The Speciation of Behavior Analysis: The Unnatural Selection of Foxes and Hedgehogs

M. Jackson Marr
Georgia Institute of Technology

David Rider has presented a stimulating analogy between speciation through isolation and the divergence of basic and applied theory and practice in the field of behavior analysis. Its publication should occasion lively and useful discussion. As the author points out, the issue of the relations between basic and applied work has been a major topic for articles, speeches, workshops, etc., and yet seemingly little, if anything, has changed to help hold the two endeavors together. Indeed, there appear to be relentless processes akin to biological evolution that drive irreversible and unpredictable changes in human enterprise:

The Moving Finger writes; and, having writ,
Moves on: nor all your Piety, nor Wit
Shall lure it back to cancel half a Line,
Nor all your tears wash out a Word of it.

A number of implications of the diversion of interests of the experimental analysis of behavior and applied behavior analysis require, I think, a more detailed treatment. It is not enough to provide a compelling analogy; how are we to deal with the outcomes? If we know, for example, that the division process is inevitable, is there anything one can do to make the transition easier? Do such divisions always imply a complete separation of conceptual interests? Alternatively, and despite apparent evidence to the contrary, is there still the possibility that productive interactions between basic and applied workers can be maintained and even enhanced?

Human evolution has become a very different process from natural selection because we, at least to some degree, can control our environment, can plan for variations in contingencies, can predict

outcomes, and can communicate all this anywhere. So, we are embedded in an "unnatural" environment and subject to "unnatural" selection. Thus, one might question the analogy on the basis of possible significant differences between essentially random, blind processes and those characteristic of a human culture. One may speak, of course, of a cultural evolution, in this case, a scientific/technological culture. Speciation of scientific fields depends upon the growth of coherent knowledge within a field of sufficient breadth and depth to maintain and develop that field. This, in turn, depends upon the exercise of effective methodologies to problems of interest to establish sufficient credibility to attract new practitioners and to develop new training programs, etc. Part of this process includes contingencies for survival of the culture. Skinner once remarked in an interview something to the effect that if a culture does not establish and strengthen contingencies for its own survival, then so much the worse for that culture.

Credibility of a scientific enterprise depends, in part, upon a kind of "ecology" wherein the methods, principles, and results of one field are connected with those of other established fields, often in complex, interactive ways. Rider's discussion of the relation between science and engineering is, for this reason, exceedingly simplistic, if not outright wrong. Unlike Rider, I will take the risk of boring the reader on this topic not simply to be pedantic, but in the service of indicating some possible significant differences between "mainline" science with associated engineering fields, and basic and applied behavioral analysis. The differences may be important in understanding some of the contingencies controlling both survival and separation of basic and applied behavior analysis.

While there is clear recognition of the integrity of engineering as a separate en-

Reprints may be obtained from M. Jackson Marr, School of Psychology, Georgia Tech, Atlanta, GA 30332-0170.

terprise, it is just as clear that good engineering, whether electrical, mechanical, chemical, aerospace, computer, or whatever, is not only founded upon an extensive net of principles from the traditional basic areas of mathematics, chemistry, physics, biology, etc., but also depends on an up-to-date understanding of those areas. If the author doubts this, I suggest he peruse a modern engineering text on quantum electronics, fluid mechanics, or chaotic dynamics. Also, there are many research domains where engineering and science blend together into a "hybrid" that does not clearly belong to either in any traditional sense. These factors are very important to understand because they weigh heavily on the issue of training. Ernst Mach's pedagogical principle that we will use as much science as we know obviously applies to engineering training; electrical engineers *need* to know a great deal of physics, and chemical engineers *need* to know a great deal of chemistry. No one doubts this. The foundations are clear, regardless of the department in which they are taught. At my institution, for example, which is reasonably well noted for both its scientific and engineering programs, *all* the engineering curricula require at least six quarters of mathematics, two quarters of chemistry, and three quarters of physics. Some curricula require, or at least strongly encourage, considerably more "basic training." In addition, much science and mathematics is taught within the engineering courses as they are applied to engineering problems. For example, electrical engineering students learn a great deal about Maxwell's equations in order to design, say, microwave transmission and receiving systems. Maxwell, of course, was a theoretical physicist who would have been pleasantly astonished to see his equations become part of the everyday repertoire of young engineering students. Many such examples could be given to demonstrate that all this training in basic science and mathematics is not a form of academic harassment to weed out uncommitted students, nor is it simply reflective of some outmoded tradition. Of course, engineering students don't

learn as much physics as a physics student, but the point is that they must learn a substantial amount in particular areas to do their work. It is also the case that physics students need to know some engineering, for example, in designing various types of apparatus. We see therefore that fields may diverge yet still maintain a close relation to each other through the exercise of common principles and mutual contributions. This is especially true at the frontiers. Basic research, for example in solid state physics, has often found rapid application in such fields as computer design.

How does basic versus applied behavior analysis fit into this discussion? Clearly, the fewer shared principles and recognized mutual contributions, the more rapidly the fields will diverge. Baer (1981) indicated that much of applied behavior analysis can be reduced to the principle of reinforcement in some guise or another plus experience with applied situations gained primarily through trial and error. In the front lines of application anything that will help is of value and there is little time, training, or inclination to engage in a behavioral analysis beyond the minimum necessary to do the job. This is the "old country doctor" approach—simple remedies at hand are by and large the best. To the extent that the principle of reinforcement constitutes the primary remedy, applied workers remind one of the line from the Greek poet Archilochus: "The fox knows many things, but the hedgehog knows one big thing" (cited in Leahey, 1987, p. xi). Here we begin to see how the relationship between basic and applied behavior analysis differs from that, say, of physics and electrical engineering. It would be as if all of electrical engineering practice was based only on Ohm's law. As important as Ohm's law is, it could not possibly bear the burden of a modern effective technology. The engineer has to be a fox, while the applied behavior analyst is seemingly content to be a hedgehog.

Within behavior analysis there are, of course, anomalies—researchers who straddle both camps successfully and who directly apply the results of basic research

to applied problems. A major example is J. J McDowell, who has been one of the most persuasive advocates of a strong tie between the two areas (see, for example, McDowell, 1989). Rider mentions work of this kind, but notes that, in general, applied people remain content with the old ways because they are serviceable enough. I would argue that a field that does not come to apply the most effective procedures available in solving its problems is ultimately doomed to extinction along with the old country doctor. What are we to conclude about a field claiming to help people, and saying that their venerable methods are good enough and that possibly more effective treatments aren't worth the trouble?

Applied areas such as cooking, engineering, or, indeed, behavior control were around in some very effective forms long before there was any science to contribute to them. People built temples, bridges, and roads, made phyllo dough, gumbo, and crème brûlée, manipulated the behavior of others through religious, economic, and political means, all with considerable skill. Science still has little or nothing useful to say about many such activities. There are notable exceptions, however. The *development* of fields like engineering or medicine depended crucially upon advances in the sciences and mathematics. Medicine probably did much more harm than good prior to the twentieth century. Science provided methods, results, and rationales for effective action as opposed to dubious, if not outright harmful recipes, seat-of-the-pants experience, and tradition. Moreover, practice fed back into basic research providing challenging problems that, in turn, advanced both efforts. An important point here is that a field such as medical practice would readily acknowledge its *continuing* debt to basic research in a number of areas. Likewise, those basic efforts would acknowledge the impetus of medical problems to their research. The areas, basic and applied, may be separate—virology, pharmacology, immunology, neurology, molecular biology, internal medicine, etc.—but the interactions and mutual stimulations are well under-

stood. Separateness does not imply that there can be no useful interactions, matings, as it were, to yield interesting hybrids. Indeed, perhaps the most desirable model for scientific/technological advance is that of hybrid vigor.

The development of applied behavior analysis was largely an outgrowth of basic behavior analysis, but somehow divergence has apparently occurred without appropriate interstimulation characteristic of some other scientific/technological endeavors. The literature cited by the author has discussed some, if not all, the important reasons for divergence. I have only emphasized one, namely the relative paucity of principles and methods from the behavior analysis laboratory typically used by applied workers.

Rider might have more carefully addressed the implications of the evolutionary model on training for a future in which the divisions between basic and applied behavior analysis grow ever wider. Assuming that there are sufficient contributions emerging from the laboratory, and not everyone would agree on that point, then the key to maintaining interactive ties must be training—broadly defined—including curriculum design, field and laboratory experience, meetings and workshops, all these things and more held together by some form of accreditation. It is the accreditation, properly structured and administered, that could provide a needed set of contingencies to help maintain functional ties between basic and applied work. It seems obvious that the sort of productive interaction possible in other scientific/technological endeavors could not occur in a context of concerted mutual ignorance.

As the author emphasizes, however, there are those who would say that basic and applied behavior analysis have virtually nothing in common. If so, then why not split up the Association for Behavior Analysis into separate organizations with separate meetings, journals, etc.? Why not also modify the curricula and other aspects of academic training programs so that students don't waste their time on all that basic science or applied stuff they don't need? Why not set

up applied programs equivalent to the California "professional" schools of clinical psychology? What's more, why not do these kinds of things *now*, and avoid all that internecine struggle and nonsense that has made the American Psychological Association look ridiculous? Rider might have resonated more with a behavioral audience by tracing the comparable history of the separation of a group considering itself to represent science in APA to form APS, the American Psychological Society. The clinicians and other professional groups had come to overwhelm their basic science colleagues and ultimately to disenfranchise them. Recent APA conventions reflect the separation; there is little to attract either basic or applied *scientists*.

The APA story also points out that the divergence of basic and applied behavior analysis is not only about scientific or technological turf, but about "professional" status and how to deal effectively with a public that pays the bills for treatment. In this context, comparisons with the divergence of a traditional science and its associated technology may be largely irrelevant.

As Rider has persuasively argued, however, any of the above prescriptions (and descriptions) may also be irrelevant since the issues have been addressed many times before with little apparent effect. But are things really as he, and those who went before, describe them? Most of the references to this topic are a decade or more old. One might have thought that by now we would have bid adieu to each other and gone our separate ways. Such is clearly not the case. Perhaps the most compelling demonstration of this is the annual meeting of the Association for Behavior Analysis. The program is designed to give expression to both basic and applied interests, interesting papers of both kinds are well attended by both parties, and the administrative structure includes representa-

tives of both camps. Our current president is an eminent researcher in basic experimental analysis.

Contrary to Rider's assessment that basic experimental analysis of behavior hasn't changed much over the years, there have occurred many innovations appropriate to relations between basic and applied work. The most significant is a considerable increase in human operant research. Associated with this research is a rapidly growing interest in verbal behavior: rules, acquisition of verbal operants, interactions between the development of relational frames and problem solving, etc. It seems very likely that these kinds of efforts will find expression in applied contexts. On the applied side, there has been significant progress in such areas as instructional technology, for example, precision teaching, that goes beyond any simple application of a principle of reinforcement to encompass and highlight the challenging and highly important problems of skill acquisition.

In conformity with Rider's analogy, a decade ago we could not have predicted these events. The course of scientific progress has always been as unpredictable as the generation of new species, and over a much, much shorter span of concern. But it seems possible that changing directions of basic research, new developments in applied contexts, and more proactive efforts in organizations such as the Association for Behavior Analysis are establishing and strengthening contingencies for survival of functional relations between basic and applied behavior analysis whether we want it or not.

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