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Background and Change in B.F. Skinner's Metatheory From 1930 to 1938

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From 1930 to 1938, B.F. Skinner developed, and then altered in several ways, a scientific metatheory or philosophy of science. In the present article, the reflexological background of his early metatheory is described, and the problems it created for him are discussed. Difficulties in his early metatheory and discoveries in his rat research brought metatheoretical changes that were announced in his publications of 1935, 1937, and 1938. The present article suggests several themes to characterize his metatheoretical development between 1930 and 1938.

B.F. Skinner's *The Behavior of Organisms* (1938) was his first scientific book. Though it is often taken as a starting point for an exposition of his behavior theory in textbooks on learning theory, the book itself is the end-product of a complex personal and professional development from 1930 to 1938. In his second autobiographical volume, Skinner (1979) has detailed a great variety of experiences in this period, but is more reserved in suggesting themes of his development up to his 1938 book. In the present article, we provide a *thematic overview* of changes, during this period, in his metatheoretical commitments—foundations of the meaning of scientific terms, criteria for a behavioral unit, the purpose of explanation in behavioral psychology, and so on. Describing such changes ought to come before piecing out their antecedents in Skinner's life history. Therefore, we will stand fairly clear of biographical details (Skinner, 1979), and will construct an overview from his position papers of 1931, 1935, and 1937, closing with a brief look at his *Behavior of Organisms* for illustrative manifestations of changes that we have singled out.

In this article, we will not attempt to place Skinner's development into detailed relation with other behavior theorists. The fact that Skinner did not closely follow the psychology literature (Skinner, 1979, p. 179) raises questions about the degree and kind of influence that the surrounding

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field of behavior theorists exerted on the B.F. Skinner of 1930-1938. Rather than decide on these questions of influence, we will describe Skinner's development more or less in abstraction from the broader field of behavioral psychology.

Finally, we will not try to decide whether the conceptual themes we elucidate actually guided Skinner in his laboratory research; or were themes which marked only his more conceptual papers of 1931, 1935, and 1937; or are simply rational reconstructions in our exposition. Answers to such questions require more detailed biographical exploration and textual analysis than are apropriate to the overview that is our objective. Since we *can* use these conceptual themes to characterize Skinner's metatheoretical development without answering these questions, we will postpone their consideration.[1]

Background: Determinism and the Reflex

Determinism has been a perennial obsession of behaviorists in the twentieth century, and so it was with B.F. Skinner during his graduate-school years of 1928-1931. But so it was with numerous writers—Loeb, Jennings, Watson, Crozier, E.B. Holt, L.J. Henderson, and others whom Skinner read—whose pronouncements reflected the turn-of-the-century issue of mechanism and vitalism in the biological sciences. These writers were not the first to be concerned with determinism, and the issue is plainly visible in Descartes' writings on psychology (Descartes, 1649/1967).

Descartes' assumption that the human will is free was made in the context of his conviction that the domain of material causation included human and animal bodies. He regarded the living body as an engine whose observable behavior results from motions that are generated by the heat of the body's vital spirits and governed by the arrangement of its anatomical parts. The springboard for his mixing of theological and scientific-explanatory notions was the commonsensical idea that a human being could be judged morally responsible for some action only if he "could have done otherwise" than he did, which Descartes construed as meaning that a culpable action was not fully determined by the antecedent conditions (rendered as configurations of stimulus energies or motions by Descartes) which led up to the action or "caused it." The result was that when Descartes took the culturally defined classes of cupable and nonculpable acts, they were abstractly described as "voluntary" and "involuntary" and were given a particular causal-scientific

[1]Of an earlier draft of the present article, Professor Skinner remarked: "I have never thought about intellectual behavior in those terms [the names of themes we elucidate in this article]. I wish you could make it clear that you, not I, are using these words to describe what I was doing" (B.F. Skinner, personal communication, 12 October 1984).

interpretation: a voluntary action enjoyed a system of mediation involving the uncaused action of the soul upon a minute portion of the brain, the pineal gland, whose movements altered the direction of flow of vital spirits from the ventricles of the brain along motor nerves out to the muscle groups which execute that particular action. By contrast, involuntary acts were elicited by stimuli, and the form of their execution was dictated by fixed anatomical connections, mediated by the "centers of reflexion," between receptors and effectors. The identification of human involuntary activities with Descartes' "reflex" acts meant that the terms "voluntary" and "reflex," terms from different domains of discourse, were placed in a simple and enduring opposition as one instance of the philosophical issue of determinism.[2]

Descartes' hydraulic model of neuromuscular action ran into difficulties in subsequent research. However, by the middle-to-late-1700's, a number of investigators, such as Whytt in England, Legallois in France, and Haller, Unzer, and Prochaska in the Germanic states, had established a generalized correlation between the condition of the spinal cord and what were to be more systematically identified as "reflex actions." The principal evidence for this gross correlation was, first, that certain actions remained after the organism had been decapitated; and, secondly, that destruction of the spinal cord abolished these actions. By the mid-1800's, the opposition of the terms "voluntary" and "reflex" had received an anatomical interpretation in terms of a distinction between activities that are particularly associated with the brain and those that depend on the spinal-cord-plus-medulla portions of the central nervous system, respectively. According to Sherrington (cited in Stirling, 1902/1966, p. 86), it was Marshall Hall, in the 1830's, who was most responsible for linking an anatomical division to a complex behavioral distinction (cf. Hall, 1833, pp. 638-642; see Skinner, 1931, pp. 434-436). This anatomical division nicely fitted the conservative tendency to distinguish the province of mind as an arena of human freedom and of the "higher" principles of human life from the merely physical and causally determined functioning of some bodily actions of humans (i.e., reflexes) and of the behavior of

[2]The opposition of voluntary and reflex acts is no simple opposition, but a diverse contrast of a philosophically interpreted class of morally culpable human activities abstracted out of complex social judgment, on the one hand; and responses later identified by their characteristic form, typical eliciting stimuli, and their neuroanatomical mechanisms in the contrived preparations of the physiological laboratory, on the other. One could even mount a reasonable argument that the contrast of volition and reflex is an example of a "category mistake" (Ryle, 1949), for the two terms come from different conceptual domains and have meanings and roles that do not stand in simple logical opposition. Nonetheless, the historical contrast of volition and reflex was made centuries ago, and it continued to dominate throughout most of the nineteenth century; it was aligned with the established dichotomies of mind and body, brain and spinal cord, and the spiritual and material domains.

animals (reflexes and reflex chains or "instincts"). Whether this is an accurate historical assessment of Hall's place in the development of the reflex doctrine is of little concern to us here (cf. Leys, 1980), since it is what B.F. Skinner took to be the significance of Hall's distinction.

Skinner's Agenda in 1931

According to Skinner (1931, pp. 434-439), the concept of reflex had gradually come to be burdened with a number of interpretive properties, such as that the reflex is innate; that it is performed unconsciously and involuntarily; that it is automatic, stereotyped and mechanical; in general, that it is "rigidly determined" by the stimulus and not by any additional principle of the organism except its given anatomical structure and physiological condition. Skinner blamed Hall in particular for saddling the principle of the reflex with these interpretive features and therefore for erecting conceptual barriers, "metaphysical and superfluous interpretations" that are contrary to an appreciation of the fruitful and—this was Skinner's reading of history's teleology in the scientific study of behavior—ultimately correct hypothesis concerning even the most complex of human and animal behavior: namely, that it is all "reflexive." (He called this idea "the generalized reflex hypothesis," and we will retain his useful label.)

Such a philosophically tainted opposition as that of reflex and volition could not fail to arouse the critical disapproval of an anti-metaphysical leveller such as the young B.F. Skinner. One of his tasks was, therefore, to purify the reflex of its "superfluous and metaphysical interpretations" so as to reveal the empirical core meaning of the concept. That core meaning would then be useful in a new, descriptive behavioral psychology, which he was trying to develop in the early 1930's.[3]

Skinner's paper, "*The Concept of the Reflex in the Description of Behavior*" (1931) is a brilliant exposition of a program for the reflex. At the heart of his paper is an acute awareness of "the conflict between an *observed necessity* and *preconceptions of freedom* in the behavior of organisms" (Skinner, 1931, p. 431, emphasis original). Skinner's objective was to resolve the conflict in favor of necessity or determinism. In so doing, he attempted to combine, with mixed success in his paper, the roles of visionary and critic. As a visionary, he endorsed the deterministic "generalized reflex hypothesis"—the notion that the entire behavior of the organism is a complicated function of the present stimulus situation—which

[3]"I am trying to define a special science concerned with describing the behavior of organisms, and I want to save and to define the reflex as the logical instrument for that description" (B.F. Skinner to E.G. Boring, December 14, 1930. Quoted with permission of Professor Skinner.)

he saw as the historically inevitable outcome of the thorny development of a science of behavior. Simultaneously, he is the sharp-eyed critic ever on the watch for speculative tendencies which seem, in the history of the sciences, to have always produced confusion; the reader is not surprised to find that "preconceptions of freedom," for example in Descartes' account of voluntary action, are indefensible. Skinner's therapeutic recommendation is the spartan and self-denying maxim that we should "remain at the level of our observations" (Skinner, 1931, p. 450). If we remain at that level, we find that the empirical concept of reflex is independent of volition because volition is not an empirical construct. So the reflex cannot meaningfully be characterized as involuntary (see footnote 2).

His analysis of concepts—here he is at home in the role of critic—is marked by a homogeneous and flattened quality. There is a radical levelling: the synapse is *only* a construct, he writes; a Realistic interpretation of the synapse as an unobserved but nonetheless real physicochemical system is not only *unnecessary* but *wholly gratuitous*; the pupillary reflex is *nothing more than* an observed stimulus-response relationship; and so on. All phenomena seem to stand on the same level of importance: the *total* behavior of the organism is an *exact* function of incident stimuli. Speaking metaphorically, there is no slack in this tight enterprise, and the determinism which Skinner (1931) defends is of a totalistic sort. Even the literary style of the paper contributes to this atmosphere.[4]

In addition to its critical aims, his 1931 paper involved a visionary agenda concerned with the defense of philosophical determinism. He defended the thesis that the behavior of organisms is characterized by "observed necessity," and he attempted to operationalize "necessity" as equivalent to the fact that "a given response is observed invariably to follow a given stimulus" (Skinner, 1931, p. 446; cf. Scharff, 1982). This is not to say that all behavior is involuntary, since that term is deemed nonempirical, but only that all behavior "depends on" enviornmental events.

Background: Extension of the Reflex

In the first sentence of his 1931 paper, Skinner announced his intention to defend the "extension of the concept of the reflex to the descrip-

[4]The expository style of Skinner's 1931 paper is revealing. The view which he defends is presented in unqualified and bold statements that contain such words as "wholly," "solely," and "only." Words that convey uncertainty or qualification—"probably," "perhaps," "possible"—never qualify the view which he defends but occur in relatively innocuous contexts. The view which he rejects is dismissed as simply metaphysical, unscientific, reactionary, and inevitably overthrown in the course of scientific progress. He says he spent a great deal of time improving the literary qualities of the dissertation from which his article was taken with only minor changes (Skinner, 1979, p. 71).

tion of the behavior of intact organisms" (Skinner, 1931, p. 427). He based this idea on a historical development which had become quite noticeable around the middle of the nineteenth century in physiology and in the life sciences more generally.[5] There were a number of attempts, both experimental and speculative, to establish that portions of the central nervous system above the spinal cord obeyed the principles of the reflex. This extension of the reflex was facilitated by a number of factors, which Robert Young (1970) has detailed, including the later suggestion from evolutionary theory that there is a functional continuity between the primitive, lower functions typical of the spinal cord and the higher, evolutionarily newer functions characteristic of the cerebral cortex of the brain. In truth, this extension of the reflex concept had been carried out experimentally and speculatively ever since sensory and motor localization was demonstrated in the spinal cord in the early-to-middle 1800's. As we have already noted, by the early 1800's, most investigators thought reflexes were delimited to spinal cord functioning, the laboratory study of which had yielded the bulk of evidence concerning reflexes. But in the "progress" of the life sciences in the nineteenth century, this conservative and theologically palatable delimitation of reflexes was gradually weakened.

Most of the research on reflexes was done with decapitated or spinal-cord-injured animals, animals in which surgical intervention precludes the influence of the brain (mind) on spinal cord function. Originally the surgical reduction of the organism to a "reflex preparation" may have been prompted by methodological considerations and matters of convenience. The procedure of severing brain from cord limited the animal's ability to move about, giving the experimenter greater control over the effective stimulation; repeatedly, in methodological refinements, such control was found to be essential for discovering the regularities that are reflex laws (cf. Granit, 1967, chap. 2). Moreover, the procedure dissociated the spinal reflexes from the matrix of "higher controls" and more complex behavior patterns in which it was supposed that reflexes normally existed, though in a form that was obscured by their smooth integration into such larger activity patterns. By means of spinal and other preparations, the experimenter could intensively study reflexes as putative units of more complex behavior and as the expression of correlated anatomical structures in the cord.

[5]"The advance of mechanism" in the life sciences is the global historical construction in which Skinner saw his own research. The idea of such an advance is a large topic, for which we will suggest only a couple of writings that provide helpful orientation: Smith (1973) for the complex philosophical background; Goodfield (1960/1975) for subparts of physiology; Young (1970) for sciences of the central nervous system; Daston (1978) and Jacyna (1981) describe reactions to the alleged implications of mechanism for ethical theory; Gray (1968) more briefly touches upon its manifestations in psychology.

A substantial amount of research was done with spinal preparations throughout the 1800's. By the early 1900's, a number of specialized spinal and brain-stem preparations had been distinguished by the point at which the neuraxis was transected. A different profile of reflexes was found for each preparation, and the profile was "richer" for the higher preparations, those in which progressively more structures beyond the cord were left intact. Moreover, by the late 1800's, advances in localization of sensory and motor functions in cerebral cortex made it plausible to think that the cerebral cortex as a whole was made up of localized sensory and motor "centers," just as in the spinal cord. It would be tempting to think of the intact organism as a kind of limiting point on the upper end of this series of increasingly more complex preparations (e.g., Skinner, 1930a, pp. 59-60), with the low spinal preparation constituting the bottom end of the series.

That notion seems to suggest that the behavior of the intact organism is really a sequence of reflex movements simultaneously and serially compounded into the integrated acts of the organism, a possibility usually called "reflexological." As an alternative to this reflexological hypothesis, Skinner offered a more abstract version, which he called the *generalized reflex hypothesis*: "the behavior of an organism is an exact . . . function of the forces acting upon the organism" (Skinner, 1931, p. 446). He certainly was aware that discoveries which were supportive of this idea had come from the intensive study only of contrived laboratory preparations, and this awareness figured prominently in the development whose background we are sketching. In the case of spinal preparations, it was a long inferential leap from the limited activity which could be provoked by artificial stimulation in these preparations to the reactions of the intact, surgically unhampered organism to objects that make up its natural setting. There simply was no research which closely demonstrated that the complex behavior of the *freely moving organism* was in fact a complicated resultant of the stimulus forces acting through an anatomical network structurally similar to the prototypical reflex arc. Such demonstration would be a prodigious achievement, and Skinner rightly took the hypothesis to be "beyond immediate experimental demonstration" (Skinner, 1931, p. 446).

Those who were committed to the hypothesis engaged in the practice of "extending the reflex principle" either through actual reflexological research or through speculation. Actual reflexological research might involve an empirical demonstration of the stimulating forces which control the components of complex acts, thereby achieving a satisfactory demonstration of the "reflex nature" of these complicated acts considered *in partibus*. The work of Sherrington (1906) and Magnus (1924) on posture probably best exemplified this strategy, and Skinner was well acquainted with their research from coursework and reading. This type of extension required systematic inclusion and exclusion of the "components" of the complicated behavioral sequence. A second case involved conceptual or speculative extension to highly practiced, "automatic" acts whose occurrence and modulation require no deliberation. This case involved a large methodological departure from the reflex concept because one could not appeal to a demonstration of the stimulating forces and of the neuroanatomical pathways involved, which typically were not known in detail. A third and final case involved the hopeful, in-principle extension of the reflex concept to *all* learned behavior (e.g., Meyer, 1911; Smith and Guthrie, 1921; Watson, 1914, 1925). In this, the most speculative type of extension of the reflex, the technical shortcomings present in the second case were further exaggerated. Although extensions of the first type were part of the normal-science process of empirically identifying reflexes in intact organisms, extensions of the second and third type were always a matter of interpretation.

A strategic ploy in the second and third types of extension of the reflex was to redefine "stimulus" to mean an object or entire situation, to enlarge "response" to include complex activities, and to treat the relationship of the activity to the object or situation as like that of a localized muscle movement to its eliciting stimulus in a true reflex (i.e., a reflex newly discovered in the first type of extension; cf. Skinner, 1931, p. 445; Smith and Guthrie, 1921, pp. 39-46; Watson, 1919, pp. 10-13; 1930, pp. 6, 11-19, 41-44). This expanded denotation of "stimulus" and "response" rested on an *assumption* that the terminology of stimulus and response and the results of reflex studies would be extended successfully to the "molar" level through the discovery of a reflexological mechanism—for the present, the mechanism had to be assumed—which constructs the "molar" behavior out of simpler reflexes, as in the chain-reflex hypothesis of Watson (1914) and in the idea that lever-pressing is a chain (Skinner, 1932a, pp. 31-32; 1932b, pp. 38-39); as an alternative, the theorist might treat the molar behavior as itself a reflex, as Watson occasionally did (e.g., Watson, 1930, pp. 41-44), and as Skinner did in treating the complex act of pressing a lever as a reflex which he designated: "lever——press." Both techniques involve an extrapolation. Historically the rub is that at the very time—roughly the teens and twenties of the present century—when the accomplishments of reflex physiology inspired a psychological reflexology, they provided little *direct* confirmation of the latter enterprise.

Skinner's Early Metatheory: Spontaneity and Variability

The background we have sketched, that of reflexology and behaviorism, would seem to cast Sherrington, Pavlov, and Watson as Skinner's intellectual ancestors. This might be so for the 1928-1929 period, but by 1930-1931 other influences were of equal importance. W.J. Crozier was certainly an important figure in Skinner's development during the graduate-school and postdoctoral years (Herrnstein, 1972; Skinner, 1967, 1979). In the writings of both individuals one finds the same vigorously anti-metaphysical stance; the same impatience with "obscure thinking" and "unfortunate mysticism" that underlie "arguments in favor of 'free-will' control" (Crozier and Hoagland, 1934, pp. 90-91); and the same concern with experimentally defending determinism (1) by quantitatively demonstrating "relationships between measured features of performance of an organism and values of a known controlling variable" (Crozier and Hoagland, p. 4), and especially (2) by showing that observed variability in behavior from occasion to occasion is no support for indeterminism. A close reading of Skinner's early papers (e.g., Skinner, 1930b, 1931, 1932a, 1932b) underscores Crozier's importance.

Like Crozier, Skinner saw that two features of behavior serve as *prima facie* evidence against determinism. First of all, and especially in the case of freely moving organisms, behavior often occurs "spontaneously," in the absence of identifiable correlated stimulating forces; this is the problem of spontaneity. Secondly, even in the laboratory, a reflex typically shows noticeable variation from one elicitation to another, despite efforts to assure constancy of stimulating conditions; this is the problem of variability. Skinner's reading in the history of physiology suggested to him that experimentally observed spontaneity and variability had often served as the reason for positing nonphysical agencies to explain whatever behavior exhibited these two features. As a result, the idea that "a given response is observed invariably to follow a given stimulus" (Skinner, 1931, p. 446) often appeared to be false, and one could be tempted into concluding that the concept of reflex was probably inapplicable—not even applicable "in principle"—to most of the observed behavior of organisms.

This problem of variability was of a highly general nature: not only did it bother researchers like Crozier, but it had come to be regarded by many as the Achilles heel of the behaviorism of the 1930's. If we may put the matter crudely and somewhat in caricature, we could say that the early behaviorism of Watson and his kind was so enamored of determinism that its hypothetical organism was a "mindless reflex machine," its movements "rigidly" controlled by the present stimulus circumstances and persisting obstinately in obedience to past regularities (Kitchener, 1977, pp. 13-16). Slogans such as "mechanistic," "reductionistic" and "simplistic" are to be commonly found in the resulting discussion, and representative examples occur in McDougall (e.g., 1923, chap. 2; 1930, pp. 13-15), R.B. Perry (1918, pp. 11-15), and Tolman (e.g., 1920, esp. pp.

217-227; 1925). Though it is difficult to state the underlying assumptions in a way that is technically satisfactory and, at the same time, reflects the philosophically immature discussions of the period, the following version of psychological reflexology will serve for the purpose of exposition:

(a) Complex ("molar") behavior consists of component ("molecular") reflexes linked or associated serially and simultaneously during learning.

(b) Association is simple and automatic, it merely adds parts and does not contribute new properties.

(c) The components are stereotyped in execution, for they are reflexes determined by specific stimuli.

This model implies that a complex learned act has no emergent properties, for it is merely a resultant of the linkage of the simpler behavioral *units* into a chain, as determined by physical properties of the units, such a prepotency, contiguity, number of repetitions, and so on. The Gestalt psychologists and "purposivists" rejected this view of molar behavior. Moreover, according to this reflexological model, what the organism "really learns" is necessarily those specific S-R connections which were actually formed in past experience; and the classic experiments on response equivalence (Lashley and Ball, 1929; Macfarlane, 1930) had successfully attacked that notion. Lashley's (1929) research on cortical correlates of simple habits also led him to repudiate the "switchboard" view of cortical function which was taken for granted in the reflexological camp. In addition, some were to argue that the reflexological model assumed far greater stimulus constancy in the environment than it was ordinarily possible to ensure in experiments on habit formation in freely moving organisms. Others were to emphasize the opposite consideration that, because the reflexological model expected complex learned acts to be relatively stereotyped in their execution from occasion to occasion, observed variability in responding is necessarily an embarrassment to the reflexological model. The embarrassment is precisely the problem of variability we have been considering, and most theorists dealt with the problem in at least a cursory fashion (e.g., Guthrie, 1930, p. 417), some even trying to build in some machinery for a theoretical deduction of the phenomenon of variability (e.g., Hull, 1930, pp. 244-248; 1943, p. 319), without altering their fundamental commitment to reflexological ideas of the sort we have sketched above. In that model, variability is clearly a problem occasioned by a "molecularist" reliance on reflexes as building blocks of "molar acts." It is not unexpected, then, that the problem of variability was keenly felt by the young behaviorist B.F. Skinner, concerned, as he was, to extend the reflex to "molar" behavior.

But as sources of embarrassment to determinism—especially in its form as the reflex formulation—spontaneity and variability are contingent features of behavior: both rest upon "the possibility or the impossibility of the experimental demonstration of stimulating forces" (Skinner, 1931, p. 435; cf. McDougall, 1923, pp. 43-57). That such demonstrations had been a recurrent feature of reflex investigation was the polemical thrust of Skinner's (1931) history of the reflex, which contained several examples of how investigators identified the stimulating forces for behavior that had previously been deemed "spontaneous" and, therefore, the product of volition, mind, or "soul." A continuation of this experimental extension of the reflex into the domain of the soul was to be expected: "it was implicit in the nature of the reflex that it *should*, in the course of its growth, disfranchise volition" (Skinner, 1931, p. 436, emphasis added; see also p. 437; and, more recently, 1971, p. 96). In his 1931 paper, he eventually concluded that it was "difficult to discover any aspect of the behavior of organisms that may not be described with a [reflex] law" (Skinner, 1931, p. 454); his argumentative task in 1931 was to clear the way for the extension of the reflex to the behavior of the freely moving organism by conceptually neutralizing the idea that reflexes are involuntary, unlearned, and unconsciously performed. He regarded these descriptions as "superfluous, metaphysical" interpretations which the reflex had accumulated in its history and which had served as *prima facie* grounds for dismissing the possibility of its continued extension to ever more complex behavior. (This is not the place to examine the adequacy of Skinner's historiography: see Coleman, in press. For the present, we need only note that it treated the extension of the reflex as a progressive and durable historical trend.)

Most fundamental among the reflex laws were the *primary laws* of the reflex, in which a dependent variable measure of a reflex is sampled at different levels of a treatment, such as the intensity of the eliciting stimulus. In the classic treatise on reflexes (e.g., Sherrington, 1906), primary laws of reflex latency, duration, and amplitude had been determined; schematically, they could be represented as $R = f(S)$, where R is a response measure, and S is a quantified characteristic of stimulation, such as its intensity.

Such primary laws express in the language of functional relations the philosophical theme of determinism with which the reflex presumably had always been very closely associated; correlatively, observed variation in these relationships, even when care had been taken to assure the same conditions of stimulation from one occasion to another, had been regarded (again historically) as impeaching the concept of reflex, a point we have already remarked. The problem of variability had been handled in a variety of ways. The most dramatic defense of the reflex was to "discover" the stimuli or antecedent conditions with which the observed variation was correlated, as in the case of Rudolf Magnus's (1924) investigations of the role of proprioceptive stimulation in posture and Pavlov's study of the role of conditioned stimuli in glandular secretion (Pavlov, 1906; 1927/1960, pp. 3-7), as Skinner noted (Skinner, 1931, pp. 436-437).

A somewhat different solution to the problem of variability was to work out the *secondary laws* of the reflex (Skinner, 1931, pp. 451-454). Secondary laws specify quantitatively the dependence of the primary reflex relationships on values of various other factors, collectively referred to as "third variables"; in so doing, they encompass in a higher lawfulness the observed *variability* in the effectiveness of eliciting stimuli. The general expression of a secondary law of the reflex is: $R = f(S,A)$, where A is the third variable (Skinner, 1931, p. 452). Since primary laws of the reflex express the exact conditions of the S-R correlation and amount to an implicit denial of the *spontaneity* of that behavior, the two kinds of reflex laws together handle the problems that spontaneity and variability pose for a deterministic science of behavior.

While the primary laws are important in demonstrating the reflexiveness of some action, the extension of the reflex into the domain of "psychological" explanations depended more on secondary laws. These laws operationally clarify or redefine psychological constructs in terms of the organism's varied but determinate responsivity to physical agencies. For instance, hunger "drive" is to be clarified in terms of the operations, typically abstinence and satiation, that make food more or less effective to elicit eating (e.g., Skinner, 1930b). Drive is therefore a third variable, of which the primary S-R relationships in the organism's eating behavior are themselves a determinable function. The variability that suggested the term "hunger drive" in the first place *should properly be understood simply as variation in the strength of correlated reflexes*, variation which is demonstrably related to specific operations on the relevant independent and third variables (Skinner, 1931, p. 454; 1932a, pp. 33-34).

Even though Skinner described the history of the reflex as one of successful extension of the concept through discovery of eliciting stimuli in the primary reflex laws, the identification of eliciting stimuli for lever-pressing played no role at all in his own research. Skinner's laboratory research resembled Crozier's program of seeking stimulus-behavior correlations in the behavior of the organism-as-a-whole (see reviews of early research in Crozier, 1928, and in Crozier and Hoagland, 1934), rather than on the reflexological thesis that this "molar" behavior was composed of reflexes. In a sense, Skinner bypassed the primary laws, the laws that demonstrate some behavior to be reflexive, and *assumed* that lever-pressing is elicited by the stimulation afforded by the lever. His initial goal was to delineate the group of *secondary laws* which belong under the rubric of "hunger drive" (Skinner, 1930a, 1930b, 1932a, 1932b). In the investigation described in his first single-authorship article, his intention was to determine the time course of the variation in "eating reflexes"—a variation which had prompted unnamed others to appeal to a psychic state of "hunger," he claimed—and thereby to "measure" hunger as a third variable. His critical aim was to preclude explanation of the variability in eating by appeal to a fictitious inner "state of hunger"; by accounting for the variability, he hoped to extend the reflex approach and vindicate determinism. In his research in the early 1930's, he carried out the theme of extension of the reflex by encompassing the psychological topics of drive and satiation, conditioning, extinction, and discrimination as classes of secondary variations in reflex strength (Skinner, 1930b, 1932a, 1932b, 1932c, 1933a, 1933c, 1933d). Skinner's attitude toward psychic or neurological hypothetical states remained positivistic in the period we are examining; he declared for remaining at the level of observation, and defined the reflex as an observed correlation of stimulus and response events.

Skinner's Early Metatheory: Constructing a Behavioral Unit

A definition of the reflex as a correlation of specific *events* must come to terms with the potentially troublesome fact that the quantitative features of the reflex vary with many nondefining properties, that is, with conditions that did not enter into the original definition of reflex. For example, the resting posture of the organism or preparation affects the quantitative relationship, and so does the temperature of the spinal preparation, and a host of other variables as well that are peripheral to the investigator's specific research interests. (From coursework and other reading, Skinner was acquainted with the range of conditions that affect the strength of reflexes.) If these conditions *have* to be specified in the definition in order to guarantee reproducibility of results, the generality of the definition is reduced with each additional qualifier; such a reduction would be distressing to someone who wished to extend the concept of the reflex (Skinner, 1931, p. 427).

To deal with the unwanted importance of nondefining properties, various kinds of experimental restriction had become customary either for excluding the nondefining properties (e.g., surgically) or for holding them constant at some conventional value (cf. Skinner, 1931, pp. 447-448). Unfortunately, although reproducibility is noticeably improved by techniques of restriction, there is no guarantee that different results would not have been found if the investigator had chosen different methods of restriction, methods which singled out some other nondefining properties. An *a priori* justification of the particular chosen methods for achieving reproducibility through restriction would be metaphysically repugnant, of course. In this line of thinking, specific laboratory operations designed to improve reproducibility begin to appear incorrigibly arbitrary (e.g., Israel, 1945; Petrie, 1971, pp. 148-152). Such arbitrariness threatened to undercut the positivistic insistence that scientific laws are impersonal and general.

At this crucial point in the argument of Skinner's 1931 paper, the reader will look in vain for an answer to the implied criticism that operational definitions are arbitrary (Skinner, 1931, pp. 449-451). Instead of answering that criticism, Skinner turned to a related but distinct difficulty: if different operations yield quantitatively different reflexes, then the reflex "parts" of behavior which had been isolated experimentally may not be the same as the actual constituent reflexes within the global activity of the freely moving, intact organism. (This would also be a sensitive issue for Skinner, since he was greatly concerned with extending the reflex from its historical dependence upon the artificially reduced preparations of physiology to the intact, freely moving organism with which a behavioristic psychology is concerned; moreover, reflexology was the model of behavior that he was favoring at the time.) There follows, in his 1931 paper, a very strong denunciation of the assumption that entities (i.e., reflexes) exist apart from laboratory operations, and of the assumption that our observations of reflexes in the laboratory might be thought to approximate the presumptive reflexes in the behavior of the freely moving animal outside the laboratory (Skinner, 1931, pp. 449-450). Skinner treated these assumptions as "wholly gratuitous" (Skinner, 1931, p. 450), perhaps having taken his cue from Sherrington's widely remarked doubt concerning the "reality" of the simple reflex (Sherrington, 1906, p. 7). Moreover, Skinner rejected as pseudoquestions such questions as whether the experimentally isolated reflexes *exist* in the global behavior of freely moving organisms outside the laboratory, even though the reflexologists whom he admired (e.g., Sherrington, Magnus) had carried out just such a reflexological program. His lapse into dismissive rhetoric is quite foreign to the otherwise restrained style of his 1931 paper, and we are inclined to judge the defensively aggressive tone as symptomatic of a disturbing inability to answer the more fundamental issue of arbitrariness. We need not rest on such personal features, for it is also likely that Skinner was expressing "positivistic" reservations about speculative contemporary versions of the reflexological thesis in psychology, for instance, the chain-reflex explanation of serial acts (e.g., Smith and Guthrie, 1921, pp. 100-105; Watson, 1919, pp. 270-273; 1930, pp. 207-210, 219-220; cf. Crozier's disapproving comments on Watson in Skinner, 1979, p. 44). He may even have been affected by a rather general questioning of the reflexological model in American psychology at about that time (Coleman, 1981, pp. 208-211), although that seems less likely, since he was not following the psychology journals closely (Skinner, 1979, p. 34). Moreover, his 1931 paper is a defense of the reflex, not a criticism. Whatever the reason for his outburst against pseudoquestions, the problem of arbitrary operations was left unresolved in his 1931 paper.

Skinner therefore found himself committed to a metatheory which encouraged "restricted preparations" (see below) and dismissed as "pseudo" the kind of questions which would show that the products of such an enterprise are arbitrary, lacking in generalizability. Instead of answering the problem of arbitrariness, Skinner dismissed a Realist interpretation of scientific concepts on the ground that it puts forth pseudoclaims of existence that cannot be justified operationally. The critic had used operationism in his quest for certainty; but he had inadvertently hemmed in the visionary who embraced the generalized reflex hypothesis (Skinner, 1931, p. 446).

Skinner's (1931) operationist position committed him to a Nominalistic definition of behavioral units in terms of very particular (and therefore disturbingly "arbitrary") operations, and to a criterion of exact reproducibility of results for deciding on a behavioral unit. Therefore, in his 1931 paper, Skinner ended up very close to the Nominalist idea "that every possible restricted correlation [of stimulation and behavior] is an independent unit in itself" (Skinner, 1935a, p. 43), because the concept of a reflex "has no scientific meaning apart from its definition in terms of . . . [specific] experimental operations" (Skinner, 1931, p. 450). As he stated near the end of his 1931 paper: "The reflex remains . . . *an* observed correlation" (Skinner, 1931, p. 451, emphasis added). This particularistic definition of stimulus and response relations soon prompted E.G. Boring to remark, in the context of Skinner's treatment of eating as an "ingestive reflex," that: "Not only may you have a sugar-reflex as distinguished from a salt-reflex, but you have a stuffed-olive-reflex as different from an anchovy-reflex" (E.G. Boring to B.F. Skinner, November 4, 1932).[6]

A philosophical Nominalism—with all of its traditional commitment to the particular against the universal, and to the merely conceptual (*flatus vocis*) status of constructs which Realists presume to regard as independently existing "entities"—was the position on behavioral units in which Skinner left himself in his early metatheory of 1931. That this position was eventually unsatisfactory is suggested by the changes he made in his 1935 papers. That it was unsatisfactory even at the time (i.e., 1931) is indicated by the following consideration: Skinner's principal motivation in his 1931 paper was, after all, to defend the extension of the concept of the reflex from reflex physiology to "the behavior of intact organisms" (Skinner, 1931, p. 427). If the concept of reflex had no meaning apart from its Nominalistic definition in terms of the specific features of

[6]Letter in possession of Harvard University Archives; quoted with permission of Professor Skinner.

(admittedly) arbitrary operations, then the concept lacked the breadth or "extensivity" to tolerate variation within the defining operations. Moreover, defined in terms of a particular set of operations, it could not be extended to cases in which admittedly different but analogous operations were used (cf. Boring's remark, above). Even within the spartan framework of his 1931 paper, these consequences were unpalatable, for Skinner had set out to defend the *extension* of the reflex.

A solution to his dilemma—a dilemma he did not explicitly acknowledge in his 1931 paper—emerged not from a conceptual frontal attack, but from unexpected discoveries in his lever-press preparation. He announced his solution in a paper on the generic nature of psychological concepts (Skinner, 1935a); simultaneously, he distinguished his lever-press preparation from Pavlov-type preparations in a paper on two types of conditioned reflex (Skinner, 1935b). Changes utilized in his solution are the subject of the next two sections.

Metatheoretical Change: Response Rate

Skinner's entire approach to dependent variables seems to have come from the physiological literature. He purchased Sherrington's *Integrative Action of the Nervous System* (1906) in the late fall of 1928[7] and would have been reading it in conjunction with Hudson Hoagland's course in physiology at that time as well as in subsequent semesters (Skinner, 1979, pp. 17-18). A year later—March to May of 1930—he was trying to get the scientific publishing houses of Blakiston and of Williams and Wilkins interested in his proposed translation of Rudolf Magnus's *Körperstellung* (1924). His year of formal coursework in physiology (academic year 1928-1929) acquainted him with physiological studies of fatigue, refractory phase, and the like. These latter phenomena were time-dependent and involved kymographic display of change in a recorded response property as a function of repeated elicitations.

Though his recording method went through a number of modifications before it became the cumulative recorder (Skinner, 1956), several of his earlier versions were recognizably of the kymograph family. For instance, the "Parthenon" study, described and sketched in Skinner (1956), involved hand-tracing onto a moving paper strip the distance a rat moved out of a wooden tunnel (the "Parthenon") as a function of time; Skinner studied the inhibition of the rat's movement by a noise of a calibrated loudness. This method resembles the tracing procedure he was using at about the same time in a study of ant locomotion on an inclined plane. The ant study was conducted in the spring of 1929, and the writing was

[7]Professor Skinner allowed me to determine the flyleaf date-of-purchase of several books in his personal library.

completed in the early summer; it was published under the authorship of Barnes and Skinner in 1930 (see also Skinner, 1979, pp. 19-20).

This conceptual and methodological framework for the quantitative description of reflex-like behavior was applied by Skinner to the study of eating. He may have been led to that topic by Curt Richter's (1927) review of studies of hunger and eating sometime in 1929, or by reading Walter Cannon's classic treatise on *Bodily Changes in Pain, Hunger, Fear, and Rage*, which he purchased in late 1929 (see footnote 7). Magnus (1924) had already studied ingestion as a chain of reflexes observable in midbrain preparations, and the swallowing reflex was a standard textbook example of a reflex chain, so Skinner was on conceptually familiar ground in his first single-authorship research publication, a brief report to the National Academy of Sciences in the spring of 1930 (Skinner, 1930b). In his study, food-deprived rats were required to push open a small door in a chamber to get access to a food tray and to remove a food pellet from the tray. The rate of occurrence of this response declined smoothly within roughly a two-hour period, at which point his rats abruptly ceased to press the panel.

Skinner found the quantitative course of decline in this hunger-motivated behavior to be well described by a power function of time, $N = kt^n$, in which N is the cumulative amount of food taken (and eaten). He reported that the exponent n was relatively constant at about 0.7 over a number of conditions which affected the constant k. While the particular value of n was not especially important, its constancy "indicates that it is, in effect . . . the description of a process" (Skinner, 1930b, p. 437), namely the process by which the ingestion of food reduces the facilitating "condition of hunger" on which the momentary "strength of the eating reflexes" depends. Variability does not really impeach the reflex "if the variability is itself lawful" (Skinner, 1931, p. 434), or if the experimenter is able to identify "the antecedent changes with which the activity is correlated . . . and thus establishes . . . the reflex nature of the behavior" (Skinner, 1931, p. 437). Skinner's National Academy paper specified one of the "conditions of elicitation of certain eating reflexes," as the title proclaimed; and thereby not only did he determine the time course of decline in the facilitating condition of hunger under his experimental conditions, but he also achieved an empirical specification of "hunger" which would justify treating it as a "third variable" of which reflex strength is a function, rather than as an immaterial or psychic principle that underlies variability and is contrary to the reflex formulation.[8]

[8]Such a demonstration must have been gratifying, for it constituted an operational clarification of the concept of drive in the manner of Percy Bridgman's *Logic of Modern Physics* (1927), which Skinner had recently purchased (September, 1929) at the urging of his friend, Cuthbert Daniel. Moreover, Skinner's operational clarification of drive vindicated his deeply suspicious regard of mentalistic terms in the vernacular (Skinner, 1979, p. 80; see also Skinner, 1932a, p. 34; 1935a, pp. 58-60).

Skinner's use of a response-rate measure would eventually carry him away from reflexology, but at the time he tried to fit the measure into a reflexological framework. At first, he admitted that, although rate of eating as a measure of the strength of constituent eating reflexes (locomotion, food seizure, chewing, swallowing, etc.) was somewhat unusual in the physiological literature, it was "at least the most convenient measure at hand" (Skinner, 1930b, p. 434). In a publication two years later entitled "Drive and Reflex Strength" (Skinner, 1932a) he offered a reflexological justification, pointing out that as hunger declines, the several reflex components of the complex ingestive act undergo *secondary changes*:

(a) the speed of each component is reduced;
(b) the interval between each component increases;
(c) the interval between separate ingestive acts increases;
(d) the latency of the ingestive act to presented food increases;
(e) external inhibition or distraction of the eating behavior becomes progressively easier to demonstrate.

These are all features of reflexes which could be found in the reports of Sherrington and investigators of other reflex systems. But one could speak of the global "strength" of eating reflexes only if the particular changes noted above correlate significantly. As he had pointed out earlier, the concept of reflex strength is "by intention a description of a group of concurrent changes" (Skinner, 1931, p. 452),[9] and if they do not correlate highly then one cannot use the concept of strength nor can one speak of hunger as a "unitary process" (Skinner, 1931, p. 453). In his "Drive and Reflex Strength" paper, he admitted that it would be difficult experimentally to isolate the component reflexes of the complex ingestion sequence to determine whether the assumed correlations could in fact be demonstrated.

In lieu of such demonstration, he claimed: "A combined measure of several [of these component reflexes] is available . . . in the rate at which the rat eats" (Skinner, 1932a, p. 23, original emphasis removed). Consequently, having made implicit simplifying assumptions about the temporal infrastructure of response rate, he justified his use of this measure as a *quantitative resultant of the secondary values of the reflex components of ingestion*. It seems highly probable that this decision was strengthened, first of all, by his previous discovery of orderly change in rate of eating as a function of time. Moreover, the approximate constancy of the exponent n

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[9]The concept of reflex strength was formalized in his 1931 paper as a purely Nominalistic, Machian abbreviatory construct for correlated changes in several reflex dependent variables (Skinner, 1931, pp. 452-453). As Spence (1966) and Verplanck (1954, pp. 291-292) have noted, the concept of reflex strength is an intervening variable in the sense clarified by Meehl and MacCorquodale (1948). Though Sherrington (1906) had spoken somewhat metaphorically of reflexes as strong or weak, fresh or fatigued, the concept of reflex strength was largely implicit in his treatise and subordinated to ideas about synaptic phenomena.

over experimental conditions not only implied that hunger was indeed a unitary process (as casual use suggests) but, more important, suggested that change in response rate adequately reflected a presumably uniform change in the state of facilitation of the component reflexes which comprise the global ingestive sequence.

Metatheoretical Change: The Conquest of Variability

Skinner's reflexology eventually succumbed to research findings. In his paper entitled "Drive and Reflex Strength: II" (Skinner, 1932b), using the same methodology, he found that if the ingestive act was preceded by an "arbitrary initial reflex" of pressing a lever which dispensed a pellet of food into a food tray, the function describing the decline in rate of lever-pressing over a session was also a power function of time, $N = kt^n$, where N is the number of lever presses, and again the exponent n was 0.7. That this equation was independent of the specific initiating act—for this conclusion, the arbitrariness of lever-pressing was an important property—implied that the equation was quite general, possibly "independent of *whatever* reflex may initiate the behavior" (Skinner, 1932b, p. 46, emphasis added), a conclusion incompatible with reflexology.

To propose, as we do, that this discovery of generality was a decisive one may seem odd at first glance. After all, Skinner (1932b) had simply discovered that no matter which of two rather different responses initiated the eating sequence, the data on satiation described a power function with the same exponent. (He would soon be testing even further the generality of the function with an exercise wheel [reported in Skinner, 1938, pp. 354-356].) But Skinner regarded his discovery as an empirical solution to the problem of variability that had been troubling him as an apparent violation of the metaphysical principle of determinism (see previous discussion, above).

Up through his 1931 paper, Skinner had defended determinism, first of all, by appealing to historically recurrent instances (e.g., Pavlov, Magnus) of the discovery of eliciting stimuli, which yielded *primary* laws of the reflex. Skinner admitted that a rate measure "has only an indirect reference to the detailed behavior of the organism" (Skinner, 1932b, p. 38), and therefore departed from outstanding features of the reflex methodology. However, he had outlined a second tactic, that of encompassing variability under a *secondary* law of the reflex; his power function for satiation of panel-pressing (Skinner, 1930b) exemplified this second tactic. Skinner's (1932b) discovery of an unexpected generality in his power function provided him with a third and more powerful strategy, the notion of a generic behavior unit, a "response class." He had already conceded that his use of unrestrained organisms did not allow him to "control the behavior of the rat adequately enough to insure . . . the elicitation of an invariable response" (Skinner, 1932b, p. 46). And yet the smooth change in response rate which he recorded during a progressive reduction of hunger seemed to be unaffected by the variety of movements of which pressing the lever consisted. As he noted in closing his paper, "We are justified in speaking of *one* initial reflex only because . . . this diversity [in ways of pressing the lever] does not affect the regularity of the recorded behavior," and why this happens is explained by the general validity of the power law: "The change in rate is independent of the precise nature of the initial reflex" (Skinner, 1932b, p. 47).

In effect, *he had neutralized the problem of variability* which he had taken so seriously in his conceptual-historical paper of 1931. Between 1932 and 1935, Skinner went even further, to find that reliable, orderly changes in response rate as a function of several third variables—satiation, conditioning, extinction, distraction, discrimination, etc.—could be demonstrated in apparent, relative independence of the particular properties of the "molecular" constituents of lever-pressing. Moreover, he carried out his extension of the reflex not by assuming that the frontal tactic of discovering eliciting stimuli would continue to be carried out successfully—he had not attempted this strategy in his own research, since he already knew that the eliciting stimuli for pressing were "the visual lever," "the tactile lever," and so on—but by experimentally demonstrating that, within tolerable limits, there *really are* molar regularities ("defining properties") in behavior. Correlatively, some measureable aspects of behavior are irrelevant to the molar regularities which the behavior exhibits. By implication, much of the variability he observed in his preparation was irrelevant to those general laws and, therefore, *no real embarrassment to determinsim*, despite the absence of demonstrated strict necessity.

Metatheoretical Change: A Generic Behavioral Unit

Collectively these discoveries—reinforced no doubt by a host of "influences" that are outside the scope of an overview article—brought about a substantial shift from Nominalism to Realism in Skinner's metatheory. This shift colored Skinner's theoretical work up through *The Behavior of Organisms*. We will touch upon this topic at the end of this article.

Skinner has remarked of his article on "The Generic Nature of the Concepts of Stimulus and Response" (Skinner, 1935a) that it "may be regarded as a sequel" (Skinner, 1935a, p. 63) to his 1931 paper. While that is chronologically and topically true, the statement does little to suggest how extensive a revision the article made in his metatheory of 1931. His 1931 paper had defined a reflex as a correlation of stimulus and response events. Philosophically speaking, Skinner was committed early to an ontology of *events* (Alexander, 1920/1966; Bergson, 1889/1910; Bridgman, 1927, pp. 95-97; Whitehead, 1920, chap. 3 and 4; 1925, pp. 145-164, 171-176; cf. Zemach, 1970; see Skinner, 1976, pp. 280, 319; 1979, pp. 41, 353-354), and therefore had been suspicious of Realist existence-claims for *entities* (e.g., Skinner, 1931, pp. 449-451; see also Skinner, 1932a, pp. 34-35). Since a reflex was an observed coincidence of stimulus and response (Skinner, 1931, pp. 439, 445, 451), his vindication of determinism 1931-style consisted in discovering that more and more behaviors are really reflexive: in each case we demonstrate "necessity," that is, we find that the observed coincidence of S and R events holds under repeated observation (Skinner, 1931, p. 446). Consequently, reproducibility provided the main criterion for a behavioral unit, as we noted above, in our discussion of criteria for a behavioral unit in his 1931 metatheory.

By contrast, the B.F. Skinner of 1935 was not tied to particular events defined by specific, and apparently arbitrary, laboratory operations. A reflex is now to be defined as a correlation of stimulus and response *classes*: "the 'stimulus' and the 'response' entering into a given correlation are not to be identified with particular instances appearing upon some given occasion [as he had claimed in 1931] but with classes of such instances" (Skinner, 1935a, p. 57). The classes of S and R are defined "at a level of restriction marked by the orderliness of changes in the correlation" (Skinner, 1935a, p. 55) when a third variable is experimentally manipulated. That is, exact reproducibility is no longer the criterion for a behavioral unit in Type I preparations such as lever-pressing, but rather we require smoothness of secondary changes (i.e., secondary laws) of the reflex. Exact reproducibility remains as a behavioral criterion in "restricted preparations" such as those of reflex and Pavlovian (Type II) research, where an extension to the behavior of the freely moving, surgically intact organism is not the immediate research objective. Determinism is vindicated in Type I research not by strict replication (i.e., "necessity") but by a less obvious kind of orderliness, and that orderliness is a newly discovered property of behavior. This property—"molar" orderliness despite "molecular" variation—is suppressed from notice by arbitrary procedures that guarantee reproducibility in the restricted preparations favored by "the extreme particularist," by which designation he backhandedly refers to his own metatheory of 1931. Not only had he solved the problem of variability, but he showed that the insistence of early behaviorists on the deterministic model of a "mindless reflex machine" was a red herring: "The reproducibility of non-defining properties is *unimportant*" (Skinner, 1935a, p. 43, emphasis added). We might say that Skinner's (1932b) discovery permitted him to shift away from the reductionist viewpoint embodied in reflexology and to espouse a descriptive emergence contained in the idea of "molar" behaviorism (cf. Holt, 1931, pp. 256-257; Tolman, 1932, chap. 1). Emphasizing yet another philosophical topic, we could say that in his metatheoretical development there was a shift from necessity to lawfulness (cf. Scharff, 1982).

Skinner's (1935a) paper on the generic nature of reflexes justified many of his technical departures from traditional and more restricted ways of studying reflexes (e.g., his use of response rate, unrestrained animals, "response" defined in terms of its consequences, etc.) without his earlier reflexological appeal (Skinner, 1932a). His adoption of a criterion given by "smoothness of curves for secondary processes" (Skinner, 1935a, pp. 56-58) in developing a behavioral unit reduced the importance of identifying and controlling eliciting stimuli, which were necessary for the criterion of reproducibility, according to Skinner. His complex argument for generic concepts of stimulus and response in no way counseled abandonment of the concept of reflex, but only argued against the use of extremely restricted preparations in the study of behavior, a fault which he placed against Pavlovian preparations and which his own methodology presumably avoided (e.g., Skinner, 1935a, pp. 44-45).

Moreover, his more relaxed metatheory of 1935 allowed him to answer questions he had had to rule out earlier as pseudoquestions. For example, he had rejected as meaningless the question whether the reflex is "a unitary mechanism" (Skinner, 1931, p. 450). But in 1935 he was able to conclude that his chosen reflex of lever-press "behaves experimentally as a unitary thing" (Skinner, 1935a, p. 45), since it showed smooth changes in secondary processes despite topographical variation. Finally, though his metatheory of 1931 had involved an enthusiastic embrace of reflexology, the metatheory of 1935 recognized a research enterprise that was sufficiently different from that of physiology (including Pavlovian research) to be demarcated explicitly. Thus his idea of two types of conditioned reflex.

Metatheoretical Change: The Operant as a Realistic Construct

Skinner had been accumulating discoveries which showed that his rat lever-press preparation differed in important ways from the canine salivary preparation reported by Pavlov in his *Conditioned Reflexes* (1927/1960). Skinner had found that his type of conditioning could be accomplished in a single trial (Skinner, 1932c). Despite convincing efforts, he had been unable to demonstrate disinhibition of the extinguished lever-press response in his preparation. As early as 1932, Skinner had published the distinction between his and Pavlov's preparation (Skinner, 1932c). His "two types of CR" paper was submitted to the *Journal of General Psychology* at the same time as his paper on the generic nature of concepts (June

4, 1934), and it formalized a distinction to which he had already become attached.

In his two-types paper, Skinner continued to treat lever-pressing as a reflex, though of a distinct type from Pavlovian (Type II) conditioned reflexes. He was soon to discover that he had not gone far enough in making his distinction of Types I and II (Skinner, 1935b), for his distinction was immediately criticized by Pavlovian researchers Jerzy Konorski and Stefan Miller (Konorski and Miller, 1937). Skinner's reaction was to re-draw the distinction somewhat and, in the process, to postulate the existence of operant behavior (Skinner, 1937). Elsewhere we have argued that the concept of the operant was intended to defend Skinner's distinction of two types of conditioning (Coleman, 1981). Here we will simply call attention to the manner in which the notion of operant behavior reflected Skinner's metatheoretical change from Nominalism to Realism, the change we emphasize in the present paper.

Depending on just how one takes the apparently ontological claim that "there is also a kind of response which occurs spontaneously" (Skinner, 1937, p. 274), and depending on how one reads his carefully worded expressions of the idea that "there is no external eliciting stimulus" for operant behavior (cf. Skinner, 1938, pp. 19-21), an interesting set of possible interpretations arises. One might take a Nominalist-Instrumentalist interpretation of the concept of operant, in the manner of Skinner's (1931) earliest opinion on scientific constructs, and sidestep questions of the *existence* of operant behavior by treating "operant" as merely a "conceptual expression" (Skinner, 1931, p. 443) which ties together the classes of behavior he and other psychologists had studied in contradistinction to behavior studied by physiologists (Skinner, 1938, pp. 422-424). Such a move was certainly available to Skinner in 1931, for he had done precisely this sort of thing in his Nominalistic treatment of the synapse as a "conceptual expression for the conditions of correlation of a stimulus and response" (Skinner, 1931, p. 443). Moreover, he had evidence to support the more easily defended, Nominalistic version of the idea that "operants lack eliciting stimuli": he had found that his Type R (Type I in his 1935 distinction of Types I and II) lever-press preparation yielded results which diverged from Pavlovian, Type S phenomena (Type II in 1935).[10] If Type S preparations define "eliciting stimulus" operations, and if "All conditioned reflexes of Type R are *by definition* operants"

[10]For example, he had found that the *latency* of discriminated lever pressing (Type R) does not change during extinction, while Pavlov had reported that latency of the conditioned salivary reflex (Type S) increases in extinction. These and other data which show that the same or analogous dependent variable measures do not show comparable changes under the same treatment in Types S and R preparations are to be found in *The Behavior of Organisms* (1938, pp. 239-241, 328-338). We have discussed at greater length the character of Skinner's (1937) distinction of operants and respondents (Coleman, 1981, pp. 222-224)

(Skinner, 1937, p. 274, emphasis added), then the empirical divergence of Types R and S indeed indicates that "operants lack eliciting stimuli." That conclusion, however, extends only to the experimentation that grounds these limited definitions of "eliciting stimuli" and "operant," and therefore can only be given a Nominalistic interpretation as conceptual shorthand for all these laboratory findings. Moreover, he had conducted no experimentation to determine whether his Type R behavior was conditionable under Type S manipulations, and the resulting Nominalistic conclusion could itself be only provisional. It seems implausible, therefore, that Skinner (1937) endorsed a Nominalistic interpretation of the concept of the operant.

By 1937 Skinner had moved so far away from his earlier Nominalist suspicion of existence-claims that a Realistic interpretation of the concept of operant—that is, that there is a class of behaviors which has the property of occurring spontaneously, in the absence of eliciting stimuli—seems quite a bit more plausible. A Realistic interpretation of operants goes beyond actual experimentation in regarding operant behavior as a more extensive behavioral class which includes not only the behavior studied in Type R preparations but in addition "the greater part of the conditioned behavior of the adult organism" (Skinner, 1938, p. 19).[11] A Realist's interpretation proposes that an operant is spontaneous in the sense that *there are no originating environmental forces in its occurrence.* The claim that operant behavior is spontaneous is an unverifiable negative existential assertion, certainly not the kind of claim to which the B.F. Skinner of 1931 would readily have assented. (This disparity is not surprising: his youthful history of the extension of the reflex had been a heroic story of "the experimental demonstration of stimulating forces" [Skinner, 1931, p. 435] which shows each time that yet another behavior is really a reflex rather than a spontaneous product of the soul or mind. But his own research involved no such demonstration of eliciting stimuli and concomitant elucidation of primary laws of the lever-press reflex. So, the operant represents minimally a later adjustment of Skinner's concepts to the actual character of his laboratory practices. Ontologically, of course, it goes beyond serving merely as a Nominalist "marker" for a set of treatment-behavior relationships, which is the claim we have professed.) The

[11]In 1931 Skinner fulminated against questions concerning behavior *outside* the laboratory and in which the term reflex is used; the questions were ruled out as pseudo because the term "reflex" had meaning only in virtue of operations *inside* the laboratory (Skinner, 1931, pp. 448-450) and could not figure meaningfully in question about events taking place outside the laboratory. But beginning in the mid-1930's, Skinner claimed of his Type I conditioned reflexes that this was the category for most of the adaptive, learned behavior of the adult organism *outside* the laboratory (Skinner, 1935b, p. 75; 1938, pp. 19, 45, 115). It is obvious that his early epistemological caution had given way to more expansive pronouncements.

development we have described, especially his leaning toward a Realist scientific ontology, also helps to make sense of some curious aspects of *The Behavior of Organisms*, and we will close this overview with a brief, illustrative look at this topic.

Illustrations

In *The Behavior of Organisms*, one finds a "system," ostensibly patterned after physical chemistry (Skinner, 1938, pp. 434-435; see also Skinner, 1979, pp. 100-101). The applicability of "system" in the physical-chemistry sense depends on (a) the successful isolation of the system from extraneous influences and (b) the assumption that a measured effect (output) is quantitatively determined by the values of the relevant variables (input) that may independently be altered.[12] But the determinism which a system exemplifies is of a rather "loose" sort—thermodynamics serves as the best-known instance of this feature (Skinner, 1938, p. 432)—in the sense that micro-processes which mediate the input-output regularities may take any of a variety of concrete forms, so long as their operation contributes to the "molar" regularities. As a result, the theorist has a great deal of latitude in making simplifying assumptions, idealizations, and pictorial models of "molecular" events which mediate "molar" regularities. The flavor of the explanatory enterprise of 1938 is entirely different from the reflexology which Skinner favored in the early 1930's. In place of a homogeneous, "mechanical," one-level determinism, there is an expansiveness, a tolerance of heterogeneity in mediation, and a more focused effort to integrate quantitative molar regularities. These are attitudes appropriate to a theorist, and we could denominate here yet another shift which Skinner exhibited from 1930 to 1938, a shift from Machian researcher to inventive theorist. Since this change may represent more of a biographical role shift than a change in metatheory, it is enough simply to take note of it. We will move on to a consideration of the "reflex reserve" as an illustration of Skinner's metatheoretical development.

The reflex reserve in *The Behavior of Organism* is a complex idea, which

[12]From his very first articles, Skinner followed these conditions as precepts for success. He took steps to ensure that his animal subjects were free of extraneous influences, the control of which Pavlov had shown to be essential in obtaining orderly data (see Skinner, 1932a, p. 26; 1938, pp. 26, 55-57; 1956; 1979, p. 88). Secondly, he showed an interest in quantitative relationships, probably following Crozier's research strategy (Skinner, 1930b; 1956; 1979, pp. 58-60). In unpublished records dating to the 1929-1930 period, there are a number of trial efforts to locate quantitative relationships of behavior and treatment, in which it is fairly clear that Skinner was searching for almost *any* kind of regularity, as his own account indicates (Skinner, 1956, pp. 224-225; 1979, pp. 54-56, 59-60). I thank Professor Skinner and the Harvard University Archives for allowing me to examine these laboratory records.

reflected in part Skinner's earlier metatheoretical tendency to regard a scientific term as "merely a construct"; it also reflected aspects of Skinner's subsequent metatheoretical development from 1930 to 1938; the incompatibility of these two viewpoints probably underlies some of the conceptual ambiguity in his 1938 theory (Verplanck, 1954, pp. 279-282, 291-294). Following his earlier metatheory, Skinner called the reserve "a convenient way of representing" the behavioral results of operations, and thus "no local or physiological properties are assigned to it" (Skinner, 1938, p. 26). As a mere "convenient expression," the reserve was just a step beyond the standard physiological practice of fitting idealized functions to data points, a practice in which he had engaged in his first scientific publication (Barnes and Skinner, 1930). But his subsequent research on extinction of lever-pressing gave him the needed material for working out the further notion of a "reservoir of responses." In his first paper on the rate of extinction (Skinner, 1933a), he used a logarithmic ($N = k \log t$) extinction "envelope"; an envelope is an idealized upper limit of the cumulative record of extinction, not a best-fit curve (see the critical discussion in Verplanck, 1954, p. 280).[13] In a paper of the same year and devoted to resistance to extinction, he used a specific numerical aspect of the total number of responses in extinction as "a proper measure of the amount of conditioning" (Skinner, 1933d, p. 427, original emphasis removed). He was soon to find that extinction drive level altered the momentary response rate but not the total number of responses that would be emitted in extinction, suggesting a fixed reservoir of behavior (a constant number of responses) resulting from each reinforcement. The decisive experiment was probably his paper on disinhibition in his rats by introducing various disturbances during extinction. Using again the earlier assumption that response rate in extinction should not exceed its envelope (Skinner, 1936, p. 130), he reasoned from his failure to observe disinhibition that "Extinction . . . [is] the mere exhaustion of the effect of conditioning" (Skinner, 1936, p. 129), which implied that the Pavlovian construct of inhibition is not required for an account of extinction, and therefore ought to be eliminated.

This development culminated in the idea of a reservoir of "total available activity" to which reinforcement added responses and extinction removed them (i.e., failed to replace them as they were "used up" in responding). "Momentary strength is proportional to the reserve and therefore an available direct measure" of the reserve (Skinner, 1938, p.

[13]In the early use of the envelope, Skinner admitted the possibility of overshooting the envelope (Skinner, 1933a, p. 124, Figure D), a possibility that must be allowed in a mere graphic convention. In his later exposition of the same data, that possibility was not permitted (Skinner, 1938, pp. 74-78), which suggests that the envelope had become more than just a convention and served instead as an upper limit for an assumed process. See the discussion of this matter in Verplanck (1954, pp. 280, 292-293).

26). Momentary rates generated from this reserve were also affected by variables which did not affect the number of responses in the reserve (i.e., "learning") but only the proportionality of that number to the momentary rate (i.e., performance).

The construct of reflex reserve allowed Skinner to encompass in a simple semi-quantitative model a large number of experimental data. For instance, he was able to explain that the constant response rate which develops during FI schedules is the result of the fusion of separate response envelopes that are generated by successive reinforcements in the reflex reserve, such that any segment of the curve will "apparently contain the responses remaining to the preceding curves" (Skinner, 1938, pp. 117-118). The explanation was a *tour de force*, strengthened by the display of data (Skinner, 1938, p. 118, Figure 28) that are consistent with the interpretation. In the exposition he cogently argued for the existence of responses that *would have appeared*.

Though Skinner *could still have used* the reflex reserve as a convenient and economical way of integrating data, occasionally the reserve was assigned properties that go beyond even a rather elaborate summarizing role, at which point it clearly became a hypothetical model of the sort that he later castigated as "theory" (Skinner, 1950).[14] For example, he discovered that stimulant drugs such as caffeine and benzedrine enormously elevated response rate in extinction, independently of food motivation, a result which severely embarrassed the idea of an economy of responses in the reflex reserve. To say that "the drug . . . multiplies the responses existing in the reserve . . . is little more than a figure of speech," Skinner admitted (Skinner, 1938, p. 416); but so is the notion of "strain" on the reserve, to which he appealed elsewhere (Skinner, 1938, pp. 293ff). These matters have been discussed accurately by Verplanck (1954, e.g., p. 293).

Our illustrations suggest that portions of *The Behavior of Organisms* reflect the uneasy juxtaposition of Skinner's enduring positivistic caution which we traced back to around 1931 along with his more tolerant and expansive Realist metatheory that was first published in 1935. A more thorough historical analysis of his book awaits another occasion, and the same should be said for a study of personal, social, and other biographical sources of the conceptual shifts we have described. Perhaps this overview will prompt investigation of these and related topics.

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[14]For example: the same *minimal* integrative/economizing role is served by the concepts of reflex reserve and synapse. But while the synapse had received a pointedly Nominalistic treatment by Skinner in 1931, the reserve had a more Realistic and, at the same time, more ambiguous status in 1938 (e.g., Skinner, 1938, pp. 420-423). The synapse, too, was given at this time a much more Realistic treatment in Skinner's idea of a "structural neurology" in contradistinction to a "conceptual neurology" (Skinner, 1938, pp. 418-423). In the earlier treatment of the synapse there was a marked reluctance to go beyond the idea that the synapse is *only* a construct (see Skinner, 1931, p. 443).

References

Alexander, S.H. (1966). *Space, time, and deity.* New York: Dover. (Original work published 1920)

Barnes, T.C., and Skinner, B.F. (1930). The progressive increase in the geotropic response of the ant *Aphaenogaster. Journal of General Psychology,* 4, 102-112.

Bergson, H. (1910). *Time and free will: An essay on the immediate data of consciousness* (F.L. Pogson, Trans.). London: Allen and Unwin. (Original work published in French 1889)

Bridgman, P. (1927). *The logic of modern physics.* New York: Macmillan.

Cannon, W.B. (1915). *Bodily changes in pain, hunger, fear and rage.* New York: Appleton-Century.

Coleman, S.R. (1981). Historical context and systematic functions of the concept of the operant. *Behaviorism,* 9, 207-226.

Coleman, S.R. (in press). When historians disagree: B.F. Skinner and E.G. Boring, 1930. *Psychological Record.*

Crozier, W.J. (1928). Tropisms. *Journal of General Psychology,* 1, 213-238.

Crozier, W.J., and Hoagland, H. (1934). The study of living organisms. In C. Murchison (Ed.), *A handbook of general experimental psychology* (pp. 3-108). Worcester, Massachusetts: Clark University Press.

Daston, L.J. (1978). British responses to psycho-physiology, 1860-1900. *Isis,* 69, 192-208.

Descartes, R. (1967). The passions of the soul. In E.S. Haldane and G.R.T. Ross (Eds. and Trans.), *The philosophical works of Descartes* (2 vols.) (Vol. 1, pp. 331-427). Cambridge: Cambridge University Press. (Original work published 1649)

Goodfield, J. (1975). *The growth of scientific physiology.* New York: Arno. (Original work published 1960)

Granit, R. (1967). *Charles Scott Sherrington. An appraisal.* Garden City, New York: Doubleday.

Gray, P.H. (1968). Prerequisite to an analysis of behaviorism: The conscious automaton theory from Spalding to William James. *Journal of the History of the Behavioral Sciences,* 4, 365-376.

Guthrie, E.R. (1930). Conditioning as a principle of learning. *Psychological Review,* 37, 412-428.

Hall, M. (1833). On the reflex function of the medulla oblongata and medulla spinalis. *Philosophical Transactions of the Royal Society of London,* 123, 635-665.

Herrnstein, R. (1972). Nature as nurture: Behaviorism and the instinct doctrine. *Behaviorism,* 1, 23-52.

Holt, E.B. (1931). *Animal drive and the learning process.* New York: Holt.

Hull, C.L. (1930). Knowledge and purpose as habit mechanisms. *Psychological Review,* 37, 511-525.

Hull, C.L. (1943). *Principles of behavior.* New York: Appleton-Century-Crofts.

Israel, H.E. (1945). Two difficulties in operational thinking. *Psychological Review,* 52, 260-261.

Jacyna, L.S. (1981). The physiology of mind, the unity of nature, and the moral order in Victorian thought. *British Journal for the History of Science,* 14, 109-132.

Kitchener, R.F. (1977). Behavior and behaviorism. *Behaviorism,* 5, 11-71.

Konorski, J., and Miller, S. (1937). On two types of conditioned reflex. *Journal of General Psychology,* 16, 264-272.

Lashley, K.S. (1929). *Brain mechanisms and intelligence: A quantitative study of injuries to the brain.* Chicago: University of Chicago Press.

Lashley, K.S., and Ball, J. (1929). Spinal conduction and kinesthetic sensitivity in the maze habit. *Journal of Comparative Psychology,* 9, 71-105.

Leys, R. (1980). Background to the reflex controversy: William Alison and the doctrine of sympathy. In W. Coleman and C. Limoges (Eds.), *Studies in history of biology* (Vol. 4, pp. 1-66). Baltimore: Johns Hopkins University Press.

Macfarlane, D.A. (1930). The role of kinesthesis in maze learning. *University of California Publications in Psychology,* 4, 277-305.

Magnus, R. (1924). *Körperstellung.* Berlin: Springer.
McDougall, W. (1923). *Outline of psychology.* New York: Scribner's.
McDougall, W. (1930). The hormic psychology. In C. Murchison (Ed.), *Psychologies of 1930* (pp. 3-36). Worcester, Massachusetts: Clark University Press.
Meehl, P., and MacCorquodale, K. (1948). On a distinction between hypothetical constructs and intervening variables. *Psychological Review,* 55, 95-107.
Meyer, M. (1911). *The fundamental laws of human behavior.* Boston: Badger.
Pavlov, I.P. (1906). The scientific investigation of the psychical faculties or processes in the higher animals. *Science,* 24, 613-619.
Pavlov, I.P. (1960). *Conditioned reflexes* (G.V. Anrep, Ed. and Trans.). New York: Dover. (Original work published 1927)
Perry, R.B. (1918). Docility and purposiveness. *Psychological Review,* 21, 1-20.
Petrie, H. (1971). A dogma of operationalism in the social sciences. *Philosophy of the Social Sciences,* 1, 145-160.
Richter, C.P. (1927). Animal behavior and internal drives. *Quarterly Review of Biology,* 2, 307-343.
Ryle, G. (1949). *The concept of mind.* London: Hutchinson.
Scharff, J. (1982). Skinner's concept of the operant: From necessitarian to probabilistic causality. *Behaviorism,* 10, 45-54.
Sherrington, C.S. (1906). *The integrative action of the nervous system.* New Haven: Yale University Press.
Skinner, B.F. (1930a). The concept of the reflex in the description of behavior. Unpublished doctoral dissertation. Cambridge, Massachusetts: Harvard University.
Skinner, B.F. (1930b). On the conditions of elicitation of certain eating reflexes. *Proceedings of the National Academy of Sciences,* 16, 433-438.
Skinner, B.F. (1931). The concept of the reflex in the description of behavior. *Journal of General Psychology,* 5, 427-458.
Skinner, B.F. (1932a). Drive and reflex strength. *Journal of General Psychology,* 6, 22-37.
Skinner, B.F. (1932b). Drive and reflex strength: II. *Journal of General Psychology,* 6, 38-48.
Skinner, B.F. (1932c). On the rate of formation of a conditioned reflex. *Journal of General Psychology,* 7, 274-286.
Skinner, B.F. (1933a). On the rate of formation of a conditioned reflex. *Journal of General Psychology,* 8, 114-129.
Skinner, B.F. (1933b). The measurement of "spontaneous acitivity." *Journal of General Psychology,* 9, 3-23.
Skinner, B.F. (1933c). The rate of establishment of a discrimination. *Journal of General Psychology,* 9, 302-350.
Skinner, B.F. (1933d). "Resistance to extinction" in the process of conditioning. *Journal of General Psychology,* 9, 420-429.
Skinner, B.F. (1935a). The generic nature of the concepts of stimulus and response. *Journal of General Psychology,* 12, 40-65.
Skinner, B.F. (1935b). Two types of conditioned reflex and a pseudo type. *Journal of General Psychology,* 12, 66-77.
Skinner, B.F. (1936). A failure to obtain "disinhibition." *Journal of General Psychology,* 14, 127-135.
Skinner, B.F. (1937). Two types of conditioned reflex: A reply to Konorski and Miller. *Journal of General Psychology,* 16, 272-279.
Skinner, B.F. (1938). *The behavior of organisms.* New York: Appleton-Century-Crofts.
Skinner, B.F. (1956). A case history in scientific method. *American Psychologist,* 11, 221-233.
Skinner, B.F. (1967). (An autobiography). In E.G. Boring and G. Lindzey (Eds.), *A history of psychology in autobiography* (Vol. 5, pp. 387-413). New York: Appleton-Century-Crofts.
Skinner, B.F. (1971). *Beyond freedom and dignity.* New York: Knopf.
Skinner, B.F. (1976). *Particulars of my life.* New York: Knopf.
Skinner, B.F. (1979). *The shaping of a behaviorist.* New York: Knopf.

Smith, R. (1973). The background of physiological psychology in natural philosophy. *History of Science, 11*, 75-123.
Smith, S.S., and Guthrie, E.R. (1921). *General psychology in terms of behavior.* New York: Appleton.
Spence, K.W. (1966). Foreword to the seventh printing. In C.L. Hull, *Principles of behavior. An introduction to behavior theory* (paperback ed., pp. vii-xvii). New York: Appleton-Century-Crofts. (Original work published 1943)
Stirling, W. (1966). *Some apostles of physiology* (facsimile reprint). London: Dawson at Pall Mall. (Original work published 1902)
Tolman, E.C. (1920). Instinct and purpose. *Psychological Review, 27*, 217-233.
Tolman, E.C. (1925). Purpose and cognition: The determiners of animal learning. *Psychological Review, 32*, 285-297.
Tolman, E.C. (1932). *Purposive behavior in animals and men.* New York: Century.
Verplanck, W.S. (1954). Burrhus F. Skinner. In W.K. Estes et al., *Modern learning theory* (pp. 267-316). New York: Appleton-Century-Crofts.
Watson, J.B. (1914). *Behavior. An introduction to comparative psychology.* New York: Holt.
Watson, J.B. (1919). *Psychology from the standpoint of a behaviorist.* Philadelphia: Lippincott.
Watson, J.B. (1925). *Behaviorism.* New York: Norton.
Watson, J.B. (1930). *Behaviorism* (rev. ed.). Chicago: University of Chicago Press.
Whitehead, A.N. (1920). *The concept of nature.* Cambridge: Cambridge University Press.
Whitehead, A.N. (1925). *Science and the modern world.* New York: Macmillan.
Young, R. (1970). *Mind, brain and adaptation in the nineteenth century.* New York: Oxford University Press.
Zemach, E.M. (1970). Four ontologies. *Journal of Philosophy, 67*, 231-247.