Volume
2
Issue Number 4

THE BEHAVIOR ANALYST TODAY

A Context for Science with a Commitment for Behavior Change

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INITIATING AN AFFAIR: HUMAN GEOGRAPHY AND BEHAVIOR ANALYSIS

William Norton

Abstract

Geographers study physical environments, human behavior that changes physical environments, and resulting regionally distinct landscapes. As such, geography faces the challenge of being both a physical and human science, a challenge resulting in an uncertain disciplinary identity. Within human geography there is a significant but erratic history of objectivist analyses, including work in cultural geography and behavioral geography. However, most contemporary human geography rejects objectivist analyses, favoring instead subjectivist ideas related to developments in such areas as cultural studies. There are important links between human geography and psychology, especially concerning environmental and cognitive approaches, but behavior analysis has been either ignored or misunderstood.

It is not unusual for behavior analysts to bemoan the fact their work is sometimes inadequately or unfairly represented by other psychologists, especially in the context of the introductory textbook (Jensen & Burgess, 1997). Writing as a human geographer, I might add that behavior analysis has received minimal attention within the academic discipline of human geography and even such minimal attention has typically misrepresented this approach to the study of human behavior.

Behavior analysts might not be surprised to hear about the lack of interest and characteristic misrepresentation of their work within human geography. However, they might be surprised to hear that human geographers have regularly claimed human behavior as core human geographic subject matter. Thus, human geography has a “long tradition of studying environment and behavior interactions” (Kitchin, Blades, & Golledge, 1997, p. 555), being concerned with "questions of human behavior to the same degree, though not necessarily in the same way, that the other social sciences are” (Ginsburg, 1970, p. 293). According to the *Dictionary of Human Geography*, the discipline is “concerned with the spatial differentiation and organization of human activity and its interrelationships with the physical environment” (Johnston, Gregory, Pratt, & Watts, 2000, p. 353).

If such is the case, behavior analysts might wonder: Why is it we do not know more about this discipline and why has it not made effective use of the concepts and principles of behavior analysis? Human geographers might respond by noting that their discipline has displayed much uncertainty about subject matter and approaches, accompanied by an almost alarming tendency to abandon established approaches at the expense of newer approaches. Behavior analysts might have different responses to these questions that focus on some of the limitations of their work (Hayes, 2001).

The purpose of this paper is to seek to uncover past and present links between human geography and behavior analysis. The paper is organized into three sections. First, the history and goals of geography are summarized. This history introduces the complexity of geography as both a physical (physical geography) and a human (human geography) discipline, a complexity that behavior analysts and other psychologists will readily appreciate. Second, the characteristically tentative and flawed links between human geography and behavior analysis are outlined, with emphasis on the subdisciplines of cultural and behavioral geography and on the current preference for subjectivist rather than objectivist approaches. Third, there is a concluding discussion anticipating the contents of a proposed second paper focusing on the challenges of and prospects for conducting behavior analytic studies in human geography.

INTRODUCING HUMAN GEOGRAPHY TO BEHAVIOR ANALYSTS

“The discipline of geography is difficult to define in a few phrases. Unlike many other scholarly fields, it is not characterized by a discrete subject matter or method or even philosophy” (Gaile & Willmott, 1989, p. xxiv).

Geography has a long academic pedigree with important contributions made by early Greek, Chinese, and Islamic civilizations, and steady growth from the fifteenth century onwards in Europe. Prior to the late nineteenth century, central concerns were with mapping what proved to be an ever-expanding known world and with providing written descriptions of lands and peoples. As such, geography has always been concerned both with the physical world of climate, landforms, soils, and vegetation, and with the human world of population distribution, settlement patterns, agriculture, and industry. The principal common bond between these two interests is a concern with how physical and human characteristics are distributed on the surface of the earth, specifically with how they are located such that there are distinct regions—areas of the earths' surface displaying common physical and/or human landscape features. Indeed, by the late eighteenth century, Immanuel Kant identified geography as the study of regions. A second longstanding bond between the two relates to relationships between physical and human worlds, with emphases on possible physical geographic causes of human landscape features and on human modifications of physical geography. These two bonds continue to be evident in contemporary geography.

There is a penalty to pay for this breadth of interest. By the nineteenth century, when the various academic disciplines were formally delimited and institutionalized within a growing university system, geography had an uncertain status, being a physical and human discipline at a time when these two types of discipline were increasingly separate. Geographers proposed various definitions of their discipline in the late nineteenth century with the two most significant being geography as the study of regions and geography as the study of human and nature relations and related landscape creation.

From the 1920s until the mid-1950s the regional approach dominated. Regions were delimited and described with emphasis on a gradual correction or verification of facts. The alternative landscape approach focused on the evolution of human landscapes emphasizing the impact cultural groups had on the physical landscapes they occupy. In the mid-1950s both regional and landscape approaches were criticized for lacking an explicitly scientific focus. With the key concern being to explain the location of geographic facts, an objectivist approach developed based on a positivist philosophy and with theoretical and quantitative content. This spatial analytic approach was a major concern from the late 1950s until about 1970. Since about 1970, there has been an increasing separation of physical and human geographic interests. Physical geography is allied to other physical sciences, while human geography has experienced a series of re-inventions and transformations that, in accord with trends in social science generally, typically involve a preference for subjectivist rather than objectivist approaches.

Contemporary geographers continue to express frustration and discontent at the uncertain status of the geographic discipline. A few quotes from leading geographers suffice to make this point. Reflecting on a long and distinguished career, Haggett (1990) observed that geography occupies "a very puzzling position within the traditional organization of knowledge .... it is neither a purely natural nor a purely social science" (p. 9). In some cases, geographers argue for a single discipline that integrates physical and human geography. The classic position is that "it is in bridging the gap between physical and human phenomena that geography finds its distinctive role" (Wooldridge & East, 1951, p. 25). A more recent version of this view is that it is "the roles that place and its locational attributes play in natural and human processes occurring on the Earth's surface that are at the heart of geographic inquiry and knowledge" (Gaile & Willmott, 1989, p. xxv). Other geographers question the legitimacy of separate human and physical geographic disciplines. For example, Orme (1985) argued that "geography without a physical base is sociology" (p. 259), while Stoddart (1987) argued that "outside a more general framework physical geography loses its coherence" (p. 330).

But there is a different view. Johnston (1996) stated: "I find the links between physical and human geography tenuous, as those disciplines are currently practised. The major link between them is a sharing of techniques and research procedures, but these are shared with other disciplines too, and are insufficient foundation for a unified discipline" (p. x). Notwithstanding the long history of links between physical and human geography, especially in the area of environmental studies, the position stated by Johnston (1996) is a fair reflection of contemporary North American geography with typically separate textbooks, college courses, and specialist journals for the two interests. Tellingly, in 2001 the leading American geographical publication, the *Annals of the Association of American Geographers,* introduced separate sections for physical (environmental sciences) and human (people, place, and region) geography. This paper acknowledges this fundamental division being concerned only with human geography.

Removing, or perhaps merely ignoring, confusion related to the traditional dualism of geography does not result in a neatly defined discipline of human geography. Contemporary human geography exhibits what some identify as an alarming diversity of subject matter and method. Harvey (1990) noted a "seeming inability or unwillingness to resist fragmentation and ephemerality" (p. 431), while Eyles (2001) worried the discipline was becoming "almost terminally irrelevant" (p. 41). The best explanation for such comments of distress appears to be the post 1970 theoretical uncertainty of human geography with a corresponding lack of focus and direction. The current embracing of cultural studies, postmodernism, and poststructuralism is especially noteworthy in this respect. Eyles (2001) concluded: "I must say the incredibly nuanced theoretical and philosophical debates, the frequent lack of attention to methodological rigor, and the liberal borrowings from other disciplines have left me feeling that geography is largely irrelevant and that the world has passed it by" (pp. 60-61).

BRIEF ENCOUNTERS: HUMAN GEOGRAPHY AND BEHAVIOR ANALYSIS

Behavior analysts might question why they ought to be interested in an academic discipline so uncertain of its' own identity. The answer is that much human geography is indeed concerned with human behavior, specifically behavior contributing to modification of physical landscapes and related creation of human or cultural landscapes. At the risk of oversimplifying, human geographers have used two approaches conceptually similar to the underlying logic of behavior analysis. The first concerns attempts to provide a conceptual basis for the study of human and land relations and the second concerns the behavioral geography that emerged from the spatial analytic interest.

Searching for Causes of Human Behavior: Cultural Geography

Several approaches to the study of human and land relations adopted a perspective that might be considered implicitly behaviorist. Environmental determinism, an influential argument until the mid 1950s, is based on the premise that physical environment controls human behavior. Scholars from the Greeks onwards accepted this view and it was a part of the newly institutionalized discipline of geography in the late nineteenth century. Explaining interest in this approach, Taylor (in Spate, 1952) stated: "as young people we were thrilled with the idea that there was a pattern anywhere, so we were enthusiasts for determinism" (p. 425). Two modifications of this perspective, environmentalism possibilism and environmental probabilism, allow culture to play a role.

The landscape approach advocated by Sauer (1925) is the most influential approach in favor of culture as cause of behavior modifying landscapes with the key argument being that: "Such behavior does not depend on physical stimuli, nor on logical necessity, but on acquired habits, which are its culture. The group at any moment exercises certain options as to conduct which proceed from attitudes and skills which it has learned. An environmental response, therefore, is nothing more than a specific cultural option with regard to the habitat at a particular time" (Sauer, 1941, p. 70). Behavior analysts might be heartened to read this statement and to hear Sauer was the doyen of cultural geographers from the 1920s until about the 1970s. However, only a few practitioners, notably Carter (1968) and Zelinsky (1973), concerned themselves with the conceptual implications of the landscape approach and even then the concern was with links to the superorganic concept of culture from anthropology (Kroeber, 1917) and not with behaviorist concepts from psychology.

Working within this landscape approach, cultural geographers conducted research with behaviorist overtones. For example, there is a considerable body of literature recognizing the role played in human behavior in landscape by what Hudson (1994) labeled, the "authority of tradition" (p. 3), with different ethnic groups behaving differently in similar environmental contexts. More specifically, with reference to American frontier movement east of the Great Plains, Newton (1974) identified an Upland South culture possessing eleven preadaptive traits facilitating successful expansion and related landscape change. In much of the work on ethnic landscapes and preadaptation the concept of rule-governed behavior is implicit.

The first explicit recognition that the landscape approach might be interpreted as adopting a behaviorist position referred negatively to the "behaviorist claim that habit should be construed not as thought but as activity" (Duncan, 1980, pp. 194-195). This critical interpretation of the landscape approach was not rebutted and proved highly influential, contributing to the emergence of alternative approaches to cultural geographic study based on a variety of subjectivist social theoretical and cultural studies ideas.

The Rise of Behavioral Geography(ies)

The positivistically inspired spatial analysis that dominated human geography briefly during the 1960s arose in opposition to the descriptive empiricism of regional geography and to the perceived atheoretical character of the landscape approach. Along with normative theories, models, hypothesis testing, and quantitative methods, spatial analysis incorporated a mechanistic conception of humans derived from economics but also in accord with similar conceptions employed in other social sciences. The initial flowering of behavioral geography was an innovative but uncertain component of spatial analysis.

Some geographers took an interest both in overt behavior and in the role played by human thoughts and knowledge, an interest that was a critical response to the assumption of rational human behavior employed in spatial analysis. Focusing on the world as it is rather than as it ought to be, this was an engagement with developments in cognitive psychology and produced a body of research using such concepts as mental maps, cognition, and perception. Other geographers turned to ecological and environmental psychologies, an engagement prompting publication of a new journal, *Environment and Behavior*, in 1969. Focusing on behavior and the environmental settings in which it occurs, human geographers studied especially the perception of and responses to environmental hazards. Both cognitive and ecological/environmental versions of behavioral geography are outlined in Aitken, Cutter, Foote, and Sell (1989).

There were some suggestions concerning the possible merits of adopting a behaviorist philosophy. Most notably, Golledge (1969) identified the learned basis of behavior and the law of effect, and suggested human geographers pursue the work of such psychologists as Guthrie, Skinner and Estes. In similar vein, Downs (1970) argued for behavioral geography as the science of human behavior and spatial decision making, while Harvey (1969) referred favorably to stimulus-response psychology. These proposals were not well developed at the time and have not lead to a human geography informed by behaviorism and employing the concepts and principles of behavior analysis. However, several areas of research employed ideas sympathetic to behaviorist logic. Two examples are noted.

The push-pull model of migration assumed environmental determinants of movement, specifically identifying negative push factors at the immigrant source area and positive pull factors at the immigrant receiving area (Bogue, 1969). In this model, the behavior of moving is a response to specific environmental stimuli with the intended consequence of improved well-being. More generally, Chapin (1974) developed a model to explain human activities that recognized the role played by motivated behavior aimed to satisfy individual wants through activity in the environment. In both of these examples, the basic concept is operant conditioning, referring to the environment reinforcing behaviors that are most adaptive and effective in achieving reinforcers and avoiding or escaping from aversive stimuli, but in neither case was there explicit integration with the behavior analytic literature.

From about 1970 onwards, behavioral approaches evolved in two different directions. First, humanistic geography moved the behavioral interest further from its' spatial analytic roots. Condemning earlier work for being dehumanizing, this approach centered on humans as active agents, on verstehen, and on participant observation. Second, cognitive approaches were increasingly favored on the grounds that "the pattern of human phenomena on the Earth's surface was best understood by examining the thoughts, knowledge, and decisions that influence the location and distribution of those phenomena" (Kitchin, Blades, & Golledge, 1997, p. 557). This analytic behavioral geography retained the scientific method but rejected components of positivism seen to be unnecessary, such as the claim that a researcher was a passive observer of an objective reality and the claim that facts and values could be separated. The key argument of analytic behavioral geography "is that human beings respond to the environment as it is perceived and interpreted through previous experience and knowledge" (Couclelis & Golledge, 1983, p. 333). This research tradition is detailed in Golledge and Stimson (1997).

The Road Not Taken

A third possible direction for behavioral geography—a road informed by behaviorism—was not followed. Humanistic behavioral geographers had a different agenda while, confusingly, analytic behavioral geographers claimed they were "particularly sensitive to the excesses of the 'operant-conditioning' school of Skinnerian behaviorism" and noted the "more moderate 'stimulus-response' approaches of Watson, Hull, etc" (Couclelis &

Golledge, 1983, p. 338). Reflecting a general hostility towards objectivist approaches, both humanistic and analytic versions of behavioral geography condemned behaviorism without engaging in meaningful debate, often failing to distinguish between the various versions of behaviorism. For example, Pipkin (1979) asserted: "No matter how much we prefer to focus on overt behavior and to eschew mentalistic concepts, we cannot emulate the extreme behaviorist stance, rejecting theoretical structure in general and unobservable variables in particular" (p. 311). Similarly, Gold and Goodey (1984) stated: "behaviorism viewed human behaviour in terms of stimulus-response relationships in which specific responses could be attached to given antecedent conditions" (pp. 544-545). More recently, Pile (1996, p. 36) described behavioral geography as behaviorist and identified both Watsonian and Skinnerian versions of behaviorism as being stimulus and response centered.

Failure to recognize the several different versions of behaviorism meant human geographers viewed behaviorism in overly simplistic terms. In particular, there was no meaningful consideration of radical behaviorism and of behavior analysis. Inevitably, then, human geographers remain unaware of the important changes occurring in behaviorist logic and practice in recent years, especially the convergence of behaviorist and cognitive approaches (Slocum & Butterfield, 1994).

Overall, behavioral geographers failed to engage seriously the work accomplished by behavior analysts. The tendency was to reject any and all behaviorisms without attempting a critical review of psychological literature. This failure is regrettable but unsurprising as, by the 1960s, human geographers were disenchanted with, indeed embarrassed by, the simplistic logic of environmental determinism and, accordingly, most approaches suggestive of environmental control of behavior were viewed unfavorably. Perhaps this failure explains the dismissive comment by Relph (1984): "Since I have never been able to establish just what 'behavioral geography' is and how it distinguishes itself from other sorts of geography, I have assumed it to be a version of B.F. Skinner's behaviorism somehow transferred from psychology to geography" (p. 209).

There is a further explanation for the failure of human geographers to engage seriously the work of behavior analysts, namely invitations were lacking. Behavior analysts conducted research on topics of minimal interest to human geographers, employed specialist vocabulary, and published in specialist journals.

Expanding Horizons

On the basis of the account so far, behavior analysts might question if there are any prospects for a behavior analytic informed human geography. After all, if human geographers proved unable to turn to behavior analysis at the height of the spatial analysis movement it seems unreasonable to suggest they might do so within the context of a contemporary human geography primarily inspired by a body of subjectivist ideas. Indeed, there is little evidence today that human geographers wish to debate seriously the use of any objectivist research procedures. But there are some positive indicators.

There is a growing body of argument favoring naturalism, the view that the social sciences can be studied in the same way as the natural sciences. Most notably, Hutcheon (1996) presented a powerful and detailed argument for an evolutionary naturalism in social science, an argument that included a sympathetic review of radical behaviorism. "We seldom pause to reflect that the premises of naturalism are also the philosophical prerequisites for any behavioural or social discipline attempting to be *scientific* in fact as well as in name" (Hutcheon, 1996, p. viii). Similarly, Kuznar (1997) argued for a scientific anthropology: "when contemporary anthropologists analyze and evaluate accounts they are abandoning the basic tools of scientific analysis—logic and empirical data" (p. ix). In human geography, Entrikin (1991) identified the naturalism in both environmental determinism and the landscape approach noting: "The natural historian offered an attractive model for those seeking to establish the scientific moorings of the study of the areal diversity of culture and human attachment to place" (p. 73).

Importantly, two human geographers recently introduced concepts that are implicitly behaviorist. Appleton (1990) proposed human behavior in landscape be studied with reference to animal behavior, emphasizing biological drives and denying the relevance of human imagination and creativity. Habitat theory is the idea of spontaneous human response to, rather than rational appraisal of, landscapes, with learned patterns of behavior being secondary to inner needs. Prospect-refuge theory is the idea that the ideal environment is one humans can retreat to safely, meaning it is a refuge in which they cannot be seen and also one providing the opportunity to observe surroundings, meaning it serves as a prospect.

Wagner (1996) asked how we might behave more appropriately towards each other and towards the environment. The answer was that we are born to show off, to strive for what is called, Geltung: “human beings are innately programmed to persistently and skillfully cultivate attention, acceptance, respect, esteem, and trust from their fellows” (Wagner, 1996, p. 1). Personal Geltung explains both social relationships and human behavior in environment, for example relating to the need to respect both other people and places, to moderate population growth, and to challenge spatial monopolies of power. The ambitious agenda implied by these ideas has parallels in behavior analysis: “A major role of applied behavior analysts is to help people act in ways that will have long-range benefits for the actors and for humanity” (Malott & Malott, 1991, p. 239).

There is another reason for suggesting the time may be ripe for a rapprochement between human geography and behavior analysis, as behavior analysts are actively seeking to expand their horizons. Most notably, some behavior analysts do not have a wholehearted commitment to a radical behaviorism ignoring cognition. Although there are differences of opinion concerning the extent to which it is necessary for behaviorists to incorporate cognitive concepts in their analyses, some “behavioral psychologists now concede that reference to cognitive mechanisms is necessary to provide explanations of behavioral regularities" (Smith, 1994, p. 215). Other behaviorists contend they analyze cognition under the general rubric of such behavior-analytic concepts as rule-governed behavior and establishing operations. "A distinction was gradually drawn between behavior shaped directly by its consequences and behavior under the control of a rule. It was a distinction that not only breathed new life into the field, it unequivocally linked behavior analytic research and cognitive processes" (Vaughan 1989, p. 98).

CONCLUDING THOUGHTS

As this account suggests, human geographers have barely engaged with behavior analysis. References to Skinner are few and the specific concepts and principles of behavior analysis have not been discussed. In retrospect, it is evident that the positivistically inclined spatial analytic movement of the 1960s was a missed opportunity. The past thirty years are characterized by human geographic excursions into a variety of primarily subjectivist approaches—exceptions to this generalization are the references to naturalism and the habitat, prospect-refuge, and Geltung concepts. Accordingly, at this time it appears there is only limited prospect for a behavior analytically informed human geography.

This limited prospect might be improved if two things happen. Behavior analysts might fruitfully investigate the human geographic literature and begin to think in terms of landscapes as they are related to behavior, specifically the behavior of individuals as members of groups. In such investigations there is a need for behavior analysts to seek to address a wider audience of social scientists through non-specialist journals and through the use of more accessible language.

But the principal onus is on human geographers to apply the concepts and principles of behavior analysis in their studies. A subsequent paper aims to develop this claim through identifying means by which human geographers might begin to apply some concepts and principles of behavior analysis, specifically the concept of rule-governed behavior. The context for a proposed second paper is a focus on group identities, such as national, ethnic, and religious identities, and on the landscapes these groups occupy, value, and change.

Rather than yet another brief encounter, the time is ripe for a full-blooded affair. Both human geography and behavior analysis deserves no less.

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STRATEGIC INTERDISCIPLINARY RELATIONS BETWEEN A NATURAL SCIENCE COMMUNITY AND A PSYCHOLOGY COMMUNITY

PART 1: THE DEVELOPMENT OF THE CONTRASTING PARADIGMS

Lawrence E. Fraley

Abstract

The scientists and scholars of behavior–environment relations, since the inception of their discipline, have debated the matter of how best to organize their discipline and nudge it along an appropriate course of maturation. Two main alternatives have emerged: (a) Infiltrate the already organized discipline of psychology and convert it into a natural science discipline with a properly useful kind of focus on behaviors, (b) Establish an independently organized natural science discipline existing apart from organized psychology. The 300–year development and emergence of the non-behavioral natural sciences have provided relevant history lessons pertinent to the current dilemma. That history suggests that the behavior analysis community has been experimenting for half a century with the less promising alternative.

THE ROYAL SOCIETY

In 1645 civil war raged through England, although as war was conducted in those times, certain classes of people could ignore it sufficiently to go about their business. In keeping with the traditional and prevailing belief that all worthwhile wisdom was derived from the ancients, England's two universities, Oxford and Cambridge, were exclusively devoted to the study of ancient languages and the works of ancient philosophers, religious and secular. Science, founded on observation and experiment, was relatively new and had no place in the curriculum of those universities. At neither Oxford nor Cambridge could one study the works of Galileo, who had founded mechanics, Gilbert, who had founded experimental physics, or Kepler, who had derived the laws of planetary motion (Andrade, 1960).

However, the works of such men of science were admired and studied within an informal community of well read and influential English noblemen, some of whom were churchmen and some of whom were actual practitioners of the new experimental philosophy. Following a decade or more of somewhat regular if informal meetings devoted to a sharing of their common interests in natural philosophy, what two years later would become The Royal Society was formed by 40 men at Gresham College in 1660. It received its Royal Charter, and its name, in 1662. The Royal Society, as it was known thereafter, was for a long period perennially short of funds both to support the scientific activity of its Fellows and to publish results. Nevertheless, it did so to the extent that its limited resources permitted (Andrade, 1960).

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The rise of modern Western science proceeded on various fronts throughout Europe mainly through the work of an often loosely knit community of individuals, but it was The Royal Society that most represented the organizational embodiment of science and functioned as the hub of European scientific activity, in part because it welcomed foreign members. The Royal Society gradually became a kind of clearing house for the advance of scientific activity throughout the European continent.

From the outset, The Royal Society maintained a strict organizational autonomy as its most important organizational resource (Andrade, 1960; Purver, 1967). From the time of the founders, a policy of religious tolerance was strictly observed, yet The Royal Society was noted for the maintenance of its scientific integrity through a deliberate disengagement, or emancipation, of science and philosophy from the coercion of particular religious systems. Even so, the early Royal Society was entirely a product of the religion–dominated culture in which it emerged. The Society reflected a prevailing view among its members that had been elaborated by Francis Bacon (1561-1626).

As Purver (1967) concluded after her scholarly review of Bacon's works: "Bacon …saw science as the notification of a universal language to be learnt by the scientist in the service of God for the benefit of man" (p. 147). From that perspective, the world was God's creation, and science was an extension of religion. Nature was God's product, while scientists were nature's interpreters. Through science, God could be accorded a more informed kind of credit for nature, while humankind would benefit from a more effective exploitation of nature. That was the Baconian view, and the founders of The Royal Society generally espoused that notion. "…The debt to Bacon was overwhelmingly philosophical rather than scientific…Bacon was the great formative influence on the Society's concept of science" (Purver, p. 6).

Thus, the Royal Society emerged in the midst of a culture dominated by general religious mysticism that was expressed through particular disparate systems of religious ideology and practice. With most of its Fellows given to mystical postulates, The Royal Society nevertheless set forth the kind of constitutional provisions necessary to maintain the individual scientific pursuits of its members and also to support their collaborative scientific endeavors. The organizational arrangements in support of scientific activity maintained by the Society tended to keep its Fellows under natural contingencies of involvement with their respective scientific subject matters.

Thus, the Baconian intellectual compromise with mysticism, upon which The Royal Society was founded, allowed the scientific implications of natural philosophy to unfold under predominantly natural contingencies—a process that carried to the emergence and development of the early scientific disciplines. All justified in their incipient stages as the human interpretation of God's creation, and as the means to a more effective utilization of God's provision for humankind. (The belief that God had done it all for humans in the first place, perhaps a gratuitous assumption, had always been incorporated into the predominant Christian religious interpretations of the world).

This kind of intellectual compromise, featuring scientific work conducted according to philosophically mystical justifications, worked well for the emergence of the physical and biological sciences. Nature was analyzed and duly exploited, while the keepers of the mystical sources of truth and wisdom were appeased by the contention on the part of scientists that it was all for the revelation of divine creative techniques—a kind of explication that would putatively lend credence to God's miracles by taking a bit of the mystery out of them. This way humans could better comprehend them and hence more wisely appreciate them—and in the process avoid letting so many of the fruits of those miracles go to waste. After all, God's humanly intuited intention was that people should make use of nature, which was construed as a gift to humankind from God.

Yet one prominent piece of nature—one particularly important domain of natural phenomena—was going neglected. Where was the natural science of *behavior*? Human behavior, especially the verbal behavior that connoted the intellectual superiority of our species and its dominant status in the world, was always of the utmost important to all human affairs—yet a science of that important domain of phenomena, cast in terms of its own level of analysis, had not arisen in parallel with the physical and biological sciences.

The Royal Society and the remainder of the early European science community relied on the Baconian justification for natural philosophy and science, a rationale that seemingly justified both religious tolerance and the potentially intrusive probing of God's works that the aims of the Royal Society portended. That reliance on the Baconian perspective worked well to foster mathematics and the emergence of the physical and biological sciences. However, such a Baconian justification could not work for a natural philosophy and science of behavior that potentially could render redundant the whole foundation of religious mysticism in which the Baconian justification of science was grounded in the first place.

In fact, the prevailing Baconian basis for an observational and experimental natural philosophy intrinsically precluded a logical construing of behavior—especially human behavior—as subject matter for scientific investigation. Behavior was regarded as the manifest will of God or, occasionally, of the will of Satan, his evil counterpart. Presumably, the control of behavior by God or Satan could be exerted directly, or it could be exerted indirectly through agents of God or Satan, who shared a kind of extension office that was established within the body of each behaving person and known as the *mind*. The divine mystical agent on duty there was the soul, although some saw it through a more secular filter as the self—or, if separate, as both. The agents of good and evil, respectively possessed of delegated powers divine or devilish, were thought to share control of the body, and perhaps at times to compete to do so.

Commenting on the non-emergence of behavior science during that period, historian Thomas Hardy Leahey (1997) noted,

> One might have thought that in an age that glorified humans, there would have been an outpouring of psychological studies, but there was not. Authors wrote to exalt humanity, to establish humans' proper place in nature but not to study them. Even the most scientifically oriented of all Renaissance philosophers, Sir Francis Bacon, simply modified the faculty psychologies of the middle ages. (p. 97)

The failure of behavior science to keep pace with the emerging physical and biological sciences can be better understood in light of the prevailing explanatory quandary in which the natural philosophers found themselves. As Leahey (1997) put it:

> Nature philosophy was an ambitious attempt to explain happenings in the world without reference to supernatural beings, but tended to impute to matter the very magical powers previously granted to gods, demigods, angels, and demons. Magnetism, for example, is quite mysterious. Certain metallic substances have the power to attract or repel other metallic substances, and it had been easy to attribute the powers of magnets to enchantment by a wizard wielding supernatural powers. Nature philosophers rejected supernatural explanations of magnetism, however, and said that magnets naturally possess the power to attract and repel metals by themselves, without the intervention of magical powers.
>
> ....Nature Philosophers were scientific to the extent that they sought to explain events by reference only to natural causes, but they failed to spell out mechanisms by which forces such as magnetism worked, leaving them somewhat between science and magic. (pp. 97-98)

Nature philosophers could not give a functional accounting for the behavior of certain metallic substances near magnets, so the power of magnets was attributed to the *intrinsic nature* of magnets. It provided no explanation, but at least it was an implicit way of denying that mystical or occult forces impelled the observed events. It amounted to an early approximation of the current scientific practice of settling for ignorance until a scientific answer can be found (e.g., Skinner, 1953, p. 13).

The human body presents as a big organic machine that exhibits vast measures of behavior, and, as with the magnets that attracted metal items, how bodies manage to produce behavior was as pregnant a question for the nature philosophers as it has been for people of all times. However, the nature philosophers, with their Baconian view of what they were doing, were not in an epistemological position to give bodies and behavior the same intellectual treatment that they gave to such things as magnets, rocks, gases, plants, planets, or projectiles. In fact, the concept of a similar science of behavior remained largely unconditioned, and it did not seriously occur to the nature philosophers to focus on the nature of a human being and its behavior with the same natural perspective from which they viewed the remainder of nature. The mind of Man was too close to God, too akin to God, for such intrusive inquiry. Sorting out the intricacies of some more remote piece of God's created world was one thing. Focusing that sort of analytical inquiry on what was construed to be a soul was quite another.

A SCIENCE OF BEHAVIOR: DAVID HUME AND THE EARLY CONCEPT

The year was 1739. The Royal Society had had its charter for 77 years. By the reckoning of some scholars and historians of those times, about a century had passed since the rudiments of modern science had first been applied to natural events. David Hume, the celebrated English philosopher and historian, had just published his comprehensive treatise on behavior. While Hume never became a Fellow of The Royal Society (P. Byrne, <paul.byrne@royalsoc.ac.uk>, researcher with The Royal Society, personal communication, February 25, 1999), Hume brought to the literature of Western thought a conceptual analysis of human behavior informed by a philosophy of science founded in the tradition of experiment and observation. It was a promising historical moment, for it seemed that early modern science was embarking on a study of behavioral phenomena that might parallel the earlier intellectual triumphs in understanding physical events. Hume was suggesting that the tested and increasingly respected analytical practices of science now be focused on matters of human behavior, to which Hume referred in the language of his day as "moral subjects."

His "attempt to introduce the experimental method of reasoning into moral subjects" (Hume, 1888, original title plate of 1739) was a struggle against his own misconceptions—a struggle that necessarily occurred in the context of the traditional assumptions that his very work was intended to overcome. Hume also had to render his literary products in the medium of language that had evolved to reflect those false assumptions. Nevertheless, the possibility that human behavior might soon yield its mysteries to an effective science inhered in Hume's ideas. Hume's treatise revealed that he was part of the tradition of natural philosophy, a tradition that Hume deemed capable of sustaining the rigor and style for such a scientific inquiry. Here is Hume's own description of his undertaking (Hume, republication of 1888):

> *Tis evident, that all the sciences have a relation, greater or less, to human nature....Even Mathematics, Natural Philosophy, and Natural Religion, are in some measure dependent on the science of Man... 'Tis impossible to tell what changes and improvements we might make in these sciences were we thoroughly acquainted with the extent and force of human understanding, and cou'd explain the nature of the ideas we employ, and of the operations we perform in our reasonings.... If therefore, the sciences of Mathematics, Natural Philosophy, and Natural Religion, have such a dependence on the knowledge of man, what may be expected in the other sciences, whose connexion with human nature is more close and intimate? The sole end of logic is to explain the principles and operations of our reasoning faculty, and the nature of our ideas: morals and criticism regard our tastes and sentiments: and politics consider men as united in society, and dependent on each other. In these four sciences of Logic, Morals, Criticism, and Politics, is comprehended almost every thing, which it can any way import to us to be acquainted with, or which can tend either to the improvement or ornament of the human mind. Here then is the only expedient, from which we can hope for success in our philosophical researches...to march up directly to the capital or center of these sciences, to human nature itself... From this station we may extend our conquests over all those sciences, which more intimately concern human life, and may afterwards proceed at leisure to discover more fully those, which are the objects of pure curiosity. There is no question of importance, whose decision is not compriz'd in the science of man; and there is none, which can be decided with any certainty, before we become acquainted with that science. In pretending therefore to explain the principles of human nature, we in effect propose a compleat system of the sciences, built on a foundation almost entirely new, and the only one upon which they can stand with any security. And as the science of man is the only solid foundation for the other sciences, so the only solid foundation we can give to this science itself must be laid on experience and observation. 'Tis no astonishing reflection to consider, that the application of experimental philosophy to moral subjects should come after that to natural at a distance of about a whole century... (pp. xix-xx)*

But it has been a long time since David Hume. The promising emergence of a science of behavior did not clearly and directly develop with continuity from that origin.

A CONTEMPORARY ASSESSMENT AND PERSPECTIVE

Hume's promising lead was not followed. Among Hume's scientific successors, observation and experiment were not applied to describe behavior and its controlling relations from a new perspective that cast behavior as a purely natural phenomenon. Those scientific practices, when applied to behavioral phenomena, were instead applied primarily to corroborate traditional mystical postulates about human behavior. That has long been accomplished by pursuing a scientific explication of the supposed real-world implications of those mystical assumptions (Leahey, 1997).

After Hume's time, another 150 years were required on that slowly developing track for psychology to emerge organizationally by finally declaring disciplinary independence on a foundation of experimentalism. The new psychologists were progressively more inclined to accept that behavior was much controlled by events in the context in which it occurred, and they relied less on communicated dogma having only tenuous links to the reality of behavior and the events that surround it. The psychologists, in the tradition of The Royal Society, presumed to abandon the latter sort of philosophy as a feature of their revolution.

But that left the early psychologists with only some procedural or methodological rules for objective inquiry plus the enormous task of generating a new functional philosophy of their developing science. Most psychologists, like the traditional philosophers from whom they had distanced themselves, continued to be burdened with the intellectually debilitating assumption that a person is possessed of the supernatural capacity to generate behavior *the nature of which represents more than could have followed functionally from any environmental precursors.* Certain classes of verbal behaviors continued widely to be accepted as prima facie evidence of such mental supersession of natural function. This had been the essence of the famous confirmation of the behavior–generating mental agent (a.k.a. "I," "me," "you," or "they") announced by Descartes in the early 1600s: "Cogito, ergo sum," (Leahey, 1997, p. 113).

Despite the popularization of Decartes's succinct capture of the notion, even then it was hardly a new concept. Most people since antiquity had intuitively accepted that implication. The incipient experimental science of the early psychologists was therefore much in need of philosophical respect for natural causation, because the formality through which references to the soul were dropped in favor of references to a body-directing self amounted to little more than a shift from religious to secular mysticism. Speaking of ***selves*** instead of *souls* does nothing per se to reduce explanatory reliance on mysterious initiative powers when such capacities of the soul continue to be attributed to the newly recognized self–agent.

One school of early psychological thought construed mental events to have a purely physiological basis, Its adherents believed that studies of the brain could and should serve as the foundation science .of both mind and the outwardly exhibited behavior that the mind was presumed to determine (Leahey, 1997). Physiological psychology, however, operated at the wrong level of analysis to support a practical science of behavior, because answers to most of the important questions in a practical behavior science are not derived from references to the variables of the behavior–mediating nervous system. Instead, the nerve–related events are slave functions of environmental events, and it is the behavior–*environment* relations that anchor the practicality of behavior science. Even at its own analytical level, physiology is a natural biology–based discipline that, in its explanations of nerve functions, does not tolerate explanatory reliance on a mystical internal body–driving agent, an allowance for which has always been demanded, or at least tolerated, by enough psychologists to make the accommodation of spirit powers a definitive characteristic of their discipline.

The majority of recruits to psychology have not been predisposed to abandon the cultural reliance on the ancient physical–metaphysical dualism often described as the body-and-soul or mind-and-body distinction. That predisposition to give behavior a fundamentally mystical accounting is one of the of most prevalent of modern superstitions, as has been true since antiquity.

Most of the contemporary psychologists who subscribe to that form of mysticism fashionably refer to the soul as the self (at least on weekdays). However, it is usually clear that the self is presumed to be endowed with the mystical power of agency—the putative capacity to initiate behavior through the pro-active exercise of mental activity. Reliance on such a willful self is, in turn, the basis of the concept of personal responsibility. In that still prevalent school of psychology, mental science is widely practiced and supported, because it is construed as a foundation for what some, in modern times, have called "moral science," a phrase that means, roughly, the study of cultural strictures and the behaviors that respect them. According to that view, the cultural mission of psychology is to explicate, scientifically, the mental faculties, so that the findings can then be exploited in service to whatever moral and ethical agenda is entertained within the psychology community—which traditionally has closely matched the general Judeo-Christian moral agenda that prevails throughout Western culture.

When, in the mid-twentieth century, a strict natural science emerged under the banner of behaviorism, and did so in the midst of a psychology community that was still heavily laced with such mystical views (Skinner, 1938), few people wanted a *natural* science of behavior (see Skinner, 1953). As followers of the natural science perspective arose in their midst, traditional psychologists maintained a public deportment characteristic of modern scientists, and many psychologists exhibited a passing infatuation with natural science principles. Nevertheless, the psychologists remained preoccupied with the mysteries of what was presumed to be a behavior–originating mind, and few professionally established psychologists abandoned that quest to join the small emergent band whose members were committed to a *natural* science of the functional relations in which behavior plays a role (Fraley, 1997).

Modern psychology, primarily in service to a residual and largely immutable basic assumption of the spiritual essence of human beings, has become preoccupied with efforts to reinterpret physiological findings in ways that lend support to the kind of cognitive theories that explain how an agentive mind could function. Increasingly, psychologists have depended heavily on their own peculiar interpretation of the work of neural physiologists to support claims of scientific progress in their own cognitive discipline. Because the production of behavior is mediated by the nervous system, intraneural activity necessarily accompanies any kind of behavior. For those who are predisposed to entertain some form of secularly or religiously inspired spiritual mental agency, physiological correlates of that neural activity are compelling evidence of the proactive mental mechanisms assumed to represent the origins of behavior. Although mentalistic recipes for the manifestation of some behaviors may require raw ingredients in the form of elements of *information* imported from the environment, the behavioral chef is a proactive mental agent whose operations are what presumably is being detected by intraneural physiological measures.

During the 1900s the behavior analysis movement arose within organized psychology, although not as an extension of traditional psychological thought. Its new behavioral philosophy supported a strictly natural science of behavior–environment relations. The emerging behavior analysis movement gained a following largely among the students of B. F. Skinner, especially during the third quarter of the century. In 1974 the behavior analysts met in Chicago to organize the Association for Behavior Analysis. However, while that organization was created apart from the traditional psychological organizations, it was created by people who considered themselves to be psychologists or who uncritically accepted as *psychology* the foundation science underlying their applied work. From the outset, in spite of a minority of members who hoped to create an entirely independent natural science discipline, the Association for Behavior Analysis remained under the political control of those who continued to think of themselves as psychologists. Following Skinner's lead, they were committed to the conversion of psychology to a natural science discipline.

By the end of the twentieth century, the minority of behavior analysts who continued to promote the circumvention of the organized discipline of psychology and who supported the establishment of an independent natural science of behavior found themselves increasingly ostracized from the political mainstream of the behavior analysis movement. The members of that minority continued to insist that a body of superstitious scholars and practitioners cannot be converted to a philosophy of naturalism nor convinced to adopt its derivative science of behavior merely by adducing scientific evidence of the efficacy of the perspective of naturalism. However, within the behavior analysis community, increasingly, members of that minority were accused of advocating an unhealthy political polarization. When the separatists pointed to what they construed to be important differences between the implications of approaching behavior respectively from superstitious and naturalistic perspectives they could find themselves accused of intellectual arrogance. Ad hominem quibbling aside, among the majority faction, the minority position favoring complete disciplinary independence was regarded, in general, as impractical.

Now, after most of a century devoted to the disciplinary experiment called "psychology," during which that organized discipline has suffered a persistent but non-lethal infection of behavioral naturalism, we are in the early stages of a new scientific emergence of an independently organized discipline. This new discipline relies on an explicitly natural philosophy of science and also features a different kind of interface with its ambient culture than is characteristic of behavior analysis.

This new movement is organized under the rubric of behaviorology. The term *behaviorology* denotes the comprehensive independent natural science discipline devoted to the study of the relations between behavior and the measurable events that determine and control behavior in a functional way. Behaviorology encompasses technical, scientific, and philosophical domains. The philosophy of this discipline is called radical behaviorism (*radical* in the sense of fundamental or root), and the basic science is an experimental analysis of behavior. Also found within the discipline of behaviorology are various developing technologies of behavior.

Behaviorology is currently organized around two principal professional organizations. One is the

International Behaviorology Society (IBS), an organization focused on philosophical and scientific issues that has now sponsored more than a dozen annual conventions. The other is The International Behaviorology Institute (TIBI) and its related association, which focuses on education and training opportunities and publishes a general disciplinary magazine entitled *Behaviorology Today.* A comprehensive history of the emergence of the organized discipline of behaviorology has been published (see Ledoux, 2001). The cultural mission of the organized discipline of behaviorology is to establish the study of behavior–environment functional relations among the traditionally organized natural sciences and to provide basic scientific foundations capable of supporting the behavior-related work of practitioners in any applied field or profession.

This new scientific movement brings the natural philosophy of science to the study of behavior. Natural philosophy did not come to inform the study of behavior as an extension that can be traced continuously back through psychology and its philosophy predecessor to Hume, but rather, as an intellectual product that was transferred to the study of behavior from the physical and life sciences—thus continuing a philosophical tradition stretching back through the natural sciences as they evolved from astronomy and mathematics into modern physics, chemistry, and biology (Michael, 1993).

SCIENCE AND SUPERSTITION IN CONTEMPORARY CULTURE

We live in a mystical culture the implications of which penetrate to nearly all aspects of our discipline (e.g., Raloff, 1996). Nearly everyone indulges in at least some explanatory reliance on mystical variables, a habit so widespread that it could be said to characterize the human species. The adjective *mystical* applies to that which purportedly has, or pertains to, a spiritual or ethereal identity not apparent to the senses nor measurable in terms of mass, distance, time, temperature, electric charge, and a few other more esoteric physical dimensions that together are definitive of the physical world. From the traditional mystical perspective, a mystical variable or event can occur in violation of the deterministic postulate insofar as it can arise spontaneously, without functional connection to a natural history of its own. Such an event, following its spontaneous mystical manifestation, can then presumably affect the course of real future events.

By general agreement, the most definitive feature of *our* species is the verbal capacity that connotes its intellectual superiority among species, and people seldom object to a qualitative ranking of advanced *non*-human species, such as chimpanzees or whales, on the basis of their evolving verbal capacities. People are more inhibited about applying a similar qualitative gradation *within* our species, although that extension of the concept follows logically.

People generally accept, as a widely applicable criterion for intellectual maturity, (a) the extent to which an individual exhibits a verbally supplemented control of the environment and (b) the rapidity of an individual's verbally supplemented behavioral adaptation to environmental changes. If those are valid criteria by which to gauge intellectuality, then, as the accomplishments of the natural science disciplines have been implying for the past few centuries, superstition characterizes intellectual immaturity. That is because explanatory reliance on superstitious verbal constructs circumvents the functional analyses that are required for the support of practical intervention technologies. Functional analyses expedite more effective reactions to environmental changes. The practice of natural science, which centers on functional analyses that improve behavioral efficiency and effectiveness, thus affords better support for precisely the kind of behavioral manifestations that are widely accepted as evidence of intellectual maturity.

Many people who regard themselves as natural scientists and who work in natural science fields are also superstitious, but their explanatory recourse to superstition tends to pertain to questions that fall outside of the scope of their own scientific work. Typically, they are superstitious about someone else's subject matter, not their own.

Consider an example. Suppose that we compare the intellectual maturity of two people, both of whom enjoy popular recognition as *natural* scientists. One is an organic chemist who spends a professional lifetime sorting out the kinds of molecular bonding that holds together a certain class of petroleum derivatives. The other natural scientist studies the functional relations between behavior and environment and specializes in equivalence relations of the kind explicated by Sidman (1994).

Suppose the chemist believes that the world was created rather quickly by a God who maintains a certain level of managerial control over that creation and to whom prayerful entreaties can be directed for divine interventions into human affairs. That chemist also believes that the behavior exhibited by a human body is directed by a mystical resident agent to which that chemist refers on some occasions as the "soul" and on other occasions as the "self." That willful behavior–directing agent is presumed to operate mainly from its accommodation within a construct called a *mind,* which that chemist conceptually superimposes on the brain.

In contrast, the scientists whose subject matter is behavior per se cannot logically rely on superstition for explanations about their own behavior–related subject matter. That is because superstitious activity of *any* kind, and with respect to *any* topic, consists of a class of illogical verbal behavior. However, that general kind of dilemma is not faced by natural scientists who work to explain the structural integrity of molecules (or any non–behavioral aspects of nature—even the origins of the universe). Thus, our behavior scientist's answers to somewhat general questions about those kinds of non–behavioral events need not have as much reliability as his or her answers to questions about behavior. Suppose, in this case, that our hypothetical behavior scientist knows relatively little, in the scientific sense, about advanced chemistry or cosmology, and therefore could exhibit any prevailing superstitions pertaining to such matters without directly affecting his or her own behavioral studies. However, instead of succumbing to that tendency, let us suppose that the behavior scientist, who is more likely to recognize the fallacy in *any* superstitious behavior, also resists recourse to superstition with respect to questions from *other* peoples' fields. Our hypothetical scientist of behavior–environment relations instead accepts temporary ignorance in cases of unanswered questions about events studied by other kinds of specialists.

If we were asked to rank the maturity of intellect of those two scientists on the basis of such evidence, would not most of us assign a higher rank in intellectual maturity to the person who resisted superstition, not only in his or her *own* field, but with respect to phenomena in other peoples' fields as well? As natural scientists, would we also not argue that natural science in general is undermined when people who work in one natural science field promote or tolerate mysticism about the subject matter being studied by the natural scientists who work in another field?

Students are best served when their training programs present the relative efficacy of each of the various ways in which people think about the phenomena of concern. If natural science leads to better control over the environment than its superstitious alternatives, then it follows that students in natural science programs should be learning why that is so. They should also be exploring the potential implications of their entertaining superstitious indulgences about events in other natural science fields of study.

However important such training in comparative philosophy and science may be for students in the natural sciences, the capacity of a natural science training program to provide that curricular facet is substantially compromised when it must occur in philosophically heterogeneous departments where the work of some faculty members with whom details of such a curriculum must be negotiated is informed by philosophies rooted in superstition. Faculty members who treat their own subject matter superstitiously seldom consent to a curriculum of comparative philosophy in which superstition in general, and their kind in particular, will suffer invidious comparisons.

Consider the kind of comparative analysis of paradigms that is especially important for students in the natural sciences. People are always under contingencies to control their environments. When a particular event manifests under peoples' notice, explaining or accounting for that event is a step on the path to gaining control over events of that kind. Across the verbal history of the human species, two major approaches to such accounting have emerged.

The more ancient and simplistic approach has been to ascribe events to mystical powers, extrinsic or intrinsic, that are custom crafted to produce the observed event. Why did that lightening stroke occur? A God made it happen, perhaps simply by picking up the bolt and hurling it—or, according to a more sophisticated idea, by *willing* that it happen. A common prevailing assumption has always been that nature obeys the will of a God. Why did George throw his book across the room? The George self–spirit that inhabits his body willed that his body execute the throw, and his body then followed the orders of that ethereal body–directing, mental–dwelling agent. While most people intuitively understand that individuals frequently act under control of their own (often private) verbal behavior, that verbal behavior can seem to be self-generated. That assumption, lingering since antiquity, has been compelling. The need to account for such internal verbal generators typically has been met with explanatory recourse to a kind of mystical hypothetical construct that can serve as such a self–agent. The presence of such a self–agent, in turn has often been explained as an installation effected through an exercise of will by a still more powerful and equally mystical *external* agent (e.g., the infusion into a human body of a spiritual soul by the Judeo–Christian God). In chapter 9 of *Beyond Freedom and Dignity*, B. F. Skinner provided a somewhat detailed address of the agent issue (Skinner, 1971).

The other approach to accounting for an observed event has been to assume that the world and all of its aspects are natural in the sense that all events can be defined in terms of detectable and measurable variables. That assumption is a philosophical aspect. According to the concept of naturalism, events are linked functionally to other events. Any event is determined by the functionally related prior events that have led to it. We proceed to trace events along this temporal track by looking for order (functional relations) relating pairs of events along the sequence of events of concern. We then describe those relations and test the validity of those descriptions by predicting events that will occur on similar occasions in the future. As valid relations are identified on that basis, we then intervene to control the dependent variables by manipulating the independent variables. Our increasing capacity to do those sorts of things in an organized and systematic way is described as the maturing of our sciences (Skinner, 1953).

Why did that lightening stroke occur? An imbalance in localized and opposite static charges had accumulated until the air between those separated concentrations of charge no longer resisted the flow of electrons between them to restore the balance. We recognize that while that is descriptively true and that explanations in such terms are usually adequate for our explanatory purposes, a functional accounting that features a more precise specification of relevant variables awaits the development of better science than is currently available. However, given the above conditions, as described in currently available terms, the lightening stroke then followed inevitably as the only thing that could happen. Why did George throw his book across the room? A target of opportunity presented, and given such a target plus George's history of conditioning, the throw may be said to have been probable. Actually, as determinists are prepared to argue after the fact of the throw, the throw was inevitable (Fraley, 1994, 1997; Skinner, 1953, p. 112).

The comparative test of quality for these two explanatory approaches inheres in the implications of each approach for gaining control of the environment. Over the past few centuries, with the emergence of the natural physical and biological sciences, the functional approach has won that contest decisively. Only with respect to a natural science of *behavior* does the culture continue to maintain significant resistance to natural science. As the saying goes, "in God we may trust, but on science we surely depend." Nevertheless, as the first clause implies, the second clause has not been meant to include a natural science of human behavior–environment relations.

If it is true that better control of the environment follows from functional analyses than follows from indulging in superstitious accountings, then the residual penchant for mystical crutches represents the trailing edge of human intellectual development. The problem is that this trailing edge, so defined, represents the vast majority of living individuals, and that troublesome circumstance limits the strategic options for the organization and development of an independent natural science discipline of behavior.

Obviously, the cultural victory won by natural science in rendering everyone dependent on its products has not meant the abandonment of superstition at the cultural level, in part because natural science is so effective that only a small minority devoted to its mastery and practice was required to effect what has been called the scientific revolution and to continue the pursuit of its implications. While that history has represented a powerful demonstration of the effectiveness of the natural science epistemology by the minority that was capable of representing it, the assumptions underlying the natural sciences remain alien and unwelcome to most people. Even today, the Baconian justification of science still dominates the culture. In some vague way, God, through vast mystical powers, is responsible for all of creation. Science is still widely regarded merely as our way of understanding how that divine product works if not how it came to be—an understanding that allows us to exploit aspects of it to our benefit.

At the same time, a substantial respect for the power, if not the epistemological essence, of natural science has reached to just about everyone through peoples' increasing reliance on its technological products. Not surprisingly, nearly all of the many and diverse verbal communities in which mystical thinking prevails act, in spite of their often rampant mysticism, to co-opt the powerful image of natural science. Mystical communities may affect pretenses to compatibility with natural science and may even employ scientific methods in pursuing activities that they foist as demonstrations of that putative compatibility. This is especially evident with respect to phenomena that the relatively small organized natural science community has not yet managed to draw protectively under the umbrella of its scholarly responsibility—human behavior in particular. A common example is Christian Science (Wilson, 1961) which seems to represent little more than a nominal relation to science. In academic circles we see what are called the *social sciences,* which employ valid scientific methods to pursue the implications of what are often mystical postulates about the nature of human beings and their behavior (e.g., that behavioral activities are presumably directed or influenced by willful self–agents that can then be held responsible for the implications of the behavior that they initiate).

Behavior is so important that no modern population can presume to conduct its business without some kind of dependence on behavior–related sciences, yet people cannot be expected to support or entertain sciences that threaten their most basic postulates. Social science, as it has come to be defined in the practical affairs of the academy, differs from natural science in that mystical postulates are often entertained as bases for scientific inquiries. Minds and selves are simply postulated, and scientific methods are then applied to studies of their nature and implications. In contrast, the kind of functional analyses that characterize the natural sciences, when applied to behavioral phenomena, provide a kind of accounting that leaves progressively less operating room for mystical origins and for the functional autonomy central to the concept of either the willful self or the spiritual soul as tenant manager of the body.

Physics, the natural science of matter and energy, can only push the idea of divine creative activity along an endless course of retreat that now stretches to the extreme of the Big Bang. In contrast, a natural science of behavior affords a much more efficient approach by philosophically and scientifically depreciating the general epistemological basis of superstition through which mystical concepts of omnipotence manifest in the first place. That is, physics can only provide accountings that, with respect to specific matters of scientific concern, leave less and less that seemingly needs to be accomplished through divine intervention. Thus, in the traditional natural science communities, the eventual abandonment of superstition in general must then be left to occur as an intuitive leap that the case–by–case investigations have rendered increasingly compelling.

However, in contrast, a natural science of *behavior–environment relations* is the science of conceptualization in the first place. A natural science of behavior subsumes the science of *verbal* behavior (Skinner, 1957) and hence the science of epistemology. Its attack does not merely put deities out of work; *it conceptually decapitates any god who may be posited to do the work.* While physics can eliminate an explanatory reliance on God on a phenomenon by phenomenon basis, a natural science of human behavior simply undermines superstitious concepts like a deity in the first place. Thereafter, no need remains for an interminable series of phenomenon–specific demonstrations of God's redundancy. An institution like the Roman Catholic Church could survive the loss of its protracted battles with the astronomy of Galileo and the biology of Darwin (Begley, 1998, p. 51). However, such institutions could not in the same way survive a protracted battle with the science of Skinner, because a natural science of behavior includes a science of philosophy (Fraley, 1999) by which theological foundations can be subjected to the same class of analytical scrutiny to which our solar system and our genes have been subjected.

Much less threatening to a predominantly mystical culture is a kind of behavior science that does not question the mystical basic assumptions about the nature of humans and their behavior, especially the concept of the internal body–directing agent, and focuses instead on the mechanisms by which the body produces what are construed to be the behavioral manifestations of such an agent and on the topography and implications of those behaviors. Vast numbers of scholarly inclined people have a well conditioned affinity for sciences of that kind, and they flock in great numbers to the training programs of the traditional social sciences where that approach predominates.

Nothing logical prevents social phenomena from being studied from the natural science perspective, a fact that seems to support the somewhat tentative and sporadic efforts of the Association for Behavior Analysis (ABA) to advertise behavior analysis as a *natural* science of behavior–environment relations that would include social relations. However, ABA has never officially endorsed the establishment of an academic home for a natural science of socio–behavioral phenomena among the natural sciences within universities—a kind of departmental home for a natural science of behavioral and socio–behavioral phenomena that would coexist *competitively* with traditional social science departments located elsewhere on college campuses.

The largely mystical general population, faced with the task of educating its people, supports universities to explore the diverse implications of the basic assumptions that prevail within that population. Within contemporary universities, a few natural science programs are tolerated. Those programs, beneficiaries of a long history because they pertain to subject matters that, from the Baconian perspective, were deemed appropriate for scientific study in the earliest days of modern science, focus primarily on the study of non-behavioral phenomena—for example, the study of energy, the structure of matter, and the evolution and internal functioning of life forms. Some additional programs combine special applications of those more basic natural sciences—for example, applications for the study of cosmological phenomena, the crust of the earth, and classes of problems that define various agricultural, meteorological, engineering, and medical specializations. By now, the progressive functional analyses of the phenomena under study in the traditional natural science fields have pushed mystical accountings so far away from everything deemed relevant and important that today studies in those fields can be conducted without reliance on any mystical postulates. Those traditional natural science disciplines and their offshoots have become attractive fields of study for people who have no personal need to confirm such mystical assumptions.

However, many other persons cannot tolerate their own emotional reactions to the parsimonious functional accountings that characterize the natural sciences or to the temporary state of conserved ignorance, as is so often required in the natural sciences. Such people, including those who still want to study science in order to reveal the relations between the phenomena under study and the mystical postulates that they bring to such studies (Begley, 1998; Woodward, 1998) tend to get only limited satisfaction from work in fields where study is conducted according to natural science. In those fields, the practitioners remain under contingencies to pursue the discovery of functional relations to an extent that leaves no intrinsic role for mystical forces. Persons who harbor fundamental superstitions, if working in a natural science field, more often draw support for their personal superstitious views from events that appear to lie safely beyond the scientific frontier of their own field of study (Begley, 1998). They may also entertain fallacious misinterpretations about the limitations of their own capacities to measure. For example, they may continue to insist incorrectly that the concepts of probability theory and chaos theory pose valid challenges to the deterministic nature of functional causation upon which the natural science are based (Fraley, 1994).

Reliance on superstition is a reaction to one's own lack of effective responding. That persons of renowned intellectual stature (Begley, 1998; Woodward, 1998) often exhibit this illustrates the fact that the human intellect is multifaceted. It reveals that persons whose exercises of parts of that intellect lead to substantial achievements, including the scientific kind, can be intellectually bereft in other facets of their lives.

For instance, if a renowned chemist expresses unsophisticated statements of a superstitious nature pertaining to the crust of the earth, we may be dismayed that such a respected person got through an undergraduate curriculum without a good course in earth science. One may even pass along a geology primer as a gift to that person. We may be puzzled as to why a person of such intellectual repute could not get past such blatant superstition in another field when recourse to superstition is so carefully avoided in that person's own field. Some people may readily excuse such a contradiction simply because geology is not that person's specialty. However, within the natural science community, intellectual maturity requires that one's avoidance of superstition transcend the phenomena that are under one's personal scientific investigation, and community members who fail in that regard suffer a measure of disrespect.

SCIENCE AND SUPERSTITION IN ACADEMIC SETTINGS

Within universities, the simplistic myth may be promulgated that the natural sciences are defined, not by their natural epistemology, but by the subject matters upon which they focus. That allows the mystical majorities that typically dominate university governance to block the expansion of the natural sciences to include the study of behavior–

environment relations. The traditional natural sciences have been confined to studies of "nature" in the common sense of animals, plants, air, water, and rocks—or, more abstractly, matter and energy and their transformations. Some people entertain the simplistic notion that the natural sciences focus on events mostly inhering in the outdoor realm, and certainly not impinging on those important behavior–related aspects of humanity that comprise what they construe to be the subject matters of the *social* sciences.

I recently probed that convenient misconception by attempting to persuade the undergraduate Liberal Studies Program Committee at my university to allow credit in the *natural* science category for students who take my natural science course on behavior–environment relations. It is a comprehensive introductory course on human behavior that adheres strictly to a natural science treatment of the subject matter. Nevertheless, because of its behavior–related subject matter, acknowledgment that it could be a *natural* science course was formally withheld regardless of its strict adherence to an ontological and epistemological naturalism that rendered it alien within the social science departments of the university.

On July 1, 1998 the College of Arts and Sciences at Temple University was abolished as part of an even larger university–wide reorganization. The departments from the dismantled College of Arts and Sciences were divided between two new colleges, the College of Liberal Arts and the College of Science and Technology. The departments of physics, chemistry, biology, and mathematics, among others, went to the College of Science and Technology, while the psychology department went to the College of Liberal Arts (Donald A. Hantula, personal communication, February 21, 1999; Philip N. Hineline, personal communication, February 23, 1999). Issues of political and economic balance among the colleges were necessarily factors in such placement decisions. However, it was possible in terms of disciplinary considerations to distinguish psychology from the pure natural sciences in spite of a behavior analytic faction within the psychology department faculty that lent a measure of ambivalence to its epistemological character. The result was a kind of organizational placement for psychology that would have been much more difficult to justify for physics, chemistry, or biology.

The differences extend beyond philosophical issues. Some important, if seldom discussed, contrasts also pertain to the ethics that are observed in the respective academic operations of social science and natural science units. When faculty members with natural science perspectives on behavior are forced by the decisions of university governmental bodies to operate within social science units, those somewhat isolated philosophical naturalists can find themselves in the midst of a somewhat alien ethical community. Natural science begins with its natural postulates and builds upon those foundations with accumulating descriptions of experimentally verified functional relations that leave no room for alternative epistemological frameworks. The epistemological approach of the natural sciences is one of their most exclusive and definitive characteristics.

The teaching in natural science courses, defined in ways that comport with the prevailing natural epistemology, is not continually interrupted to teach what would be redundant superstitious alternative perspectives on the subject matter. For example, within geology departments, courses in the stratigraphic fundamentals of ground water normally are not interrupted—in the misguided interest of fairness, intellectual diversity, or tolerance—to teach either a foundation unit on divine creationism nor a technological unit on water dowsing.

In the natural science community, recourse to superstition does not represent a worthy intellectual alternative. Intellectual development is construed to follow a track along which the earlier or lower extreme is characterized by recourse to superstition and represents intellectual immaturity. The later or upper extreme is characterized by a philosophy of naturalism and represents intellectual maturity. The value of a community member, and the respect accorded to that individual, is proportional to the degree to which that person's behavior comports with the philosophy of naturalism.

Applied to the history of the culture, the emergence of naturalism represents an important milestone in the intellectual progress of the human species. Applied to the intellectual development of an individual, personal intellectual progress is marked by the capacity to rise above superstition, first with respect to one's immediate practical concerns and then with respect to all matters in general.

However, in the traditional social sciences, the foundation behavior science quite often rests on mystical postulates, and the training curriculum builds through the incorporation of theories that, while generally compatible with those postulates, may or may not incorporate mystical concepts. Thus, the social sciences represent a philosophical milieu that is laced with proliferating explanatory theories—all unfolding in an atmosphere of ambivalence about the place of superstitious postulates that, nevertheless, is characterized by the reality of their predominance.

With the integrity of their philosophical and scientific foundations so insubstantial, the social sciences typically include applied disciplines for which no definitive paradigm can be identified. Therefore, among the social sciences, no valid basis is thought to exist for claims of epistemological righteousness that match those in the natural science community. Thomas Hardy Leahey, a prominent contemporary historian of psychology, concluded about paradigmatic unity in the field that presumably provides the basic science for the social science community: "I now believe that there never has been a paradigm in psychology, and to think so obliterates vital differences between thinkers lumped together in a supposed shared 'paradigm'" (Leahey, 1997, p. xvii). Thus, in social science communities, to teach exclusively any one epistemological perspective is construed to be somewhat unethical, and it is even more unethical to teach exclusively a single set of related theories. In such an academic atmosphere, survey–of–theory courses tend to predominate, and a principal quality measure pertains to the diversity and sometimes to the contradictions among not only the theories *but also among the epistemological frameworks* being included in those surveys.

A natural scientist, organizationally restricted to a social science unit, can then be subjected to whatever pressures represent the enforcement of that prevailing ethical position. In the social science milieu, seemingly legitimate objections may be raised against any course that adheres exclusively to a strict natural science approach.

The philosophical dichotomy in departments where natural scientists of behavior–environment relations must work cooperatively with scholars of mind–body dualism is often mistakenly described in terms of potentially equal worthiness. In academic circles the philosophical differences are typically cast as parallel intellectual developmental trends in a way that comports with an *equal–alternatives* model. That is, the philosophical dichotomy is presented as if progress toward intellectual maturity had, at some earlier point, reached an equi–potential fork in the road at which began two parallel branches, one of which each new student can be encouraged righteously to choose.

Overlooked, or necessarily ignored, is the fact that one of those alternatives leads students toward a career spent pursuing the implications of superstitious postulates. Insofar as the human intellect is the most important and definitive human characteristic and is arguably the most important of an individual's personal resources, the steering of students, by their educators, to a lifetime investment in superstition can be viewed as a kind of crime against humanity, because it represents the deliberate degradation of a person's intellect. From such a perspective, an educator who would *knowingly* do that to a student is a kind of criminal. While superstitious faculty members are in no position to gauge their own superstitious behavior nor to evaluate its implications, that excuse does not apply to faculty members whose convictions represent naturalism.

Natural scientists of behavior–environment relations who adopt a strategy of disciplinary development that disperses them within social science units thereby place themselves under the constraints of the kind of harmful educational ethics that prevail in such units. Those ethics *require* that equal curricular time be allocated to what natural scientists realize is the degradation of the intellect of students through encouraged recourse to superstition. Those compromised natural scientists are also expected to exhibit their personal approval that this be done as a show of respect for such misapplications of the principle of balance in the curriculum. They may have placed themselves there in service to the quixotic notion of overhauling those superstition based units, but what they themselves become and what they must do to students in that futile quest may, in the judgment of history, be inexcusable.

By far the largest number of programs in contemporary universities pertain in some way to human behavior, and their treatment of behavior relies primarily on mystical foundations—especially on the general idea that behavior is largely the product of an indwelling willful self–agent that is *responsible* for exhibited behavior. The conceptual foundation for programs in law, government, business, recreation, social services, and many others thus rely on a tenet of personal responsibility. The basic anchor discipline has long been traditional psychology (or in some cases a simplistic abstraction of it), which to varying degrees of explicitness posits or tolerates a human body driven by a mysterious agent that operates cognitively by way of a pro-active and seminal mind. Some of its putative mystical initiative capacities are delineated in the following quotation:

> *One of the main assumptions of cognitive science is that cognition is a form of computation. The mind is seen as a complex system that receives, stores, transforms, retrieves and transmits information. Information is represented in terms of symbols which are manipulated in terms of formal processes—thus the analogy with computation. (McTear, 1988, p. 13)*

For example, Chomsky (1957, 1965) explained the human capacity to master language in terms of an implicit set of mentally innate rules collectively called a *generative grammar*. He argued that these implicit rules allow persons both to construct sentences that are linguistically acceptable when spoken and to understand such sentences when heard. Such an explanation underrates and largely neglects the capacity of operant conditioning, an explanatory appeal to which affords a robust account of grammar in general and its nuances in particular—all in terms of naturally occurring functions (Skinner, 1957). Furthermore, to posit the physiological mechanisms that would be necessary to support such a theory may overtax the capacity of evolutionary biology. The selection process in biological evolution does not yield body parts that have initiative capacities. Biological evolution produces body parts that have only reactive capacities. In any case, Chomsky's thesis, which is proffered to account for a class of complex verbal behavior, amounts to little more than saying that people do it, because they do it.

As usual with modern cognitive science, such an accounting remains vague on the question of whether at the core of such an explanation is an implicit reliance on a mystical mental agent. Such an agent, if required, would presumably be empowered, perhaps to *generate* the putative rules of language (or at least to *choose* an appropriate rule from an innate menu and *manage* its application). On the other hand, if recourse to the role of a mental agent is not implicit, this explanation may be interpreted to rely exclusively on some concept of natural neural hardwiring. Therefore language, when somehow initiated, emerges with linguistic propriety as an inevitable and automatic consequence of innate neural structure. That, presumably, would leave nothing for an agent to do.

That ambiguity about essential reliance on mysticism exemplifies a trend in contemporary academic circles whereby direct references to an internal agent have dropped from fashion, perhaps because they so blatantly expose explanatory recourse to superstition. Modern cognitive psychologists seldom refer explicitly to such an agent. They merely allude to it. A student is left to infer, or not, the reality of an obscure part of the nervous system that putatively can receive, store, transform, retrieve, and transmit—presumably performing such feats under the coordination and direction of a mystical self–agent. That particular part of the nervous system, called a *brain* in the natural sciences, is often called a *mind* by psychologists—a term that is more conducive to mystical interpretations than is *brain*. In the alternative, the student may assume that these kinds of events occur automatically, perhaps because the structure of the brain is such that those kinds of events occur as inevitable or automatic reactions to certain kinds of stimulation.

Students can bring their preconceived mystical postulates to such studies without fear of direct contradiction. In such a training model, the students reflect a *mystic in–then mystic out* characteristic. At most, students may have to adjust some of their more simplistic superstitions—but that can easily be rationalized as evidence of scholarly intellectual maturation with little damage to the fundamentally mystical foundations that those students continue to entertain. Mystical people, like other types, expect to see an increase in the sophistication of their views, and in behalf of that outcome they gladly pay university tuition.

Psychology emerged a century ago out of various intellectual traditions respectively propitious to both mystical and natural accountings for behavioral phenomena (Leahey, 1997). However, the realities of the recruiting pool determined the character of the maturing discipline. The vast mystical majority in the general population supplied a multitude of recruits who arrived for their studies in psychology with a Baconian perspective on behavior science to which they were prepared to accept only superficial scholarly modifications that did not challenge their superstitious postulates. A number of those students subsequently became faculty members and inherited the training mission, while others became professional practitioners. Thus, the prevailing ratio of mystics to naturalists was carried forward from the general population (probably with a modest shift toward naturalism) to the ranks of students in training, and finally into the professional cadre of the discipline. The mystical majority in psychology simply reflected the mysticism extant in the general population, a condition that continued to characterize psychology as it became better established across the twentieth century.

With its focus on human behavior, which is generally recognized as an essential subject matter, organized psychology grew large. Because students serve as the human resource fuel on which any expanding profession depends, economic pressure to fill seats in the multiplying psychology classrooms intensified. However, relatively few recruits arrived with a naturalistic propensity, mainly because the culture was producing relatively few of them in the first place, and also because those types were often attracted to the already established natural science disciplines where they were more conceptually comfortable.

Among modern cognitive scientists, some of whom prefer no longer to call themselves psychologists, are many whose work has carried far into intricate physiological analyses upon which to base their psychological interpretations of the nervous system. That work has often become so involved with physiological detail that whether the mind in which those people believe is construed at root to be a seat of mystical power or is instead regarded as a manifestation of natural evolution is a distinction that may no longer matter in any important sense. In the attempt to obtain independent confirmation for their elaborate theoretical mental constructs, cognitive theories are brought to corresponding physiological evidence for both confirmation and explication at the physiological level of analysis. One or more courses in physiological psychology are required in many psychology training programs. Having turned to biology, in particular to neural physiology, to which they bring their mentalistic theories, the cognitivists relate the mentalistic events featured in those theories to presumably correlated physiological neural events.

But the elaborate constructs of the cognitive psychologists are neither suggested nor implied by the relations of behavior to the physiology of the nervous system. The impetus and germination of cognitive constructs is largely a product emerging out of a tradition of non-scientific metaphysics that cognitive psychologists now bring to the findings of a legitimate natural science for confirmation, but they are marching down a one-way street. Nothing from the natural science of physiology would likely spawn a reconstruction of cognitive psychological mental constructs. Absent the historically accumulated body of mentalistic cognitive constructs, the cognitive model of a mind would not likely reemerge from the work of neural physiologists or brain scientists.

The neural physiological events that are occurring when a person is behaving provide valid accountings for the respondent or operant *manifestations* of behaviors by organic bodies, but those neural physiological events do not account for the *origins* of those behaviors. For those, we must look to the environment. Even there, we will not find moments of spontaneous origination. Instead we will find only the links in chains of function, the accumulation of which we call the *natural history* of whatever behavioral event is of analytical concern. Nature is exclusively reactive.

Part 2 will analyze in detail the nature of the challenge that behavior analysts confront in their quest to change psychology, explore the feasibility of the alternative, and comparatively assess the economics that inhere in each approach.

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STRATEGIC INTERDISCIPLINARY RELATIONS BETWEEN A NATURAL SCIENCE COMMUNITY AND A PSYCHOLOGY COMMUNITY PART 2: CHANGE VERSUS CIRCUMVENTION

Lawrence E. Fraley

Abstract

The discipline of psychology provides the vast ambient culture with a scientific pursuit of its mystical assumptions about human beings and their behavior. The postulates that serve as the foundation for traditional psychology, like the postulates that inform the work in any discipline, are largely immune to the change effects of scientific evidence. Behavior analysts seek the resources of the psychology establishment, while psychologists, in general, seek to associate with the powerful image of natural science. Many natural scientists would prefer to see psychologists adhere more closely to the postulates of naturalism. However, under neither side's motive—nor any motive—can evidence change the fundamental nature of the other intellectual faction, because the postulates to be changed function as the principles by which the adduced evidence is interpreted in the first place. An integral natural science discipline focused on behavior is possible only if the behavioral community organizes effectively for its development and maintenance, but current arrangements ignore the lessons of history and do not serve that outcome. Students in behavior analysis are entitled to a more accurate revelation of what they confront in the struggle to induce change in psychology and a more substantial defense, if that is possible, of the proposition that they devote their lives to the pursuit of that quest at the expense of the integrity of their own discipline.

REDUCTIONISM VERSUS EXPANSIONISM

Biological evolution always produces minimal solutions to problems, and they are always structural in nature. In that slow selectional process, new physiological structures are produced only as transmogrifications of old ones. Those changes in physiological structure yield the extensions of behavioral capacity that inhere in the new structures. Studies in structural biology, whether focused on the macro–level of organisms or the micro–level of physiology, have a reductionist character insofar as most biological structure is to be understood in terms of its history of simpler or more primitive precursors. An encountered biological phenomenon of apparent complexity is typically to be understood through its analytical reduction to the combination of simpler structures and processes from which the more complex events of current interest were derived biologically through evolutionary processes.

Natural scientists of behavior, operating at their own level of consideration, study the complexity of behavior processes through a similar reductionist approach. Exhibited behavior, in spite of its often seeming complexity, similarly manifests through a limited number of basic behavioral processes that remains conceptually resistant to expansion (although, rarely, that does happen) . Natural scientists of behavior typically approach the investigation of a new, seemingly complex and at first mysterious behavioral phenomenon by looking for the more elemental or basic and perhaps primitive behavior processes that may in some way be combining to produce the new behavioral manifestation of interest.

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A typical example pertains to adjunctive behavior (Falk, 1961, 1971, 1981, 1984, 1986, 1993, 1995; Falk & Tang, 1988; Staddon, 1977). Adjunctive behavior was originally seen as a strange phenomenon that to some investigators implied new underlying behavioral processes, but which has been shown, to the satisfaction of many, to be explicable in terms of elemental and well understood basic processes (Fraley, 2001, Chap. 16). Natural scientists are under contingencies to keep things simple, and in the natural sciences professional status accrues to those who can reduce initial complexity to combinations of simpler processes with minimal recourse to newly proposed basic concepts. The set of concepts evoked in the construction of a valid explanation exhibit a one–to–one correspondence with the features of the event under investigation, but an excessively elaborate conceptual repertoire can result in the attribution of fictitious features to the phenomenon of analytical concern.

For example, a mentalistic science of mind does not build with a set of basic behavior processes, but with accumulating theories the elements of which are typically metaphors (e.g., mental information processing) and hypothetical constructs (e.g., *information*). Many mentalistic accounts start with *information*, to which a variety of complex changes are said to occur. In such intellectual treatments, information may enter the mind already possessed of meaning (as mentalists may define *meaning*), or raw information may subsequently acquire meaning through mental processes. However, insofar as both information and meaning, like the pulling force called *suction*, do not exist, the construction of such theories can easily get off to a fallacious start and remain divorced from reality.

A discipline that builds through the accretion of theories that are based on metaphors and hypothetical constructs affords its scholars opportunities to enhance professional status mainly through the invention of new and more intriguing theories. Scholars are thus under contingencies to invent ever more complex and encompassing theories that may have to gain acceptance through the explicit negation of some previous theories—or perhaps, more commonly, through their neglect. To attract attention to each new round of theories, the theories are often identified by affectedly cute nominal phrases that are coined to imply something new and different.

In fact, it may not be clear whether the emergence of such an idea represents a new theory in the technical sense of that term. It may be, at best, the reorganization of some old ideas into a new configuration or context (I recently watched a psychologist on television discussing the implicitly new *theory* of *"emotional intelligence"*). In any case, cognitive, mentalistic, and emotion psychologists are under professional contingencies to complicate their discipline and to appear as masters of the ensuing complication, which continually dilutes the essential scientific integrity of their own discipline (e.g., Fraley, 1998).

LIMITS ON THE PERSUASIVENESS OF SCIENTIFIC EVIDENCE

Scientific evidence does not readily challenge fundamentally mystical concepts. Rather, evidence derived through scientific methods is subject to interpretation in terms *of* those basic mystical assumptions. In disciplines tolerant of mystical postulates, the careful adherence to scientific method when pursuing their implications lends apparent credence to those postulates, but, under the prevailing epistemology, scientifically produced evidence can do little to challenge those basic assumptions. The capacity of scientific evidence to produce change in verbal behavior can rarely penetrate to the level of the basic assumptions by which scientific evidence is interpreted in the first place. That is not what scientific evidence tends to do, or is usually allowed to do.

Consider the nature of verbal behavior itself, especially the private kind that we call *thought*. Primitive people rather easily recognized the often-beneficial controlling effects of thought on their own environment–effecting behaviors. However, they often lacked the analytical sophistication to notice the controlling effect that the environment was effecting on thought. They certainly failed to recognize that thought per se was merely another kind of behavior, and that thought, in turn, exerted functional control on other behavior, including, in come cases, more thought. They also remained largely oblivious to the articulated history of chained functional relations (i.e., the *conditioning history*) that completely accounted for any given instance of thought. Under those conditions it was easy to assume that thought originated in a proactive agential mind of a mystical nature.

In today's modern world of scientific sophistication, such fallacies surrounding something as simple as private verbal behavior seem associated perhaps with intellectual immaturity and certainly with ignorance. The small subset of people that is capable of teaching the science necessary to analyze and get past the fundamental fallacy of the mystical mind has for some time proffered its intellectual wares in the academic marketplace with little success. The fact that most people in the world, including a substantial fraction of those whose thinking is widely respected, continue stubbornly to indulge in such assumptions implies that those ancient fallacies are being maintained by far more than their intrinsic logic.

Typically, people will have made enormous personal investments on the basis of their fundamental beliefs—investments of various kinds that would be jeopardized were those basic assumptions to be abandoned. In the course of a lifetime much that is of importance comes to rest on such foundations, so those assumptions tend to be preserved in the face of logical challenges. An old kernel of wisdom warns of the difficulty in trying to persuade people to adopt ideas that imply that kind of substantial adverse change. Given the costs implicit in the behavior that would comport with certain newly discovered truths, there are truths that some people cannot afford to know. Such an individual remains stubbornly immune to evidence that supports those new ideas at the expense of old ones.

Consider a typical mentalistic psychologist, perhaps a faculty member in a university. The person may or may not come from a religious background, but given the nature of the general population from which that person was recruited into psychology, the odds strongly favor that kind of personal history. Since childhood the person has probably believed strongly in an omnipotent and creative God. Such a person presumes that a human being is a direct or indirect product of divine creative power. While all life necessarily operates through physiological processes, human life is presumed, nevertheless, to occur as the manifest essence of a spiritual multifaceted self. The spiritual self exercises mystical powers as does God, but, according to most versions of such cultural lore, human beings have been endowed with only a small and much weaker allotment of such powers. That is, God, through the exercise of *divine* will, can effect events on a universal scale, while an agential body–driving self, through the exercise of the more puny *human* will, can compel only the movement of parts of its host body. Those movements are called *behaviors*.

From such a perspective, the world of physical reality may be construed as a superficiality that inheres, as a divine superimposition, on the more important and extensive mystical domain. According to such a view, the physical world has been created merely for the sake of human senses so that we can engage in necessary life functions during the brief period of our entrapment in that physical world. People refer to that period as their *lifetimes*. According to that view, we experience life in the physical world for a relatively brief episode of qualitative testing before we can be certified for any particular eternal status in the far more important and limitless mystical domain. This common and familiar perspective, or elements of it, is entertained within the general population. It reflects one of several related variations, any one of which could characterize an average citizen of our predominantly mystical culture even before such a person turned to psychology as a career choice.

In this hypothetical example, before we let that career commitment occur, we can add some typical cultural embellishments. Let us suppose that the person is a member of a loving and nurturing family consisting of parents, siblings, relatives, and friends. The person's welcome in the bosom of that family is contingent on sharing in its common belief system, which typically would run along the lines discussed above. Eventually, the person may bond with a life partner or spouse. An important criterion in that selection is the compatibility of that individual with respect to those kinds of fundamental assumptions. A partner or spouse would not likely remain comfortable in such an intimate marital relation with a person who did not share and act on the basis of such fundamentals. Children may come to that union, and our hypothetical person indoctrinates those children with that same set of postulates and concepts, having been taught to do so as a proper exercise of parental responsibility. By this time, the individual in question has long been interpreting all new data pertinent to life and behavior in terms of those mystical fundamental assumptions about the nature of human beings and their behaviors.

Now, let us project this hypothetical person into a higher education institution and resume our tracking as this person who, in the role of college student, is selecting a training program. When previewing psychology, nothing about how psychology is presented for consideration by prospective students could be expected to discourage this person from opting for that major. However, engagement in psychological studies would be construed by this person merely as an opportunity to get technical about how the detailed implications of the person's superstitious fundamental beliefs manifest behaviorally given the physiological constraints of animal construction. The person may hope to be of assistance in nudging along the divine plan as a result of better understanding how its behavioral aspects work at the social and biological levels. In that respect, our person would be exhibiting the four hundred year old Baconian view of the purpose of science (Purver, 1967).

The individual then gets one or more degrees in psychology. They could be from any university in the world, because persons with such a perspective are universally welcome in psychology, and, beyond some fine-tuning, the curricula to which they are subjected do little or nothing to force a conceptual retreat from that view. As behavior analysts have long been painfully aware, there is a limited amount of a natural science of behavior–environment relations that their typically small minority is permitted to introduce into the curricula of psychology departments—proffered under the prevailing ethics with which they are constrained to present it. This mounts little challenge to the culturally imparted mentalistic mysticism, whatever its source and particular characteristics. Keller and Schoenfeld (1950) long ago testified to the difficulty of such a teaching task even under more favorable conditions.

Let us suppose that, after graduating with a sufficiently advanced degree, our hypothetical but quite typical person gets a job on a university faculty as a psychology professor. Next comes a period of career development in which the person gains professional and academic status through an immersion in the pursuit of new and potentially popular theories. These usually make sense, or at least take on special importance, only in the context of those same fundamental assumptions about human beings and behavior with which the person began so long ago.

With vast numbers of such mentalistic psychologists working on a wide variety of practical problems—and often doing so under strong natural contingencies that tend to maintain their practical contact with the realities of those problems—it comes as no surprise that some of them will produce highly effective outcomes in spite of both the tenuously relevant science and inauspicious philosophical fundamentals that they may entertain in the abstract (Fraley, 1998). The behavior–controlling capacity of mystical postulates, within limits, can be superseded functionally by the natural contingencies to which people's work subjects them. Epstein (1984, 1985) relied heavily on that principle of natural supersession in arguing for an independent discipline that would be open to persons of *any* philosophical bent. He entertained the problematic assumption that the natural contingencies that inhere in properly focused laboratory and fieldwork would overcome the corrupting effects of superstitious fundamental assumptions. If under those circumstances, important scientific developments occur in an area to which the few available natural scientists have not yet turned their scientific attention, or in an area in which those who have done so have not yet succeeded. Then we are left with mentalists who are postured to tout their competitive successes as evidence of a more effective paradigm—a convenient and often exploited fallacy.

Reviewers who are critical of articles that support disciplinary independence sometimes cite such successes by mentalistic colleagues as a valid reason to let that fallacy stand unchallenged (e.g., Wulfert, 1997). A logical aspect of a strategy of infiltration, which may be undertaken to promote internal changes in a superstitiously mentalistic discipline, is the feigning of respect for the approaches of the hosts into whose disciplinary community the intrusion is being attempted. I too would congratulate cognitive or mentalistic psychologists on their successful and valuable solutions to practical problems. However, if I was referring those accomplishments to a behavioral colleague, my point would be that, if such mentalistic people had a philosophical repertoire that shared in controlling a kind of behavior that natural contingencies could supplement rather than have to overcome, then, potentially, those people could have accomplished even more.

Returning to the extended example of our now well-established cognitive or mentalistic psychology professor, at this point, let us to introduce you, the reader, into this hypothetical scenario. You are cast in the role of a behavior analytic colleague who now comes to that person's attention, because you are adducing scientific evidence of various kinds and weaving it into logical arguments in an effort to persuade that person to change. You know that superstitious people cannot behave in a valid scientific way with respect to any events that evoke elements of their superstitious explanatory repertoire. That is, for example, they cannot react in valid scientific ways to events attributed either to God or to self–agents. Furthermore, when such events constitute parts of whatever subject matter defines the field in which you and they work, you necessarily regard whatever intellectual products those colleagues produce as less reliable than corresponding products derived as implications of a natural philosophy and science. Just as you realize that rain dances do not increase rainfall, you also see the logical transparency in succumbing to such intellectual shortcuts as conjuring mysterious neural deep structures to account for the production of grammatical speech.

Perhaps you prefer that the person stop acting as if behavior originated in a mind either as the explicit or implicit will of a semi-autonomous, body-controlling self-agent. Perhaps you prefer that the person stop acting as if the path to useful behavior technologies must necessarily wind through descriptions of physiological intricacies the details of which are presumably correlated with hypothetical cognitive mechanisms that are of dubious worth in the first place. Let us assume that you would prefer to see your cognitive colleague adopt some basic scientific principles pertaining to the *functional* nature of behavior, especially to the control of behavior by the environment—and then to apply those much more parsimonious fundamentals to analyses and explanations of behavioral events. You would prefer that your mentalistic colleague's behavioral technologies were derived as applied implications of such functional relations, because behavior technologies spawned in that way have proven to be more efficacious than practices born in service to superstitions.

Your strict respect for such functional analyses makes sense only if the world is an entirely natural place in which there can be nothing for mystical powers to do. Your kind of analyses, characteristic of natural science, precludes recourse to the kind of autonomous self upon which your mentalistic colleague has always relied uncritically for behavior–related explanations. Let us further assume that your adduced evidence is compelling. You are prepared to present sound data–based arguments in behalf of an ontological and epistemological approach that is rooted in the postulates of naturalism. You are even prepared to reveal how the postulates of naturalism, unlike their superstitious counterparts, arose in the first place, not as arbitrary contrivances of convenience, but instead as logical inductions from a wealth of practical experience. Our traditional psychologist should be persuaded—or so you are supposed to believe if you have been trained as a loyal soldier of the behavior analysis cause.

To that traditional psychologist, the enormous cost of the conversion that you urge (measured by the aversive consequences of behavior that would comport with a new naturalistic perspective) is sobering. That cost would be extracted not only from that person, but from others about whom that person cares deeply and fondly. Such a conversion would leave the individual conceptually alienated from that person's own extended family and friends, who could never be expected to understand what such a person had become nor to accept it. The person's more immediate family could be cast into turmoil; a marriage could even be wrecked, and relations with children could be strained when the person's behavior no longer comported with the social training that that person had provided for those children.

On the job, the person would no longer be welcome within the circle of professional associates with whom the person had invested years of collegial networking. Furthermore, with this individual's formal training opportunities already long spent, the person's general status as an established professional would be eroded, because the person, who had long been a well trained, skilled, and accomplished *psychological* scholar/researcher, would be a relatively unskilled novice in the new natural science alternative. A respected academic would be cast back to the professional level of a student in need of a comprehensive education in what to that person would be a whole new philosophy and science. And finally, because of having been subjected to respondent emotional conditioning by the mystically tolerant scientific community in which the person has developed professionally, our traditional psychologist is now emotionally self-punished by any personal behavior that disrespects the traditional superstitious beliefs of the psychology community. In common terms, although it may seem logical, the person finds disturbing the conceptual merchandise that you are peddling.

My behavior analytic acquaintances keep insisting to me that the decades long quest to effect such conversions represents the most appropriate way, or at least the necessary way, to effect the emergence of an organized natural science discipline that is focused on behavior–environment relations. Most behavior analysts continue to insist that such a subversive infiltration of superstitious social science disciplines represents the best available course of action. It may seem that they should know better, and, in a sense, perhaps they already do. Nearly everyone will attempt to teach even their children to realize that people who are fundamentally different must circumvent one another rather than wasting their respective resources on strategies of conversion. "Don't talk politics or religion to strangers" is a common admonition, because persuasion is impossible and alienation is likely. Another humorous and well circulated if indelicate folk version of this same cautionary advice goes this way: "Don't try to teach a pig to sing. It can't be done, and it angers the pig."

Traditional psychologists do not want to be converted, and in several important ways they cannot afford such conversions. Behavior analysts (of *all* people) should know that. As members of our culture, behavior analysts certainly are aware of the relevant principles, which are common enough to pass as cultural lore. With respect to the probability of effecting conversions, the reverse is also true. Like a superstitious individual whom a behavioral person may be trying to convert, that behaviorist too, at this late date, probably cannot afford to abandon the commitment to naturalism, and for the same kinds of reasons that the mystical counterpart cannot be converted to naturalism.

Such conversions are not *impossible*. However, for those who have taken a critical look that what is actually involved in effecting them, that kind of change tends to rank low on the feasible alternatives list. Compare the conversion approach with training a new person. The resources required to take a young and intellectually unspoiled person and engineer the production of a new behaviorologist are substantially less than the vast resources that would have to be expended to convert an established traditional psychologist into a competent and productive natural scientist of behavior–environment relations. (See Fraley, 1995, for a parallel discussion about the relative costs of producing exemplary citizens by [a] the conversion and overhaul of long–conditioned felons, or [b] the direct production of new citizens.) The continued pursuit of such a fifty–year–long well failed strategy by behavior analysts implies some of what behavior analysts cannot now afford to know.

TO CHANGE THEORIES AND TO CHANGE POSTULATES

Among the various tactics in the strategy to change psychology, behavior analysts have bolstered the experimental tradition within organized psychology. Many academic behavior analysts who work in psychology departments are associated with the experimental aspects of the business. Over the years the natural scientists, many operating in an experimental capacity, have generated copious amounts of data–based evidence to confirm the practical effectiveness of their own philosophical and scientific perspective. Many of them have professed the hope that such demonstrations would, over time, lessen the prevailing explanatory reliance on hypothetical internal constructs within psychology and promote greater reliance on functional analyses that feature real variables in both experimental and conceptual contexts. The shortfall of that movement has continually perplexed those reformers. Murray Sidman (1986) referred to the reluctance of some psychologists to use the body of knowledge that had accumulated in behavior analysis as a kind of "scientific malpractice" (p. 44).

However, basic assumptions about the nature of a subject matter, accepted uncritically without evidence and often brought from earlier origins to the study of that subject matter, determine to a large extent what is considered to be important in that subject matter. For example, both psychologists and behaviorologists may seek solutions to the same kind of personal and social behavior problems. However, regardless of their address of common problems, we observe an inordinate preoccupation among modern cognitivists with the internal workings of nervous systems, especially brains. To their natural scientist counterparts, explications of intraneural events are clearly not as relevant to personal and social behavior problems as is the delineation of the functional relations between the troublesome behavior and the environmental events that define the context in which that behavior occurs.

Nevertheless, if one believes that one is constructing explications of how a self works—perhaps from the Baconian perspective of understanding God's creative miracle, or perhaps from the perspective of untangling the mysteries of a willful if more secular self—then it makes sense to focus on the workings of the mind. In the context of such beliefs, that apparent center of agential function remains the most important and fascinating aspect. The influences of the environment on behavior, even when pervasive, can hardly become as important as the workings of the awesome and wonderful machine that is mistakenly thought to operate initiatively on that environment. If one believes that input from the environment is only providing targets for behavioral responses that have to be originated spontaneously through willful exercises of an autonomous behavior–generating mind, then that mind and its mystical operations will seem to be of central importance. However, if finally people who entertain that view were intellectually to breach that fallacy, far more than that the construct of mind could crumble. By the same logic, so could other similar mystical constructs in which those people often have even greater personal investments. If behavior is not an expression of a mental body–governing mini–God, then a question is raised about whether other kinds of phenomena require the ministrations of a big God in the sky.

In a community of natural scientists, persuasion is accomplished by way of scientific evidence, but that process of change works better on theories than on postulates. Consider an example that illustrates their relative susceptibility to change: People long ago started looking into space, and the sweeping trajectories of most everything observed there seemed to support the notion of a central earth around which everything else revolved. That theory comported with more than the limited and casual observational evidence then available: It was also compatible with the postulate that the human species and its world represented the special and probably unique creation of an omnipotent God who presumably would have bestowed centrality on such a masterpiece. Aside from the fact that observational evidence seemed to confirm it, the divine positioning of such a special creation at the center of the universe seemed logical. The slow but steady accumulation of contradictory evidence adduced by science gradually compelled the abandonment of the central earth theory, even by most members of the majority that espoused the mystical postulates. However, for most such believers, the postulate that the human species and its world were the special creations of an omnipotent God easily survived the abandonment of the supportive though non-essential central earth *theory*.

The capacity of scientific evidence to bring about change in theories does not so easily reach to the level of the basic assumptions, or postulates, by which that evidence is interpreted in the first place. Unlike theories, postulates are largely immune to evidence. That is equally true of the thinking that is exhibited by natural scientists, but in a natural science community the immune postulates are compatible with the kind of functional analyses that characterize the natural sciences. Consider, for example, the postulate that all physically detectable events have a functional (i.e., natural) history that is defined by a physically detectable and functionally related chain of events. That chain of functionally related events is presumed not to be subject to mystical intervention. The discovery of a seemingly spontaneous event would logically challenge that postulate, but in a natural science discipline, evidence of an apparently spontaneous event is simply not interpreted or accepted as such. Instead, a search is maintained for evidence of what is assumed to be its functional causality however obscure those relations may be. The continuing failure of such a search, regardless of its duration, remains the occasion to conserve an explicitly designated and openly declared state of ignorance until such time as an answer can be generated through methods that do not violate the basic postulate of naturalism. Faith in the reality of a still hidden function is not shaken merely by the failure of attempts to confirm it.

If the question tenaciously resists being answered in that scientific way, the nature of the question per se will be challenged before the postulate of naturalism will be abandoned. A simple example pertains to accounting for the pulling force of suction. Although supported by copious evidence, an analysis of the suction force cannot carry to the discovery of any rational causal mechanism, a dilemma from which we escape by questioning the question until we recognize that the question per se pertains to a fallacy. Thus, the postulates that comprise a natural philosophy for science help to maintain a persistent search for functional relations, which is important because those relations, once discovered and accurately described, are then exploitable in the kind of practical technologies that enhance the prosperity of mankind.

In contrast with a postulate, a theory can change with the accumulation of more and better evidence, because theories tend to be more contextually specific and to be derived from implicitly limited evidence that is interpreted according to the more general and fundamental postulates. On the other hand, a postulate, unlike a theory, does not undergo that same kind of intrinsic alteration on the basis of evidence. Postulates are mostly taught, and a postulate is more subject to change at the cultural or group level than at the individual level. That is, a postulate is more subject to a change in its ratio of believers to non-believers than to its abandonment by a single believer. The mechanism for that kind of change inheres in the respective conditioning of individuals: For example, the ratio of believers to non-believers is shifted when a newly developing individual is conditioned to behave in ways that comport with the postulate in question. An individual may also be conditioned in such a way that subsequent arrangements to teach a given postulate prove unsuccessful with that individual—perhaps because an antithetical postulate has already been taught—a perennial lament of behavior analytic teachers of traditional psychology students (e.g., Keller & Schoenfeld, 1950).

Although basic philosophy at the postulate level is usually just taught and, absent some special pre-conditioning, is accepted uncritically, postulates as well as theories can be induced from evidence. The rare initial manifestation of a postulate normally occurs as a grand induction based on a long history of practical experience. Because, once in place, such basic philosophy subsequently shares in the interpretation of data, data that seemingly contradict a postulate and which may be introduced to challenge it are instead merely interpreted in light *of* the postulate.

Regardless of whose postulates are more worthwhile, the likelihood of contemporary behavior analysts persuading traditional psychologists to abandon postulates tolerant of unnatural causality would seem to be about the same as the likelihood of traditional psychologists persuading contemporary behavior analysts to abandon their allegiance to the postulates of naturalism. The question is not whether it can be done, because at least theoretically it is possible, albeit at great cost. The question is why we should bother with the attempt when history has demonstrated a much more efficacious and expeditious course of disciplinary development.

Although most behavior analysts lend their support to efforts to bring about fundamental changes in the discipline of psychology, the behavior analytic community is poorly organized to effect that kind of much heralded intellectual epiphany on the part of psychologists. Given a person who is already indoctrinated with mystical postulates (whether of religious or secular origin), a coherent scientific community would require exclusive contact with that person over a long period of time before such a grand induction of naturalism would be expected. The improbability of meeting both kinds of requirements—for extensive and exclusively arranged experience—appears to be confirmed by the extreme rarity of such conversions, and especially conversions effected by behavior analysts who disperse themselves thinly throughout the relatively vast and well organized psychology community.

For every traditional psychologist that finally experiences a shift in what is deemed important and relinquishes explanatory reliance on hypothetical internal constructs (mystical or not), the prodigiously reproductive psychology community recruits and trains a legion of new members who are fundamentally mystical. Those new recruits proceed to interpret the subject matter in accordance with the mystical postulates that they brought to their psychology training—and with which they will interpret (or reinterpret) any situation–specific evidence proffered from a nearby behavior analytic ghetto within the local psychology community.

Sectional endnote. Mentalistic psychologists typically entertain notions of an agential mind that acts initiatively in some autonomous or semi–autonomous way to effect what would amount to miraculous interventions. Such putative actions of the self–agent interrupt the natural functional chains of events with the implication of mystical causality—a process often connoted in the phrase *mind–body dualism.* The false reality of such an illogical impossibility may be accepted uncritically as a postulate, perhaps early in life, and become the basis of a enduring superstitious perspective on the nature of human beings and their behavior. That cultural indoctrination with superstition often occurs under the direct or indirect managerial reach of religious agencies, but it can also occur in a secular vein as a general implication of the cultural lore. Whether of religious or secular origin, the superstitious character of the relevant verbal repertoire may go unrecognized by the person who expresses it, although such persons, while oblivious to the superstitious nature of their own postulates, may critically recognize the superstitious nature of the differently originated postulates of others.

Consider a non–religious person who nevertheless has been indoctrinated with the general cultural assumption that the vitality of persons is the manifest expression of their rather autonomous self agents. Upon becoming a psychologist, that person may then address, from that familiar mystical perspective, the same practical behavior–related problem that a behaviorologist may be addressing from a strictly natural science perspective—for example, the resistance to change in fundamental beliefs that is being examined in this article. That psychological author may allude to the religious kind of superstition and attempt to analyze the resilience of any concepts that may be supported by religious postulates. Such a secular psychologist may even relate the intransigence of those beliefs to some asserted biological basis that underlies the putative workings of the miraculously agential mind upon which that psychologist's arguments rely. It usually goes unnoticed by such a secular psychologist that the idea of an agential and somewhat autonomous self in putative control of a body is just a mini–version of the idea of an agential and autonomous god in putative control of a universe. For example, see Lester (2000), which exemplifies points in the earlier sections of this paper about the thinking of typical mentalistic psychologists while at the same time affording an opportunity to contrast the differing treatments of the same topic (viz., the resilience of postulates). Lester's article is cast in terms of the secular mysticism of psychology, while this article reflects the natural science of behaviorology.

ORGANIZED DISCIPLINES: EMERGENCE AND TRENDS

The long quest by behavior analysts to change psychology was begun explicitly by B. F. Skinner in the early part of the twentieth century (Skinner, 1979, p. 38). Throughout that continuing crusade the fact that scientifically adduced evidence has little effect on basic assumptions has gone neglected, and therein lies the strategic error. With very rare exceptions, that is not a kind of effect that scientific evidence can normally produce. That is why, during competition for cultural dominance between an organized natural science community and an organized scientistic community that is grounded in mysticism, a strategy by one to change the other through evidence–based persuasion offers little promise of success, at least across the professional lifetimes of those who are making the attempt.

As an organized natural science evolves from its rudimentary origins toward cultural preeminence, a general strategy of circumvention has proven the more feasible alternative, and that would seem to apply also to a natural science of behavior–environment relations (Fraley, 1997, 1998; Fraley & Ledoux, 2002). The other well established and independently organized natural science disciplines (e.g., physics, chemistry, and biology) have shown the way.

For example, in the seventeenth century the church held a political and social grip on most aspects of the culture through the promulgation of a fundamentally mystical ideology pertinent in various ways to a broad spectrum of human activity. Yet The Royal Society of London (Andrade, 1960; Purver, 1967), when it was organized in the middle of the seventeenth century, did not recommend that its members could best promote the advance of science by calling themselves priests or clergymen and dispersing themselves throughout the various branches of organized religion. Rather than promoting such a dispersion of its membership, or organizing under the formal auspices of the church, the Royal Society organized independently. It became a center of organized science, holding its own meetings, generating its own body of literature, sponsoring scientific ventures, and, conducting its own training activities (which, in those times, consisted mainly of the fellows educating each other through the circulation of their respective works).

In general, the Royal Society organized itself in a way that maintained a degree of intellectual isolation from the much more superstitious remainder of the culture, an organizational scheme that was necessary for the nourishment and development of what has become modern natural science. In the kind of organizational environment that the Royal Society created, its members could come more freely under the natural contingencies of scientific activity and also under the contrived contingencies maintained by the scientific community to promote such activity. Members of the Royal Society focused their studies on aspects of the external environment. However, because a science of behavior–environment relations that could controvert behavior–related superstition had not yet emerged, the Royal Society uncritically projected the Baconian rationalization that its activities were undertaken to broaden the human understanding of God's creative miracles. Importantly, the Royal Society did not expend its energies on the folly of a strategic infiltration of the organized religious establishment in some misguided effort to divert religious practitioners away from their superstitious ways.

In contemporary culture, with the intrusive control of the church weaker and less pervasive than in the seventeenth century, a somewhat similarly influential role is now played in a more secular vein by the organized discipline of traditional psychology. In response to the extensive cultural establishment of the psychology enterprise, for half a century the majority voice within the Association for Behavior Analysis has endorsed the strategy of its members infiltrating organized psychology. This is an attempt to gain control of the cultural mission of organized psychology while subsisting on the resources of the organized psychology discipline. However, that approach runs counter to a long and compelling historical tradition that demonstrates how, most successfully, to develop a natural science discipline in the midst of a philosophically alien culture.

The endorsement of that approach by an organization that, at least in some contexts, purports to represent the *natural* sciences, is prima facie, a logical contradiction. That may, in part, be explained by the fact that the Association for Behavior Analysis has many members who reflect the Baconian view of why science is practiced. Dr. Richard Malott, of the psychology department at Western Michigan University, has shared with interested colleagues his data from a survey that he conducted at the 1994 convention of the Association for Behavior Analysis, an organization that formally defines behavior analysis as a *natural* science. According to those data, 3l% of members polled said “yes” when asked “Do you believe in God?” Among that 31% who said that they believe in God, 80% indicated without explanation that, in their view, science and religion are not in conflict as explanatory systems.

Unfortunately, whenever the variables that define whatever God is supposed to do become the subject matter of scientific inquiry, those explanatory systems are in direct and irreconcilable conflict. This is a problem that superstitious yet scientifically oriented individuals typically resolve by reassigning God to a role that keeps God’s miraculous interventions safely remote from the events under scientific study. That remoteness is usually arranged along the dimension of time, and an extreme version of that kind of conceptual disposal correlates God’s intervention with the Big Bang. Proponents argue that, after that creative moment, God stepped back to let the universe according to its own natural processes. Through that conceptual device, everything following that seminal instance of divine intervention operates in a natural way that is suitable for scientific study. However, even this extreme expulsion of God from the arena of scientific inquiry may not help a cosmologist who entertains a superstitious concept of initial divine creation yet whose scientific studies of the origins of the universe carry to the Big Bang.

Most natural science communities, apart from the specifics of their scientific agendas, entertain two noteworthy objectives. One is to foster the continuing development of the natural science epistemological alternative, which those communities represent—that is, to see to the intrinsic improvement of the naturalistic philosophy, so that, on the merit of its definitive qualities, the effectiveness of the natural science epistemology can be maximized. Because philosophy manifests as verbal behavior, the task of tending philosophy is fundamentally a task of contingency management (Fraley, 1999). The main objective of the qualitative enhancement of the *philosophical* product is to improve the quality–control of *scientific* practice, which is the primary function of a philosophy of science.

A second and somewhat incidental objective of the qualitative enhancement of the philosophical product—perhaps in the long run, a second general objective—is to replace the alternatives to natural science within the culture in the belief that superstition does not optimally support effective practices. Technology, behavioral or otherwise, is effective to the degree that control is acquired over the independent variables in the relevant functional relations. However, absent an appropriate quality–controlling philosophy, the discovery of functional relations, their analyses, and the identification of their constituent variables may all be preempted by expedient reliance on mystical explanations. It therefore matters in that important way whether or not mystical postulates share in the control of a person’s practical behavior.

The mysticism that prevails throughout the culture represents the underlying problem for those who would advance a natural science of behavior. That cultural penchant for recourse to superstition has adversely affected all of the organized scientific disciplines to varying degrees. The more global mission of the natural sciences (namely, supplanting the cultural reliance on superstition with more effective functional accountings) is generally directed at the culture at large.

However, the behavior analysts, in planning their contribution to that general mission, face a dual dilemma. First, the behavior analysts must solve the problem of how best to deal with the entanglement of their discipline in the fabric of organized psychology. Second they must solve the problem of what to do about their own carelessly recruited and now sizable mystical minority. Those superstitious behavior analysts can operate only at the superficial level of scientific methodology by engaging in practices that are selectively adopted from the ambient scientific community on the basis of compatibility with their own kind of superstitious assumptions. To address an often neglected but increasingly important distinction, people who respond superstitiously to the variables that define aspects of the subject matter cannot operate as natural scientists of that subject matter. Natural scientists are such precisely because their scientific repertoire is quality controlled by the natural philosophy of science.

The philosophical impact on the culture at large by the traditional natural sciences is of a magnitude not yet equaled by the behavioral community. For example, thanks to the natural physical sciences, explaining the observed behavior of an automobile by insisting that it represents the executed will of an internal automotive spirit is widely regarded as ignorant and perhaps implicit of deficient intellect. Yet, if we replace the inorganic body of the automobile with a biological unit and resort to the same kind of mystical explanation to account for its behavior, vast numbers of people then take that kind of explanation seriously.

However, compared with the alternative of a functional analysis, that mystical explanation of why an *organic* body behaves represents the same kind of immature intellectualism as does the attribution of vitality to automobiles. Thanks to the cultural impact of the natural physical sciences, many people are quick to note that the automobile actually behaves entirely under the control of its environment, a principal aspect of which is its driver. Then, for lack of training in a corresponding natural science of *behavior*, those same people fail completely to notice that the driver, too, behaves under the direct control of its environment. Lifetimes spent in various scientific and sanctimonious affectations cannot elevate the epistemological ranking of that way of thinking. Such reliance on the internal body–driving agent remains an indulgence in superstition. Insofar as sophistication of intellect is the most definitive characteristic of a human being, it follows logically that the indoctrination of a child with any kind of superstition is a contra–human cultural activity. By the same logic, so is the pointless participation in university training programs that further damage the intellect of students by adding layers of sophistication to their superstitious indulgences.

Superstition aside, mechanics and physiologists alike may continue to concern themselves, in a respectably scientific way, with how bodies (whether mechanical or biological) work internally to produce the outward effects that we call their behaviors. Nevertheless, we can consider the relative importance of the subject matter in the relevant classes of scientific investigation: (a) the internal activities within a body when that body is behaving, and (b) the relations of that behavior to the environment in which it occurs. If a body is malfunctioning, the science of its internal operations is relevant and important. On the other hand, if the behavior of a body is ineffectively or inefficiently affecting its external environment, then a science of behavior–environment relations becomes relevant and important.

In the vast socio-cultural arena of human activity, the behavior–environment interactions typically (a) occupy the concerns of more people, (b) pertain to a broader range of practical and important problems and their solutions, and (c) involve more that is of critical importance to human well being. In general, we bother with cars more because of *what they can do* than because of what they are, and that is true of people as well. True, in a given context either kind of scientific concern can be indispensable. However, when a major scientific discipline of behavior is organized to serve the culture, it needs to be focused more on what people do, and why, than on what they are—an assertion verified by even a casual perusing of behavior–related job postings.

Some modern cognitivists, may be drifting closer to natural science through an immersion in neural physiology. However, they remain preoccupied with the wrong level of analysis for the study of the practical behavior–related events that psychology has been organized to address. That misdirection originally derived its importance from mystical postulates handed down to modern psychologists by their predecessors. However, given the kinds of behavior problems that psychologists purportedly seek to address, their continuing preoccupation with behavior–related intraneural activity at the expense of attention to behavior–environment relations leads them away from practical opportunities to intervene effectively. In light of their announced concerns, the relevance of their questions continues to imply a kind of quality–control failure in the focus of their scientific activity—the legacy of basic mystical assumptions about the nature of human beings that have yet to be abandoned.

These observations about where the main scientific thrust should be focused in a practical behavior science does not discount the potential worth to behaviorologists of supplementary findings from physiologically based intraneural studies. While the internal workings of a behavior mediating body may remain somewhat peripheral to the concerns in a discipline of behavior–environment relations, the valid products of *any* neighboring natural science discipline may prove to be of some relevance. Such findings must be imported directly from valid biologically based physiological studies without their having first been embellished with superstitious interpretations imposed by mentalistic psychologists.

ACADEMIC AND PROFESSIONAL IMPLICATIONS

Within our generally mystical culture, vast numbers of people have been conditioned to respect the power of science. Concurrently, they continue to respect a mystical essence that they believe to underlie behavioral phenomena, and they want a "science" by which to study that mysterious source of vitality. They want their universities to sponsor training programs that are cloaked in an image of scientific respectability yet which tolerate their mystical postulates. Within those universities, they prefer academic departments that are devoted to pursuing, with apparently respectable scientific methods, what are presumed to be the practical implications of their own unwavering superstitious assumptions, especially those pertaining to the nature of human beings and their behaviors. Traditional psychology departments have thrived by providing that kind of service for the ambient culture.

Within the community of mentalistic scholars have arisen some modern cognitivists who have immersed themselves in neural physiological studies. Such intellectual forays into the physiology of brain science often garner the kind of respect within the culture that is reserved for the broad field of biology as one of the traditional natural sciences. However, two points are relevant: (a) neural science is not behavior science, and (b) cognitivists who approach physiology from the mystical perspective of traditional psychology have never experienced difficulty in interpreting the physiological activity that is associated with human behavior as if it were evidence of a willful mind at work. Regardless of any infatuation with a physiological approach to studies of the nervous system, psychology departments in universities continue to pursue kinds of curricula that assumptively define the nature of human beings mainly in the broadly appealing and commonly accepted terms of mental self–agents. Physiological findings are then interpreted as explanations of how minds initiatively accomplish their putative tasks such as selecting and putting into motion the behaviors that the body then exhibits.

Psychology departments gain public support by meeting that public need, and they fill their classrooms with student majorities that expect such training—tuition–paying majorities that psychology departments cannot afford to disappoint. Such departments are expected to offer programs that reliably serve the vast ambient population, and the primary cultural contribution of contemporary psychology departments is to lend scientific verisimilitude to the popular notions of mind–body dualism. As the cognitive psychology presented in popular textbooks implies, an ethereal self-entity is taken seriously (e.g., Lefrançois, 1999). Its locus in a mind is taken seriously. Its putative capacity to initiate cognitive functions and to spawn emotions are taken seriously—functions that, most importantly, may be construed, at the pleasure of the student or professor, to bridge the gap between the metaphysical and physical worlds.

Organized psychology is publicly presented as if it represents quality science. It is now deeply entrenched in the cultural niche reserved for the most effective science of human behavior, in part because psychology has little apparent competition. Competition with psychology for cultural dominance in behavior science would arise naturally from the emergence of an independently organized natural science of behavior–environment relations. However, most members of the largest extant organizational expression of the natural perspective on behavior (i.e., the behavior analysis movement) are preoccupied with a seemingly futile and arguably contra–historical strategy to infiltrate organized psychology and effect fundamental changes.

Both the psychological community and the natural behavior science community can be productive, but their products differ in nature. In one of those communities an essential product is actually a *by–product,* namely, an enhanced public credibility for mystical behavior–related postulates. Such a science must manifest in forms that serve and protect the interests of those who have invested in mysticism. The essential product in the other community is simply the capacity to control behavior in accordance with the values that prevail among the controllers. The followers of neither discipline can establish legitimate claim to some intrinsic essential superiority, but with respect to the capacity to gain control over practical behavioral events, those in the natural science camp are rightfully convinced of their advantage.

The two communities, one a natural science community and one a traditional psychology community, both assert the effectiveness of their respective sciences. Both claim to have developed useful behavior–technologies, but like a ball game, regardless of the bluster, one side eventually wins the contest, and how best to play the game remains an important issue. One community proffers an intellectually controlled yet pragmatic approach to long life and prosperity through behavior–related practices for coping effectively with the environment. It exploits the principle that effective technologies of any kind are based on gaining control of the independent variables in the relevant functional relations. The other community, under the same category of promises, conditions people to rejoice in their scientifically bolstered faith in comfortable mystical mental constructs. To do that, functional behavior-controlling relations between environment and behavior must go largely neglected, which abandons the most important path of progress toward practical behavior–controlling technologies. The differences, both in the respective approaches and in their outcomes, are substantial, real, and measurable.

This distinction and its implications have been described in other terms. For instance, in recent years it has often been said that while behavior analysts seek to gain control of behavior and hence to engineer prescribed behavioral outcomes, the psychologists seek to predict and understand behavior. That line of argument, reiterated by Wulfert (1997), traces to Hayes & Wilson (1995). It has been argued, however (Fraley 1998), that such a casting of the distinction could hardly be acceptable to psychologists themselves. This represents little more than a euphemistic way that behaviorists have invented to suggest that mystically based psychological analyses usually lead to less effective behavioral technologies than those derived on the basis of their own epistemological foundations.

Some behavioral colleagues continue to argue that cognitive and other mentalistic foundations deserve respect, because they do occasionally lead to, or support, better practices than those derived from within the natural science paradigm. However, those arguments rely on an invalid implication of the facts. The vast numbers of psychologists can mount far more studies of any phenomenon that are likely to originate among the much thinner ranks of their natural science counterparts, and in many cases critical aspects of those studies are controlled by the natural contingencies that inhere in the investigative situations. Scientific activity, or at least important parts of it, often occurs under natural contingencies that, to some extent, can neutralize the ideological perspective of the investigator (Fraley (1998). That is why water dowsers often find ground water.

What, finally, we may ask, is a "behavioral psychologist"? The enigma of the "behavioral psychologist" is easily resolved: A behavioral psychologist is *not* a psychologist. B. F. Skinner was never a psychologist. He tried to change psychology into a natural science of behavior so that he and his followers might be at home in an epistemologically overhauled community (Skinner, 1979), and to that end he allowed himself to be called a psychologist (see also, Fraley & Ledoux, 2002). Psychology, however, has not become the natural science discipline that the adjective *behavioral* connotes.

Contemporary psychology is contaminated by the intrusion of a modicum of natural science epistemology. Many behavior analysts insist that, in a practical sense, the organized discipline of traditional psychology is politically or scientifically vulnerable to transformation by its small natural science minority. That seems problematic, because psychology, as an academic discipline, exists as the scientistic academic representative of the vast mystically trusting general population. Psychology is widely and generally supported to represent that vast majority at the academic roundtable as the discipline that is expected to lend scientific credence to the mystical assumptions entertained by that majority. Whether a devotee of psychology is pursuing the implications of what is construed to be a vitalizing projection of a small increment of divine agency into a human body, or whether such a self–agent is thought to be of more secular origination, does not matter in this context. Either interpretation serves the point.

However, the sponsoring culture can hardly be expected to tolerate a naturalistic turning of its champion discipline by insidious intruders peddling what, from the predominant cultural perceptive, is an alien ideology. Within universities, psychology departments that promote too much of the philosophy of naturalism (and the kind of behavior science it supports) can expect the imposition of corrective actions to restore a mystically tolerant curriculum (Fraley, 1998, see Editors Note, pp. 94–95).

The natural science community appears to have little valid reason to anticipate a faster change in the fundamental epistemological nature of the organized psychology discipline than that already occurring within the general population. Most observers do see a long slow decrease in superstitious behavior within the general population, usually as measured across centuries. I find it easy to stipulate to that rate of progress on the part of behavior analysts in their efforts to convert psychology into a natural science, because it is already occurring for reasons that transcend their efforts. Some who are critical of disciplinary independence for the natural science of behavior–environment relations and instead insist on changing psychology imply that that may be the only feasible rate at which psychology can be changed, and they seem prepared to live with it. However, to settle for that rate of progress toward cultural adoption of a natural science of behavior–environment relations is to do little more than to snuggle up to the copious resources of organized psychology and wait for the world to abandon superstition (while claiming to be leading the charge).

The behavioral movement has produced only limited change in the textual delineation of traditional psychology as revealed by the contents of typical eclectic general and applied psychology textbooks. The behavioral contributions, often in the five to fifteen percent range, appear more as appendages than substitutions and are seldom compatibly integrated. "Behavioral" material is often set apart under section headings such as "The Behavioral View of (…whatever)…." (For examples in the area of education, which is my own applied field, see the educational psychology textbooks by Biehler & Snowman,1990; Borich & Tombari, 1995; Lefrançois, 1999; and Pressley & McCormick,1995).

Today, the label "behavioral psychologist" denotes little more than membership in isolated pockets of hushed revolutionaries, many of whom, making a virtue of necessity, continue to pursue the well-failed strategy of quiet infiltration—usually *very* quiet. The movement led by Skinner attempted to visit upon the psychology community a science and philosophy that were, and are, incompatible with those of that community. The behaviorists have been unable to effect more change in psychology, because that kind of change requires that the psychologists become something entirely different at the most fundamental level, and usually at much too high a price.

The behavioral pseudo-psychologists remain camp followers. The huge community of organized psychology has dealt with them in the manner that any large community may deal with a troublesome and alien, though occasionally useful, minority. It practices apartheid. It approves of having most behaviorists reside well away from the center of psychology communal activity in remote "homelands" like the Association for Behavior Analysis. It is something akin to the establishment of a *reservation* for Native Americans who, at the time, were deemed too different in cultural fundamentals to function as citizens of the American community at large.

For the smaller number of behavioral persons who, for a variety of reasons, must be kept and tolerated closer to home in the midst of the psychology community, something akin to the segregated school or township has been developed. In the American Psychological Association (APA), they are kept in the anomalous Division 25, the only APA Division based strictly on epistemological difference. This is a way of formally denoting that whatever behaviorists are in the way of science philosophers, they differ in some fundamental way from all others who can lay a more righteous claim to a place in the psychology community.

The behavioral people themselves initiated a number of these features of their own apartheid. Their self–isolation supports the argument that, while they would usurp the "psychologist" label (and thus position themselves to share in the benefits and resources reserved in our culture for the psychology enterprise), the behaviorists know very well that they are different.

What, then, is the relation between organized psychology and its organized natural science alternative? The incompatible assumptions of both groups are minimally subject to alteration on the basis of evidence and are instead the basis for interpreting evidence. Those differing foundations also support behavioral technologies that not only differ formally but portend differing capacities to control behavior-environment relations—the point underlying the *control*–versus–*understand* distinction (Wulfert, 1997). Given such extensive and fundamental differences plus what is at stake culturally, the disciplinary relation between organized psychology and organized natural science is inherently adversarial. Even when members of the respective communities ignore each other, those two groups must complete for whatever cultural resources are allocated to the support of a behavior science for dealing with practical behavior–related issues.

Adversaries may, in certain ways, respect one another. Honor may span the breach, and ethical rules may evolve to govern the fair conduct of contests. But neither the respect nor the honor need manifest as a diminution in the integrity of either side. Neither side can readily erase the adverse implications for its own interests that inhere in the other's intellectual mode. In one way or another, most contests that arise must simply be won, or what is at stake will be lost. Occasionally a stalemate that delays a resolution can outlast the waning urgency of an issue. However, any natural scientific community that, in the long run, is attempting to occupy a legitimate place at the roundtable of the natural sciences will probably have to stand not only well apart from psychology but in clear philosophical demarcation from it.

Focusing on the university, more proper disciplinary relations can be proposed: A university is created for the generation of new behavioral repertoires and to test their efficacy in behalf of the host culture. That testing may be grand and elaborate. Regardless of the field or subject matter, scholars, whether they describe what they are doing in these terms or not, arrange for the evocation of new behaviors. They then put those discovered behavior-controlling relations to various tests, describe those new relations in the literature for collegial critique, and teach what survives to students. Those new behaviors and their controlling relations are exported, in the repertoires of those students, to the host culture at large where they are subjected to practical long–term testing in various applications.

Within the university, alternative repertoires frequently emerge. Ideas may be in conflict, but within a university that is acceptable and predictable. Such differences can vary in kind. Differences of opinion about the merits of conflicting theories merely await concurrence on the basis of more valid data.

However, another class of disagreement—one that rests on incompatible assumptive foundations of the ontological and epistemological kinds—features incompatible basic beliefs. Those disparate postulates portend incompatible implications that remain irreconcilable. Within my own university, a few programs tout the virtues of general scientific epistemology. In contrast, one program teaches explicitly that the uncritical adoption of prescribed faith is a worthwhile intellectual strategy—that man is best served by embracing comforting custom–tailored mystical answers to important but difficult questions. Other programs of that genre, addressing behavioral phenomena, teach to the comprehensive and profound implication that the essence of man resides in what, at root, can only be a mystical personal autonomy. At the same time, other programs teach a diametrically opposed concept of a human being as a natural product of selection processes whose behavior is capacitated by the body structure of the moment and is functionally controlled by the environment.

Within the university, the members of these various sub-communities find themselves in a culturally sponsored contest of ideas, and the implications of the outcomes are important. One perennially relevant issue pertains to the decorum and protocol to be exhibited by persons within the academy—particularly, the code of conduct that the members of differing intellectual factions should follow in those ideological confrontations.

Feigning respect for ideas that one believes to be invalid—and perhaps harmful—is an old strategic ploy, but costs may be attached. No absolute prohibition prevents an extant and well organized discipline from being ill-conceived and from supporting training programs that lead its followers to technologies that are less effective than others. What a program teaches may have ominous implications. It is irresponsible to regard a given discipline, its training programs, and the implications of its subject matter, uncritically at face value, as if it represents nothing more than an inconsequential alternative. I have heard respected faculty members publicly posit the mentalistic approaches to behavior and the natural science alternative as options without implications, as if they afford a choice no more importance than the availability of a green and blue sweater when dressing for a trip to the grocery store. Such professors thereby compromise the conduct of their cultural assignment within the university, sometimes in service to a futile strategy of ideological subversion, and sometimes merely to purchase a reduction in the social tensions of their academic milieu.

That is an unfair and inappropriate thing to do, especially when it is permitted to mislead students. The students are there to confront alternative and conflicting ideas, and the distinctions among the curricular alternatives that they are confronting should be drawn as accurately and as distinctly as possible, always in terms of the evidence. Unfavorable comparisons are in some cases inevitable, but to blur those distinctions and gloss over their implications respects a false propriety. The students are denied the opportunity to learn techniques by which more critically to assess their educational options (a valuable skill that their tuition should have purchased), while their faculty members compromise professional ethics. Academics have been looking one another respectfully in the eye for centuries and comparatively adducing evidence–based cases for their own views at the expense of their counterparts' ideas. The conclusion that that is neither a nice thing nor a proper thing to do represents a kind of self–serving corruption of the academic ideal.

A natural scientist properly respects the essence of the university by recognizing the right of others to be there. This does not mean forgetting that some of them are conceptual adversaries or that a well established discipline can rest on a foundation of invalid assumptions and be doing more harm than good. The requisites of proper university etiquette do not require that a professor acknowledge potential merit in the ideas of others in which little or no promise is evident. The proper etiquette within the institution, beyond a general politeness, requires only that the professors in one faction insist on the job security of those in other factions, along with their own job security. They should continue until the sponsoring culture at large no longer gains worthwhile advantages from the ideas of a given faction.

Members of the general academic community can always be found who do not clearly exhibit such an academic etiquette, nor insist on it, nor defend it. In that failure they share responsibility for the erosion of the kind of ethics that insure the intrinsic capacity of the university to fulfill its cultural mission. The privilege of expulsion ultimately resides with the culture at large that sponsors the university. That privilege is typically exercised through students enrolling elsewhere as job markets for given kinds of noncompetitive specialists evaporate. Threats to job security should not inhere in ideological disagreements among faculty members. Sometimes, a university faction is permitted, unfairly, to protect itself from public exposure of the relative quality of its ideas through hostile political connivance directed against those within the university who think differently about the subject matter. However, the degree to which that occurs measures one kind of internal governmental and administrative failure.

Once the university hires you, promotes you, and grants you tenure, you are supposed to be licensed to bring your behavior under control of your disciplinary subject matter and to let your intellect develop as that approach may propel it. The university, in accordance with its advertised mission, should be obligated to support you in that personal program of maturation. If you evolve intellectually as a natural scientist within the context of a maturing career, the university is getting a result of precisely the kind that it should be constituted to produce. A university is not supposed to be the private political property of its intrinsic epistemological majorities, and preventing such a usurpation requires the strong political voices of its independently organized disciplines—especially, in this case, a discipline representing the natural science of human behavior.

Behavior analysts in general tend to insist that they are promoting a natural science of behavior. However, natural science appears to gain little when behavior analysts proactively help ontologically and epistemologically alien sub-communities run scientific programs that apply scientific methods to pursing the unreliable implications of mystical postulates. The fact that today that is the only kind of job available to many behaviorists simply measures the misdirection in strategy by those who have been responsible for the evolution of their discipline.

One result is that today the voice of an organized natural science of behavior–environment relations does not ring within the university. Instead of organizing that discipline to exhibit integrity within a university, its leaders too often, and with much success, have settled for the disorganization of behavior analysis within universities by endorsing the dispersal of its members among ideologically alien academic communities. That trend is now accelerating in part because there are fewer and fewer full-fledged natural scientists of behavior-environment relations to oppose it. That is because, by now, most of those who assert themselves to be such, were actually trained within those ideologically alien communities in programs that represented substantial curricular compromises. With extremely rare exceptions, a student in a social science academic department cannot encounter a complete training curriculum that would prepare a person to be an effective participant in an integral natural science discipline. The contemporary ranks and files of the behavioral movement are filled with individuals who, because of their compromised training, are only approximations of the natural scientists that some purport to be—approximations that, with each passing generation, are less so. Increasingly, too, that training is provided by "behavioral" faculty members who themselves are incapable of recognizing the difference.

Many natural scientists of behavior may hope to see their discipline contribute appropriately in the kind of grand academic forum of ideas that, ideally, a university represents. However, that can be hastened only by arranging a better organizational pulpit from which to press an explicit comparative analysis of what they and the traditional psychologists have respectively to offer and from which to defend their right to do so. Furthermore, when the behaviorists have moved across the hall into natural science departments of their own, the psychologists are no longer their critical audience. The behaviorists will have circumvented any need to change how psychologists think, and the intractable problem of how to do that will have evaporated. At that point it would matter only that a graduate of the natural science department, when hired to fill some behavior–related position, be more effective than psychological counterparts who may have been available to fill the same position.

In the long-term best interests of the natural behavior science community, as a practical matter, its definitive relations with psychology should be honorably, ethically, and quietly competitive. Those who adhere to a natural science perspective should define themselves in ways that place them outside of psychology, and they should concentrate on developing their independent discipline. The community of natural scientists and scholars of behavior should adopt, as its primary long-term cultural objective, the establishment, within universities, of independent departments of behavior-environment relations.

In such a natural science department some of the students would be prepared for the maintenance and expansion of the philosophical and theoretical framework of the discipline. Other students would be trained in more focused programs that produce professional, behavior-related clinicians. Those departments would also produce persons generally skilled in basic behavior science who could then adapt those basic fundaments to behavioral specializations in a wide variety of non-clinical fields ranging from advertising to zoo management.

The curriculum, beginning with general behaviorology, must build upward through successive prerequisite levels of complexity and specialization. I once counted those sequences of prerequisite courses in the physics major at my university and found that the most advanced course is at level seven with six successive prerequisite courses. A curriculum based on superstitious fundamental builds laterally through the accretion of theories and practices that typically bear relatively little necessary relation to one another. In contrast, a training curriculum in a natural science builds vertically as the relations at each level imply new variations and combinations that define the next level of complexity and sophistication. Those two approaches to curriculum development are not compatible, and they occur only in their respective kinds of departments.

The Association for Behavior Analysis (ABA) should no longer waste its resources on a quixotic quest to change psychology. Instead, ABA should be amassing a fund to endow both chairs and scholarships in new independent academic departments of the natural science of behavior–environment relations whenever and wherever they emerge *apart* from existing psychology departments. The Association for Behavior Analysis should also focus its influence on the issue of the internal structure of universities in ways that promote the creation of natural science departments devoted exclusively to behavior–environment relations.

Such an independent department is a liberating creation. It can offer a natural science training curriculum that is much more comprehensive and of much greater epistemological integrity than anything possible within a typical psychology department (Fraley, 1998). Irene Grote, at the University of Kansas, responded in opposition to my contention that the natural science of behavior–environment relations belongs in its own academic home (Grote, 1997). In doing so she nevertheless referred fondly to her own opportunity to receive masters level training in the *independently organized* behavioral department at the University of North Texas. She included that period of her own training in a special qualitative category that she described as the "best of all possible worlds." When the behavioral training available in the independent department at North Texas University is compared with the compromised approximations available in most psychology departments, it becomes obvious why one would hold in higher regard the training opportunities in an independent department.

THE CRITICAL AUDIENCE

I have attempted here, as in earlier cited works, to reveal how many of the traditional defenses of the strategy to infiltrate and induce change in the discipline of psychology do not withstand a critical analysis. I remain interested in any rebuttals of my views that may be mounted by behavioral colleagues of opposite persuasion. However, behaviorologists like me, who are actively engaged in operating new and independent organizational platforms for this discipline, do not constitute the most critical audience for those presentations. Students and new professionals in the *behavior analysis* movement continue to be drawn into the strategy of infiltrating psychology on the basis of vague implications and propagandistic assurances that such a course is necessary, worthwhile, and feasible. Before any more behavior analytic students are asked to commit their professional lives to a quest that erodes the integrity of their own discipline, they deserve a much more sound presentation making clear why they should do so. That view should be explicated with such logic and rationale that it is not subject to easy rebuttal.

Let us call upon those leaders of our discipline who continue to insist that a natural science of behavior cannot best develop, nor best serve the host culture, from the platform of its own independent natural science identity and organization. Let them explain and justify that position to the new people who are going to inherit our disciplinary mission, however that mission is ultimately to be construed. Meanwhile, many *behavioral* individuals from all stages of professional development, who have some professional life left, remain concerned about how best to expend it in behalf of the natural science of behavior–environment relations. As part of their efforts to keep themselves informed on the issues surrounding the development of their discipline, subscribing to *Behaviorology Today* would be prudent (a magazine published by The International Behaviorology Institute, 9 Farmer Street, Canton, NY 13617–1120; see also http://www.behaviorology.org).

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PERSONALITY, PERSONALITY "THEORY" AND DISSOCIATIVE IDENTITY DISORDER:

WHAT BEHAVIOR ANALYSIS CAN CONTRIBUTE AND CLARIFY

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Abstract

Behavior analytic accounts of Dissociative Identity Disorder, formerly known as Multiple Personality Disorder, are rarely presented in depth. This lack of recognition is due to misunderstanding the applicability of the behavior analytic position on personality, abnormality, and related issues. Arguments are made here that a behavioral analysis of Dissociative Identity Disorder demystifies and clarifies these behaviors. Behavior analysts can communicate to a wider audience by addressing more phenomena of a clinical and popular interest.

In Phelps (2000) an argument was made that behavior analysis has more relevance to personality and especially "multiple personality" than is commonly presented. Some of the arguments of Phelps are reiterated here and expanded upon.

When behavior analytic accounts of personality or abnormal behavior are introduced, the discussion is usually brief, with references to faulty learning, inadvertent conditioning experience or aberrant behavior models. The brevity is to be valued; it shows the behavior analyst's hesitation to speculate in the absence of data as to how a particular behavior was acquired (Thompson & Williams, 1985). Further, behavioral theorists are reluctant to attribute explanatory or causal status to mental or intrapsychic or other variables inherent to the individual as a cause of the individual's behavior (Skinner, 1974). Nevertheless, this hesitation to speculate has led many writers to conclude that since behavior analysts have little to say or they say the same things repeatedly about different behaviors, behavior analytic contributions are irrelevant (Phelps, 2000). On the other hand, psychoanalytic, humanistic and cognitive theorists can also be accused of saying the same things about very different behaviors. A proposal is made here to re-evaluate behavioral accounts of personality and their relation to Multiple Personality Disorder (American Psychiatric Association, 1987), now called Dissociative Identity Disorder or DID (American Psychiatric Association, 1994).

WHAT IS PERSONALITY IN BEHAVIORAL TERMS?

In 1937, Gordon Alport catalogued some 50 definitions of personality. Little has changed except there are now more definitions and theories of personality; most refer to internal or intrapsychic variables that in vaguely defined ways cause a person's behavior but do not refer to personality as being behavior (Hayes, Follette, & Follette, 1995; Pronko, 1988). Conversely, few behavioral theorists have written extensively about or defined the behaviors of personality (Phelps, 2000). Since personality is behavior, other writings are pertinent without specifically addressing personality or granting privileged status to personality. Behavioral theory *is* personality theory. For instance, Skinner (1953) argued that personalities represent "topographical subdivisions of behavior" and that a particular personality was "tied to a particular type of occasion . . . a given discriminative stimulus," (p. 285). Some twenty years later, Skinner echoed his prior position: "a self or personality is at best a repertoire of behavior imparted by an organized set of contingencies." (Skinner, 1974, p. 149). In their extensive treatment of personality and learning, Dollard & Miller (1950) stated that "Human behavior is learned… We also learn fears, guilt, and other socially acquired motivations… factors which are characteristic of normal personality," (p. 25.) Correspondingly, Eysenck (1959) stated his position on personality as being, "personality as the sum total of actual or potential behaviour patterns of the person, as determined by heredity and environment," (as quoted in Chesser, 1976, p. 291). Bijou & Baer (1966) saw personality as the acquisition and effects of contingencies between "social reinforcement for social behavior, under social SDs," (p. 721). In 1984, Harzem interpreted a personality (characteristic) as being "a cluster of functional relations between (1) a set of variables and (2) the already-established behavior patterns of an individual," (p. 391). In his own behavioral system, Staats (1993) gave a

Author's Note: I would like to thank Carl Cheney and Charles Lyons for their helpful comments on an earlier draft of this paper. Request for reprints should be sent to the author at the Dept. of Psychology, South Dakota State University, Brookings, SD 57007 or at Brady_Phelps@sdstate.edu.

definition of personality as, "personality is composed of specifiable, learned behaviors," (p. 10).

Interbehavioral theorists have defined personality as Kantor (1924) wrote, "we cannot consider personality to be anything more than the individual's particular series of reaction systems to specific stimuli," (p. 75). In comparable terms, Pronko (1980) defined personality as "the total series of a given individual's interactions with the relevant stimulus objects," (p. 201). In consonant papers, Keller & Schoenfeld (1950) and Kohlenberg & Tsai (1991) addressed the term "self" much as others above defined personality. Keller & Schoenfeld described the self as "a word that is meant to designate the ability to speak of (be `aware' of) one's own behavior, or the ability to use one's own behavior as the SD for further behavior, verbal or otherwise," (p. 369) and "the `Self,' in short is the person, his body and behavior and characteristic interactions with the environment, taken as the discriminative objects of his own verbal behavior," (p. 369). Kohlenberg & Tsai discussed self from the perspective of the individual, as one who reports self-observations of their specific personality, "the experience of the self lies in specification of the stimuli controlling the verbal response `I'," (p. 128-129). Lastly, Hayes (1984) and Hayes, Kohlenberg, & Melancon (1989) discussed how our verbal environment shapes our behavior into having a sense of self or to experience our environment (seeing, feeling, hearing, etc.) from a distinct perspective of "you" (Phelps, 2000).

Here, these theorists argued that the people composing our social milieu refer to us with the term "you" used in different ways; on some occasions, you is used to refer merely to us as a physical body, as a person may say to us, "I saw you bleeding in the emergency room,"; in other circumstances, our verbal environment shapes our behavior and models for us to see ourselves seeing from our own perspective, i.e., from a perspective of you, where seeing refers to experiencing and interacting with the world (feeling, hearing, moving, etc.). Now consider the following question from different perspectives, "If you lost your arms and legs, would you still be you?" (Hayes, 1984. p. 103). From the perspective of one as mere physical body, or "My body *is* me," the answer would be no. The answer, from the outlook of the individual with a perspective of you is yes; you could still envision yourself seeing yourself as you. That is, our verbal environment teaches us a general tendency to respond to our own observations of our own behavior verbally and give us "a sense of self" or to acquire and have self-knowledge as a result.

The commonalties in these behavioral definitions are obvious. Personality consists of behavior-environment contingencies, being subject to control and modification by the environment. Further, personality or the self cannot be given explanatory or causal status for other behaviors, except as part of a behavioral chain or as discriminative stimuli for further behavior. Instead, the terms personality and self are behaviors in need of explanation and identification of their causal variables, (Skinner, 1974). Finally, each of these definitions points to personality as being highly consistent yet still malleable, within limits imposed by the environment and the individual's heredity. Pronko stated: "everything is in a state of flux; so is personality. An inventory of one's personality would stop only with the death of the individual." (Pronko,1980, p. 201). Our personality repertoires are stable and variable as a function of historical or present environmental events; the concept of any individual having "multiple personalities" is implicit in behavioral definitions of personality, (Kantor, 1924; Skinner, 1953). Skinner (1957, 1989) also discussed different repertoires of personality or self observable either by other individuals or the person so behaving, traceable to environmental contingencies. Although amongst the definitions cited here only Eysenck explicitly acknowledges the role of genetic variables, other behavioral writers do not dismiss hereditary factors as being a distal yet functional variable in determining behavior (Skinner, 1974). Some readers may not agree with including Eysenck as being a behavioral theorist but Eysenck's definition of personality describes personality as behavior.

Other "behavioral" writers that have addressed personality have seemed reluctant to define personality in precise behavioral terms but instead have proposed that personality is a product or output of a complex interaction of internal (but not necessarily genetic) and external variables (Bandura, 1999; Mischel and Shoda, 1999).

In contrast to the behavioral views of personality, the spectrum of conventional "personality theories" approaches the subject matter as though it were something we can only speculate about, as if we were studying exobiology. This loose speculation leads to the multitude of personality theories and a field that hardly seems to have human behavior as its referent. Perhaps someday a new specialty in psychology will emerge as the investigation of "Theories of Personality Theory" to study and perhaps rationalize the proliferation of personality theories.

WHAT IS MULTIPLE PERSONALITY?

The behavior pattern commonly known as Multiple Personality Disorder but now called Dissociative Identity Disorder (American Psychiatric Association, 1994) might be considered only a recent phenomenon. This behavior, however, was described in every DSM system since its inception (American Psychiatric Association, 1952, 1968, 1980, 1987, 1994); in addition, Flournoy (1900) described similar behaviors at the turn of the century. With a liberal interpretation, the self-report of the Biblical demoniac in Mark resembles those of multiple personalities: "my name is legion, for we are many," indicating that these behaviors are possibly ancient.

The diagnostic literature shows the definition of multiple personality as changing markedly over the editions of the Diagnostic and Statistical Manual of Mental Disorders. In the DSM-I, these behaviors were termed Dissociative reaction, (American Psychiatric Association, 1952), which came to be called Hysterical neurosis, dissociative type in the DSM-II (American Psychiatric Association, 1968). In each of these, multiple personality was not viewed as a discrete disorder but was grouped with somnambulism, amnesia, and fugue states. Only in the DSM-III does Multiple Personality Disorder appear as a separate diagnostic category, with a description of this behavior. This disorder's defining features were proposed being "the existence within the individual of two or more distinct personalities, each of which is dominant at a particular time," (American Psychiatric Association, 1980, p. 257). The DSM-IIIR of 1987 gave nearly identical defining features as "the existence within the individual of two or more distinct personalities or personality states," (American Psychiatric Association, 1987, p. 269). The defining features evolved further in the DSM-IV where this behavior pattern came to be termed Dissociative Identity Disorder. Its features became, "the presence of two or more distinct identities or personality states that recurrently take control of behavior," (American Psychiatric Association, 1994, p. 484). Both the 1980 and the 1994 definition bear close resemblance to defining features of individuals with behavioral repertoires referred to as “mediums,” “channelers” and “psychics,” (Hines, 1988; Spanos, 1994). In these latter three categories however, the differential personality repertoires, the differential self-report, and the differential remembering are under the control of more obvious public discriminative antecedents.

The subtle change in the 1994 definition is notable; distinct personalities were no longer seen as existing *within* the person or as a part of the person, but the behaviors displayed different states or conditions. This definition is less organismic or person-centered and more behavioral-environmental in theory than earlier versions. With the reader’s indulgence, the personality is behavior with variations or as "topographical subdivisions of behavior, occasioned by discriminative stimuli and controlled by reinforcement contingencies." A person whose behavior is consonant with this diagnostic label displays a personality showing more variability than that of the "average or normal" individual; the individual lacking one coherent personality displays a personality repertoire which is very divergent, with large variation in the contingencies between antecedents and responses. The antecedents, i.e., people, places, events, etc., of the individual in question occasion more responses of an idiosyncratic nature which are maintained by reinforcement contingencies singular to that individual. Along this approach, one writer took the new definition to mean that the individual displaying these behaviors could no longer be described as having more than one personality. Instead, the person should be viewed as having less than one whole, coherent personality (Sapulsky, 1995). Likewise Kohlenberg & Tsai (1991) reported that such individuals might not have acquired all the characteristics of a stable, singular personality.

HOW COMMON ARE THESE BEHAVIORS?

The frequency of multiple personality has been debated over time. While some descriptions of these behaviors occurred early in the 20th century, but from the 1920s to the early 1970s, there was a marked deficit of cases (Spanos, 1994). Kohlenberg (1973) termed these behaviors as being “relatively rare” as did Caddy (1985); other reports saw it as very numerous in number late in the last century. For instance there were more cases reported from the mid-1970s to the mid-1980s than in the previous two hundred years (Orne, Dinges, & Orne, 1984). Curiously, the substantial increase in reported cases has occurred almost exclusively in North America. This behavior pattern is rarely diagnosed in the United Kingdom, France, and Russia; no case has ever been reported in Japan (Spanos). In North

America, the bulk of reported diagnoses is made by a small minority of professionals. Most professionals go through an entire career and rarely if ever see such behaviors, (Mersky, 1992; Modestin, 1992; Spanos).

The dramatic increase in the reported numbers of cases has been attributed to differing factors. Possibly, cases which were un-diagnosed in previous decades are now being diagnosed due to greater vigilance for these behaviors; it has also been proposed that the label is being applied (and over-diagnosed) to individuals whose behaviors are readily suggestible (American Psychiatric Association, 1994). One can reasonably conclude that the prevalence of Dissociative Identity Disorder was disputed in the late 20th century and still is disputed. Some readers may also dispute the validity of this diagnosis as the DSM-IV, unlike earlier versions of the DSM, does not provide any diagnostic reliability information, (American Psychiatric Association, 1994). Hopefully the up-coming DSM-V will clarify the situation.

WHAT CAUSES THESE BEHAVIORS?

Theories attempting to characterize and explain these behaviors are as diverse as the paradigms that propose them. Psychoanalytical theory argued these behaviors as being motivational, motivated and driven by a defective or inadequate identification with the same-sex parent and the abrupt loss of a substitute model (Horton & Miller, 1972; Sackheim & Devanand, 1991). Bowers et al. (1971) argued that a sense of self-loathing and self-alienation leads to the development of multiple personality. Dissociation has also tentatively been attributed to extreme self-hypnosis (Bliss, 1980, 1984; Hilgard, 1977) and neurological aberrations or epileptic post-ictal activity (Gur, 1982; Schenk & Bear, 1981).

HOW DOES BEHAVIORAL THEORY ACCOUNT FOR THESE BEHAVIORS?

While Skinner (1953) had suggested we all might display multiple personalities, Kohlenberg (1973) first proposed a learning theory account for multiple personality (Phelps, 2000). It can be argued and observed that each of us has differing amounts of variance in our personality repertoires to the point that a common question may arise: "How many personalities do we actually *have*?" The question isn't how many personalities do we *have*, but how many behavioral repertoires are each of us capable of *performing* or *exhibiting*?

Viewing personality this way, it is obvious that we all execute multiple personalities, with differing grades of behavioral excesses and deficits, beyond what is "normal." These behavioral variations are due to our unique histories of differential stimulus control, reinforcement and punishment contingencies and observational learning experiences (Phelps, 2000). That is to say, we may behave very differently in a lecture hall than when in a church, synagogue or mosque. Any individual no doubt behaves very differently when with one's mother than when with friends at a convention. Despite the variability, an observer would still see "It's still Joe" or that there was enough stability or generalization in Joe's personalities across all contexts for Joe to be recognized as the same person.

THE CONTROL OF SELF-REPORT

Conversely, with the behaviors labeled Dissociative Identity Disorder (DID), the variability between behavioral repertoires is very high, possibly so extreme that the repertoires don't compose one consistent personality repertoire (Sapulsky, 1995). The person him- or herself may even report being a different person, complete with a different name or "identity." While the behavioral variability is more extreme here, it is still on a continuum with the average person. We all exhibit several personality repertoires and there are obvious circumstances of threats of extreme punishment or the potential for deprived reinforcements under which any person might claim to be a different person (Sackheim & Devanand, 1991). Among the behaviors correlated with a diagnosis of DID, self-report is less controlled by public, environmental events and more controlled by events which are private to the person giving the self-report (Kohlenberg & Tsai, 1991; Phelps, 2000). The most apparent question is, what type of experiences could account for this extreme behavioral variability, in the self-report of being a different person, with differences in sex, age, race, physical appearance, etc,?

Commonly, these individuals frequently report having suffered drastic neglect or abuse during their childhood (American Psychiatric Association 1994; Murray, 1994). Reports of a history of childhood abuse are no doubt seen as the defining feature of DID in the minds of many clinicians, as individuals with DID-like behaviors may also display post-traumatic symptoms (American Psychiatric Association, 1994). These reports don't enlighten much since child abuse and neglect sadly isn't rare but the prevalence of these behaviors, while in dispute

(American Psychiatric Association, 1994) isn't nearly as common as abuse. Some of these remembered reports of abuse have been considered suspect since the individuals exhibiting these behaviors give highly variable self-reports of their histories. It has also been argued that some of these reports of abuse may have been suggested and prompted by overzealous therapists (Spanos, 1994).

In relating variations in self-observations and self-reports to the consequences delivered by others, the behavior analyst sees a straightforward connection and interaction. Much self-observation and resultant self-report comes from experiences with, observations of, and inquiries from others (Skinner, 1974). Conceptually, a person with behavior so labeled has had learning experiences that resulted in extreme behavioral variance as well as self-reports of their behaviors. The behavioral variances aren't as clearly related to obvious public events, however, as they are in the person who does not exhibit the behaviors labeled as being DID (Kohlenberg & Tsai, 1991).

Kohlenberg & Tsai argued that any individual has the experience of "being someone else," typically as part of a child's imaginary play and these behaviors can be occasioned and reinforced and or punished by the social environment. Having different aspects of one's self or "being someone else," accompanied by different subjective states of remembering and emotion, because of so behaving, can become a very adaptive behavior under some specific circumstances. When experiencing repeated physical or emotional punishment, being somebody else could provide means of escape or avoidance when no other means of escape or avoidance is attainable (Kohlenberg & Tsai, 1991). The child cannot be unaware of the horrible happenings, but the child can come to be unaware that the aversive events are happening to them. By "being someone else" who needn't remember the trauma, the child can distance him or herself from the abuse and still maintain some coarse approximation of a normal emotional relationship with the abuser. From the perspective of the abused person, "My daddy does nasty things to that other little girl, but only because she is so naughty, but my dad loves me and has never done anything bad to me."

The culmination is an individual who never acquires a complete personality, self, or an experience of being one coherent "I" controlled by both public and private events. Instead, the individual who experienced the history of abuse has more than one personality repertoire, primarily controlled by private events (Kohlenberg & Tsai, 1991). On the other hand, during more normal acquisition of personality repertoires, an individual will increasingly engage in being the same person, with these behaviors occasioned and maintained by public and private events; being someone else does not have significant adaptive value.

DIFFERENTIAL REMEMBERING

Besides engaging in different personalities, another aspect of the extreme behavioral variance in this disorder is that of amnesia, or an inability to remember beyond what is considered average (American Psychiatric Association, 1994). In other words, besides extreme variability in behavior and self-report of identity, the self-reports of experiences the person has had also varies widely (Coons, 1994). When exhibiting some personalities, the person reports a history of abuse but not necessarily all the present circumstances. When another behavioral repertoire is exhibited, past abuse may not be reported but the present is reported clearly. It is this behavior that intrigues many. Clinicians and the lay public alike seem to want to know "Is it all *in there*?" That is, are all the memories and experiences stored somewhere in the mind or brain of this person?

From the behavior analytic point of view, remembering (or failing to remember) is a behavior, more or less likely to occur as a function of its antecedents and consequences (Grant, 1982; Grant & Barnet, 1991; Phelps & Cheney, 1996; Skinner, 1974); storage and accessibility are replaced with probability of remembering. With that clarified, one could say that some or most real experiences can be remembered (potentially) and reported; to remember we must arrange the environment to increase the probability that we will behave in the future as we are now behaving (Phelps & Cheney; Skinner, 1989). But in the cases of individuals with the behaviors of DID, the person is reluctant or unable to remember or report some experiences until that person is in a different situation or the reinforcement contingencies change. Then, the person may change personality repertoires and can remember and report different experiences.

The vivid and lucid imagery of the past that is reported by these persons when displaying differential personalities corresponds with Skinner's "conditioned seeing" (Skinner, 1953). A person may come to see stimulus Z, not just when Z is in fact present, but also when other stimuli that have frequently accompanied Z are present. That is, if I can remember and reinstate the emotional behaviors of my past, I can come to see and hear aspects of my past. If I do not remember how I felt in the past, I am less likely to see, hear or otherwise re-experience the past again (Phelps, 2000). Hallucinations in our remembering like other hallucinations are highly context dependent (Hobson, 1994).

This differential remembering/reporting is also on a continuum in degree, not in kind, from the average person's behavior. We all remember, or fail to remember, as a function of discriminative stimuli. These discriminative stimuli, some of which are self-generated in our verbal behavior, and the reinforcement and punishment contingencies in effect at a given time, enable our remembering behavior. Environmental stimuli guide or prompt remembering just as stimuli guide or facilitate other behaviors (Donahoe & Palmer, 1994; Grant, 1982; Grant & Barnet, 1991; Phelps & Cheney, 1996; Skinner, 1974). But these individuals show behavioral variance in remembering and personality in response to highly specific and subtle stimuli, probably more in response to covert behaviors called moods, thoughts, etc., than the average person. This difference in controlling factors of these persons' verbal behavior is the key to conceptualizing these behaviors (Kohlenberg & Tsai, 1991; Phelps, 2000).

SELF-OBSERVATION AND CONTROLLING EVENTS

To this point, some of the typical behaviors labeled as DID have been described in behavior analytic terms. While a complete account of the behaviors conceptualized under the DID label is not likely, a reasonable accounting of most of these behaviors can be framed, using established behavioral processes.

To pursue this further, the variance in self-report of identity and experience by individuals whose behaviors have been labeled as DID may be based disproportionately on inaccurate self-observations made without seeking verification from the social environment. Simply put, such individuals may attend more to their own observations expressed and reiterated in their own verbal behavior and less upon the observations and reports of others. That is to say, when in Rome and unsure of what to do, persons with DID-like behaviors may not attend to or imitate the behavior of other Romans as models. Instead, these persons may arrive at an inflexible self-produced verbal governance (Fine, 1992) by which to behave or they may attempt to engage in what they judge to be appropriate behavior by observing their own behavior without using social comparisons. Keller & Schoenfeld (1950) described the person as having "the ability to use one's own behavior as the SD for further behavior, verbal or otherwise" (p. 369); here, the person uses their own behavior as a discriminative event to a greater extent than the normal individual.

Since abnormality is defined by its context, and since we are frequently less adept at self-observation than we are at observing the behavior of others (Skinner, 1974), this in and of itself could lead to aberrant behavior. But individuals with DID-like behaviors persist in their self-observations and reports, even in the face of contradicting evidence from others. They claim to be different persons when in fact there is only one and the same person (or body) present. These individuals have dissociated their self-observations and resulting reports from the reports of others. As a result, they have observations that are not as controlled by the public environment but are instead a function of their own distorted verbal governances (Fine, 1992).

Such inaccurate self-observations may be under the control of reinforcement contingencies other than those exerted by other individuals. In the past, the person with the now present DID-like behaviors learned to attend to and rely more heavily upon his own observations of how he felt, what he needed, whether he was "good" or "bad," etc. (Fine, 1992; Keller & Schoenfeld, 1950). This behavior may have either been due to neglect and abuse, both of which were possibly delivered without regard to what the child did. The behavior might also have been present before the abuse but only emerged as adaptive responses while experiencing the abuse (Kohlenberg & Tsai, 1991).

EMOTIONAL BEHAVIORS AND CONTROLLING CONTINGENCIES

During abuse, emotional outbursts such as crying and responses to pain, which were originally respondent behaviors (Fordyce, 1976; Rachlin, 1985; Turk & Rudy, 1990), caused still more abuse and therefore came under control of avoidance and escape contingencies (Kohlenberg & Tsai, 1991). Pain-inflicted crying led to more pain being inflicted. Crying, smiling, and other emotional displays, which were originally respondents, could come to be under the control of operant contingencies, in an attempt to avoid further punishment. In addition, the care giver's abuse may have been erratic and difficult to predict but was still the focus of attempts by the abused person to predict and avoid further abuse. As a result, the abuse victim may have come to exhibit behaviors and emotions capriciously and histrionically; at other times, virtually no affect would be exhibited. These attempts at self-control from the erratic stimulus events and contingencies were probably not often successful in avoiding or escaping abuse. The person being abused could never learn to predict what events produced or avoided abuse or reinforcement (love) and increasingly would come to attend to him- or herself since other individuals provided unreliable antecedent events as occasions for how to behave.

In the present, however, the former victim has potentially "heightened" operant control of emotions and personality behaviors when confronted with uncertainty or stimulus conditions reminiscent of the past. These individuals are often very skilled at altering their personality repertoires to control others (Spanos, 1994). Kohlenberg & Tsai (1991) reported that these individuals are vigilant and actively attentive to the therapist's discriminative stimuli as to what behaviors will be reinforced or punished. At the same time, different personalities may be displayed with no obvious change in any public, environmental stimuli.

Some writers report that this disorder may only become apparent to a professional or others when "different people" attend meetings, interviews, or therapy; that is, the same individual attends but with a different self-report of identity, memories, and personality behaviors (Sackheim & Devanand, 1991). In so doing, individuals displaying these behaviors can receive a great deal of reinforcing attention from professionals for engaging in these behaviors. Individuals demonstrating behaviors correlated with a diagnosis of DID may be reassured of no further abuse and may be encouraged to try to "be themselves" in as many ways as they “need” to be. The different self-reports and personality repertoires become a source of reinforcement for the formerly abused victims and the professional alike (Spanos, 1994). The risk here is that the verbal repertoires of a person with degrees of behavioral variability could be shaped iatrogenically to reporting to be a divergent person by professionals zealously looking for this disorder (Fahy, 1988; Merskey, 1992). To quote one skeptical critic, "the procedures used to diagnose MPD often create rather than discover multiplicity," (Spanos, p. 153).

DIFFERENTIAL "INTELLIGENCE" AND PHYSICAL SYMPTOMS

This behavior pattern has been conceptualized as being largely a difference in verbal behavior, but other differences are reported to exist and are marshaled as evidence for this disorder. That is, the individuals who exhibit these behaviors are reported to be different in intelligence and pharmaceutical needs, and have different corrective prescriptions for vision, allergies, and so on (American Psychiatric Association, 1994). Some of these reported differences are explainable in the analysis presented here. For instance, a person's intelligence quotient score consists of his ability to answer specific types of questions and his attempts to perform some nonverbal tasks. Some of these are a person's verbal behaviors (Staats, 1963), in that the person, when displaying some personalities, does not "know as much" as when executing other personalities. The person simply answers fewer questions correctly when performing Bob’s repertoire than when performing the personality repertoire of Jose. In terms of nonverbal tasks, "I can't figure this one out" or "I don't know what to do here" can end the trial, just as performing slower or faster can alter the score. The score is taken as a measure of intelligence when all that are being measured are test-taking skills (Staats, 1993) which are largely self-reports. The reported differences in corrective lenses are explainable by differential self-report but the differences in medical conditions may be more difficult to explain.

Pain complaints, paralysis, blindness, etc., also consist of a self-report of a private event. Each of these may be accompanied by publicly observable behaviors such as wincing, reluctantly moving, reporting or appearing to be unable to move or see (Fordyce, 1976; Skinner, 1974). Both the self-reports and the public display of these differences are under stimulus control of the different personality repertoires. When such an individual displays a specific personality, the self-report of pain or other symptom comes or goes with the other behaviors. Originally, the public signs of pain were authentic afflictions in the past as the result of abuse; months or years later, such indications could be self-produced, rule-governed behavior as part of the personality repertoire. These pains and related behaviors could be reinforced and shaped into a "real" affliction by well meaning others as the verbal behavior acquired differential stimulus control of operant pain behavior. The reports of pain and the display of pain-related behaviors can persist as operant behavior maintained by its consequences in the absence of the original painful stimuli (Bonica & Chapman, 1986; Fordyce, 1976; Rachlin, 1985).

As for the reports in the literature of allergic and other responses being present in some personalities and not i n others, these too can potentially be accounted for via verbal behavior mechanisms. There are reports that individuals can develop rashes, a wound or a burn or other physiological symptoms in response to another's verbal suggestions, i.e., under hypnosis (Barker, 2001), although it has been argued that many of these symptoms are likely self-inflicted when observers are not present (Johnson, 1989). Verified reports of hypnotically-induced dermatological changes are difficult to substantiate; such effects are difficult to produce and are not as common an occurrence as often reported (Johnson). These reports are not all due to the acts of the person showing the symptoms; instead, these symptoms may be due to an interaction of verbal behavior and conditioning mechanisms, (Barker). Verbal behavior can also facilitate the development of stimulus control via respondent or operant conditioning (Skinner, 1957). If an experimenter were to flash a light in your eyes and then shock you, then you would be expected to recoil to the light after some number of such pairings. If the experimenter were to explain the contingency between the light and the shock, it would be expected for you to recoil to the light after fewer trials (Wilson, 1968). Such instructions are " . . . not intended to change the subject's beliefs about what events are to occur, but about the contingency between them" (Boakes, 1989, p. 385).

Relating this to the differential report and display of symptoms is not a big leap. Here, the individuals who display the divergent personalities have self-instructed and subsequently conditioned themselves to display symptoms when performing different behavioral repertories. Over time, the symptoms may come under the stimulus control of the emotions displayed, in addition to the person's verbal behavior, and appear spontaneous to the person him- or herself. To support the argument for conditioning mechanisms producing somatic symptoms, biofeedback has successfully been applied to treat autonomic dysfunctions as diverse as dysmenorrhea and seizure activity (Adler & Adler, 1989), hypertension (Dubbert, 1995), and psoriasis (Goodman, 1994), among others.

HOW SHOULD THERAPY ADDRESS THESE BEHAVIORS?

From the foregoing arguments, therapeutic interventions for persons displaying the behaviors labeled as DID must consist of extinguishing a reasonable share of the behavioral variability in the personality repertoire and reinforcing behavioral stability and generalization; literally, to shape one personality. In an ABA research design, Kohlenberg (1973) reported increasing the frequency of specific behaviors composing one personality of an individual who exhibited DID-like behaviors by differential reinforcement of that personality repertoire. Upon returning to baseline and extinction, these behaviors returned to baseline levels.

Other techniques would involve the client role-playing and rehearsing social interactions and experiencing some situations expected to elicit and occasion "normal" emotional behaviors. Price and Hess (1979) reported success at "reintegrating" the personalities in a dual personality individual by teaching assertiveness skills via role-playing. Caddy (1985) also used assertiveness training and shaping in "reintegration" of a varied personality repertoire. The therapist might videotape the client as they behave, to use for feedback and in shaping and instructing more "cohesive" behavior. Therapy could also dictate a means of teaching the client to engage in more "social-referencing," or seeking public feedback in more instances of what is acceptable behavior. Whereas you or I might ask, "Did you see (or hear) something?" when we are unsure of seeing or hearing, individuals whose behaviors are consistent with the label of DID may have to learn to ask, "Am I still behaving as me?" The therapist could not completely answer this question but family members and significant others could. This process would have to continue until the person reports being the same individual with the same experiences, and has less observable variability in their personal repertoire.

Even if a therapist were to try to pursue such an intervention (most would probably not), this process could be drawn-out and arduous, due to the multiple sources of control that would require intervention. This could possibly sabotage the efforts by those who, with the best of intentions, attend to and reinforce the personality variability. Based on this account, control of the behaviors in this pattern would be difficult for anyone to establish. Even the therapist who occasions and reinforces the variance is not exerting control unless unpredictable behavior is the target behavior. As a result, these individuals likely have been in therapy for years and will continue to seek and need therapy for years to come (American Psychiatric Association, 1994).

CONCLUSIONS

This paper has attempted to apply a behavioral analysis to Dissociative Identity Disorder. Why should anyone conclude that a behavioral analysis of this disorder offers any more than other theoretical positions? Behavioral theory treats personality as behavior and identifies the known environmental variables that determine these behaviors. Behavioral theory *is* personality theory, without granting unnecessary, special status to the behaviors of personality. Other paradigms either reject personality as behavior or attribute causation to inaccessible, internal, and often poorly defined variables (Bliss, 1980, 1984; Bowers et al., 1971; Gur, 1982; Hilgard, 1977; Horton & Miller, 1972; Schenk & Bear, 1981). The same operant variables that occasion and control personality no doubt have a role in Dissociative Identity Disorder. As an alternative to the "ill-defined" variables criticized above, behavioral theory would argue that the person's verbal behavior (overt and covert) and the bases for the person's relevant verbal behavior, as well as their self-observations are variables to be functionally analyzed and manipulated in understanding the behaviors labeled DID. No claim is being made here that the person's verbal behavior is *the* functional variable behind dissociative behaviors. As Beck stated, "To conclude that cognitions cause depression is analogous to asserting that delusions cause schizophrenia," (Beck, 1991, p. 371). A person's verbal behavior can play multiple roles in interacting with other behaviors, as antecedent stimuli, as concurrent behavior, or as stimuli that have acquired reinforcing or aversive properties, (Skinner, 1957), or as functional variables that either "complement" or override control by other operant contingencies (Catania, Matthews, & Shimoff, 1982, 1990).

Unless a reader is willing to look at the evidence for the effectiveness of behavior analysis, the arguments made here are moot. Some readers, behavioral or otherwise, may consider any discussion of these behaviors to be a waste of time or even an indulgence in "pop psychology" since they don't really "exist". However, the behaviors labeled as DID receive a great deal of attention from the lay public and in clinical training programs. Therefore, behavior analysts should take the time to explain their analysis of these behaviors; after all, Skinner (1945) spent considerable time analyzing psychological terms, as did Dollard and Miller (1950). It is not however, productive to discuss this behavior pattern as a unique instance of behavior as it is merely an instance of behavioral variability.

While behavior analysts are hesitant to address this and similar behavior problems, other explanations are being widely read. Behavior analysts have important but unrecognized arguments to contribute to the discussion.

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THE PAST AND FUTURE OF BEHAVIOR ANALYSIS IN DEVELOPMENTAL DISABILITIES:

WHEN GOOD NEWS IS BAD AND BAD NEWS IS GOOD

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Abstract

This article provides a brief historical overview that outlines the temporal contiguity of developments in both behavior analysis and developmental disabilities, illustrating how each has contributed to the other. Consideration is then given to what the successes and failures suggest for the future. Behavior analysis has had a major impact in the field of development disabilities. This is readily apparent from an examination of the literature, where behaviorally-based interventions for individuals with developmental disabilities proliferate. This is also seen in the curricula of training programs in special education which typically contain course content and textbooks on behavioral approaches; in the number of advertisements for positions in developmental disabilities in which skill in behavior analysis is a qualification. More examples include the results of litigation mandating provision of services based on behaviorally-based practices, and from policy, regulatory standards, and legislation regarding use of behaviorally based assessment and treatment in various situations (e.g., Reid, 1991). That's the good news. On the other hand, there have been, and continue to be, notable failures and sources of dissatisfaction. As will be discussed, that is also the good news. It can therefore be useful to examine the evolution, sources, and nature of this good news. This article, then, will (a) provide a brief historical account that outlines the temporal contiguity of developments in both behavioral analysis and developmental disabilities, and (b) consider what the successes and failures suggest for the future.

HISTORICAL DEVELOPMENTS IN BEHAVIOR ANALYSIS AND DEVELOPMENTAL DISABILITIES

1940s. Behaviorism began emerging as a philosophy following Skinner's *The Behavior of Organisms* (1938), as a result of dissatisfaction with the tradition of "seeking a solution for the problems of behavior elsewhere than in behavior itself." (As a frame of reference, this development would have been described in the parlance of the times as "the cat's pajamas.")

At the same time, a custodial model characterized the field of developmental disabilities. Many individuals with mental retardation resided in institutions, where programs were directed almost entirely to providing basic physical care and general types of stimulation. Because persons with mental retardation were considered to be uneducable, systematic training was not provided.

1950s. Developments in this decade emerged from Fuller's (1949) study with a young man with profound mental retardation who did little except lie on his back with minimal movement, and who was thought incapable of learning. Fuller injected a warm sugar-milk solution into the man's mouth following any movement of the man's right arm and, within four sessions, the man was moving his arm to a vertical position three times per minute. (To anchor it within a cultural context, Fuller would have been considered "a cool cat.")

Following Fuller's (1949) study and the publication of Skinner's *Science and Human Behavior* in 1953, other researchers began to use the methodology of the experimental analysis of behavior to determine whether principles of behavior demonstrated by Skinner in the laboratory were valid with humans. The *Journal of the Experimental Analysis of Behavior (JEAB)* began in 1958 and published behavioral research (with both humans and other animals). Much of the research that occurred with society's neglected and disenfranchised members contributed to the development of behavior analysis as applied to humans but was not designed for any socially significant purpose. Thus, persons with disabilities and mental illness contributed more to behavior analysis than behavior analysis benefited them during this time. However, this development set the stage for recognition that learning could occur and behaviors could be changed in individuals previously thought to be "hopeless."

1960s. Research in behavior analysis rapidly progressed from extensions of behavioral principles to persons with developmental disabilities for the sake of showing generality, to applying those principles to the analysis and treatment of important problems. Much of the research in developmental disabilities during this transition was therefore an outgrowth of basic research. It was a logical extension, fueled by

Author Note: Portions of this article were based on: Neef, N. A. (April, 2000). *Contributions of behavior analysis to advances in developmental disabilities*. Invited address at International Conference on Behaviorism: Theory and Philosophy, Morgantown, West Virginia.

Skinner's extrapolations, that if behavior could be systematically changed through applications of the principles of behavior, it could be changed for the better. The *Journal of Applied Behavior Analysis* began in 1968 as the applied counterpart to the basic research reported in *JEAB*, and Baer, Wolf, and Risley (1968) articulated the defining characteristics of applied behavior analysis. Research in developmental disabilities established that basic skills and adaptive behavior repertoires could be taught. Token economies were commonly used. (These developments would have been called "groovy".)

Within the field of developmental disabilities, social movements established the right to receive treatment and education. This was significantly influenced by pioneering behavioral research that challenged myths regarding the educability of persons with mental retardation. With the successes demonstrated in behavioral research, there was increased recognition that individuals with developmental disabilities could benefit from educational programs. This was associated with a change from custodial to habilitative programs; principles of behavior were applied to teach adaptive skills such as toileting, feeding, dressing, and language skills, and to treat problem behaviors. The developments during this decade ushered in the deinstitutionalization movement.

1970s. A major event in developmental disabilities was the passage of the Education of the Handicapped Act (PL 94-142), which included a mandate to develop individualized education programs. (This development would have been called "far out".) The focus on objective measurement of behavior clearly reflected the influence of behavior analysis, and concern with individualization was consistent with the emphasis of behavior analysis on single organisms. Individualized education programs required specification of present levels of performance, short-term instructional objectives presented in measurable terms leading to annual goals to be achieved, and evaluation of procedures with objective criteria for determining progress toward goals.

This corresponded closely to approaches in applied behavior analysis, except that applied research was concerned not only with whether the individual had learned the target behavior, but whether the procedures produced that outcome. The emphasis, however, was more on demonstrating the effectiveness of procedures in changing behavior than on understanding their operations. At the same time, applied behavior analysts were finding less of relevance to their interests in the basic research literature (Baer, 1981; Michael, 1980). *JEAB* published substantially fewer studies on the experimental analysis of human behavior, which declined in 1970 and reached its lowest point of 4% of all its studies in 1980 (Buskist & Miller, 1982). The increased technological focus of applied research, on the other hand, appealed to the demand for practical solutions to human problems (in fact, *JABA* circulation was at its highest point during this decade), but was lamented by some behavior analysts (e.g., Michael, 1980; Pierce & Epling, 1980). Despite the recognition that behavior was learned through a history of environmental contingencies, the reinforcement histories that led to the development of behavior were generally disregarded in developing interventions. Instead, it was assumed that etiology was irrelevant because immediate history could override the effects of prior history. Unknown prior histories were therefore treated as inevitable sources of variability. As a result, research and intervention in developmental disabilities involved teaching new repertoires or altering existing ones by superimposing reinforcement and/or punishment contingencies onto whatever unknown contingencies currently maintained the behavior. The effectiveness of those procedures therefore depended on their either being sufficiently powerful to override whatever variables were maintaining problem behavior or on serendipitously addressing the maintaining contingencies without knowing what they were (Lattal & Neef, 1996). As a result, efforts to address habilitative or educational goals of individuals with developmental disabilities often relied on default technologies of punishment or contrived reinforcement.

This posed several problems in interventions for problem behaviors of individuals with developmental disabilities. First, by superimposing contingencies onto unknown operative ones, there was no consistent basis for being able to predict their effectiveness. Second, once the superimposed contingencies were removed, the operative unchanged contingencies would likely reassert their influence, creating dependence on default technologies for maintenance and thereby rather short-lived benefits. Third, this reliance often led to procedural descriptions of form rather than function, which contributed to the difficulties in predicting or producing consistent effects. In addition, it often led to a sequence of increasingly intrusive and controversial interventions, which were unsatisfactory to consumers (Lattal & Neef, 1996).

1980s. Research on functional analysis revolutionized the conceptualization and treatment of behavior disorders. Iwata et al.'s study (1994/1982) formulating a comprehensive, conceptually systematic, and standardized assessment of the function of individuals' problem behaviors signaled a major shift in behavior analysis with developmental disabilities. The focus changed from being predominantly concerned with experimental demonstrations of behavioral operation (i.e., asking "does this procedure act to change behavior?") to a concern with behavioral process and analysis (i.e., asking, "how does this procedure act to change behavior?"). This led to a change in approach in which treatments for problem behavior were selected and matched according to identified function, and to an emphasis on establishing or strengthening alternative appropriate responses that served the same function as the problem behavior. In addition, there was increased attention to antecedents and stimulus control. In the field of developmental disabilities, there was also a shift from teaching in a developmental sequence to a pronounced focus on functional skills. (These developments would have been described as "hip.")

1990s. In the 1990s, the analytic trend in applied behavior analysis continued. A review by Pelios, Morren, Tesch, and Axelrod (1999) of research published in five journals showed that the use of a pretreatment functional analysis appeared to increase the likelihood that treatments for problem behaviors would be based on reinforcement versus punishment contingencies. This conceptual focus was accompanied by a renewed interest in once again applying methodologies and findings from basic research to the area of developmental disabilities. This included, for example, applying basic research on behavioral momentum to interventions that alter the persistence of desirable or undesirable behaviors (e.g., Davis, Brady, Hamilton, McEvoy, & Williams, 1994), and the use of establishing operations to enhance reinforcer efficacy (e.g., Vollmer & Iwata, 1991). Other examples include examining modality effects in stimulus equivalence class formation as applied to socially significant behaviors (Kennedy, Itkonen, & Lundquist, 1994); and applying basic research on matching theory in identifying preferred stimuli, examining reinforcer and schedule effects, and in treating problem behavior based on examination of the reinforcement for desirable versus competing undesirable behaviors (Fisher & Mazur, 1997).

In developmental disabilities, research on functional assessment had a strong influence on policy, including recommendations and endorsements by national organizations such as the National Association of School Psychologists, the National Association of State Directors of Special Education, and the National Institutes of Health. Importantly, it has been mandated through federal legislation; the 1997 Amendments to the Individuals with Disabilities Education Act require the use of functional assessments under certain circumstances. Schools experienced difficulty implementing or interpreting functional assessments with integrity, however, and the lack of local expertise and resources has given rise to alternative functional assessment methods.

The present. Behavior analysis has had a major impact in developmental disabilities, perhaps more so than in any other area. Treatment procedures based on behavioral principles are now widely recognized as the most effective forms of psycho-social intervention. This is especially the case with autism, where the effectiveness of behavioral interventions has created a demand by parents for skilled behavior analysts that exceeds current resources. In addition to contributing to the development of an increasingly effective technology, the focus of behavior analysis on objective measurement has exposed those practices that are ineffective, such as facilitated communication and sensory integration. The natural science of behavior has allowed the field of developmental disabilities to withstand the encroachments of such questionable treatments and practices.

From 1962 to 1967, there were fewer than 50 studies involving applications of behavior analysis in the major journals concerned with developmental disabilities. Today, over 600 studies involving applied behavior analysis with developmental disabilities have been published in *JABA* alone; other journals have been established as publication outlets for such research in addition to the common appearance of behavior analytic studies in journals specializing in developmental disabilities. This research encompasses basic learning processes, self-care and daily living skills, language acquisition and communication, leisure and recreation, academic performance, vocational skills, community preparation, functional assessment and treatment of severe behavior disorders, and others. The same principles that were applied in Fuller's (1949) study to increase arm raising of a young man with disabilities while lying in a bed have been applied to teach arm raising of others with disabilities to signal a bus on their way to work, to get a waiter's attention in restaurants, and to answer teachers' questions in the classroom. (Some might call that, "like, totally awesome and way cool.")

FUTURE DEVELOPMENT IN BEHAVIOR ANALYSIS AND DEVELOPMENTAL DISABILITIES

The above history is one of which we can be proud, but never satisfied, because every change has the potential to be both praised and lamented. But even changes that are lamented might be welcomed because they serve as the stimulus for further development. Petroski (1992) likens the critical role of failure in the evolutionary process to a dentist fitting a crown, where carbon paper is used to identify points where there is not a good fit of form to context, and where change is therefore needed. It is these incongruities or irritants that occasion variations until the variations produce the desired outcomes. This is similar to a selectionist perspective (Skinner, 1953) in which variations are selected by their beneficial consequences. The risk, then, is when there is adaptation to failure, which allows problematic features or practices to persist. The process applies both to thematic research as well as to the broader practices within the cultures of behavior analysis and developmental disabilities.

As the above history suggests, behavior analysis has not been marked by complacency. Ironically, even its very success with respect to developmental disabilities has been tempered with criticisms that behavior analysis has become too focused in that area. Numerous articles, and even journal issues, have been devoted to concerns with the field (e.g., Hayes, 2001; Michael, 1980; *Journal of Applied Behavior Analysis*, 1991). There have always been tensions and resultant shifts in which a body of literature that is described by some as too irrelevant to or remote from common and pressing problems spurs "real world application" that in turn is described by others as too irrelevant or remote from basic principles. Indeed, dissatisfaction is inevitable because all developments imply a degree of failure along some dimension that cannot be satisfied without sacrificing another dimension (e.g., in terms of cost, resources, scope, precision, practicality, utility, accessibility, control, relevance, acceptability, etc.). Because it is a logical impossibility for all requirements to be met when those requirements are in conflict, it is a matter of determining to what extent and along which dimensions failure will be manifest.

An example is functional assessment in the schools, where alternatives to extended experimental analyses in analog situations necessarily sacrifice some degree of precision for practicality. The balance is a delicate one because precision without practicality (i.e., methods that yield accurate information but which are not widely adopted because of the resources required for implementation) is as useless as practicality without precision (i.e., convenient assessment methods that yield inaccurate information or conclusions). Failures along either dimension, however, may serve as establishing operations, and promote investigations that will produce closer approximations to the desired state of affairs.

Failure that results in setbacks has occurred when faulty application or bad practice is mistaken for inadequate principles or bad science. Because the public does not always discriminate technological from theoretical failures, there is the risk that it will throw out the baby with the bath water and reject behavior analytic approaches. Within developmental disabilities, dissatisfaction sometimes has been expressed in movements that are ideological in nature (e.g., self-determination, person-centered planning, positive behavioral support). Behavior analysts, too, must guard against throwing out the baby with the bath water and instead treat these movements as a useful source of data for advancing the field. Examination of changes from that perspective can suggest areas of compatibility on which we might capitalize (e.g., tying research involving choice-making under concurrent schedules to promote effective choices of persons with developmental disabilities consistent with "self-determination"). They also suggest areas in which we might devote more attention (e.g., classes of dependent variables, system-wide interventions, examination of contextual influences). Just as Baer, Wolf, and Risley (1987) suggested that, in addition to measuring changes in target behaviors, we measure problem displays and explanations that have stopped or diminished as a result, we might view changes reflecting counter-control (problem displays and explanations that have increased) as a form of social invalidity. To reject them outright is as dangerous as accepting them outright; both represent a form of complacency and adaptation to failure and therefore a threat to the vitality of behavior analysis and developmental disabilities. We can afford neither rigid adherence to our technology nor abandonment of our scientific principles.

In summary, there is cause for both celebration and contemplation. We can celebrate how far behavior analysis and developmental disabilities have come while also contemplating where it needs to go. Further development can be promoted by recognition that the good news of our success was and is made possible by the careful contemplation of our failures. In that sense, both our successes and the failures that stimulate further development are good news worthy of celebration. Party on.

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DYNAMIC CHANGES IN REINFORCER VALUE: SOME MISCONCEPTIONS AND WHY YOU SHOULD CARE

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Abstract

The effectiveness of a reinforcer in maintaining behavior (its value) changes systematically with successive deliveries of that reinforcer. Some misconceptions have impeded our understanding of these changes. The misconceptions include the idea that the changes never occur or occur only when large reinforcers are used; that they always occur; that they are always bitonic in form; that they have no theoretical implications; and that they are produced by satiation to the reinforcer. A precise characterization of the factors that alter the effectiveness of a reinforcer is essential for the theoretical understanding of operant behavior and for the use of operant techniques in practice.

For many years, operant psychologists have believed that reinforcers lose their effectiveness (i.e., their value) as they are repeatedly presented (e.g., Reese & Hogenson, 1962; Skinner, 1932). This finding has potentially important theoretical and practical implications. Changes in reinforcer value are theoretically important because they are not anticipated by most current theories. As will be argued, the changes may also contribute to the theoretical explanation of a number of operant phenomena, such as behavioral contrast and spontaneous recovery (see "Within-session Changes are not Theoretically Important"). Dynamic changes in reinforcer value are practically important because operant techniques are often used to strengthen or weaken behaviors. To do this effectively, contingency managers must maintain the effectiveness of their reinforcer or punisher as long as possible.

In spite of their potential importance, dynamic changes in reinforcer value were never subjected to an experimental analysis. Instead, they were labeled, "satiation" (e.g., Reese & Hogenson, 1962). This was regrettable because the only evidence that reinforcers lost their effectiveness with repeated presentation was that operant response rates declined after a large number of reinforcers had been delivered. Factors other than satiation (e.g., fatigue) could have contributed to these declines, but those factors were never ruled out by experimental test. Additionally, even if changes in reinforcer value did produce the declines in response rates, factors other than satiation (e.g., habituation) could have produced those changes in reinforcer value.

Author Note: Preparation of this manuscript was partially supported by grant RO 1 MH61720 from the National Institute of Mental Health to FKM. Some of these arguments were presented at the May, 1997 meeting of the Association for Behavior Analysis in Chicago, IL.
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The study of dynamic changes in reinforcer value was ignored until the rediscovery of the fact that rates of operant responding may not be constant within experimental sessions even when the conditions of reinforcement are constant across the session. Instead, rate of responding often increases, decreases, or increases and then decreases within sessions (e.g., McSweeney, 1992; McSweeney, Hatfield , & Allen, 1990). Subsequent research confirmed that these within-session changes in responding are produced primarily by dynamic changes in the value of the reinforcer For example, probe preference tests show that reinforcer value changes systematically within sessions (McSweeney, Weatherly, & Swindell, 1996a). In addition, several competing explanations for within-session response patterns were ruled out. Rejected explanations include: recovery from handling (McSweeney & Johnson, 1994), anticipation of events that follow the session (e.g., feeding, McSweeney, Weatherly, & Swindell, 1995), changes in a general motivational state (e.g., arousal, McSweeney, Swindell, & Weatherly, 1996a; 1996c), changes in interference from adjunctive behaviors (McSweeney, Swindell , & Weatherly, 1996a) or exploration (Roll & McSweeney, 1997), changes in factors produced by the act of responding, such as muscular warm-up or fatigue (McSweeney, Weatherly, & Roll, 1995; McSweeney, Weatherly, Roll, & Swindell, 1995; Melville, Rybiski, & Kamrani, 1996; Weatherly, McSweeney, & Swindell, 1995), and changes in "attention" to the task defined in several ways (McSweeney, Roll, & Weatherly, 1994; McSweeney, Weatherly, & Swindell, 1996c; Melville & Weatherly, 1996). An opponent-process explanation (e.g., Solomon & Corbit, 1974) also seems unlikely because early-session increases in responding sometimes occurred without late-session decreases, and vice versa.

As might be expected, some misconceptions developed as research about within-session changes in responding progressed. This paper discusses these misconceptions because they are impeding our efforts to understand the theoretical factors that control behavior and to put that understanding to practical use. The misconceptions include that within-session changes in response rate (and, therefore, dynamic changes in reinforcer value) never occur or occur only when large reinforcers are used; that the changes always occur; that they are always bitonic in form; that they have no theoretical implications; and that they are produced by satiation to the reinforcers.

WITHIN-SESSION CHANGES DO NOT OCCUR

One potential explanation for the initial neglect of dynamic changes in reinforcer value may have been the assumption that these changes do not occur under most circumstances. Instead, they occur only when a large number of reinforcers have been presented. More recently, some have argued that our within-session changes in responding never occur. Others argue that within-session changes are not found when animals respond for moderate rates of reinforcement (i.e., approximately 60 reinforcer per hour, e.g., Bizo, Bogdanov, & Killeen, 1998). Still others argue that within-session changes are not generally observed because these changes occur only when large reinforcers are presented. The reinforcer used for pigeons in our laboratory (5-s access to food) is larger than that used in many other laboratories.

These conclusions are not correct. Many studies report within-session changes in responding when responding is maintained by moderate rates of reinforcement (approximately 60 reinforcers per hour, e.g., Cannon & McSweeney, 1995; McSweeney, 1992; McSweeney et al., 1990; McSweeney, Roll, & Cannon, 1994; McSweeney, Roll, & Weatherly, 1994; McSweeney, Swindell, & Weatherly, 1996b; 1998; 1999; McSweeney, Weatherly, & Swindell, 1995, 1996a, 1996b, 1996c; Roll, McSweeney, Johnson, & Weatherly, 1995; Weatherly & McSweeney, 1995; Weatherly, McSweeney, & Swindell, 1995, 1996, 1998). Within-session changes in responding are also found when reinforcers are moderate in size. Our first investigations of within-session changes in responding employed rats pressing levers for Noyes pellets (McSweeney et al., 1990; McSweeney, 1992). These results cannot be dismissed by arguing that our 45 mg Noyes pellets are larger than those used in other laboratories. Subsequent experiments also showed that within-session response patterns are not altered by changes in the size of the reinforcer used for pigeons over the range studied in most operant experiments (e.g., approximately 1 to 6 s access to food). Changes in reinforcer size alter the within-session patterns only when reinforcers become very large (e.g., approximately 20-s access to food, Cannon & McSweeney, 1995; Roll et al., 1995). Finally, approximately 200 past studies, conducted in many different laboratories, using many different species, responses, and reinforcers, reported within-session changes in responding (McSweeney & Roll, 1993). Therefore, within-session changes are widely observed and are not restricted to the use of large reinforcers or high rates of reinforcement.

WITHIN-SESSION CHANGES ALWAYS OCCUR

Some argue that we believe that within-session changes in responding should always occur (e.g., Andrzejewski, Field, & Hineline, 2001). This is also incorrect. Within-session changes have been observed repeatedly when schedules provide a moderate to high rate of reinforcement (approximately 60 reinforcers per hour or more; e.g., McSweeney, 1992; McSweeney, Roll, & Cannon, 1994). Within-session changes are smaller and may not occur at lower rates of reinforcement. Systematic within-session changes may not occur for some dependent variables such as the accuracy of responding in a delayed matching to sample task (McSweeney, Weatherly, & Swindell, 1996c). Finally, we have argued that within-session changes in responding are produced primarily by sensitization and habituation to the sensory properties of the reinforcers as those reinforcers are repeatedly presented (e.g., McSweeney, Hinson, & Cannon, 1996). If that is so, then any factor that disrupts sensitization or habituation (e.g., Groves & Thompson, 1970) should also disrupt the within-session patterns. For example, habituation occurs more slowly when stimuli are presented in a varied, rather than a fixed, manner (e.g., Broster & Rankin, 1994; Davis, 1970). Consistent with this argument, within-session patterns are somewhat flatter for variable ratio (VR), than for fixed ratio (FR), schedules that provide the same rate of reinforcement (e.g., Aoyama & McSweeney, 2001). Habituation is also relatively specific to the exact nature of the stimulus delivered (e.g., Swithers & Hall, 1994; Whitlow, 1975). Therefore, it is disrupted by changes in the stimulus. Consistent with this argument, delivering several different types of reinforcers within the session reduces within-session changes in responding (Melville, Rue, Rybiski, & Weatherly, 1997). In addition, within-session changes in response rates may not be observed when frequent changes are made in the contingencies of reinforcement within the session (Andrzejewski et al., 2001). Finally, presenting dishabituators (strong, different, or extra stimuli) disrupts habituation (e.g., Thompson & Spencer, 1966). Consistent with this argument, response rates increase, reducing late-session decreases in responding, immediately after a temporary change in the nature of the reinforcer (Aoyama & McSweeney, 2001; McSweeney & Roll, 1998). Within-session changes in responding may not be observed when discriminative stimuli are changed frequently within the session (Hinson & Tennison, 1999).

WITHIN-SESSION CHANGES ARE ALWAYS BITONIC

Andrzejewski et al. (2001) argued that we believe that a rise-and-fall within-session pattern of responding is universal (p. 235). Although this bitonic pattern is common, response rate may only increase or only decrease within sessions (e.g., McSweeney & Hinson, 1992). Within-session decreases in responding are particularly large when schedules provide high rates of reinforcement (e.g., McSweeney, Roll, & Weatherly, 1994). Early-session increases in responding without later decreases are more likely at lower rates of reinforcement (e.g., McSweeney, 1992).

WITHIN-SESSION CHANGES ARE NOT THEORETICALLY IMPORTANT

A well-known psychologist argued that within-session changes in responding have few or unimportant theoretical implications. Contrary to this idea, dynamic changes in reinforcer value may contribute to the explanation of many poorly understood conditioning phenomena. The contribution of these changes to psychopharmacological phenomena (Roll & McSweeney, 1999) and to the decreases in response rates that are often observed at high rates of reinforcement (e.g., McSweeney, 1992) are discussed elsewhere. We will give a few additional examples here.

Multiple-schedule Behavioral Contrast

Dynamic changes in reinforcer value may contribute to understanding multiple-schedule behavioral contrast (McSweeney & Weatherly, 1998). Behavioral contrast refers to a change in the rate of responding during one component of a multiple schedule that results from a change in the conditions of reinforcement in the other component (e.g., Reynolds, 1961). For example, if one component of a multiple variable interval (VI) 60-s VI 60-s schedule is changed to extinction, then rate of responding during the constant, VI 60-s, component may increase (positive contrast). If one component is changed to a VI 15-s schedule, rate of responding during the constant, VI 60-s, component may decrease (negative contrast). Behavioral contrast is theoretically important because it may imply that reinforcers have a relative, rather than an absolute, effect on behavior (e.g., Herrnstein, 1970). Contrast may also have important applied implications. For example, if contrast occurs in applied settings, then every time the rate of reinforcement is altered to change the rate of a behavior, the behavior may change in the opposite direction in other settings, a potentially undesirable result (see Gross & Drabman, 1981).

In spite of its potential importance, the theoretical factors that produce behavioral contrast are not known. Dynamic changes in reinforcer value may provide part of the explanation. Notice that fewer reinforcers are delivered per session during the multiple VI 60-s extinction, positive contrast, schedule (approximately 30 reinforcers per hour) than during the multiple VI 60-s VI 60-s, baseline, schedule (approximately 60 reinforcers per hour). Assume, as argued, that a reinforcer loses some of its ability to maintain behavior each time it is presented. In that case, the reinforcers delivered in the constant, VI 60-s, component should be more effective and support a higher rate of responding during the contrast (fewer presentations), than during the baseline (more presentations), phase. This is positive contrast. Notice that the multiple VI 60-s VI 15-s, negative contrast, schedule delivers more reinforcers per session (approximately 150 reinforcers per hour) than the multiple VI 60-s VI 60-s, baseline, schedule (approximately 60 reinforcers per hour). In that case, reinforcers delivered during the constant, VI 60-s, component should be less effective and support a lower rate of responding during the contrast (more presentations) than during the baseline (fewer presentations) schedule. This is negative contrast.

This idea is parsimonious and relies only on processes that are supported by independent evidence. It is consistent with much evidence in the literature on behavioral contrast (McSweeney & Weatherly, 1998). It also makes many unique predictions. For example, it predicts that the introduction of a dishabituator (a strong, different or extra stimulus; e.g., Thompson & Spencer, 1966) in one component of a multiple schedule reduces habituation and therefore, produces positive contrast in the other component.

Spontaneous Recovery of Extinguished Behavior

Spontaneous recovery refers to the recovery of extinguished responding with the passage of time. Past theories of extinction have had difficulty explaining this recovery without making special assumptions (e.g., Pavlov, 1927). In contrast, the present idea provides a simple explanation. Habituation may occur within sessions of extinction to stimuli that are presented repeatedly (e.g., the conditioned stimulus (CS) in classical conditioning) or for a prolonged time (e.g., the experimental enclosure). If any of these stimuli help to support conditioned responding (e.g., if they are discriminative stimuli or CSs), then response rate should decrease within sessions of extinction as habituation occurs. Response rate should increase with time between sessions (spontaneous recovery) because habituation dissipates over time in the absence of the habituated stimuli.

Again, this idea is parsimonious and relies only on processes that are supported by other evidence. It is also testable. For example, as argued, habituation is relatively specific to the stimulus that is presented (e.g., Swithers & Hall, 1994; Whitlow, 1975). Therefore, conditioned responding should be restored during extinction by a change in any stimulus that supported responding during conditioning and to which the subject habituated in extinction. For example, either increasing or decreasing the intensity of the CS after the extinction of classical conditioning should restore the conditioned response.

Economic Concepts

Economic concepts were introduced to the conditioning literature in part because negative sensitivity parameters of the Generalized Matching Law (GML, e.g., Baum, 1974) were found when subjects responded on concurrent schedules that delivered qualitatively different reinforcers in the components. Hursh (1980, 1984) argued that negative sensitivity parameters occur when the component reinforcers are economic "complements" rather than "substitutes".

Finding dynamic changes in reinforcer value suggests an alternative idea. McSweeney, Swindell and Weatherly (1996c) found that sensitivity parameters were positive early in the session when rats responded on concurrent food-water schedules. The parameters became negative only late in the session. This suggested that negative sensitivity parameters may be found when subjects sensitize and habituate at different rates to the reinforcers delivered in the two components. If the within-session patterns differ markedly for the two components of the concurrent schedule, then the parameters and fit of the GML will change within sessions with the sensitivity parameter changing from positive to negative for some types of patterns.

Again, this argument is simple and testable. If it is correct, then it should be possible to measure the rate of sensitization-habituation separately for each of two reinforcers (e.g., food, wheel running). That is, the within-session pattern of responding could be measured for each reinforcer when it is delivered alone (e.g., on a VI 60-s schedule). The exact within-session changes in the values of the parameters of the GML for a concurrent VI 60-s (food) VI 60-s (wheel running) schedule should then be predictable from these within-session patterns of simple-schedule performance if the reinforcers do not strongly interact. (See McSweeney & Swindell, 1999a, for other implications for behavioral economics.)

The Regulation of Motivated Behavior

Sensitization-habituation may eventually provide a general-process contributor to the short-term regulation of many motivated behaviors (McSweeney & Swindell, 1999b). The term "motivation" usually applies to behaviors that are energetic and goal directed (e.g., feeding, drinking, exploring, drug taking). Early theories of motivation were general-process theories that attributed all motivated behaviors to a single process such as homeostasis, hedonism, instincts, drives or incentives. General-process theories were abandoned when each of these theories encountered problems. For example, no single process seemed to explain motivated behaviors that are strongly biologically based (e.g., eating, drinking) as well as behaviors that are less obviously biological (e.g., thrill seeking).

McSweeney and Swindell (1999b) provided a new general-process account of motivated behavior. They argued that subjects engage in motivated behaviors because their goal objects (e.g., food) serve as reinforcers. The strength of a motivated behavior fluctuates over time partly because subjects sensitize and then habituate to the sensory properties of the goal objects with repeated or prolonged contact with them. This idea is consistent with two fundamental properties of motivated behaviors. These behaviors decrease in strength with contact with the goal object (habituation) and increase in strength with deprivation for the goal object (spontaneous recovery). More interestingly, the idea clarifies several other widely recognized, but unexplained, characteristics of motivated behaviors. Motivated behaviors may increase in strength with initial contact with the goal, with the presentation of irrelevant stimuli (e.g., noises, shocks), with changes in the goal object, and with the presentation of dishabituators. These properties are predicted by the following properties of habituation: sensitization precedes habituation; sensitization is produced by the delivery of stimuli of moderate intensity; changing the presented stimulus disrupts habituation (stimulus specificity); and dishabituation occurs.

This simple idea also makes many empirical predictions. For example, running is often considered to be a motivated behavior (e.g., Aoyama & McSweeney, in press). In that case, running should conform to the empirical properties of habituation (see Thompson & Spencer, 1966, or McSweeney & Murphy, 2000, for a list of these properties). Consistent with this argument, Aoyama and McSweeney (in press) showed that running exhibits three fundamental characteristics of habituation: spontaneous recovery, dishabituation, and stimulus specificity. These characteristics are not compatible with the usual explanation for the cessation of running, fatigue (e.g., Belke, 1997). For example, turning the houselights on and off for 5 s (presenting a dishabituator) increased the rate of running (Aoyama & McSweeney, in press). There is no reason to believe that presenting an arbitrary stimulus would reduce fatigue and therefore, restore responding.

SATIATION PRODUCES WITHIN-SESSION DECREASES IN RESPONDING

As indicated, researchers initially attributed the loss of effectiveness of the reinforcer to "satiation" (e.g., Reese & Hogenson, 1962). This explanation apparently has wide intuitive appeal to operant researchers (e.g., Bizo et al., 1998; DeMarse, Killeen, & Baker, 1999; Hinson & Tennison, 1999; Killeen, 1995; Palya & Walter, 1997). In contrast, the sensitization-habituation explanation for within-session changes seems to violate some widely shared assumptions. For example, many assume that sensitization-habituation does not occur to the biologically important stimuli (e.g., food, water) that are usually used as reinforcers (e.g., Thorpe, 1966, p. 74). Many also assume that sensitization-habituation applies only to reflexive responding and not to the emitted or "voluntary" responses studied in operant research (e.g., Catania, 1979, p. 52). Data raise questions regarding both of these assumptions. Habituation clearly occurs to biologically important stimuli such as food (e.g., Swithers & Hall, 1994) and the responses used to study habituation are frequently not reflexive (e.g., exploration, Poucet, Durup, & Thinus-Blanc, 1988).

Operant psychologists should use technical terms, such as satiation and habituation, in the same manner as the scientists who study them. If we do not, then our idiosyncratic vocabulary will isolate us from other fields of science. In addition, terms such as "habituation" and "satiation" provide a label, rather than an explanation, for a finding until researchers give those terms empirical content. That is, for example, attributing within-session changes in responding to satiation makes no empirical predictions unless the literature on satiation is consulted for the expected characteristics of behavior when satiation occurs.

No definition of either satiation or habituation would be accepted by all researchers on these topics (e.g., Savory, 1988; Weingarten, 1985). However, the definitions that follow are compatible with the way in which many researchers use the terms. Habituation is often defined as a decrease in responsiveness to a stimulus when that stimulus is presented repeatedly or for a prolonged time (e.g., Groves & Thompson, 1970). Satiation usually refers to a decline in consumption of an ingestive stimulus (e.g., food, water) with its repeated consumption. The factors that contribute to the decline in consumption are usually called "satiety factors" (see e.g., Mook, 1996). Textbooks include lists of satiety factors that differ somewhat with the species and ingestive stimulus under study. A short list for an animal such as the rat and a stimulus such as food includes: oral factors, distension of the stomach, distension of the duodenum, increases in blood sugar at the liver and increases in cholecystokinin (CCK) in the blood (e.g., Mook, pp. 76-79). Confusion arises because habituation to the sensory properties of food is also a satiety factor (e.g., Swithers & Hall, 1994). That is, habituation to the sensory properties of food contributes to the cessation of feeding. We assume that those who attribute within-session changes in responding to satiation mean that satiety factors other than habituation produce the within-session changes in responding when they argue that satiation produces those changes. If they meant that habituation plays a role, then they would agree with our theory, but they do not (e.g., Bizo et al., 1998; DeMarse, 1999; Hinson & Tennison, 1999; Palya & Walter, 1997). When we argue that satiation does not produce within-session changes in response rate, we use the term "satiation" in this sense. That is, satiation refers to the effect of all known satiety factors except habituation.

Habituation and satiation seem to provide such similar explanations for the within-session decreases in responding that there is little reason to separate them. In fact, they have quite different implications. Suppose, for example, that candy is used as a reinforcer to alter the behavior of a child with autism. If candy loses its effectiveness because of satiety factors other than habituation, then reinforcer effectiveness could be maintained by delivering a lower-calorie candy (e.g., decreasing blood glucose) or smaller pieces of the same candy (e.g., reducing stomach distention, blood glucose, etc.). If, however, candy loses its effectiveness because of habituation, then techniques that slow habituation could be used to maintain reinforcer effectiveness. For example, habituation is relatively specific to the precise nature of the stimulus that is delivered (e.g., Swithers & Hall, 1994; Whitlow, 1975). Therefore, delivering different types of candies will slow habituation even if those candies are all high in calories. Habituation can be slowed by delivering the stimulus in a variable rather than a fixed manner (e.g., on a VR rather than an FR schedule; e.g., Broster & Rankin, 1994; Davis, 1970). Habituation can be slowed by introducing dishabituators such as lights and noises (e.g., Thompson & Spencer, 1966).

The habituation and satiety views also have different implications for how behavioral problems are conceptualized and, therefore, how they are treated. Suppose, for example, that obesity results because non-habituation satiety for food occurs too slowly. Then, for example, obese people might be urged to drink fluids before they eat to produce stomach distension. Suppose, on the other hand, that obesity results primarily from slow habituation. In that case, obesity could be reduced by increasing habituation. Again, dieters might eat a less varied diet because habituation is faster under more constant conditions. Dieters might avoid nibbling because the first few tastes of a food might yield sensitization that would increase the ability of food to serve as a reinforcer. Finally, dieters might avoid stimuli that act as sensitizers. For example, they might avoid using salt and spices. They should eat alone rather than with others and they should not watch television while they are eating.

Ironically, satiation, not habituation, was among our initial hypothetical explanations for the late-session decreases in responding. We rejected it only after the data repeatedly disconfirmed its predictions. Arguments that support habituation over non-habituation satiety factors have been described elsewhere (McSweeney & Murphy, 2000; McSweeney & Roll, 1998). We will briefly summarize only a few. To begin with, large within-session decreases in responding are reported when non-ingestive stimuli serve as reinforcers (e.g., lights, Kish, 1966; negative reinforcers, Jerome, Moody, Connor, and Ryan, 1958). Punishers may also lose their effectiveness with repeated presentation (e.g., Azrin, 1960). Non-ingestive stimuli are not usually thought to undergo satiation, but they do undergo habituation. Second, over some ranges of concentrations, steeper late-session decreases in responding are reported when less, rather than more, concentrated sucrose solutions serve as reinforcers (Melville et al., 1997). This finding is opposite to the prediction of non-habituation satiety. Faster satiety should occur for more concentrated solutions, at least for animals that regulate calories, such as rats (Adolph, 1947; Hausmann, 1933). This finding may be compatible with habituation because habituation is sometimes faster for less, than for more, intense stimuli (e.g., Thompson & Spencer, 1966; but see Groves & Thompson, 1970). Third, changing the reinforcer for a brief time late in the session increases response rate once the original reinforcer is restored (Aoyama & McSweeney, 2001; McSweeney & Roll, 1998). Response rate increases regardless of whether the change is an increase or a decrease in the amount of reinforcement delivered and regardless of whether the change produces an increase or a decrease in response rate while it is in effect. Finding such "dishabituation" is compatible with the idea that habituation contributes to the decreases in responding (e.g., Groves & Thompson, 1970; Thompson & Spencer, 1966). It is not consistent with non-habituation satiety. Providing more reinforcers should decrease, not increase, responding by producing more satiation (i.e., more blood glucose, stomach distension, CCK, etc). Finally, within-session decreases in responding are steeper when reinforcers are delivered on FR than on VR schedules even when the two schedules provide the same amounts of food (e.g., Aoyama & McSweeney, 2001). Again, this is compatible with habituation, which is often slowed by variable stimulus delivery. It is not compatible with non-habituation satiety factors. Factors such as caloric content and stomach distention should be constant when amount of food is held constant.

CONCLUSION

The effectiveness (value) of a reinforcer in supporting instrumental responding changes when that reinforcer is repeatedly presented. The reinforcer often increases in value briefly before decreasing, but it may also only increase or only decrease in value. Understanding these dynamic changes in reinforcer value is important to operant theory and to the use of operant techniques in practice. Unfortunately, some misconceptions have obscured our view. Dynamic changes in reinforcer value often occur, but they are not inevitable. For example, they may not occur if reinforcers are presented at low rates or if steps are taken to reduce habituation to the reinforcer. These changes in value are also better explained by sensitization and habituation to the sensory properties of the reinforcer than they are by non-habituation satiety factors such as caloric content or stomach distension. As a result, applied researchers should be able to maintain the effectiveness of their reinforcers for a longer time if they take steps to reduce the occurrence of habituation or to encourage sensitization.

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