized in this way: He maintains, for example, that desensitization (which is the name of a procedure and not a principle) is only a metaphor of respondent conditioning because of the obvious differences between what is done in the
laboratory and what is done in therapy. My approach, on the other hand, will concentrate on how the behavior analyst talks about a given procedure. I will attempt to show that technical terms such as reinforcement, punishment, and discriminative stimulus are frequently emitted as metaphors, even when the speaker is deliberately striving for precision. Because these are the names of principles and not procedures, we shall focus on linguistic as opposed to procedural metaphor, that is, on the degree to which a word may deviate from standard usage, rather than on how an air burst compares to a scary story. And, as already noted, we shall look for such metaphors in the contexts of interpretation and application—both activities test the veracity of principles, and both would be undermined by improper use of the behaviorist's technical vocabulary.

London is not, of course, the only author to comment on the effect of metaphor in science. The topic is, in fact, quite broad, and it has some history in the psychological literature. Moreover, that history is by no means one-sided. Most of us are familiar with at least one anecdote about how a metaphor or analogy helped bring about some important discovery in the physical sciences (see, e.g., Dreistadt, 1968). Similar contributions have been proposed for behavioral science, yet within psychology there is still considerable debate as to whether metaphor, in general, is a good or bad thing (e.g., Boswell, 1981; Hoffman, 1979; Observer, 1978, 1980, 1983). In particular, the computer metaphors of cognitive psychology have proven to be a continuing source of controversy, even among radical behaviorists (e.g., Marr, 1983; Schnaitter, 1983).

These are useful treatments but they can play no direct role in the present analysis because, although each has something to do with metaphor, none (including London, 1972) are explicitly concerned with the metaphorical rendering of terms which describe fundamental principles. It is one thing to say that Darwin somehow profited
by thinking of evolution as a "Tree of Life" (see Dreistadt, 1968); it is another matter altogether to suggest that a chemist speaking at a professional conference might be using the phrase ionic bond metaphorically. Detecting the presence of the latter phenomenon is the task at hand, and it is one which raises a definitional problem. In order to determine the extent of metaphorical language in the behavioral community, one must first decide on exactly what constitutes metaphor. Thus, we turn next to a relatively precise formulation of metaphor, and then to an examination of its prevalence in our technical terminology.

**Tact Extension: A Vehicle**

Most radical behaviorists have some familiarity with the analysis of language which Skinner presents in his classic book, *Verbal Behavior* (1957). At the center of this ambitious project is a classificatory scheme, the constituents of which are a small number of verbal operants grouped functionally by the types of variables which control them. Skinner calls the most important of these verbal operants the tact, defined as an elementary relationship in which a particular response topography (maintained by a differential history of generalized conditioned reinforcement) is evoked by a prior nonverbal stimulus. A ready illustration of the tact relationship is seen in the child who has an increased tendency to say dog in the presence of a dog, provided, of course, that this is the result of a history of consequation by (for example) parental approval. It is worth noting that the stimulus and response modalities of the tact may vary: The antecedent stimulus need not be visual; the response need not be vocal. The important thing is that a particular response form has come under the control of some nonverbal discriminative stimulus. A deaf child who emits the sign for dog at the sight (or characteristic smell) of a familiar dog is also exemplifying the simple tact relation.
Having described the elements of a basic tact, Skinner proceeds to the next level of complexity, tact extension. Tact extension occurs when a previously learned response is evoked by a novel stimulus. These stimuli achieve their control by virtue of the properties they share with the original training stimulus. If the child in our example had been taught to say dog only with respect to the family dog, and now emits the same response upon seeing a neighbor's dog, we may say that the tact has been extended. The control exerted by the sight of the new dog is due to its similarity with the original dog.

Skinner subdivides the topic of extension into a number of categories, based on the degree to which properties of the novel stimulus overlap with those of the original training stimulus. Only three categories of extension are relevant here. **Generic extension** occurs when the controlling properties of the novel stimulus are precisely those properties which defined the original stimulus. Defining properties are features or elements of a stimulus class that are considered essential by a speaker's verbal community. Thus, the child in our example was reinforced for saying dog in the presence of the first animal because it possessed the elements of "dogness" required by the verbal community. If said elements were the ones responsible for extension of the child's response to the new dog, then that extension should be classified as generic: The properties that made the new dog an effective (although untrained) stimulus were exactly the properties that the community required in the original training situation.

Skinner's next category is called **metaphorical extension**. It is a crucial topic for the present analysis and it is also the point at which we depart from his treatment in favor of one with greater internal consistency. Skinner introduces the section on metaphor with the following statement: "A second type of extension takes place because of the control exercised by properties of the stimulus which, though present
at reinforcement, do not enter into the contingency respected by the verbal community" (Skinner, 1957, p. 92). This means that in metaphorical extension no defining features are present, whereas in generic extension they all are. That is clear enough as a definition, but it does not address the many cases of extension in which a few (but not all) defining features are present in the novel stimulus. For example, if the child says dog in the presence of a cat, and it is determined that the controlling property of the cat is the fact that it has four legs, what type of extension is at work? Having four legs is certainly one of the defining features for dogs--it is "respected by the verbal community"--so according to Skinner's definition this cannot be metaphor. Yet, it is obviously not generic extension either.

With a little effort Skinner's approach could probably be salvaged. Fortunately, however, a more logical solution is available. Jack Michael has provided the field with an elegant, alternative breakdown of tact extension, and has used it in his classes on verbal behavior for approximately 20 years (J. Michael, personal communication, November, 1987). Michael's formulation (which was adopted and published by Peterson, 1978) distinguishes the varieties of tact extension on the basis of whether all versus some defining features of a class are present in the novel stimulus. If all defining features are present, the extension is generic; if some are present, it is metaphorical. By this account the previous example is clearly a case of metaphor, inasmuch as cats possess some, but not all of the defining features for "dogness."

In addition to being simple, the proposed modification also nicely handles Skinner's third category of tact extension, metonymy. When a novel stimulus evokes a tact response, in spite of the fact that it has none of the defining properties appropriate to that response, the extension (in Michael's system, and in Skinner's) is called metonymical. The controlling property in metonymy is an incidental (i.e., nondefining) feature which is common to both the original and novel stimuli. For
example, if a child is taught to say *dog* with respect to a Dalmatian, the spotted pattern (although not a defining feature of dogs) could evoke the same response if the child later saw an abstract painting which had a similar arrangement of spots. Metonymical extension does not respect the conventions of the verbal community but the mechanics of stimulus control are the same as they were for the previous two types of extension: in each case the transfer is based on common properties.

A final point to be made concerning extension is that it is usually a first-time phenomenon. Once a response has been extended to a novel stimulus and has been reinforced, future instances of control by that stimulus no longer constitute examples of extension. The formerly novel stimulus now simply evokes the response as a stabilized tact. Thus, the first person who referred to autumn as an *explosion* of color was probably engaging in pure metaphor, but the phrase was then passed along as a standard form. Of course, many extensions never get reinforced; the ones that survive presumably do so because of their usefulness to the verbal community.

How are the tact and tact extension relevant to the present analysis of behavioral language? Skinner says that the very essence of science hinges on the type of extension which it permits:

> The difference between the generic and the metaphorical tact is one of the great differences between science and literature. Scientific verbal behavior is set up and maintained because of certain practical consequences. Nothing beyond a generic extension will eventually serve...In literature there are no similar practical consequences and metaphorical extensions therefore prevail. No one will deny that they are effective; but the advantage we gain by reading Dostoyevsky or Joyce, in coming to share their "knowledge" or "understanding" of human nature, is very different from the advantage gained from scientific study. (Skinner, 1957, p. 99)

This implies—to bring the point home—that the behavior analyst is also restricted to generic extension, at least when speaking technically. If terms such as reinforcement and punishment are emitted as tacts, the nonverbal environmental antecedents which control them must possess all of the defining features required by
the appropriate scientific community. Anything less could hardly maintain one's confidence in the field's natural science status. Fundamental behavioral principles (and to a lesser extent, procedures) must be interpreted uniformly.

On that premise we shall, with the aid of the material just described, examine a sample of our most important language practices. The primary instrument of examination will be Michael's all-some-none adaptation of Skinner's analysis of tact extension. In other words, we shall match the defining features of a given concept, such as reinforcement, against the conditions of its actual usage so as to determine whether all, some, or none of those features are present when the term is invoked.

I emphasize that tact extension is merely an instrument, or vehicle, one of several that could have been chosen to reveal the degree of precision with which technical terms are rendered. To be sure, tact extension provides a particularly convenient framework because it is familiar to many behaviorists. Moreover, it is a scheme which lends itself to the rapid classification of a given verbal episode as generic, metaphorical, or metonymical extension. Nevertheless, what follows should not be construed as a general endorsement of the behavior analytic approach to language. One reason for caution is that the characterization of scientific statements as tacts, although theoretically interesting, is completely unsupported by fact. Indeed, according to the criteria which will be presented in the next section, tact analysis, apart from its ability to detect metaphor, is probably itself metaphorical. (I am indebted to H.D. Schlinger for this observation, personal communication, March, 1984.) Furthermore, it is unlikely that the examples about to be reviewed (even if they did qualify as tacts) were emitted as pure extensions. Many were almost certainly acquired and subsequently evoked as stabilized tacts. Hence, the proposed sorting by extension type will often be made with respect to the origin of a given usage, irrespective of whether the instance cited actually represents true extension.
These sources of hesitation notwithstanding, tact analysis does have an inherent logical appeal and plausibility, and that fact illustrates a paradox which will reappear throughout the balance of this paper: Occasionally a conceptual system thrives, despite serious flaws and inconsistencies, because it offers a genuine practical utility that may transcend its stated purpose. The analysis of tact extension will serve as a useful tool in this chapter; if it turns out not to be a true account of human language, my arguments will be neither enhanced nor diminished.

Having settled on a measuring instrument does not solve the problem of where to direct it. The phrase behavioral language covers a good deal of territory; any conclusions drawn about it must somehow be documented. A frequency count would be the simplest way to quantify the relative incidence of the various types of tact extension. But should the sample be taken from books, journals, lectures, or conversations? If imprecision and license are as widespread as I suggest, meaningful documentation of their occurrence would be unwieldy. (Where would one begin?) Instead, I will take the approach of illustrating with selected case examples from a variety of sources. Thus sensitized, I shall leave it to the reader, who, by listening to colleagues, attending conventions, and reading professional publications, may be personally compelled by the universality of these phenomena. Therefore, the goal here is to equip the behavior analyst with a method for screening the accuracy of pronouncements that seem to be about basic principles. Sustained immersion in the behavioral culture will do the rest.

To challenge the field's scientific foundation is a most disturbing form of criticism. However, as Sidman (1976) has pointed out, there may be no help for it: "If any science is to continue to produce new and useful knowledge, it must devote a significant effort to self criticism and questioning of its own assumptions, concepts,
and methods. Until recently, Behaviorists have done little of this..." (p. 279). I hope that the present effort will fit this spirit of constructive evaluation from within.

The Essential Features of Reinforcement

In order to detect (and then classify) an instance of tact extension, one must first know something of the environmental properties that were required by the verbal community when a given tact relation was originally taught. Sometimes these properties can only be guessed at (see Cornwell & Hobbs, 1984, for a retrospective application of Skinner's analysis of metaphor). In the case of familiar psychological concepts, however, it is sufficient to have standardized definitions with identifiable essential features. For example, knowing the definition of the term unconditioned reflex should provide the information necessary to judge utterances of that phrase as generic, metaphorical, or metonymical extensions. Such definitions are frequently produced by textbook writers who attempt to construct statements which summarize the current state of knowledge supplied by basic research.

Unfortunately, all defining features are often not explicitly stated in any given textbook definition. Even scientific writing takes a certain amount of detail for granted; so the reader must sometimes read between the lines. Consider the expression operant reinforcement and its common variants, such as contingency of reinforcement. These notions are nothing less than the conceptual engines of modern behavior analysis. Yet a relatively recent definition in a behaviorally oriented introductory text illustrates how difficult it is to completely eliminate ambiguity from the narrative format:

The principle of positive reinforcement has two parts: (1) if in a given situation somebody does something that is followed immediately by a certain consequence, then (2) that person is more likely to do the same thing again when he or she next encounters a similar situation. (Martin & Pear, 1983, p. 19)
Martin and Pear's treatment is more sophisticated than most; it makes clear that the single term *reinforcement* refers to both a *procedure* and a *process* (Catania, 1969). But there must be even more to the concept than that. According to another behavioral textbook (Catania, 1979), that something more is the addition of a qualifier to the two components already mentioned. This brings the list of defining features to three:

First, a response must have some consequence. Second, the response must increase in probability (i.e., the response must be more probable than when it does not have this consequence). Third, the increase in probability must occur *because* the response has this consequence, and not for some other reason. For example, if we knew only that a response had become more probable, it would not be appropriate to say that the response must have been reinforced; the response might have been elicited by a stimulus. It would not even be sufficient that the response was now producing some stimulus it had not been producing before. We would still have to know whether responding increased *because* the stimulus was its consequence. (Catania, 1979, p. 75)

Some may argue that this last refinement (the exclusionary clause) is so strongly implied in the first definition that it does not need to be spelled out. However, there is evidence, albeit indirect, that an incomplete definition of reinforcement can actually cause trouble. Several experiments have had their interpretations foiled by confounding the effects of reinforcement with other, antecedent processes (Higgins & Morris, 1985). (The commonest error is to mistake respondent elicitation for operant reinforcement). Although no one has empirically demonstrated that these failures were due to an inadequate conception of reinforcement, such proof may be unnecessary. As the following, hypothetical example shows, a compelling argument for precision can be made on purely logical grounds:

Suppose a child decides to experiment with a newborn sibling by pinching the infant whenever it cries. At first the infant might lie quietly, but if the older child is patient the infant will sooner or later cry. Then comes the pinch, but the pinch will be followed by more crying, and thus by more pinches, and thus by more crying, and so on. We assume that this procedure will inevitably attract the parents' attention and that they will intervene to rescue the infant. At that point the pinching may have consequences for the older child, but we must
leave the scene before the parents arrive to get back to the language of reinforcement.

In this example, a response had a consequence and responding increased. Two of our criteria for reinforcement were therefore satisfied. Yet we would be reluctant to call the pinches reinforcers or to say that the child had reinforced the infant's crying with pinches. Our reason is that we do not believe that the increase in crying came about because of the consequential relation between crying and pinches. We know enough about infants and crying and pinches to recognize that the pinches would have made the infant cry whether or not the pinches were produced by crying; the pinches by themselves would have made the infant cry even if they had been delivered without reference to crying. Our conclusion is that crying occurred because it is ordinarily elicited by pinches and not because it produced pinches. It was incidental that the first cry happened to lead to the first pinch; the infant cried when pinched whether or not it brought on the pinches itself. Whenever responses produce stimuli as consequences, the stimuli may have eliciting effects along with or instead of their effects as consequences of responding.

The vocabulary of reinforcement requires that the response have a consequence, that responding increase, and also that the increase occur because responding has its consequences and not for other reasons. (Catania, 1979, pp. 75-76)

Even if one opts for a cautious, exhaustive approach, the problem of defining reinforcement is further complicated by the fact that the phenomenon itself is not completely understood at the basic research level (e.g., Baum, 1973; Williams, 1983). It may, therefore, seem odd to judge deviations from a standard (which is just what I shall do in the balance of this chapter) when the experts themselves cannot seem to agree on a stable set of defining features. Nevertheless, this state of affairs has hardly deterred anyone's use of the term, reinforcement, whether in theory, research, or application. Thus, some form of consensus must be operative within the behavioral community, in spite of the fact that no single definition of this concept has yet captured the entire constellation of its essential features.

Can this working consensus be apprehended? Catania's (1979) three-part definition of reinforcement is probably the most comprehensive available, but it too requires supplementation. Perhaps it would be useful to make some of these implied supplements explicit. For instance, it is generally taken for granted that the phrase
contingency of reinforcement refers to a three-term relation, the constituents of which are considered to be discrete events (but see Baum, 1973). Furthermore, it is assumed that the central member of this relation, the response, is the product of just one organism, and not the combined activity of a group. Additionally, the response in question (actually the response class) can have its probability altered only by past reinforcements. Operant behavior cannot be affected by upcoming events, at least not via the standard mechanism of consequation alone. The future can sometimes be made effective by what Skinner has termed rule-governed behavior (Skinner, 1969), but this special case should not be confused with simpler processes.

These implicit restrictions on the basic definition of reinforcement are relatively straightforward, and, with the exception of the first, none are controversial. Less clear are some of the essential details of an individual conditioning episode. Of particular interest is the issue of whether temporal contiguity between a response and its consequence is a necessary feature of reinforcement. The notion that reinforcers must be delivered immediately after a response has a long history in the behavior analytic community. In certain applied settings it has been elevated to the status of a fundamental principle. But the situation in basic research is far from settled. Baum (1973), for example, has described a radically different conception of reinforcement, one in which overall rates of behavior depend on their correlation with various rates of reinforcer delivery. Inasmuch as this is a molar orientation, it denies the importance of particular response-consequence episodes; hence, the direct role of temporal contiguity is eliminated. In a similar vein, Williams (1983), in a recent review of the principle of reinforcement, has taken the position that contiguity is neither necessary nor sufficient for conditioning to occur. He cites laboratory evidence (his own and others') which suggests that contiguity, if not replaced altogether, should at least be supplemented by the concept of predictiveness. Some of
this evidence, the cognitive allusion notwithstanding, is potentially threatening. It must be said, however, that although Williams' challenges to contiguity are several and varied, he fails to completely dispense with it as an important experimental variable. Moreover, other researchers (e.g., Gleeson & Lattal, 1987; Sizemore & Lattal, 1977) continue to publish results which support contiguity's central role. At the present time, therefore, the notion remains durable; it simply will not go away.

A final elaboration of the familiar definition of reinforcement concerns the nature of consequation. Most of us speak of reinforcement and punishment in terms of the presentation or removal of stimuli. Michael (1975), however, has argued persuasively that these operations are more properly thought of as stimulus changes, rather than as the addition or subtraction of static conditions. (Temperature change can function as an effective consequence, but should it be viewed as the presentation of heat or as the removal of cold?) In its theoretical implications this is probably the least obvious addendum to the usual set of reinforcement's defining features. It is the sort of contribution whose effects become apparent only when the position is taken to a logical conclusion (see Michael, 1979, for an innovative application of this perspective to the resolution of an experimental paradox). For our purposes, however, the stimulus-change characterization of reinforcement is important primarily because it focuses attention on the moment of consequation. Along with the theme of temporal contiguity, Michael's formulation emphasizes the discrete, molecular aspects of reinforcement. We are thus encouraged to think of consequation as a situation transition, and of the entire response-consequence relation as occurring on the order of a second or seconds, not minutes, and certainly not hours or days.

Individually, these supplements to the standard definition of reinforcement are far from startling. Indeed most of them may seem trivial to the informed reader. But when taken together, and in conjunction with a rigorous textbook definition (such as
Catania, 1979), they paint a more restrictive definition of reinforcement than we might otherwise be inclined to accept. Of course, there are other, quite different interpretations of reinforcement that this list of essential features does not address. Baum's (1973) correlation-based approach has already been mentioned. Another example comes from Malott, Tillema, and Glenn (1978), who have proposed a novel, hybrid terminology. Their scheme retains a variant of the traditional meaning of reinforcement, but it also defines rewarding stimulus (the substitute term for positive reinforcer) as: "a stimulus we tend to maximize contact with" (p. 6)—not the usual fare for an introductory student of operant conditioning. Although provocative, such suggestions do not reflect the prevailing convention in the behavioral community. And, as we shall see, when speakers in that community deviate from established convention, it is not by the simple adoption of one of these alternatives.

**A Sampling of Instances**

It is time to consider some examples—to sample some actual instances of scientific tacts and, using the preceding list of essential features, to classify them by type of extension. The point of this exercise is to demonstrate that although much behavioral language is generically extended, many (possibly most) of our technical utterances are actually metaphors. Some even constitute metonymy. Of these three claims, the second is most surprising. It is surprising because metaphor should be a rarity in science. Indeed, conventional wisdom holds that scientific tact extensions are by nature generic. Recall Skinner's statement, quoted in an earlier section:

> The difference between the generic and the metaphorical tact is one of the great differences between science and literature. Scientific verbal behavior is set up and maintained because of certain practical consequences. Nothing beyond a generic extension will eventually serve...In literature there are no similar practical consequences and metaphorical extensions therefore prevail. (Skinner, 1957, p. 99)
This is not an isolated position. In a paper expressly concerned with the defining features of reinforcement, Schnaitter (1978) echoes Skinner's sentiment that behavioral tacts are tightly controlled:

An expression such as "reinforcer" is itself a tact, but one whose provenance is relative to a specialized verbal community, and where the conventional requirements of the controlling relation between events or properties of events and the response are maintained with great rigor. (Schnaitter, 1978, p. 3)

Even a criticism of sloppy operant terminology may contain a reference to the precise usage of reinforcement. Consider Sidman's (1979) comparative evaluation of the terms stimulus control and reinforcement:

Some of the more interesting gaps in our conception of stimulus control arise from our uncritical and imprecise application of the term to many different kinds of controlling relations. By way of contrast, we may cite the term, reinforcement, also a name for a controlling relation and also only inferrable. But the reinforcement relation is narrowly defined, sufficiently so that one need only infer whether or not it took place on any given occasion. Such specificity is not true of stimulus control. (Sidman, 1979, p. 126)

Assessments like these are not wholly unjustified. Generic extensions of basic terms are often the rule when nonverbal populations are involved. Imagine, for example, a typical behavior modification session with a very young hearing-impaired child. In such a case an appropriate target response might be eye contact. As the response already occurs from time to time, the procedure can be quite simple: Immediately after each eye contact the therapist inserts a small piece of candy directly into the child's mouth. By session's end the rate of response is considerably higher than it was during a preceding baseline session. Assume further that the rate drops back to near-baseline levels during a subsequent control condition, in which the same number of candy presentations are made, but on a response-independent basis. A behaviorally trained observer exposed to this sequence of events would likely emit tact responses such as reinforcer, reinforcement, and contingency, each controlled by different aspects of the situation. All forms, however, could properly be considered
examples of generic extension. Aside from meeting the minimal requirements of process and procedure, the case at hand also satisfies the implicit supplements just reviewed. The therapeutic intervention fits the framework of a standard three-term contingency (although the discriminative stimulus is unspecified). The response and consequence are discrete, uniform events. It is clear that the behavior of interest is that of a single organism. The control condition establishes that the candy achieves its rate-altering effect because of its consequential relation to eye contact. Also, inasmuch as the subject is probably nonverbal, there is little chance of contamination by rule-governed behavior. Finally, the stimulus-change character of candy delivery is apparent. And, conseuation is virtually contiguous with the response. That completes the list. The criteria for generic extension have been met.

Examples of the sort just described are easy to find in behavior analysis, especially in areas such as developmental disabilities. It follows then, that we would correctly expect a prevalence of generically extended tacts in large segments of the applied field. But that does not settle the issue of precise usage. How often have we heard a colleague refer, with some deliberation, to this or that event as a reinforcer, as in: "That was the most reinforcing committee meeting this semester."? Although it is certainly true that reinforcement produces a number of immediately discernable effects, these effects have nothing to do with the term's definition. (The crucial effect is in the future.) Indeed, when the vocabulary of reinforcement is used as a substitute for common expressions such as fun, likes, or feels good, the extension may be due to controlling properties that are completely irrelevant to the scientific community's standards. That sounds like metonymy. And, even if this type of language does not qualify as pure metonymical extension, it is obviously poles apart from the generic variety.
Between the extreme categories of generic and metonymical extension lies metaphor, the area of current interest. Because metaphorical extension is often so close to generic—the difference between all and some defining features may be as small as one—it is not as easy to spot as metonymy. As the most conspicuous form of extension, metonymy is probably also the least serious. In contrast, metaphor, by virtue of its unsuspected prevalence, directly challenges a widely held belief: that behavior analytic tact extension is exclusively generic. This in turn supports the field’s characterization as a genuine science. Thus, to the extent that metaphorical extension passes for generic, it is both more significant and less forgivable than metonymy, though neither form has a place in technical discourse.

Once one is sensitized to metaphor it is not difficult to recognize. The commonest instances of reinforcement metaphors involve the rather striking omission of one or more of its essential features. Perhaps the clearest case of this is the procedure known as contingency contracting (Homme, 1966). Put simply, a contract requires its client to earn something by doing something. When applied, for example, to the problem of weight control, a typical arrangement might be planned as follows: The subject enters the program by surrendering a large collection of valued personal possessions (or money) to the therapist. Next, short-term goals for gradual weight loss are set, and regular appointments for weight measurements are scheduled. At each goal date the subject either wins back or permanently loses ownership of a particular item (or sum of money). A variation of this format was, in fact, proven effective in an early study on weight reduction (Mann, 1972). In addition, contracts have since been successfully employed with a diversity of populations and behaviors, ranging from academic study (Bristol & Sloane, 1974) to physical exercise (Wysocki, Hall, Iwata, & Riordan, 1979). However, practical value is not at issue here. Aside
from the obvious utility of contracting as a procedure, our present concern is with how it has been portrayed in the research literature.

Almost all published reports of these programs are written in the language of responses, contingencies, reinforcers, and punishers. But notice how many of the defining features of reinforcement are violated by this usage in just the weight control study. Although we may grant that something like a response is followed by a consequence, and that the subject's weight does change in the appropriate direction, the requirements for generic extension do not end there. Virtually none of the supplementary features are in evidence. The dependent measure actually represents a complex of unspecified constituents which may vary over time, and can hardly be considered a straightforward response. (Furthermore, as I will argue later, a global appeal to the flexibility of the operant does not justify the universal application of what should be a well-defined concept.) Consequation in this example also presents a number of problems. The payoff is neither a discrete nor uniform event. Temporal contiguity has vanished: Enforcement occurs hours, if not days, after the relevant behavior. And, these interactions are not easily conceived of as momentary situation transitions.

As for confounding the effects of reinforcement with other processes, it is difficult to imagine a control procedure which could eliminate the influence of verbal behavior in a normal adult who is trying to lose weight. The usual (some would say obligatory) reversal design is no solution in this regard. Although it may demonstrate unequivocally that weight reduction is a function of the contract, it says nothing about whether reinforcement is the sole or even principal mechanism of action. The same is true of so-called component analyses. Systematic revocation and reinstatement is quite useful for isolating the necessary and sufficient elements of a package
intervention, but this tactic too offers no hint as to which fundamental behavioral processes are at work.

Add to all of this the astonishing fact that contracts often begin to affect behavior before the first consequence delivery, and the argument for metaphorical extension is complete. (Recall that reinforcement operates via past contacts.) The only conclusion derivable from these observations is that some of the defining features for terms such as response, reinforcer, and reinforcement are present in a contract whereas others are clearly absent. Yet, the procedure is almost universally rendered as a direct application of standard behavioral principles. Moreover, this is not merely an issue of theoretical interpretation. Contracts have produced a high yield in positive clinical outcome, and they are generally viewed as one of the field’s most significant and enduring contributions—all the more reason for maintaining a sharp distinction between generic and metaphorical extension when considering the terminology used to describe them.

Contingency contracting is a special case of what has long been known as contingency management (Homme & Tosti, 1965). This latter domain is so broad as to include much of what might be subsumed under the traditional heading of motivation. The contingency manager’s task, as popularly conceived, is to find manipulable reinforcers and make them contingent on whatever behavior is desired. Thus, if the problem consists of getting welfare recipients to increase their attendance at self-help meetings, a likely strategy would be to reward attendance with some form of tangible payoff. When such a technique was actually used in an applied study (Miller & Miller, 1970), its published description made the characteristic appeal to basic behavioral principles: Attendance was treated as a response, the payoffs as reinforcers, and the relation between them was termed a contingency. The overall portrayal seems to be about an instance of reinforcement, but is it accurate?
Perhaps the most suspicious aspect of this particular study is the fact that its intervention phase began with a written notice to potential attendees which informed them that free Christmas toys would be available if they came to the next meeting—as might be expected, attendance at that meeting rose sharply. Inasmuch as the subjects' behavior changed before ever making contact with the putative contingency, this initial effect must have been due to some process other than simple reinforcement. Even behavior following the first payoff is difficult to interpret. Consider the activities that make up a single occurrence of attendance: transportation, babysitting arrangements, telephone calls. Many of these bear only the remotest resemblance to a response themselves, so it is indefensible to treat the entire conglomerate as a unit. Moreover, the most relevant constituents of this group (e.g., leaving one's home) are the ones furthest removed from the consequence, thereby violating the requirement for temporal contiguity. And it will not help to invoke the unlikely possibility of an invariant behavioral chain to fill in the gaps. That overworked expedient would demand so many of its own assumptions as to be untenable. For these reasons, the extension of operant terminology to the above situation must be judged as metaphorical.

In the language of contingency management, violations of essential features may vary in both their severity and ease of detection. Sometimes, metaphorical usage can almost be inferred from the title of a journal article, as with "The Reduction of Stealing in Second Graders Using a Group Contingency" (Switzer, Deal, & Bailey, 1977). Based on the nature of the behavior and the telltale group contingency, one might guess that it was probably not possible in this setting to provide an immediate consequence for stealing. In fact, the guess would be correct. For experimental purposes, stealing (albeit a potentially discrete response) was operationally defined as the disappearance of planted items from prearranged locations. The consequence for
an infraction was the group's loss of free time, which was normally scheduled at the end of the class period. Substantial response-consequence delays for the actual culprit were therefore inevitable. Hence, this was not technically a contingency of punishment.

Other metaphors appear at first to be cases of generic extension. When one reads, for example, that childrens' standardized test scores have been raised by the application of token reinforcements for correct answers (Ayllon & Kelly, 1972), it is tempting to presume the full complement of defining features. A closer look at the procedure, however, reveals that single tokens were not delivered after individual responses. Instead, a tally was taken at the end of each subtest, and the child was then given a quantity of tokens, one for each question that had been answered correctly. The strategy has merit and it may certainly be called motivational, but the label reinforcement should not be applied to this situation, except as an acknowledged metaphor.

Perhaps the most difficult samples to judge are those in which generic and metaphorical instances occur within the same general context. Illustrations are readily found in experiments which use multiple procedures. One such study employed the addition or subtraction of access time at two urban recreation centers as a way of managing their respective problems (Pierce & Risley, 1974). The goal at the first center was recruitment. So, each time a current member brought in a new member both were allowed, on a subsequent day, to come in an hour before the usual opening time. The problem at the other site was disruptive behavior, which consisted of things like littering and misplacing equipment. Here the solution was to post rules and close the center one minute (or more) early for each rule that was discovered (on periodic checks) to have been broken. (Under these circumstances it was occasionally possible to catch an offender in the act, but that was not a necessary part
of the program.) In the first procedure, described by the authors as **positive reinforcement**, use of the standard behavioral vocabulary is undoubtedly metaphorical: Recruitment, quite apart from its temporal distance to the consequence, is no more a response than was attendance in the welfare experiment reviewed earlier. The second case is more complex. That center's method of enforcement permitted at least some offenses to be immediately followed by public notification of the time that had been lost. This could have been a genuine example of punishment, although it was not cited as such.

The point is that certain aspects of a broad intervention may completely fulfill the requirements for generic extension of a basic term. However, those aspects may not be the ones tacked by the researchers as the principal contingency of reinforcement or punishment. The study in question identified (metaphorically) recreation time as a **reinforcer** but did not go on to offer a more molecular analysis of the actual critical events. Moreover, the fact that a few generic tacts might be scattered among the metaphors—**possible** generic tacts, because additional controls would still be necessary to deal with the problem of verbal contamination—makes the entire written presentation scientifically unreliable.

In the preceding survey, I have provided only the briefest treatment of how each instance of extension deviated from the behavioral community's standard. Nevertheless, it must be remembered that even a single violation is sufficient to disqualify an apparent case of generic extension. As it happens, there is such an ample supply of clear-cut metaphor that close judgements are seldom necessary. In addition, once one has been exposed to a variety of metaphorical examples (as the reader by now has), the process of detection becomes easier. Consider the host of objections that immediately come to mind upon hearing that a methadone maintenance clinic has successfully employed **contingent reinforcement** to reduce supplemental...
drug use among its patients (Stitzer, Bigelow, Liebson, & Hawthorne, 1982). The reinforcement procedure, it turns out, consisted of providing either cash payments or clinic privileges to those participants whose urine samples tested free of benzodiazepine tranquilizers (a commonly abused supplemental drug in this population). To begin the analysis one need only recall the list of violations that plagued the contracting study on weight reduction. (Because many forms of contingency management are virtually tantamount to contracts, except that they are usually imposed rather than negotiated, there is a strong tendency to describe both techniques with the same types of metaphors.) Amorphous events are characterized as responses and consequences. Liberal references are made to various contingencies of reinforcement in spite of the fact that few of the corresponding procedures incorporate anything resembling temporal contiguity. When verbal repertoires are present their influence is seldom acknowledged, let alone controlled for. And finally, target behaviors often change in perfect synchrony with the beginning and end of an intervention. All of these objections, in one way or another, apply to the aforementioned drug study. Thus, with the aid of a few simple tests, one may quickly form an educated guess about the presence or absence of metaphor in a given linguistic sample. Of course, this initial screening must then be followed by a more cautious and detailed examination of the episode in all its particulars.

The instances just reviewed should not be thought of as unusual. Contracts and contingency management programs account for a sizable number of the articles published in the field's principal organ of applied research, the Journal of Applied Behavior Analysis (JABA). It would be a mistake to write off the metaphorical language in these reports, which are, not incidentally, subject to a thorough system of peer review, on the grounds that their authors recognize the liberties they have taken. If that were true, it would be unlikely for them to shift back and forth, without
qualification, between generic and metaphorical extension. *JABA* is a respected scientific journal. Even so, its readers are frequently exposed to both types of tacts presented side by side in issue after issue. Sometimes the mixture occurs within a single article, sometimes even within the same paragraph. There is little evidence, therefore, that anyone is aware of the serious lapses that are taking place.

The intermingling of appropriate technical terminology with that which is wholly gratuitous is by no means unique to the applied literature. Not surprisingly, similar verbal practices may also be found in those writings called interpretive or theoretical. In the following excerpt, notice how a series of clearly metaphorical examples are blended together with one or two that at least might qualify as generic cases:

As things are, for instance, the actions which we call "going on strike" are frequently followed by an increase in pay; and since such an increase is likely to have a reinforcing effect, the operant approach forces one to ask whether more could be done to reward those who do not resort to such coercion....and one wonders if more steps could be taken to ensure that there was suitable reinforcement for appropriate behaviour on the roads. Insurance companies do, of course, give a financial bonus to those who make "no claims", and this may well have a reinforcing effect on careful driving; and on operant principles it would be appropriate to introduce a system of taxation by which those with an accident-free record paid less. Similarly, when decisions are involved about handling of children, parents and teachers may well find it helpful to ask themselves whether a particular action on their own part is likely to have a reinforcing effect or the opposite. In some cases, as many of them well know, a child who displays unwanted behaviour — a temper tantrum for instance— is best left alone, since (in operant terminology) the stimuli which occur when someone pays attention to such behaviour may well have a reinforcing effect. Again if one wishes pupils in a class to contribute to discussion it is clearly wise policy to praise their efforts where possible rather than offer carping criticisms. It is also worth remembering that publicity— including unfavourable publicity— can sometimes have a reinforcing effect. Not long ago there was a controversy in the press as to whether someone who planned to make a film on the sex-life of Jesus Christ should be allowed into the country; those who wished to discourage such film making would surely have done better to ignore the proposed film rather than denounce it. *(Harzem & Miles, 1978, pp. 51-52)*

There is an undeniable commonality to the phenomena cited in this list. However, the authors appear to have been so struck with the similarity of their choices that they have overlooked some important differences among them: "These
examples show how any social situation can be viewed as a case of behaviour-being-reinforced” (p. 52). But it should be obvious that the examples in question show nothing of the sort. Instead, what they do show, given the present analysis, is something more in the way of a warning: that the price of sweeping relevance is an overly generous interpretation of basic concepts. That price, it seems, has been paid on far too many occasions. We have already witnessed metaphor being offered as the standard fare in published reports of empirical research, an arena which should reflect the community's most rigorous standards. It is reasonable, then, to suppose that the same type of slippage would exist and even increase in its prevalence as one moved from journal article to abstract treatment to casual discourse.

Is it possible that serious linguistic deviation, generally assumed to be an exceptional occurrence, is actually the rule in the applied and theoretical realms of behavior analysis? Inasmuch as a reference to reinforcement usually implies the familiar three-term contingency, there is ample potential for error: Each of the three terms, as well as the relation between them, is a candidate for extension. Furthermore, the dual dimensions of process and procedure must both be tested for accuracy when certain principles such as reinforcement, punishment, and extinction are invoked. In short, every appearance of an ostensibly generic tact is also an opportunity for metaphor or metonymy. Therefore, unlike the isolated mistakes covered in the previous chapter (on rumblings of verbal abuse), the argument here is for the ubiquity of metaphor when rigid standards, those appropriate to a scientific vocabulary, are applied with consistency. Indeed, by the criteria that I have suggested, entire subspecialties within the behavior-analytic enterprise are rife with violations of what should be conventional usage.

Just as generic extension tends to characterize most basic research (as well as a few areas of application such as developmental disabilities), it is also the case that
large segments of the applied field are prone to an extremely high incidence of metaphor. In particular, the activities known as organizational behavior management and educational technology typify those aspects of applied behavior analysis which are rendered in almost completely metaphorical language. Moreover, the theoretical literature, on which we have barely touched, is by its very nature given to an even greater degree of license in this regard. For example, The Behavior Analyst, an important outlet for conceptual articles, position papers, and the like, is full of references to political contingencies, administrative contingencies, and economic contingencies. These explicit appeals to the operant paradigm hardly begin to satisfy the requirements for generic extension. Yet, no one seems to have been deterred by that fact. (And Skinner's extensions are still to come.)

Loosely bound concepts are a mixed blessing. It is no accident that some of the behavioral movement's most metaphorical products are also some of its most creative. But this form of creativity, apart from the obvious benefits of innovation, is actually a telling weakness. It is quite unlike the straightforward extrapolation from the laboratory so familiar in the established natural sciences. The idea of a contract might well have been suggested by working with rats pressing levers. However, the respective processes involved should not automatically be blended together. Thus, whereas we may applaud the resourcefulness of the practitioner, applied researcher, or theoretician, it must be remembered that each act of imprecise usage diminishes the fundamental health and integrity of the discipline as a whole.

It is important to recognize that metaphor in behavior analysis is not only widespread; it is also not new. If one includes among the essential features of reinforcement the requirement that behavior changes must be due to the direct effects of the response-consequence contingency, and nothing else, then even most contiguous cases can be metaphorical if a verbal repertoire is present. This would
mean that metaphor could be found in the first issue of the first volume of the *Journal of Applied Behavior Analysis.* (It can.) Of course, it is true that the well established trend toward cure-directed package interventions has increased the prevalence of metaphorical extension; but seeds of this verbal practice have been in place since the field's early days. Furthermore, although the cure orientation has attracted a good deal of criticism, its actual deficiencies have thus far been misread (Minervini, 1986). Pierce and Epling (1980) give perhaps the strongest statement of what I now see as a thoroughly erroneous but widely held belief. Their complaint with the research published in *JABA* is that it relies almost exclusively upon the principle of reinforcement:

> In our review of *JABA,* we were struck by the constant repetition of studies that call on only the most elementary principle of behavior, the law of effect. This principle is demonstrated on numerous and diverse behaviors, in different settings and with different subject populations. These articles, taken together, seem to state "the law of effect works." One can imagine an equivalent development in the science of physics. If Galileo had started an applied journal there may have been numerous articles that demonstrated the law of gravity held for a) various angles of inclined planes, b) inclined planes composed of different substances, and c) diverse balls varying the size and mass. The journal could have been called the Journal of Applied Gravities as the current *JABA* could be redesignated the Journal of Applied Law of Effects. (p. 6)

Would that it were so! The problem is not that behavior analysts have generated a mass of simpleminded applications of just a few basic principles. The problem is that things which are called principles are not principles at all. Unfortunately, that revision yields a very different impression of what is truly wrong with applied behavior analysis: It is one thing to have an immature technology; it is quite another matter to have a set of procedures which only appear to be grounded in science. Even a rudimentary but genuine technology would be vastly superior to one that is merely alleged.

In retrospect, it was probably inevitable that novel developments such as contracting would someday be proposed. However, it was not inevitable that our
technical language would slip to the point of being gratuitous. One reason for the
deterioration is that many highly creative proposals worked well despite their
metaphorical nature. That opened the floodgates of unconstrained innovation. And,
so long as useful results were forthcoming, everyday technical discourse drifted,
further and further from its originally intended standard. The applied field had
become a victim of its own successes.

I began this chapter with the distinction between interpretation and application,
two ways to test a scientific principle's relevance and validity. By now it should be
clear that there is no shortage of metaphor in either domain. We tend to excuse
interpretation because it is an acknowledged break with the restrictions of the
laboratory. But this reaction may be too forgiving. It ignores the fact that
interpretations are not all of a piece. Some are more justified than others. Even when
a clinical outcome is not at stake, generic extension is still the only legitimate way to
extend the range of a basic concept. And when the shift to metaphor cannot be
avoided, it should at least be signalled. As things stand, however, the behavioral
community has grown accustomed to both modes of extension, presented together
and with equal conviction. It is not unusual, for example, to see or hear the term
reinforcement applied to hypothetical situations which could properly be described as
little more than molar, chronic betterment: improving one's condition professionally
or socially; working for tenure because it is a powerful reinforcer; being controlled by
the irresistible reinforcement of a lover—doing all the things necessary to ensure that
the relationship continues; making one's work or living environment more
reinforcing, say with the addition of a nice desk or comfortable chair. Almost any
case of self-interest or attainment of pleasure is thereby glibly explained in the
parlance of the familiar three-term contingency, frequently without so much as even a
metaphorical reference to a response. Sometimes there is at least a concrete
consequence, but just as often the putative reinforcer or punisher is an abstraction rather than a physical energy change.

Of course, one could attempt to salvage a plausible account of these behaviors by resorting to concepts such as verbal supplementation, feedback functions, or the correlation-based law of effect. However, the point is that none of those alternatives justify using the standard vocabulary of reinforcement where it does not belong. Another possible defense is the argument that most errors in theoretical extrapolation are propagated by novices. In a subsequent section, therefore, we shall consider some of Skinner's interpretations with the aim of determining whether they fare any better than what has been offered by the mainstream.

The quality of our interpretive accounts is not a matter to be taken lightly. Nevertheless, a certain amount of latitude in this area is tolerable; on occasion, perhaps, desirable. By contrast, terminological accuracy in the context of application should be a given. As we have seen, that is clearly not the case. To take a conspicuous example, recall that most contingency management programs have little to do with the concept of a contingency, at least as that notion is understood in the basic research laboratory. In fact, the actual controlling feature for the phrase contingency management seems to be the existence of an effective quid pro quo at the center of what may well be a very complex behavioral intervention. Thus, a more appropriate (although decidedly nonscientific) term for this technique would be exigency management. And as for the broader label, contingency of reinforcement, why not substitute the less impressive but more precise consistent enforcement when simple consistency—not a genuine operant principle—is all that can reasonably be claimed?

Even the commonest expressions of the practitioner ought to be reexamined from a stricter perspective. When one uses contingencies in a clinical setting, the
programmatic implications may not be very different from what takes place in an animal laboratory: In the eye contact example mentioned earlier, an actual repertoire change was effected by repeated contacts with a well-defined, immediate consequence. On the other hand, the same phrase might be invoked by a college professor who makes a practice of basing a few examination questions on material from a "recommended" textbook because he has discovered that those readings are otherwise likely to be ignored. In the latter case any reference to a contingency would indicate little more than the fact that humans must often be forced to comply with a given request. On the occasions where that force comes in the form of a systematic measure, it certainly appears as though a technical principle is somehow relevant. But standing linguistic convention cannot be changed at will. We must conclude that the concept of a reinforcement contingency is here grossly extended.

When behavior analysts make suggestions like the one just described, it seems that they have learned an important rule of professional conduct: "No contingencies, no behavior." However, the utility of that truism can lend no support to the general behavioral framework so long as the key term, contingency, is being emitted as a metaphor.

Another popular aphorism in the applied community is that we are completely controlled by short-term consequences and hardly at all by their long-term counterparts. This too is usually a metaphor. What is often meant is that short-term goals generally work better than those which are in the distant future. That may be an accurate depiction of human nature, but it is not derivable from the delay-of-reinforcement gradient. Unlike consequences, all goals are in the future.

The problem is most obvious in cases where the purported consequence, no matter how far removed from behavior, has never occurred as a consequence. Many corporations (without the aid of a behavioral consultant) have long used incentive
programs, which are somewhat similar to contracts, as an effective motivational tool. Of necessity, these must begin to affect behavior well before contact with the consequence has been made. And they must produce results with new employees as well as with those who have experienced the outcomes of previous promotions. When such campaigns are successful, the typical behavioral explanation would probably rely on an appeal to a generalized history of similar responses and similar consequences, as if a global response class had been created. Unfortunately, this argument merely answers one metaphor with another. The fact remains that the control exerted by the immediacy of an impending event is not equivalent to the control that results from a history of immediate consequation.

The substance of all this is not that the applied world has not profited from the use of metaphorical contingencies. It clearly has. Rather, the point is that even the most useful of these procedures is still defective in the sense that its linkage to basic research is, at best, indirect. If something like reinforcement is at work in situations which bear that label, it will never be understood because we will not know where to draw the line between where reinforcement stops and that something else begins. I have encountered no better illustration of this trap than the following passage from Isaac Asimov, cited by Robert D. Nye (1979) as an "outstanding" example of reinforcement:

Frankly, I tend to accept behaviorist notions. I find the behavior of people quite predictable; and the better I know them, the more predictable I find their behavior. People sometimes surprise me, I admit, but I have the feeling that this is because I don't know enough about them and not because they are capable of free will.

And, of course, my own behavior is most predictable of all; at least, to me. For instance, I respond favorably to praise. It has an extraordinary reinforcing effect on me. All my publishers and editors find this out at once. It is their profession, of course, to study the weaknesses of writers and use those weaknesses to manipulate those writers. So they carefully begin to praise the quickness with which I complete my work; the speed with which I
read galleys and prepare indexes; the cooperativeness with which I make (reasonable) revisions, and so on and so on.

All this I lap up with avidity; no one has ever handed out praise in larger servings than I can swallow. What's more, in order to get still more of it, I complete my work more quickly than ever, read galleys and do indexes with still greater speed, make reasonable revisions with at least a trace of a smile, and the result is that over the last six years I have averaged nine books a year.

Came the time when I was supposed to work on the introductions to these chapters. I was in New York; the editorial staff was in California. I sent off some introductions and heard nothing. Whereupon I grew sad and sulky and found that I didn't feel like doing any more. When I was nudged very politely by the editors, I wrote a long letter, saying that unless I knew that they liked those I had already written, I could do no further work. Whereupon the staff, suddenly enlightened, promptly sent me a letter telling me that my introductions were great and that everybody loved them. At once I sat down to write more. Periodically, they sent me kind words and I sent more introductions. It was very neat and efficient. (pp. 31-32)

Neither Asimov nor Nye happen to be behavioral psychologists. The example, however, is exactly the sort that would pass unnoticed in a college classroom, at a professional meeting, or even in a peer-reviewed journal article. It does, at first blush, seem to capture the essence of what reinforcement is all about. Nevertheless, the defining features of that familiar phenomenon are obviously not satisfied in full. Therefore, the reference does not make contact with the basic science literature. (Not even partial contact; partial matching of essential features is usually sufficient to ensure complete scientific irrelevance.)

Both of the above authors have been unduly influenced by only the most conspicuous elements of what is actually a complex process and procedure—and so has most of the behavioral community. The cumulative effect of this error has led to an enormous blurring: We have failed to distinguish the specific process of reinforcement from the broader notion that behavior is affected by its consequences. That may not have an immediate impact upon the routine work of the practitioner. But long-term theoretical implications for the discipline are more serious. Genuine
scientific principles demand more than just a close resemblance between technical terms and the conditions which they are said to describe.

Other Metaphors

Not all metaphors are about reinforcement contingencies, per se. However, as with the collection just reviewed, the most dangerous types of metaphors are those which are the most plausible. Perhaps no one is misled by application of the term autoshaping to a program which uses floating targets to control the direction of male urination (Siegel, 1977), this despite several direct citations of basic experimental studies. Similarly, when one reads an article entitled, "Fixed-Interval Work Habits of Congress" (Weisberg & Waldrop, 1972), it should be apparent that a number of conceptual dimensions have been stretched. On the other hand, we might be inclined to take seriously a proposition that, within the field of social work, there may be a lean reinforcement schedule for "adherence to a rigorous behavior analysis base" (Polster & Dangel, 1981, pp. 165-166). And a reference to a book as a discriminative stimulus (Moore, 1980) is likely to arouse no suspicion whatsoever. (Books certainly can function as discriminative stimuli—for responses like grasping, balancing, or throwing—but an explanation of the effects achieved by their informational content cannot be tossed off so easily.)

Also virtually unquestioned is the longstanding axiom that tokens and money are conditioned reinforcers. We must resist the temptation to support this convenient bit of extrapolation with the fact that these so-called conditioned reinforcers do indeed work as expected. At this point it should be clear that the defining features of reinforcement go well beyond that. The examples of tokens and money not only raise most of the problems encountered in the preceding section; they are burdened with the additional puzzle of how such stimuli come to acquire their reinforcing properties.
Does the standard pairing analysis really provide a tenable account after all these years of implicit acceptance? Has it ever been borne out empirically? It is odd that conditioned reinforcement, a phenomenon so full of mystery at the basic level, seems to be so well understood in the weakly controlled context of application.

All of these metaphors are interesting in their own right. By now, however, the reader has been sufficiently equipped to screen such instances and sort them according to whether they are generically or metaphorically extended. I will, therefore, address only two particularly troublesome cases before moving on to an examination of Skinner's extensions.

**Shaping** by the method of successive approximations, although not a principle of behavior, is one of the most powerful procedures in the behavior analyst's arsenal. As originally defined in both laboratory and clinical settings (see e.g., Michael & Meyerson, 1966), it referred to a technique whereby one initially reinforces whatever currently available topography is closest to a desired target response. This differential reinforcement then produces a new distribution of response forms from which an even closer approximation can be selected. By gradually changing the criterion for reinforcement in this manner, a "novel" response can eventually be generated.

Over the course of the last two decades, this once unambiguous concept has succumbed to a variety of metaphorical interpretations. The term shaping is now used to describe almost any sort of intervention which achieves its end in a gradual fashion, usually by breaking some complex behavior into small units. The shift has not gone unnoticed. In a popular behavioral textbook, Martin and Pear (1983), after restating the familiar conventional wisdom regarding the integrity of behavioral language, lament the fact that shaping has taken on multiple meanings:

As we pointed out with regard to the term positive reinforcement..., behavior modification could not be nearly the effective technology it is if its terms were not defined so precisely. Unfortunately, the technical term "shaping" is not
always used as precisely as it should be...increasing applications of the procedure have been accompanied by increasing instances of vagueness in the use of the term. It is not uncommon to hear students of behavioral psychology, when talking about behavioral programs, say, for example, "That teacher really shapes good reading skills in his students." Even some recognized authorities have written statements such as "Shaping is used to develop new behaviors that have never been performed by the individual, such as reading, feeding, and dressing." It should be clear, however, that one cannot develop these behavior simply by waiting for and then reinforcing closer and closer approximations to them; they are just too complex for that. (pp. 65-66)

Given the evidence presented in this paper, the authors' confidence in the field's overall linguistic health can be viewed only as an act of faith. However, their critique of shaping is a step in the right direction. The vagueness they speak of has become pervasive. My own shaping anecdote comes from a remark made by a grading assistant. We were involved in a group grading session and came across an answer that, except for its syntax, was clearly incorrect. The assistant suggested that inasmuch as it was early in the semester, we should give credit for the answer and increase the stringency of our requirements on subsequent examinations--a proposal intended to shape more accurate test answers.

The extension was not surprising, considering that it came from a student. Even so, it is important to recognize that slippage in this area is not confined to the occasional offhanded comment by an undergraduate. Nor (as is true for metaphor in general) should the current state of affairs be thought of as without historical precedent. JABA's premier issue includes an article which equates shaping with the following program: A noncooperative child "...was initially reinforced for all responsive verbalizations in proximity to children, subsequently only for such verbalization in potentially cooperative situations, and finally...only for full-blown cooperative play" (Hart, Reynolds, Baer, Brawley, & Harris, 1968, p. 74). Shaping may have had a role in this study. In spite of that, it is the wrong label for the entire progression just described.
The common element in most shaping metaphors is, for want of a better term, **incrementality**. (The phrase *incrementality mentality* has a rhythmic appeal which I cannot resist.) Incrementality probably constitutes excellent advice in many applied settings, but it derives no direct support from the experimental laboratory. It would be best to subsume **shaping by the method of successive approximations** under this larger umbrella (of generalized incrementalism), and retain its original restrictive meaning for technical purposes. Although *procedural* metaphors are not as serious as metaphors of basic principles, they can still cause needless confusion and should be avoided. Shaping would be an ideal place to begin reversing the marked deterioration of this important and practical aspect of behavioral terminology.

Our second focus in this section is on one of the most fundamental principles in modern behavior analysis: the *operant*. Perhaps the most significant property of the operant is that it is not a formal unit of analysis. Skinner made this clear when he tied the concept's definition to an empirical base. He argued that the specification of a response class "...is successful if the entity which it describes gives smooth curves for the dynamic laws" (cited in Schick, 1971, p. 414). So concerned are some behaviorists with the antiformal characterization of the operant that it has become a recurrent theme in the literature. In a provocative defense of the behavioral orientation, Branch and Malagodi (1980) remind us that operants are not confined to key pecks and lever presses:

> The concept of the operant is certainly not so restricted as to apply only to easily repeatable, laboratory free operants. Even in the research literature, operants have extended from twitches of individual muscle fibers to generalized, abstract classes such as imitation. Operants are as operants do; that is, they are functionally defined units of analysis. (p. 35)

The breadth gained by this approach is obvious. But an extremely flexible treatment of the operant may also be its undoing. By defining the unit as whatever enters into an orderly functional relation with a consequence, we are bound to blend
fundamentally different processes. For example, writing and sharpening the pencil
with which you write both become operants under this formulation, provided we can
demonstrate that each is predictably sensitive to some form of consequent stimulation.
To a certain extent this is desirable. At some point, however, we ought to insist that
there be limits. (This insistence does not necessarily raise the issue of whether those
limits would be natural or imposed.) I am not advocating that operants be relegated to
simple muscle twitches. On the other hand, it does not seem reasonable to assume
that they should encompass an activity as large as the production of a novel.
Skinner's original intention was to aim for something like induction in the
determination of usable response classes. There is little evidence of that tactic (if it
ever existed) in contemporary behavior analysis—now the problem of class is usually
solved on an a priori basis. Thus, few of today's "operants" were truly discovered in
any scientific sense.

It is time for a reassessment of the operant. As an initial corrective, I suggest that
the operant remain flexible, but only within the context of an inflexible,
nonmetaphorical contingency. Skinner's program of investigating emergent units
was never actually carried out. Closer verbal adherence to the defining features of
terms such as reinforcement and contingency might well be conducive to the
empirically driven elucidation of valid operant classes. Beyond that, I am uncertain as
to where or even how to set the outer limit for this difficult concept. I am willing to
guess, however, that a line should be drawn somewhere between eye contact and the
so-called generalized response classes. It should be acknowledged that virtually
nothing is known about the details of what happens when conseuation affects such
broad categories of behavior as imitation (e.g., Baer, Peterson, & Sherman, 1967) or
compliance (e.g., Goetz, Holmberg, & LeBlanc, 1975). The first might be partially
salvaged by a more complex analysis, but both stretch the concept of the operant well

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past its traditional borders. This is not to deny the research evidence that these units can, in fact, be modified. Rather, the question is whether they are part of the operant domain.

In considering the answer to that question, we should not be misled by the apparent sufficiency of obtained correlations between operational definitions and the "smooth curves" which they yield. Operationalism is no help in and of itself because of its ability to confer respectability upon even the most undeserving of psychological constructs. As for the smooth curves, they can hardly inspire one's confidence when the dynamic law at issue (reinforcement) is itself a metaphor. In sum, functional relations, no matter how impressive, are seldom better than their constituents.

A second adverse effect of the infinitely expandable operant is that it encourages a tendency to label classes of behavior with terms borrowed from the lay vocabulary. Skinner warned against this practice in spite of his belief that stimuli and responses could be apprehended only as generic quantities:

> The generic nature of stimuli and responses is in no sense a justification for the broader terms of the popular vocabulary. No property is a valid defining property of a class until its experimental reality has been demonstrated, and this rule excludes a great many terms commonly brought into the description of behavior. (Skinner, 1938, p. 41)

The popular vocabulary is attractive as well as deceptive because it gives the appearance that we are prepared to handle the entire range of human action--eating behavior, writing behavior, even scientific behavior. However, although we may discover an interesting and orderly relation between the rate of one's writing projects and the speed of their publication, that is no reason to call the first entity a response or the second a stimulus. Skinner's cautionary advice (which he himself seems to have ignored in later works) is still worth taking today: All that is behavior does not automatically belong to an experimental analysis of behavior; it must be earned piecemeal.
The above arguments should not be construed as an endorsement of the topographical error. I am not recommending that the operant be defined formally. Yet, having said that, it is also time to acknowledge that a functional approach must be bounded by something more than just an orderly relation. If it is not, operants will appear to be everywhere, frequently without warrant. Consider the amount of territory that the unit is expected to cover in the following example. A recent commentary on the subject of verbal-nonverbal (say-do) correspondence states that the problem need not be overcomplicated:

The analysis of verbal regulation in correspondence training has the theoretical complexity and sufficiency of the operant analysis of antecedents, behaviors, and consequences. The applied and theoretical stakes may seem higher when we are dealing with verbalizations and their potential influence over behavior, but it is (thankfully) only the operant again, in its undisguised beauty. (Stokes, Osnes, & Guevremont, 1987, p. 164)

Part of this beauty is undoubtedly related to the broad relevance that is the logical outcome of a functional definition. But beauty can be subversive. We should not be lulled by it into an uncritical acceptance of the operant as a concept without limits, even if those limits are not presently known.

**Skinner's Extensions**

As Skinner is chief progenitor of the current behavior analytic movement, his words deserve special attention. This is especially so because he has advanced the case (which I have quoted twice thus far) that in scientific verbal behavior only generic extension will do. How does his own language fare in this regard?

Skinner is not primarily known for his personal applications of basic principles. Instead, his contributions are mostly interpretive. Nevertheless, these theoretical accounts have always relied upon concepts derived from an experimental analysis:

It is often pointed out that I have specialized in the behavior of rats and pigeons, and it is usually implied that as a result my judgment about people has
been warped, but at least sixty percent of what I have published has been about human behavior. I have discussed government, religion, psychotherapy, education, language, incentive systems, art, literature, and many other human things. And so, of course, have thousands of other people, but I do not believe I have offered my readers just more of the same; for that is where the other 40 percent comes in. In writing about human affairs I have always stressed the implications of an experimental analysis of behavior, an analysis which was, indeed, first carried out on lower species, but which was eventually extended to human subjects with comparable results. (Skinner, 1978a, p. 16)

But the reality of that supposed transition is precisely the question at hand. And it goes to the very nature of the extension to which Skinner refers. If the crucial realms of interpretation and application are cluttered with metaphor, the transition from laboratory to clinic may be an illusion.

The collection of Skinner's interpretive works is not without its critics. Two forms of criticism with special relevance for this paper have been nicely summarized by Baron and Perone (1982). They report that Skinner has been charged with a tendency, referred to by Thouless (1963) as "anecdotalism," to portray casual observations as facts. The offenses that Thouless cites occur in a programmed text by Holland and Skinner (1961). For example, in one series of frames, the authors of this program state that a man who brings candy to his wife to end an argument may actually be reinforcing her arguing. Thouless points out that no research has shown that wives "will react as simply and predictably" as pigeons in an experimental chamber. In his reply, Skinner (1963) admitted to the use of anecdotes, but said that his intention was merely to illustrate and that the examples in question were not presented as actual hard data.

A second accusation, closer in spirit to the present argument, is that Skinner's analyses of social behavior and cultural design are given to "egregious" extrapolation (Watts, 1975b). Watts offers a thorough, scholarly, and evenhanded treatment of the relationship between Skinnerian behaviorism and political science. As such, it is too broad to review here. However, some of the specific complaints include Skinner's
loose application of technical terms and the inability of his organizing principles to deal with the ambitious range of topics which he attempts. Skinner's answer was based on what might be called the astronomy defense. He argues that although astronomers are interested in "...events occurring in outer space, under conditions beyond any hope of experimental control, and mostly unreproducible on the earth," they are able to handle their subject matter with "...facts about gravity, radiation, pressure, temperature, and so on, obtained under the controlled conditions of the laboratory" (Skinner, 1975, p. 228).

The astronomy defense, if it applied, would be flawless. Unfortunately, behavior analysis has nothing remotely like the basic science (physics) that supports astronomy (Watts, 1975a). Moreover, as Baron and Perone (1982) point out, it currently does not even have a parallel to the observed phenomena of astronomy. Psychologists have never put together an empirical base of systematically obtained field observations. Hence, they lack both the tools of explanation as well as a clear picture of exactly what is to be explained.

No critic should object to the process of interpretation, per se. What is objectionable about some of Skinner's interpretations is not that they are analogical, or speculative, or even anecdotal; these can all be appropriate modes of extension. Rather, it is the quality and precision of his analogies that are often questionable. For despite his urgings to the contrary, Skinner's extrapolations are shot through with metaphor, and unacknowledged metaphor at that. This is what makes them truly egregious.

On occasion, Skinner seems to be of two minds regarding how much latitude is acceptable for a given basic concept. For example, in a section entitled, "The Range Of Reflex Action," he seems to be saying that reflexes play only a very limited role in what is generally considered as significant human behavior:
We now see that the principle of the reflex was overworked. The exhilarating discovery of the stimulus led to exaggerated claims. It is neither plausible nor expedient to conceive of the organism as a complicated jack-in-the-box with a long list of tricks, each of which may be evoked by pressing the proper button. The greater part of the behavior of the intact organism is not under this primitive sort of stimulus control. The environment affects the organism in many ways which are not conveniently classed as "stimuli," and even in the field of stimulation only a small part of the forces acting upon the organism elicit responses in the invariable manner of reflex action. (Skinner, 1953, pp. 49-50)

Yet, a few pages later, when discussing "The Range Of Conditioned Reflexes" (which presumably cannot extend beyond the range of responses in the spectrum of unconditioned reflexes), Skinner's restrictive tone has disappeared:

Training a soldier consists in part of conditioning emotional responses. If pictures of the enemy, the enemy's flag, and so on are paired with stories or pictures of atrocities, a suitable aggressive reaction will probably occur at the sight of the enemy. Favorable reactions are generated in somewhat the same way. Responses to delectable foods are easily transferred to other objects....The successful salesman is likely to buy his customer a drink or take him out to dinner. The salesman is not interested in gastric reactions but in the customer's predisposition to act favorably toward him and his product which...also follows from the pairing of stimuli. (Skinner, 1953, p. 57)

Do these cases survive close scrutiny--where are the unconditioned responses? In fairness, Skinner does hint that the last example has more to do with the creation of a conditioned reinforcer than a conditioned response. Even so, a detailed analysis is not provided, thereby leaving the impression that "favorable reactions" and "predispositions" are rendered understandable within the respondent paradigm. And it is noteworthy that in a chapter devoted to reflexes, the most interesting and relevant illustrations are also the most metaphorical.

A similar contradiction is evident in the following passage on superstition:

Superstitious rituals in human society usually involve verbal formulae and are transmitted as part of the culture. To this extent they differ from the simple effect of accidental operant reinforcement. But they must have had their origin in the same process, and they are probably sustained by occasional contingencies which follow the same pattern. (Skinner, 1953, p. 87)
This is far too broad a sweep to be covered by the notion of accidental reinforcement. The metaphor itself is somewhat forgivable but the tactic, once again, seems to be: "Qualify, then override the qualification."

Some of Skinner's extrapolations are excusable on the grounds that they are so obviously analogical. Much of his writing on social behavior and the evolution of cultures is of this type. Specifically, he has long made use of a parallel between natural selection, the selection (by consequences) of responses in an individual's repertoire, and the selection of large-scale cultural practices (Skinner, 1971). However, he does not actually assert that the respective processes involved are identical (though one may wonder at times about just what sort of connection is being proposed). Thus, there are implied limits to his system.

Other aspects of the social analysis are presented more forcefully. Cultural values are reinforcers (Skinner, 1971). A law is a "...statement of a contingency of reinforcement maintained by a governmental agency" (Skinner, 1953, p. 339). These are more than mere analogies. They draw their explanatory power directly from the conceptual framework of the experimental analysis of behavior.

And the treatment of the individual's behavior is no less explicit. Skinner (1953) says, for example, that an author's inability to write may be attributed to the effects of operant extinction (induced by a series of rejected manuscripts). The question here is not whether he has gone beyond his facts but whether the extrapolation is in good faith. Even with quantitative evidence that writer's block is a direct function of number of rejections, the account would still be completely metaphorical. Furthermore, it is a metaphor which is clearly intended to pass as generic extension.

Of course, as I have already argued, some of the most objectionable interpretations are those which contain an unacknowledged scattering of generic extensions embedded among the metaphors, or vice versa. Skinner has demonstrated
a certain sensitivity to this kind of blending. Consider some of his 1938 comments on the definition of a response class:

The existence of a popular term does create some presumption in favor of the existence of a corresponding experimentally real concept, but this does not free us from the necessity of defining the class and of demonstrating the reality if the term is to be used for scientific purposes. It has still to be shown that most of the terms borrowed from the popular vocabulary are validly descriptive—that they lead to consistent and reproducible experimentation. We cannot, with Watson...define a response as "anything the animal does, such as turning toward or away from a light, jumping at a sound, and more highly organized activities such as building a skyscraper, drawing plans, having babies, writing books, and the like." There is no reason to expect that responses of the latter sort will obey simple dynamic laws. The analysis has not been pressed to the point at which orderly changes emerge. (p. 42)

Contrast that with the following suggested list of operant behaviors:

While we are awake, we act upon the environment constantly, and many of the consequences of our actions are reinforcing. Through operant conditioning the environment builds the basic repertoire with which we keep our balance, walk, play games, handle instruments and tools, talk, write, sail a boat, drive a car, or fly a plane. A change in the environment—a new car, a new friend, a new field of interest, a new job, a new location—may find us unprepared, but our behavior usually adjusts quickly as we acquire new responses and discard old. (Skinner, 1953, p. 66)

Somewhere between 1938 and 1953 the conservative approach seems to have been abandoned. In the second statement, "more highly organized activities" are mixed together with simple motor topographies. Have we any reason to claim that the dynamic laws of operant conditioning have as much to say about the complexities of socialization as they do about using a hammer? If the concept of a response class requires boundaries, then why not the concept of an operant response class?

A similar blending occurs in the analysis of punishment. In describing his unusual view of this phenomenon—unusual because he says that its effects are due entirely to a combination of escape and avoidance—Skinner (1953) chooses a revealing set of illustrations. His examples include taking candy from a baby, spanking a baby, disapproval, and taking money away by legal fine. The point is that
without the present analysis of extension, the last example might slip in undetected. A legal fine may fit the barest aspects of Skinner's unorthodox definition of punishment, but including it with the rest blends distinctly different processes under a single label. Moreover, the work in the basic laboratory supports the first two cases, does not directly address most types of disapproval, and has even less to offer regarding the scientific status of legal fines.

If these criticisms are sound, Skinner is guilty of a serious miscarriage of technical language. However, perhaps his only real weakness is ambition. Near the end of his seminal work, *The Behavior of Organisms*, he is quite cautious about raising the possibility of a broad extrapolation from the laboratory:

The reader will have noticed that almost no extension to human behavior is made or suggested. This does not mean that he is expected to be interested in the behavior of the rat for its own sake. The importance of a science of behavior derives largely from the possibility of an eventual extension to human affairs. But it is a serious, though common, mistake to allow questions of ultimate application to influence the development of a systematic science at an early stage. I think it is true that the direction of the present inquiry has been determined solely by the exigencies of the system. It would, of course, still have been possible to suggest applications to human behavior in a limited way at each step. This would probably have made for easier reading, but it would have unreasonably lengthened the book. Besides, the careful reader should be as able to make applications as the writer. The book represents nothing more than an experimental analysis of a representative sample of behavior. Let him extrapolate who will.

Whether or not extrapolation is justified cannot at the present time be decided. It is possible that there are properties of human behavior which will require a different kind of treatment. But this can be ascertained only by closing in upon the problem in an orderly way and by following the customary procedures of an experimental science. We can neither assert nor deny discontinuity between the human and subhuman fields so long as we know so little about either. If, nevertheless, the author of a book of this sort is expected to hazard a guess publicly, I may say that the only differences I expect to see revealed between the behavior of rat and man (aside from enormous differences of complexity) lie in the field of verbal behavior. (Skinner, 1938, pp. 441-442)

Those last two concessions seem to me to concede everything. Either one would be sufficiently daunting to predict a virtual discontinuity between rat and man for at
least the foreseeable future. Yet to Skinner they must appear tractable: The first (complexity) receives only a parenthetical mention. This is probably why he proceeded so quickly to the analysis of complex human behavior (e.g., Skinner, 1953). As we have noted, most of Skinner's publications (by his own tally) are about human behavior. But the problem is that this leap was made on little more than the promise of a basic science. Skinner was thus bound to suffer from premature extrapolation. It is not surprising, therefore, to find so many blends of generic and metaphorical extensions in his writing. That may be the inevitable product of ambition. In any event, one thing is clear: We have come a long way from "Let him extrapolate who will."

The Paradox of Extension

This concludes the examination of metaphorical extension in the analysis of human behavior. Throughout the chapter my tone has been uniformly negative. I have been critical of what I judge to be a pervasive deterioration in our verbal practices: The scientific language of the behavior analyst is rife with unacknowledged metaphor. Nevertheless, as with many of the complex topics in psychology, it would be a mistake to draw any single conclusion from this state of affairs. There are a number of implications to be considered, and some of them are paradoxical.

Perhaps the most startling aspect of the phenomenon, given its sheer size, is the fact that it has so long gone almost completely unnoticed. This oversight is nicely reflected in most of the articles which comment upon the health of applied behavior analysis, a literature in which any mention of a serious linguistic crisis is conspicuous by its absence. In fact, the trends usually cited as threats to the field's health are sometimes reinterpretable in light of the present orientation. We have seen, for example, that one of the conventional within-family criticisms of applied research is
that it makes repetitious use of just a few simple principles (e.g., Hayes, Rincover, & Solnick, 1980; Pierce & Epling, 1980). One such article, which attempted to document what it called "The Technical Drift of Applied Behavior Analysis" (Hayes et al., 1980), claimed to have found (among other things) that: "The majority of JABA manuscripts have always been, and continue to be, simple applications, testing the applicability of known behavioral principles" (p. 280). By the generic standard of what constitutes a behavioral principle, nothing could be further from the truth. It may be that the category which these authors call "direct applications" does indeed cover more than 60% of the research submitted to the first 10 volumes of JABA. However, their survey means only that the studies in question invoked basic principles. We simply do not know what fraction of this majority represents metaphorical appeals to basic concepts.

If the entities that populate the applied field are not genuine principles, what is their proper characterization, and why have they been employed so successfully? I submit that a great deal of what takes place in the name of applied behavior analysis has been carried, not by fundamental scientific principles, but by heuristics. Thus, many of the applications which Hayes et al. refer to as direct, undoubtedly are not. They are effective practical interventions guided by rules of thumb, that is, by metaphors of operant and respondent principles. The actual mechanisms of action are, at present, largely unknown. That may be a disturbing revelation, but it is certainly not all bad news. It merely points up another of the paradoxes of extension: Heuristics offer real utility; unfortunately, they also invalidate our claim to a true natural science.

One of the undeniable benefits of a flexible set of heuristics lies in the area of interpretation. The behavioral vocabulary, even when extended metaphorically, provides a ready-made conceptual framework with which to make points and present
arguments. That is exactly what I have done with Skinner's (1957) analysis of the tact and tact extension. The reader will recall my disclaimer regarding whether these concepts actually describe the normal verbal behavior of the scientist. Nevertheless, the scheme has proved useful as a communicative tool. And this is more than just a matter of convenience. One should not underestimate the importance of having available a system for discussing human behavior which is both comprehensive and internally consistent, as well as being explicitly nonmentalistic. These three properties in combination lend considerable support to much of Skinner's interpretive work.

Perhaps that is why his extensions are no less compelling when they happen to be metaphors. For example, we have already reviewed the case of the writer who, according to Skinner, had stopped writing because of extinction. The illustration is indefensibly metaphorical. Yet Skinner's point, as usual, is still worth taking. There are a number of reasons for writer's block, and it may be useful to distinguish the type induced by rejection from the type that is due to having nothing to say. Thus, even in the context of metaphor, an important causal relation may have been isolated. I suggest that many Skinnerian contributions are marked by this dual nature.

A second and more tangible advantage of the loosely constrained concept is its sizable yield in clinical application. Contracting is a prime example of an excellent technique that might never have proliferated if the early behavior modifiers had emphasized the fact that it was linked only heuristically to basic research. It would have been a shame to deny those persons who were helped by contracts simply for the sake of theoretical purity. We tend, with some justification, to overlook the weak or questionable source of a procedure that has clearly demonstrated its effectiveness. London (1972) has described a similar tradeoff in behavior therapy. On the one hand, he argues that treatments like desensitization are no more than metaphors or
analogies of conditioning theory. In spite of that, London does recognize that metaphors are useful as guideposts—he even uses the word heuristically—and concludes that: "...none of this has anything to do with whether anything works for any practical problem. Desensitization may be a poor deduction from conditioning, but it is a fine treatment for phobias" (p. 918).

Metaphorically based interpretations and applications can clearly be beneficial forms of innovation. However, the creativity they bring comes at a price. When a theoretician employs a metaphor, all that is known about the generic case is implicitly and mistakenly extended to the metaphor. When a practitioner uses an extinction program that is not really extinction, it may or may not work. Documenting the mistakes and failures attributable to metaphor would be a challenging and worthwhile exercise. I suspect, though, that there is a greater cost associated with imprecise technical language, and that it is one which is even more difficult to discern than either of the types just mentioned.

Whenever advances such as contracting are derived more from the heuristic value of the experimental analysis of behavior than from the details of its discoveries, the entire scientific enterprise is compromised. Science demands that we say with precision whether contracting is actually anything like key pecking, at least insofar as the terminology of reinforcement is concerned. If the processes are distinct, and we subsume them under the same label, an important gap in our understanding will have been missed. To fail to detect gaps of this kind is to lose both the impetus and the direction for further experimental inquiry—if not reinforcement, what is at work in contracting?—and the loss is always at the expense of genuine explanation. In Skinner's own words, a premature or insubstantial explanation can function "...to allay curiosity and to bring inquiry to an end" (Skinner, 1957, p. 6). On the other hand, when metaphorical extensions are recognized as such, the boundaries of
scientific knowledge become clear. This permits a direct attack on the limiting cases of a principle. And, it encourages a pursuit of systematic inference in the manner which Platt (1964) has suggested in his well-known paper on inductive method. The alternative, forever easy to slip into, is to blur the discipline's other borders, slowing or stopping true discovery, having "explained" everything.

If the end of inquiry comes too soon in the world of application, its acceptance is easier still in the case of interpretation. Consider the concept of a response class, and its common rendering in the theoretical literature as a fluid complex of adjustments and adaptations. A particularly risky but familiar pattern is that upon designation of some object as a putative reinforcer, the response class becomes whatever one does to obtain it. The usual rationale is that the members of this class are functionally related by virtue of their common consequence; nevertheless, a good deal of rigor is lost here. Although it is undeniable that many actions can accomplish the same end, that may be the only feature shared with the response-class notion. The adjustment process whereby a student obtains a degree is neither explained nor described by the standard operant vocabulary, and the simple identification of an important environmental relation merely begs continued analysis--yet none is likely to be forthcoming. As with the metaphors of the practitioner, metaphor in interpretation often serves to abort further inquiry. Skinner is, of course, a principal transgressor in this regard. However, I hope that the preceding sections have convinced the reader that his expansive style has been fully embraced by a number of the field's lesser luminaries. As more and more of these extensions find their way into print, a significant body of metaphor is steadily and somewhat officially absorbed by the behavioral culture.

For the reasons stated above, I submit that the real cost of the behavior analyst's poorly controlled verbal practices is that they promote a false sense of science. The
robustness and veracity of our principles have become self-fulfilling prophecies. Their essential status is rarely questioned. Perhaps this is because, once struck by the inherent wisdom of the three-term contingency, no one seems able to resist seeing it in every facet of daily life. The fact that many situations offer only a partial complement of a concept's defining features is apparently a weak deterrent to indiscriminate usage. Thus, we incautiously employ the observed phenomena of human behavior to compel the psychological reality of our language system—and it all comes true. It is exceedingly difficult, therefore, to recognize and address the overarching paradox of extension, namely, that we have made far too much of a good thing.

A deviation of this size must be the result of several factors. One of the most obvious is the lure of the shibboleth. A verbal repertoire founded on laboratory principles virtually assures membership in the scientific fraternity. The temptation is not unique to behavior analysis, nor even to psychology in general. It is a by-product of any endeavor that aspires to join the ranks of the established natural sciences. Unfortunately, it is also not the type of trap that lends itself to self-correction.

Another sort of pressure that tends to favor a relaxation of strict linguistic convention has to do with the issue of subject matter. Research in the experimental analysis of behavior has usually been concerned with the study of relatively straightforward motor topographies. The actual unit of analysis, as is often pointed out, is not based on response forms but rather on functional classes. Even so, a few highly stereotyped topographies nearly always emerge, thereby ensuring the correlation between switch closure and discrete skeletal movement. Scientists and clinicians alike play their strongest hand when they confront behaviors of this kind. However, who would support a science of discrete motor responses? Many topographies are trivial, especially in comparison to the possibility of a regular foray
into the exciting realm of social behavior, or even scientific behavior. (The way in which scientists conceive their experiments is much more intriguing than the way they shake their test tubes.) It follows, then, that most interesting human behavior will almost inevitably give rise to metaphor. As Skinner (1983) has recently said, in a charge leveled at nonbehavioral psychology, "A great many things can be talked about when standards are less rigorous" (p. 9).

These factors (which may fairly be paraphrased as prestige and ambition, respectively) certainly account for some of the ubiquity of metaphorical language. Yet, there is an additional pair of forces to be considered, belonging more to perception than motivation, and their combined influence is sufficient to explain any residual slippage in our technical verbal practices. Briefly, the behavioral community has been seduced by the power of partial similarity coupled with success; partial similarity, because that is the essence of metaphor and the mainstay of heuristic guidance.

Surely, the first behavior modifiers must have realized that their programs lacked features such as temporal contiguity; in the early days these discrepancies probably gave them considerable pause. But they were pioneering a new technology, and, once their initial efforts began to work, the tentative atmosphere quickly dissipated. Soon, the discrepancies ceased to be troublesome. After all, a functional relationship had been established. Was that not the principal empirical test for a reinforcer?

The problem is that reinforcement is a specific process, and no one was prepared to recognize that it should not cover all orderly relations between behavior and its consequences. A few well-placed practical failures of metaphorical reinforcement would have gone a long way toward maintaining the integrity of our technical language. Of course, they would also have severely restricted the scope and general
relevance of applied behavior analysis. As things turned out, the field's relevance is now quite broad, but its depth is far less extensive than we would like to believe.

Skepticism Renewed

To this point, I have described a scientific promise, the conventional wisdom which fuels it, and two forms of evidence that, I believe, force a complete reconsideration (perhaps even a retraction) of the entire happy proposition. The promise is that a science of human behavior holds great hope for humanity, indeed, that it is necessary to save the world from certain disaster; the conventional wisdom says that behavior analysis, although young, offers just the solution, and that it will eventually develop into a mature sister of the established natural sciences; and the undermining evidence consists of the applied-basic split and the unacknowledged prevalence of metaphor.

These two paths, which have brought me to my own apostasy, lead to the same general conclusion: that the scientific basis of applied behavior analysis is mostly an illusion. The split hints at such a conclusion; the presence of metaphor compels it. This stands in direct opposition to the widely held conviction, referred to by Michael (1985) as the normal relation, that basic research, applied research, theory, and application all share the same basic principles. If I am right, the promise of a science of human behavior, along with the belief that the beginnings of that science are already under way, both warrant a renewed skepticism. So much for the nature of the proposed reconsideration. Assuming that my essential characterization is correct, what are its implications?

The first is that we have, within applied behavior analysis, two separate domains which have up to now been treated as one. They are both called technology, but that term should be reserved for only those interventions which actually employ scientific
principles, that is, where the processes and procedures involved may be described with the generically extended "tacts" of the operant vocabulary. This category will prove, by far, to be the smaller of the two. The second and much larger domain ought to be named simply, technique. It includes both the set of applications in which no appeal is made to basic principles as well as the set which invokes these principles, but does so metaphorically. Of course, it is the latter aspect of the second domain that is often mistaken for genuine technology. None of this is meant to imply that, for practical purposes, techniques are any less valuable than technology. However, the field's aspiration to scientific status could at least be better evaluated if the two domains were kept distinct.

Several disturbing trends in behavior analysis are nicely explained if we abandon the claim to a laboratory-based technology. The applied-basic split is a good example. As we have seen, it has become fashionable to view the increasing independence of the applied area as an accident of overspecialization. Several accounts (see Minervini, 1986, for a review) have portrayed this aberration as the product of various professional exigencies, which could be depicted (albeit gratuitously) as publication contingencies, funding contingencies, and therapeutic contingencies. Yet the best reason for the split is the simple irrelevance or inadequacy of the experimental analysis of behavior. In many applied situations, its principles are just not equal to the task. Thus, the clinician's independence was inevitable--how could it have been otherwise? The split, in turn, explains the necessity for creative leaps, such as contracting, and the absence of the slow but steady type of progress that would be expected from a tight linkage to basic research. When standard principles reach their limits, it becomes necessary to get creative at the level of procedure (Azrin, 1977). Naturally, there are always pressures to maintain a
connection with the laboratory, even when none exists. Hence, the ubiquity of metaphor.

The three themes of separation, forced creativity, and metaphor, as well as an assortment of related trends (e.g., the proliferation of package treatments and the recent interest in rule-governed behavior), each point to a renunciation of all but the most restricted claim to scientific status. As if that were not bad enough, we must also reckon with the possibility that, in a strict sense, there may not exist even an incipient science of human behavior. Any true natural science, no matter how immature, must eventually produce at least one principle whose soundness is undisputed. Behavior analysts like to assume that their enterprise has moved well past this minimum criterion; but if one principle had to be chosen, it would be reinforcement. Unfortunately, a close look at reinforcement, as it is applied outside of the laboratory, is not reassuring. The dimensions of magnitude and delay, for example, have never been systematically examined for a representative sample of human behavior. Some parametric work has been carried out with dependent variables similar to those used in animal experiments. However, the area known as human operant research has, as a whole, received only sporadic attention in the behavioral literature (Buskist & Miller, 1982). Moreover, studies of the uncontaminated effect of reinforcement (e.g., Holland, 1958) are rarer still. It is not unreasonable, therefore, to pose the following question: Has behavior analysis yielded even a single fundamental principle of human behavior?

I do not propose to have the answer to that question, but it would be a mistake to approach it as a foregone conclusion. We must not forget, to borrow another Skinnerian theme (Skinner, 1972), that the flight from the fundamental always presents an attractive option. One of the obvious comforts of supposing that the discipline's foundation is in good order is that it allows us to move on to higher level
professional issues. The topics of recruitment, political survival, organizational structure (naming the field and certification, for example), and even work in the philosophy of science are all more or less tractable. And each can be dealt with immediately if the behavioral community presupposes that its basic principles are well understood.

The zero-principles argument, although it may seem bold, is no more so than its obverse. The discovery and elaboration of just one, thoroughly robust principle would constitute a tremendous achievement. Psychologists of every persuasion would be compelled to acknowledge its import. In addition, such a principle would not need to be stretched (any more than the notion of direct current would) so as to be useful in an applied setting. It is difficult to imagine another science wherein the relevance of a principle is inversely related to the complexity (hence, the reality) of the circumstances in which it is applied. Lastly, each generic extension of a true principle, from the repetitive to the innovative, would contribute something to its generality. Contrary to what Baer (1978) has stated, that is not presently the case: Every JABA study does not now add another brick to the tower of science and technology.

If the principles of applied behavior analysis are not directly responsible for its success, what is? Most of the credit, I believe, should be attributed to what might be called the general behavioral orientation. This consists of a powerful idea and a dose of ideology. The operant paradigm, irrespective of how well it is borne out empirically, is a powerful idea. That is why it spread. Even an unsophisticated version of the three-term contingency supplies a quick (and somewhat intuitive) plan of action that works, if for the wrong (heuristic) reasons. The ideology is behaviorism; a description of its influence is beyond the scope of this paper. It is
sufficient to say, however, that we probably owe less to behavioral science (such as it is) than to the philosophy which legitimized the study of behavior in its own right.

London (1972) has also questioned the usual role ascribed to principle, but he is openly contemptuous of both the content and the philosophical orientation that constitute learning theory: "What behavior therapists called theory actually served as bases for commitment or a rallying point for talking about disorder and treatment in a certain way and, more important, about acting on it within particular sets of limited operations" (p. 916). It was never really theory anyhow, as we used it, but ideology for professional purposes and mostly metaphor for clinical ones" (p. 919).

A final subject for reconsideration is the nature of the scientific promise itself. As I described it in the first chapter, a principal thrust of the promise is actually a threat: that a science of human behavior is necessary to save the world. The proposition makes a poor candidate for vigorous skepticism because it is so obviously plausible. Most of humanity's problems are ostensibly the result of maladaptive behavior. Therefore, how better to ameliorate these difficulties than by an enhanced understanding of the variables of which behavior is a function? All of this moves along with such facility that we are likely to miss an embedded assumption. The scientific promise implies that the activities which threaten mankind's survival are essentially similar to the other natural phenomena of our planet, that is, that they amount to problems whose solutions await knowledge. I suggest that there is another broad class of problems, those for which the solutions await consensus. It is this latter variety that challenges the species' continued welfare. Furthermore, it seems to me intellectually dishonest to claim that the second type of problem merely reduces to the first. Humans could not fly until there were airplanes; airplanes could be built as soon as the required information became available--an ideal problem for science. Can the same be said of war, disarmament, injustice, hunger, and the like? All of these
problems have their technological aspects, but they pale in comparison to the consensual ones. And it is naive to suppose that a science of consensus is the answer. That is the one solution which would obviate all others.

The distinction I am proposing is evident in the difference between plague and famine. In many countries, including some of the underdeveloped ones, the chief obstacle to the elimination of a new plague (i.e., a disease new to medical science) is insufficient knowledge. Famine, on the other hand, is a problem of resource allocation, where the relevant resource is money. (We have long had ample food to go round.) An international consensus would solve the problem almost immediately. Likewise for housing, illiteracy, political confrontation, and on, and on. This is not an attempt to resolve the traditional debate over the distinction between fact and value (see Skinner, 1971). Rather, I am suggesting that the denial of that distinction (or something close to it) fosters an overzealous selling of psychology. Whether or not the problems of consensus are ultimately reducible to problems of fact, they are (at least for practical purposes) not the same kinds of facts as those which the physicist or the biologist is prepared to address directly. Said another way, it is provisionally worthwhile to retain the commonsensical distinction between fact and value, at least for the purpose of focusing it on the potential merits of the scientific promise. Which of the big problems in human engineering and which of the little ones offer even a chance of yielding to additional information? Which are only superficially behavioral? These are but a few of the features of the scientific promise that ought to be reasserted as questions.

The Argument From Incipience

We now turn to two standard defenses which contradict the spirit of renewed skepticism just described. The first concerns the selection of an appropriate time
scale. Perhaps the easiest rejoinder to the charge that premature extrapolation has fostered an illusion is to simply assert that the charge itself is premature. As the following two passages indicate, Skinner is particularly fond of this tactic:

Certainly no one is prepared to say now what a science of behavior can or cannot accomplish eventually. Advance estimates of the limits of science have generally proved inaccurate. The issue is in the long run pragmatic: we cannot tell until we have tried. (Skinner, 1953, p. 20)

The commonest objection to a thoroughgoing functional analysis is simply that it cannot be carried out, but the only evidence for this is that it has not yet been carried out. We need not be discouraged by this fact. Human behavior is perhaps the most difficult subject to which the methods of science have ever been applied, and it is only natural that substantial progress should be slow. It is encouraging to reflect, however, that science seldom moves at an even pace. Progress is sometimes arrested for a long time merely because the particular aspect of a subject which is emphasized proves unimportant and unproductive. A slight change in point of attack is enough to bring rapid progress. Chemistry made great strides when it was recognized that the weights of combining substances, rather than their qualities or essences, were the important things to study. The science of mechanics moved forward rapidly when it was discovered that distances and times were more important for certain purposes than size, shape, color, hardness, and weight. Many different properties or aspects of behavior have been studied for many years with varying degrees of success. A functional analysis which specifies behavior as a dependent variable and proposes to account for it in terms of observable and manipulable physical conditions is of recent advent. It has already shown itself to be a promising formulation, and until it has been put to the test, we have no reason to prophesy failure. (Skinner, 1953, pp. 41-42).

Skinner is by no means alone in his support of the open-ended promissory note. The entire scientific community stands ever ready to wag its finger at any Philistine who dares ask for too much too soon. But, of course, one can always argue that real advances lie just over the horizon. This is the argument from incipience, and it seems to grow more strident as one decade passes into another with almost nothing to show in the way of actual technological progress. The weakness of appealing to slow scientific movement or long lags between research and practice is that neither of these excuses wear as well as they might have when Galileo was rolling balls down inclined planes. Considering the overall timespan of science, a quarter of a century in the twentieth century is a long time indeed. Extreme lags are still possible, but to
emphasize that is to seize the exception and sacrifice the rule. The modern world has produced a climate in which genuine science should flourish. For a time, this seemed to be the case with behavior analysis. The essential Skinnerian discoveries were available by 1938. In slightly more than 20 years, behavior modification was off to an ambitious start. Since then, fundamental breakthroughs have proven so elusive that the question now is whether the start was a false one (not an uncommon event in psychology).

There is an tacit recognition of the fact that early predictions of rapid success were frequently overblown. It is reflected in the way that behavioral folklore has changed over the years. At one time, for example, the token economy promised to be the catalyst of a revolution that would sweep through the mental health system. The logic was that crazy behavior was learned and maintained by reinforcement contingencies. Therefore, changing the contingencies should change the behavior. Sometimes it did. But the revolution never took place. True, there are more token economies in operation nowadays than there once were, and a sizable number of people have been helped by their presence. These are important results. However, they are not commensurate with the extent of our initial enthusiasm. Whatever became of the adage that a good deal of mental illness would someday disappear if only the behaviorist effort was allowed to proceed unencumbered?

Other aspects of our folklore have remained unchanged. In 1970, Hopkins examined some of the impediments to maintaining a behavioral program in a state institution. He concluded, with some conviction, that the problem did not lie in the sufficiency of behavioral principles, but in the intransigence of administrative personnel. The article, which appeared at the close of behavior modification's first decade, ends on a note of mild resignation: "After all, the first twenty years are the hardest" (p. 365). We may all agree that political opposition could thwart even the
potent technology of the physical sciences. But 17 years after Hopkins' original publication comes another on the same topic, and with essentially the same complaint (Hopkins, 1987). Apparently, the recalcitrance of the system may forever remain an obstacle. For present purposes, it has at least the virtue of introducing the next section; Hopkins' comments illustrate the shift from the first standard defense (incipience) to the second (oppression).

**Allegiance Without Oppression**

Inasmuch as these two elements usually operate in conjunction, the title might have been supplemented by a question: Is it possible to engender the former tendency in the absence of the latter? Opposition, despite its obvious drawbacks, provides a special type of fuel that humans seem to thrive on. Nothing invigorates the ranks of a struggling cause so well as manageable opposition; nothing, that is, except outright oppression. Enemies not only simplify our lives by encouraging consolidation and giving focus to our efforts, they also supply an inexhaustible source of excuses for what might otherwise be viewed as embarrassing failures or shortcomings. This is not to suggest that the oppressed are without merit. It is only an acknowledgement of the possibility that support can be generated by factors other than merit.

The enemies of behavior analysis are many—psychologists from competing orientations, decision makers who are insensitive to outcome data, the defenders of individual autonomy, and so on—but perhaps its oldest and most formidable adversary is mentalism. This is not just the mentalism associated with traditional psychological theory. Rather, it is the general predisposition toward inner directedness that pervades the language and culture of much of western civilization. Skinner has devoted a considerable portion of his writing to the theme that mentalism is one of the primary threats to developing the behavioral science that is needed to
save civilization (see, e.g., 1978b, 1978d). And the message has been well received; other prominent behaviorists (Branch & Malagodi, 1980) have also warned that mentalism is no less than a clear and present danger to our chances for survival.

Granting for a moment that mentalism has had a damaging effect on the study of behavior, does the same hold true for society at large? There probably is an implicit cultural belief that behavior has its origins in inside causes, but to actually concentrate on the inner life as a mechanism for change is a practice almost unique to psychology. Clinical psychologists are particularly vulnerable in this regard because their interventions and outcomes have historically been verbal. By contrast, the world outside of the therapist’s office breeds reliance on a kind of unschooled pragmatism. That is, whereas the economist may routinely blunder into explanations which are based on a crisis of confidence, how often are economic solutions directed at readjusting the mental life?

Skinner has recognized this form of self-correction, and has summarily dismissed it:

[Some would say that] a mentalistic philosophy is rather inoffensive, and it need not seriously handicap practical people. Am I not exaggerating its importance? But there must be some reason why we are not making the technological advances in the management of human behavior which are so obvious in other fields, and the reason could be our lingering commitment to the individual as an initiating agent. It is of the very nature of human behavior that seemingly trivial causes have profound effects... (Skinner, 1978b, p. 95)

No one will deny that. However, the exigencies of the real world frequently displace the mentalistic conception so readily that it may be a trivial factor after all. A fiscal policy that was truly concerned with alleviating a crisis of confidence would likely consist of television commercials and government sponsored seminars. To that extent, Skinner’s anxiety is justified. As for the broader picture, it is useful to remember that mentalism is but one type of deviation from rationality. Are not the
forces of religion, political ideology, and nationalism more serious threats to our future?

The argument from oppression is most tenable when it is reserved for groups or movements that have been deprived of the opportunity to prove themselves. Behavior analysts have never enjoyed favored standing in the wider community of psychologists. Nevertheless, there have always been behavioral strongholds; and in a few cases, even temples. Some of these have achieved admirable levels of productivity. Not one has yielded a discovery which has caused the discipline as a whole to surge forward. This may be the result of incipience or oppression or both, but it cannot be said that our most promising attempts have been completely stifled. We have tried; we have tried for a long time; and, in certain locations, we have tried unimpeded. An equitable position for oppressed causes in general is to be able to state in advance the point at which they would become responsible for their own destinies. The preceding comments should be interpreted in that spirit. 

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PART III: ON MARRIAGE AND CIRCUMSCRIPTION

I shall consider my efforts in these pages successful if the reader has been persuaded to reconsider the distance between a science of human behavior and a scientific approach to human behavior. The two are not equivalent, and at the present time it is still an act of faith to believe that the first will inevitably derive from the second. Skinner's influential book, *Science and Human Behavior* (1953), is perhaps the most articulate statement of that unwavering belief in the potential of scientific method. Ironically, his choice of the connective and may have been prophetic, for it makes the title of this work more humble (and probably more accurate) than its content (a content which clearly presupposes the possibility of a science of human behavior).

Having said that, I hasten to add a disclaimer. Nothing in my proposed reassessment is necessarily bad news for the contemporary behavior analyst. The marriage of science and human behavior has a great deal to offer mankind. To date, its advantages remain largely unmined. Of course, our contributions may not be as scientific or as momentous as some of us would like to assume, but circumscription is hardly something to be ashamed of. Tangible improvements are no less valuable to their beneficiaries just because they are only loosely connected to principles borrowed from the animal laboratory.

Neither is there any reason to question the specific place of an experimental analysis of behavior, if that activity is taken to imply simply the use of experiment to analyze those variables which affect human behavior. The same is true of so-called component analyses. Even the most sophisticated fractionation of components
amounts to little more than raw functionalism. Yet, such investigations will continue to be important irrespective of the ultimate validity of the principles which now constitute the operant/respondent paradigm. This line of reasoning has led me to a curious affirmation: The behavior analytic enterprise, notwithstanding my call to deflate its scientific prospects, is still the best game in town.

On a smaller scale, a similar paradox applies to Skinner's theoretical extrapolations. Although I have recommended that the details of his explanatory system be reappraised (on the grounds that they are littered with metaphor), it is nonetheless true that the bulk of his insights have gone unappreciated. Skinner's expositions, metaphors and all, are some of the most compelling in psychology. It is worth noting that even a thoroughgoing skeptic, who manages to reject all talk of operants, contingencies, and stimuli, may yet come away from a careful reading of Skinner with new respect for the position of strong environmentalism. And then there are the negative observations. Skinner is at his best when he is exposing the weaknesses of his opposition (the opposition includes other psychological orientations as well as much cultural tradition). Psychology requires a periodic debunking, and this may prove to be one of the Master Behaviorist's most enduring contributions.

On the other hand, as might be expected, he has been less than diligent when it comes to in-house criticism. In a recent indictment of modern cultural practices, Skinner (1987) pokes fun at several of the "nervosas" that plague democratic societies--"Caritas nervosa," for example, is the counterproductive tendency to help others when they are capable of helping themselves. But he has been virtually silent on our own collection of nervosas: gratuities nervosa (the metaphorical extension of behavioral principles); manana nervosa (the incipience defense); oppressium nervosa
(the opposition defense); and, most notably, *grandiose nervosa* (the rather fanciful notion that psychology will save the world).

When Skinner *is* on the mark, the greater part of his eloquence is attributable to the general behavioral orientation that I alluded to earlier. For many of us, one of the principal benefits of this orientation is that it fosters a set of interrelated prejudices that I contend are basically correct. As with Skinner's points, some of the most significant manifestations of these prejudices have a negative character. (A conspicuous example is the behaviorist's complete ignorance of popular psychology.)

Consider the area of teaching and learning. The behavioral orientation helps one cultivate a healthy disrespect for some of the most cherished boondoggles in the educational establishment: the scourge of inferential neurology (learning disabilities, minimal brain dysfunction, modality learning, and sensory integration); the fruitless appeal to internal dynamisms (e.g., low self-esteem); and the reflexive flights to developmentalism and genetic endowment (although both of these concepts have their place as legitimate explanations).

On the positive side (continuing with the educational example), a number of sound behavioral injunctions derive nicely from the application of a scientific approach to the difficult problems of teaching and learning:

1. Base teaching on active and consequated responses.
2. Divide learning into discrete units and require cumulative mastery of essentials before proceeding. (This is from Keller's personalized system of instruction.)
3. Whenever possible, test for productive as opposed to selective (multiple-choice) repertoires.
4. Do not hesitate to use contrived rewards when they are necessary.
5. Strive to design situations which rely on positive outcomes.

And this is an abbreviated list.
Education makes an especially appropriate example in this context of psychological advice, for it brings us back to the twin themes of marriage and circumscription. In attempting to marry scientific method to practical issues in the prediction and control of human behavior, we would do well to seek out the most fertile territory for our ventures. We should look for the largest possible effectiveness differentials (see the section entitled The Scientific Promise), that is, the spheres where our orientation and principles (both heuristic and direct) will provide the greatest gains. Education, because it depends on the construction of repertoires, offers just this sort of possibility. It is an excellent candidate for the union of problem and solution.

Some Final Advice

I shall conclude with several prescriptions. A primary goal of this paper has been to sensitize the reader so that behavioral language may be seen and heard as if through a filter. Thus equipped, it should be possible to habitually separate generic from metaphorical and metonymical extension, and to consider the effects of this separation on our professional assumptions and orientations.

In general, this will allow us to render unto Skinner what is his, and only what is his. More specifically:

1. As applied researchers, we should better establish the limiting cases of our principles. In essence, this means recognizing the boundaries of scientific knowledge. Where those boundaries are exceeded we should, as Skinner (1953) has suggested, learn the difficult lesson of "...remaining without an answer until a satisfactory one can be found" (p. 13).

2. As speakers and writers, we must avoid blending the various types of extension as if they were identical, especially in a series of sequential examples.
Spanking babies and issuing fines are not usefully grouped together under the broad heading of punishment (see the section entitled Skinner's Extensions).

3. As clinicians, we should attempt to determine whether the source of a practice is axiom, principle, or general orientation. In addition, certain types of creativity ought to be viewed with suspicion.

I have offered no new facts in this analysis. Instead, the point of the exercise has been to demonstrate the unexpected impact of the familiar. No one may be surprised, for example, to find that technical terms are occasionally mangled. If there is anything new here, it is that current estimates of linguistic violation are off by an order of magnitude. Perhaps this is understandable if imprecise language is seen as part of a larger tendency. The relentless appeal to metaphor may be only a single aspect of our unwillingness to give up, or at least move beyond, the comfort of known terrain. Soon, we no longer attempt to say what a thing is, but merely what it is like. A recent essay on the nature of understanding (Cole, 1984) reports that Confucius once said, "The beginning of wisdom is calling things by their right names" (p. 57). Perhaps it is time for the behavior analytic community to take that advice to heart, even if doing so reveals our discipline to be something that is not quite the same as genuine science.
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