Some Remarks on the Quantitative Analysis of Behavior

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This paper discusses similarities between the mathematization of operant behavior and the early history of the most mathematical of sciences—physics. Galileo explored the properties of motion without dealing with the causes of motion, focusing on *changes* in motion. Newton's dynamics were concerned with the action of forces as *causes* of change. Skinner's rationale for using rate to describe behavior derived from an interest in *changes* in rate. Reinforcement has played the role of force in the dynamics of behavior. Behavioral momentum and maximization have received mathematical formulations in behavior analysis. Yet to be worked out are the relations between molar and molecular formulations of behavioral theory.

Key words: physics, mathematical models, behavioral momentum, behavioral dynamics

Rule-governed behavior lies at the foundation of scientific practice. Mach pointed this out before the turn of the century:

When we wish to bring to the knowledge of a person any phenomena or process of nature, we have the choice of two methods; we may allow the person to observe matters for himself, or, we may describe to him the phenomena in some way, so as to save him the trouble of making anew each experiment. (1883/1960, p. 6)

Scientific knowledge, as Skinner noted, is "a corpus of rules for effective action" (1974, p. 235). The rules comprising that corpus are descriptions of the contingencies encountered in nature. The most effective form of those rules, that is, those of the greatest generality and predictive power, is embodied in the verbal behavior we call mathematics. Elsewhere I have discussed mathematics as verbal behavior, in particular to focus on the implications of viewing mathematics as a corpus of rules for effective manipulation of rules (Marr, 1986). In the present paper I wish to direct attention to the application of mathematics to the experimental analysis of behavior. Although mathematical description of operant behavior has been with us a long time,¹ there are many basic-science laboratory workers

in the field of the experimental analysis of behavior who, while certainly not eschewing quantitative description, have at least a vague mistrust of mathematical *models* of behavior. Aside from the possibility that some of these individuals are either uninterested in or untrained to follow mathematical arguments, many are perhaps so imbued with the Baconian method that "model" reeks of "hypothesis," the hypothetico-deductive method, methodological behaviorism, and the like; in sum, all those conceptual and methodological demons which, in its origin and development, the experimental analysis of behavior had sought to exorcise from its scientific soul. Many, remembering Skinner's (1972c) discussion "the flight to mathematical models," perhaps believe that *any* mathematical model of behavior is somehow tainted by Skinner's cogent criticisms. Nevertheless, by 1984 the proportion of papers in the *Journal of the Experimental Analysis of Behavior* devoted to consideration of mathematical modeling and description was then at about 40% (Nevin, 1984), nearly an order of magnitude increase since that journal was founded in 1958. Nothing like that is happening in the *Journal of Applied Behavior Analysis*, but mathematical relationships characterizing the operant behavior of nonhuman organisms have been applied with some apparent success to humans responding under similar contingencies (e.g., Bradshaw & Szabadi, 1988; McDowell, 1982). Thus, regardless of one's views on the mathematization of operant behavior, it is not only here to stay but is growing

Portions of this paper were first presented as an invited address to the Association for Behavior Analysis, Nashville, TN, May 1984. Address for correspondence: School of Psychology, Georgia Institute of Technology, Atlanta, GA 30332-0170.

¹ Jack Findley (1958) presented a quantitative description of concurrent performance in the second issue of the *Journal of the Experimental Analysis of Behavior*.

rapidly. A movement of such magnitude clearly deserves thorough treatment and assessment in terms of its sources, its values, its problems, its influences, and its future. This paper will not provide such a treatment, but I wish to touch briefly upon some aspects of those issues, particularly from the perspective of the relationships between our field and the early history of the most mathematical of sciences—physics. My thesis, in part, is that we are replicating some of the struggles physics had in its development as a quantitative science.

The mathematical treatment of nature, as we know it today, has its origin largely in the 17th century. This may seem surprising to some, as they picture Egyptian pyramids and Greek temples or think of Euclid or Ptolemy. However, with few exceptions, mathematics had essentially only two roles in ancient times: (1) as a practical application in measurement and counting or (2) as an aesthetic or mystical enterprise. The first of these had engineering and commercial value; the second, while providing powerful sources of reinforcement for the development of significant mathematics, may have actually impeded the development of effective scientific practice. The Pythagorean school, which dominated Western mathematical thought until at least the 16th century, supported the view that nature must directly reflect the properties of form and number given, for example, by Euclid's elements and Pythagorean number theory. Thus, looking to the heavens as the embodiment of perfection, it could be concluded that the planets *must* move in circles, since the circle is the perfect form. Despite observations showing clearly that heavenly bodies did *not* execute simple circular motion about the earth, theorists maintained the general perfection-of-form view by an extraordinary adjustment of parameters so that orbits were described as circles of circles, that is, epicyclic in form. Copernicus, while asserting the revolutionary heliocentric theory, still believed that the planetary orbits were circular, not because that form necessarily fit with observations, but because planets *should*

move in circles. The great Kepler, who may be given some considerable credit for initiating the modern view of the role of mathematics in the analysis of nature, nevertheless devoted enormous time and energy to a theory of planetary orbits based on the five regular solids of classical geometry. Aristotle himself had provided the essential criticism of the Pythagorean movement: "They do not with regard to the phenomena seek further reasons and causes, but forcefully make the phenomena fit their opinions and preconceived notions and attempt to *construct* the universe" (quoted by Kline, 1980, p. 116). A similar criticism of mathematical models of learning was to be espoused by Skinner (1972c) some 2,300 years later.

The Pythagorean school could be said to have deduced features of nature solely from the controlling relations of the verbal behavior called mathematics. The sources of stimulus control here are proximally verbal, even if it were possible to trace the mathematical account distally to observations of nature (Kitcher, 1983, 1988; Marr, 1986). We deem such an account "rational" as opposed to "empirical." Skinner and others have warned us not to be seduced by the inherently orderly features of a rational account; rather, the ultimate goal should be a mathematical treatment and prediction of *experimental data*.

Kepler ultimately came under sharper control of the data on planetary motion gathered largely by Tycho Brahe and, after unprecedented efforts of calculation, proposed his three laws of planetary motion. The development of physical science could proceed apace when measurements obtained through systematic observation could, through adroit manipulation, yield functional relationships effectively descriptive of natural phenomena. Observations are selective, and measurements and calculations from observations are even more selective. Kepler's success depended on his selection of features of the data that led him to note that the speed of the planets was a function of their distance from the sun, and to note, in turn, that equal areas in the

orbital planes were traced out in equal times. Finally, he *deduced* from the latter that the orbits must be elliptical with the sun at one focus (Wilson, 1972). That was the only simple form that could satisfy the area rule. All of this depended upon the accuracy and detail of Tycho Brahe's observations. These joint features of quantitative selection and refinement of technique are referred to by Skinner in his assessment of the requirements for an adequate mathematical account of behavior: "First it must be made clear that the formal properties of a system of variables can be profitably treated only after the dimensional problems have been solved" (Skinner, 1972c, p. 324).

The issues of dimension and technique are not independent (any more than "empirical" and "rational" are independent). The fundamental dimension of behavior in the experimental analysis of behavior is rate of responding. The selection of rate as the principal variable seems to have had its origin in serendipity, as Skinner gives the account in "Case History in Scientific Method" (1972b). Having fashioned a device that would yield a curve, he noted that "the curve revealed things in the rate of responding and in changes in that rate which should certainly otherwise have been missed" (p. 109). There is little explicit rationale for his believing these dimensions should be of significance except, he says (with tongue-in-cheek) that "I knew that science made great use of curves." However, rationales are provided in *The Behavior of Organisms* (1938, p. 58) and in "Are theories of learning necessary?" (1972a, pp. 74–78). The selection of rate as the appropriate dimension was controlled first by yielding data of some orderliness (given the variables Skinner *selected* to manipulate). Second, at a theoretical level, rate seemed appropriate to: (a) describing our preconception of "learning," (b) the task of a science of behavior to predict and control, that is, rate as a measure of probability of a response, and (c) the emissive nature of the operant. These three aspects are encompassed by the difficult but seemingly unavoidable construct of the "strength of an operant." Skinner's ex-

perimental technique also seemed to provide the solution to another problem of dimension, namely, the specification of a unit of behavior. As he puts it in *The Behavior of Organisms*, his formulation provides "practical isolation of a unit of behavior" (1938, p. 58).

The Skinnerian approach to the experimental analysis of behavior bears comparison with what might be called the Galileo-Newtonian explanatory axis. The development of an experimental science of motion was to be Galileo's accomplishment, and with it, the foundation of a quantitative experimental science itself. Galileo explored the properties of motion without dealing with the causes of motion—motion was *described* quantitatively by the kinematic parameters of length and time. The salient features of Galileo's method were that questions be put as simply as possible, considering only essential parameters, and that statements be in quantitative form. This provides a methodological link to contemporary laboratory research on behavior. But there is also a conceptual link that is more specific to the experimental analysis of behavior—that is, Galileo was interested in *changes* in motion. Uniform motion of bodies held no more interest than bodies at rest—the basis for the postulate to be called Newton's First Law of Motion.

Skinner's rationale for rate as the primary parameter for the description of behavior was justified in part from an interest in *changes* in rate, as, for example, in acquisition and extinction. In his own words: "The rate at which a response is emitted . . . comes close to our preconception of the learning process. As the organism learns, the rate rises, as it unlearns (for example, in extinction), the rate falls" (Skinner, 1972a, p. 75). There is a certain irony here in that the experimental analysis of behavior has been relatively little interested in either acquisition or extinction, although it has concerned itself with other aspects of rate changes, as, for example, in the description of patterns of performance under schedules of reinforcement.

Newton, who was to combine the work

of Kepler and Galileo into the grand scheme of a "System of the World," was interested in the *causes* of changes in motion. He founded the field of *dynamics*, concerned with the action of forces as causes of such change. Newton's dynamics did not incorporate the *causes* of forces, just as Galileo did not incorporate the causes of motion. Thus we see a hierarchy of description and explanation. Galileo described motion without attending to its causes; Newton explained motion by the action of forces which he, in turn, described without explaining (see, e.g., Park, 1988, for a recent discussion of these issues).

Reinforcement has often played the role of force in the dynamics of behavior; it is an agent for change in rate. Like Newton's force, reinforcement remains only at the level of description. While the forces of physics act through space, reinforcement acts through time. Considerable effort has been devoted to obtaining a consistent quantitative relationship of this action (see, e.g., Commons, Mazur, Nevin, & Rachlin, 1987).

There are other interesting parallels between the field of dynamics in physics and dynamics in behavioral analysis. The most obvious one is Nevin's measure of response strength as *resistance to change*—what he calls *behavioral momentum* (1979, 1988). Skinner's early rationale for the use of rate alone as a measure of response strength has proved inadequate for a variety of reasons. Contingencies of reinforcement engender an enormous range of rates without any obvious simple relationship between those rates and any useful notion of response strength. For example, which evinces greater response strength, responding under a simple variable-interval schedule or under a variable-interval schedule with an added inter-response time requirement? The notion of rate or even relative rate (as in concurrent schedules) as a measure of response strength has created some confusion. A consideration of dynamics would tell us that two different response rates, like two different particle velocities, may provide little information about the conditions necessary to change

the rates. If you had to be struck either by a feather moving at 100 mph or a truck moving at 50 mph, the choice would be clear.

What is needed is something analogous to what Newton called the *quality of motion*, better known as momentum. In Nevin's (1979) terms, response strength would be expressed as the resistance to change in response rate (inertia) produced by a change in reinforcing contingencies or, more generally, what he calls a *disrupter*, for example, changes in initial conditions, such as deprivation. This work holds considerable promise in establishing response strength as a quantitatively useful construct. However, behavioral momentum has not been extensively integrated into current quantitative models of responding. This is not surprising, because most of those models are concerned not with changes in responding per se, but with predicting *steady-state* conditions. Nevin himself, however, has provided some useful links between behavioral momentum theory and other conceptions of response strength. Proponents of the matching law view response strength in terms of response rate or the relative allocation of responding (or time) to alternative sources of reinforcement. Nevin shows, by considering Herrnstein's hyperbolic relation between reinforcement rate and response rate, that changes in the alternative sources of reinforcement (r_o) will influence the rate inversely as the reinforcement frequency for the rate. In addition, he points out the relationship between elasticity in the economic models and resistance to change; and, finally, relates the shaping aspects of reinforcement in reducing variability of behavior to resistance to change in responding (Nevin, 1979; Nevin, Mandell, & Atak, 1983).

A rather simple-minded metaphorical application of basic dynamics can yield functional relationships consistent with some behavioral experiments. One way to consider Nevin's measure of resistance to change is the rate at which response rate changes, that is, the acceleration of response rate as a function of pertinent variables. For example, consider the case

of changes in reinforcement frequency, where both response rate and reinforcement frequency are treated as continuous variables that may change as a function of time. The acceleration of response rate, dR/dt , is likely to be a function of the initial rate R , the initial reinforcement frequency r , and the rate of change in reinforcement frequency dr/dt . Thus:

$$dR/dt = F(R, r, dr/dt). \quad (1)$$

It is reasonable to suggest that dR/dt is directly proportional to both the initial rate and the rate of change in reinforcement frequency, and inversely proportional to the initial reinforcement frequency, that is:

$$dR/dt = a(dr/dt)R/r \quad (2)$$

where a is a constant. We do not have to have an explicit value for dr/dt to predict that, say, instituting extinction will result in a more rapid change in rate for VI 5-min than for VI 2-min, in general accordance with extant data (Nevin, 1979, 1988).

Using the chain rule, Equation 2 can be used to derive a relation between response rate and reinforcement rate.

$$(dR/dt)(dt/dr) = a(dr/dt) \cdot (dt/dr)R/r. \quad (3)$$

Thus:

$$dR/dr = a(R/r). \quad (4)$$

Integration yields

$$\ln R = a \ln r + b \quad (5)$$

or

$$R = br^a. \quad (6)$$

This power law relating response rate to reinforcement frequency seems very different from the Herrnstein hyperbola: $R = kr/(r + r_0)$, which also predicts response rate from reinforcement frequency. It is possible, however, to choose the parameters a and b such that there is a small difference between the power function and Herrnstein's hyperbola over the

typical range of reinforcement frequencies.²

By considering two alternative sources of reinforcement we can write:

$$R_1/R_2 = b_1/b_2(r_1^{a_1}/r_2^{a_2}), \quad (7)$$

a general form of Baum's matching law (1974). Equation 7 can be written in the form of Baum's law:

$$R_1/R_2 = B(r_1/r_2)^A$$

where

$$B = b_1/b_2$$

and

$$A = \ln(r_1^{a_1}/r_2^{a_2})/\ln(r_1/r_2).$$

Notice that A (a measure of sensitivity indicating the degree of overmatching or undermatching) would be, most generally, a function of the ratio of reinforcement frequencies for the two alternatives. If $a_1 = a_2$, A would be a constant independent of the relative reinforcement frequencies as Baum's law assumes. This law applies to both concurrent and multiple schedules (McSweeney, Farmer, Dungan, & Whipple, 1986).

For the most part, linear differential equations of the form characteristic of dynamic systems have not been written for the analysis of behavior, even though the variables and deterministic conceptual models are ripe for such an effort (see Myerson and Hale, 1988, for an example).

McDowell and his colleagues (McDowell & Kessel, 1979; McDowell & Wixted, 1988) have shown, however, that

² A minimal condition would require $b > 0$ and $0 < a < 1$. Additional empirical analyses would determine if the choice of parameter values corresponded to actual data and apply to new conditions. The power function dictates that response rate would increase without bound as reinforcement rate increases. If the above minimal conditions apply, then the rate of increase will fall toward zero as r becomes large. While an organism obviously cannot respond at an infinite rate, neither can reinforcers be delivered at an infinite rate. The maximal rate an organism may emit is dependent on many interacting variables, for example, the criterion response class, apparatus characteristics, schedule arrangement and parameters, etc. Asymptotic rate, as a fundamental parameter, does not necessarily have to be an inherent part of a quantitative theory of responding.

even if differential equations have not been or, indeed, could not be developed, it is still possible to treat the interaction between the organism and the environment as a dynamic system by the application of Laplace transform methods to a linear systems model. Very simply, the linear systems model views the organism as a system receiving reinforcers as input and emitting responses as output. Mathematically characterizing input and output allows, under the assumption of linearity, the derivation of a transform function specifying how input and output are related. Laplace transform methods make the analysis mathematically tractable. Even though the Laplace transform method is not used in the most effective way because of the lack of any system of differential equations, McDowell has been extraordinarily successful in applying linear systems theory to behavioral analysis. The technique requires relatively few assumptions, and, by its very nature, none of the assumptions of the theory involved "extra-episodic" dimensions (Harzem and Miles, 1978). This work promises to be one of the most widely applicable of any mathematical model of behavioral analysis yet developed.

The history of dynamics did not stop with Newton; indeed, its greatest flowering came with the conservation laws and the analytical mechanics of Euler, Maupertuis, Lagrange, and Hamilton. Euler had developed the calculus of variations, and Maupertuis, the first principle of least action, which essentially says that masses (and light) always move so as to *minimize* an integral known as *action* (i.e., the integral of a product of mass, velocity, and distance). From this principle, he could deduce Newton's laws. Lagrange generalized the principle and showed that the principle of least action and the law of conservation of energy could replace Newton's laws as the basis of dynamics. Finally, Hamilton showed that the integral of the difference between the potential and kinetic energy of a mass over the time of its motion is a minimum for the path actually taken. Kline (1980) asserts that "because Hamilton's principle embraces so many actions of nature,

it is the most powerful single principle in all of the mathematical physics" (p. 441).

A major theoretical position in the quantitative analysis of concurrent operant behavior is a *maximization* principle, to wit, that organisms respond so as to maximize reinforcement. There are different views as to whether the control is momentary or global, but the general perspective that the behavior of organisms should be in accordance with some least action principle has its antecedent in classical dynamics.

The behavioral principles of maximization have been derived more directly, however, from economic or ecological considerations, not from analytical mechanics. Ecological models focus on energy conservation and minimization, while economic models have their origin in the "rational man" theories of classical economics. Jevons, who stands as a major figure in the utility movement in the 19th century, said:

Pleasure and pain are undoubtedly the ultimate objects of the Calculus of Economics. To satisfy our wants to the utmost with the least effort—to procure the greatest amount of what is desirable at the expense of the least that is undesirable—in other words, *to maximize pleasure* is the problem of Economics. (1879, quoted in Miller, 1964)

It is possible that some kind of useful least-action principle could be derived from either Nevin's behavioral momentum theory or McDowell's linear systems analysis. If so, some of us might feel relieved to be decoupled from the "dismal science" of economics!

So far in this discussion, I have attempted to show correspondences between kinematic parameters and dynamic theory in classical physics and kinematic and dynamic processes entering quantitative analyses of behavior. The correspondences apply to the limits as well as to the accomplishments. The limits of classical mechanics began to be revealed in the early 19th century with Fourier's analysis of the flow of heat. The development of the science of heat, thermodynamics, represented a parallel, non-interacting system alongside analytical mechanics. Classical thermodynamics is often touted as an appropriate example

for behavior analysts to follow, based as it is on no assumptions about underlying processes, but rather upon postulates involving observables or derivatives of observables, for example, pressure, volume, and temperature (Skinner, 1938, 1969). Nevin (1984) has pointed out a similarity between Baum's generalized matching law and the gas law: $pV = nRT$. As any of the variables p , V , n , R , or T could be expressed in terms of the other, this is said not to be a causal law, but only to specify the *interdependence* of the variables. He comments:

In related fashion the generalized matching law describes the orderly interdependence of behavior and reinforcement, where neither is necessarily causal because reinforcers depend on prior behavior, and behavior is affected by prior reinforcers. (p. 427)

The gas law, laws of dynamics, the generalized matching law, and, indeed *most* of the quantitative theories of operant behavior are bound to the *steady-state*, meaning that the direction of time is irrelevant. As Prigogine and Stengers (1984) say of classical dynamics: "[It] seems to express in an especially clear and striking way, the static view of nature. Here time apparently is reduced to a parameter and future and past become equivalent" (p. 11). Few of us in the experimental analysis of behavior have ventured into the deep world of the irreversible. It is the *stable recoverable baseline* that is of interest, and from which the majority of quantitative models are derived. What happens in those sessions between the establishment of baselines is usually of minimal interest. An analogous conceptual trick is executed in certain problems in classical thermodynamics where it is assumed that if a process is carried out with appropriate care, it will be reversible. This is whistling in the dark, knowing full well that entropy lurks everywhere to take its irretrievable toll.

Reversibility may be the case with certain contingency-controlled behaviors; that is, one may establish a baseline performance, change the contingency, and then return to the original performance. This paradigm is, of course, at the heart

of the ABA design so popular in our field, particularly in applied contexts. Yet irreversibility is not an uncommon finding in the experimental analysis of behavior and is at the heart of the difficult problem of the behavior we call *remembering*. The general fact is that in the experimental analysis of behavior we have tended to focus on problems where irreversibility causes us little trouble, and quantitative models in our field reflect this.

Ironically, classical thermodynamics was to have its functional relationships given a theoretical foundation with the development of the kinetic theory of gases and statistical mechanics. So, once more, mechanics entered the picture, but temporal symmetry and the old determinism were no longer characteristic of description; indeed, the violation of these two aspects of classical dynamics brings us much closer to the nature around and within us than the perfectly elastic, no-friction, billiard-ball world of the introductory physics course. The other important aspect of the development of the kinetic theory of gases was, of course, the introduction of unobservable processes in the analytic treatment, a theoretical step considered dreadful by many leading physicists of the time (Mach, 1883/1960).

Molar versus molecular approaches to the quantitative analysis of behavior have also provided a lively topic for debate, although the dimensions of this debate are generally different from those of 19th century physics. First, molecular aspects of behavior are not "unobservable," they are simply relatively local, short-term features of behavior, IRTs, interchangeover times, etc. The issue is not one of different levels of analysis, because, after all, one is still talking about behavior. Second, and more interesting, I think, is that molar theorists of behavior see little order in that molecular world, and thus focus on larger dimensions of behavior. There is strong contrast here with the physicist's championing molecular processes to account for the laws of classical thermodynamics. The orderly phenomena described by those laws were shown to emerge from the *disorder* at the molecular level. Order is the product of

chaos. This remarkable and profoundly general perspective depended on analytical techniques for dealing with chaos,³ that is, the random motion of huge numbers of particles. Molar theorists of behavior have, in general, not attempted to make such derivations. Their difficulties go deeper than this as Shimp (1982) has pointed out: Some molar theorists have assumed random distributions of molecular features, where that might not, in fact, be the case.

Alternatively, the contingencies chosen to serve for the derivation of reasonably general quantitative laws of behavior may be *too* restricted in ways that engender random distributions of molecular features, or boundary conditions considerably restricting the kind of quantitative formulation possible (Catania, 1981). Considering the enormous possibility for arranging contingencies, molar approaches seem hopelessly constrained. This may simply reflect a general limitation of present quantitative models, namely, that relatively little has been done to provide a general account for the patterns of responding under schedules of reinforcement. Nevertheless, this seems inherently a task for molecular approaches, as Morse (1966) told us more than 20 years ago. To develop such a theory, however, will require more information on what Shimp (1979, 1982) calls the structure or "local organization of behavior." This kind of scientific concern takes us out of the theoretical bosom of classical physics into the physics of the 20th century.

The experimental analysis of behavior is maturing, and the principal manifestation of that growth is found not only in the sophistication of its methods, but in the sophistication of its theory. The ultimate level of sophistication in scientific theory making is a quantitative formulation that encompasses a reasonable number of facts and allows for prediction of new facts. The principal issue for behaviorists is not whether such the-

ories should be developed, but to what extent these theories appeal "to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (Skinner, 1972a, p. 69). Theory has always been a part of the game; successful experimental analysis has rarely been purely Baconian (that in itself would be a theory), but our preconceptions, formal or not, have dictated what we believed was worth observing, and what looked interesting enough to drop everything else and pursue.

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³ The term *chaos* has acquired a whole new and highly technical meaning in recent times. I plan to discuss this in a subsequent article.

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