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NATURE AS NURTURE: BEHAVIORISM AND THE INSTINCT DOCTRINE

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The main goal of this paper is to show how, in the study of action, contemporary American environmentalism absorbed intact the key concept of the nativism it displaced in the period between 1920-1935. The nature-nurture antagonism has been enacted many times in the history of psychology, in many guises. In the study of sensation and perception, of knowledge, of general mental capacity, of chronological development, of language, of individual differences in any of the foregoing, the historian can find the competing claims of empiricists and nativists, often in repeating cycles of intellectual fashion. This paper focuses, however, on just one of the many disputed areas—the study of action or behavior—and further limits itself geographically and chronologically to America in the period roughly between the two World Wars.

This instance of the nature-nurture polarity beckons historical study, first of all, because of its relative clarity and definiteness. A second reason for study is that there are unmistakable signs of the return trip of the pendulum towards the nativist side. Contemporary findings and theories in the study of cognitive development, language, visual space perception, schizophrenia, human learning ability, and the growing popularity of ethological research epitomize the shifting bias, away from environmentalist and towards nativist accounts. In the case of the analysis of behavior, however, the return trip may be unexpectedly short, for it can be shown that there was substantially less actual movement of the pendulum when behaviorism prevailed over the instinct doctrine in the early 1920's.

In the space of just a few years, the established instinct theories of behavior largely vanished, yielding to the onslaught of American behaviorism, with its ostensibly radical environmentalism. The two schools at times confronted each other literally, as when the instinct theorist, William McDougall debated, in 1924, with John B. Watson, the founder of behaviorism as a self-conscious school

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(Watson and McDougall, 1928; Herrnstein, 1969), on the subject of behaviorism. But not much of the story of how the pendulum swung can be told in actual confrontation. Instead, the swing seems to have passed through three, more or less distinct, regions, corresponding to the three main sections of this paper. First, the concept of instinct went through several transformations, making it progressively less nativistic, or, at least, less clearly anti-empiricist. Second, the concept of instinct became the target of intense criticism, by social scientists and philosophers, and rapidly fell into disrepute and disuse in American psychology. Third, the environmentalist theory of behavior that next became predominant included a principle of drive, or motivation, that embodied the final form taken by the instinct doctrine, although disguised in a new vocabulary. A comparison of McDougall's theory of instinct and Skinner's reinforcement theory— representing nature and nurture—shows remarkable, and largely unrecognized, similarities between the contending sides in the nature-nurture dispute as applied to the analysis of behavior.

Instinct in Retrospect

For a few years around 1920, there was an outburst of highly critical articles on the subject of instinct. Thereafter, the subject took a sharp turn toward obscurity in the American psychological literature, judging from the steep decline in the number of times the previously popular word "instinct" turned up in the titles of articles. The decline went hand in hand with a rising use of motivational terms, as shown by the rough reciprocal relation between the word "instinct," on the one hand, and, on the other, the words "drive," "reinforcement," and "motivation," as they turned up in the titles of psychological publications. The first book with the word "motivation" in its title seems to have been published in 1928 (Troland, 1928). Figure 1 was constructed from the annual indexes of the *Psychological Abstracts*, starting with Volume 1 (1927). Of the 113 listings entered under the four words between 1927 and 1930, inclusive, 68% were under "instinct." By 1955-58, the total had swelled to over 460, while entries under instinct had dwindled to 8%. Since the sharpest attack on instinct was prior to 1927, it is likely that the 68% is already depressed below the level one might have seen in, for example, 1915, when "instinct" was booming. The practices of indexers cannot, of course, replace intellectual analysis, but Figure 1 at least supports the claim that psychologists needed the modern vocabulary of motivational theory only after instinct had been sullied by the taint of intellectual scandal.

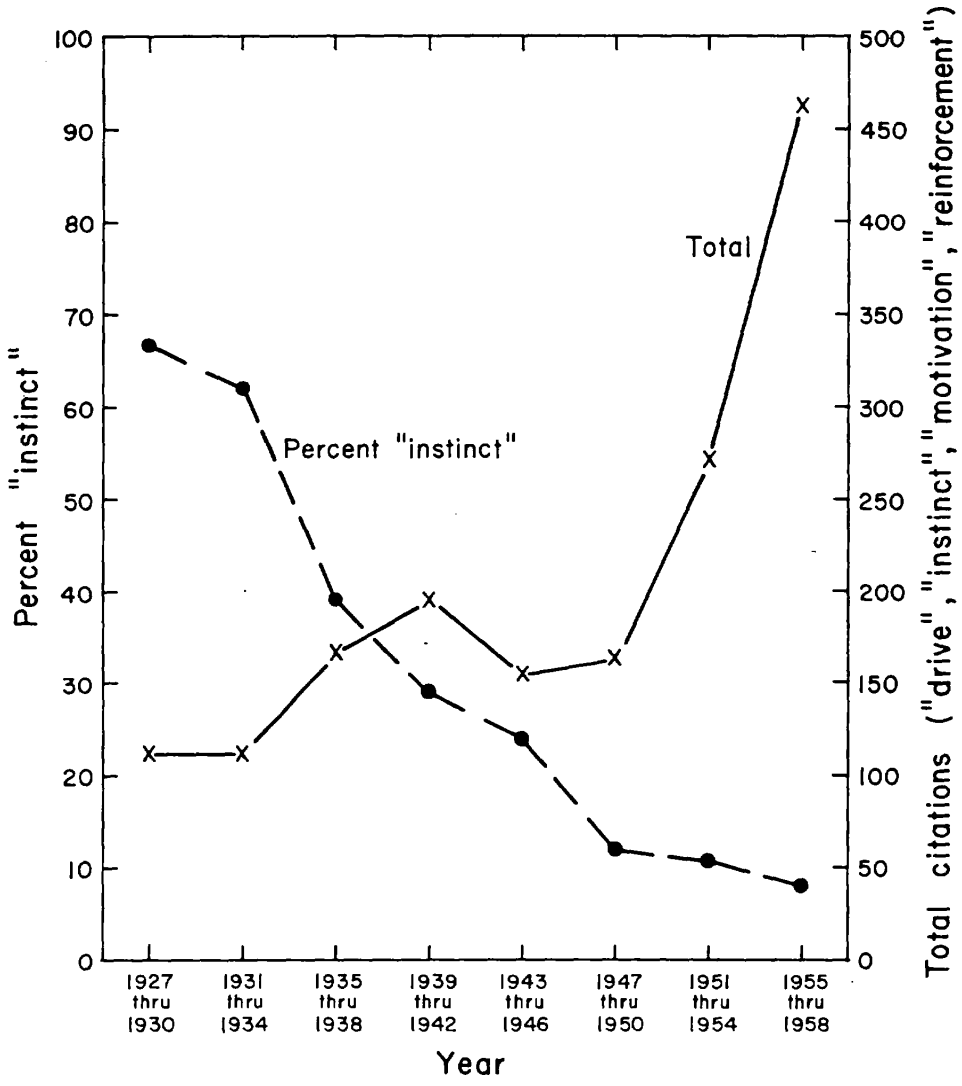

Figure 1. The number of entries in the *Psychological Abstracts* under the words "drive," "instinct," "motivation," and "reinforcement" are shown for successive four-year periods by the solid line. Of these, the percent under just the word "instinct" are shown by the dashed line.

First, what is instinct? A short or single answer should not be sought to such a question, for our intellectual ancestors bequeathed us neither clarity nor consistency. Early in the game (1895),[1] Lloyd Morgan took the trouble of assembling the multiple meanings of the term, then summarized them in a single, overwhelming paragraph:

> Instinctive activities are unconscious (Claus), non-mental (Calderwood), incipiently conscious (Spencer), distinguished by the presence of consciousness (Romanes), accompanied by emotions in the mind (Wundt), involve connate ideas and inherited knowledge (Spalding); synonymous with impulsive activities (James), to be distinguished from those involving impulse proper (Höffding, Marshall); not yet voluntary (Spencer), no longer voluntary (Lewes), never involuntary (Wundt); due to natural selection only (Weismann), to lapsed intelligence (Lewes, Schneider, Wundt), to both (Darwin, Romanes); to be distinguished from individually-acquired habits (Darwin, Romanes, Sully, and others), inclusive thereof (Wundt); at a minimum in man (Darwin, Romanes), at a maximum in man (James); essentially congenital (Romanes), inclusive of individually-acquired modifications through intelligence (Darwin, Romanes, Wallace).[2]

Out of this jumble, several key antitheses—always between instinct and something else—may be extracted to highlight the main parameters of the concept and the range over which it could plausibly vary. There was, first, the problem of instinct versus habit. Since both instinct and habit suggest some degree of fixity of action, can they be distinguished? Wilhelm Wundt (1894), the leading figure in 19th century continental psychology, did not think it was worth doing so: "Any movement that has become altogether habitual is made instinctively" (p. 395). When we laugh, weep, walk, write to dictation, play the piano, we may, according to Wundt, be acting as instinctively as when we sneeze, cough, or flinch. Instinct for Wundt was mechanized action, whether inherited like the tendency to sneeze or acquired like a well-drilled piece on the piano. For most theorists, however, the distinction between habit and instinct inescapably

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[1] Early, that is, in the post-Darwinian era. The really early, pre-Darwinian usages will be only briefly and sketchily dealt with hear. See Diamond (1971) for a discussion of the early history of the concept of instinct.

[2] No effort has been made to chase down the references Morgan cites for the authors he mentions, so that I cannot vouch for his accuracy. Moreover, by modern bibliographic conventions, his citations are often inadequate. Nevertheless, for what they are worth, here are his references, omissions and all: Claus. *Textbook of Zoology*. Calderwood. *Evolution, and Man's Place in Nature*. Spencer, H. *Principles of Psychology*. Romanes, G. J., *Mental Evolution in Animals*. Wundt, W. *Lectures on Human and Animal Psychology*. Spalding, D. "Instinct and acquisition," in *Nature*, vol. xii. James, W. *Principles of Psychology*. Höffding. *Outlines of Psychology*. Marshall, H. R., *Pain, Pleasure, and Aesthetics*. Lewes, G. H., "Problems of life and mind," in *Instinct*. Weismann. Essays, 1889. Schneider. *Der Thierische Wille*. Darwin, C. *Descent of Man*. Darwin, C. *Origin of Species*. Sully. *The Human Mind*. Wallace, A. R., *Darwinism*.

focussed on inheritance. Not that Wundt neglected inheritance, for he contrasted inborn and acquired instincts, just that he found enough in common between the two that he chose to enter them in his system under a single rubric. But Wundt was unusual, if not unique, in this respect. More typical was Lloyd Morgan, the British comparative psychologist, who, in a book (1896) based on lectures given in the United States, made it abundantly clear that habits and instincts were to be distinguished on the basis of innateness, even though he knew that the threads of inheritance and acquisition were woven into the seamless fabric of animal behavior.

The antithesis between inheritance and habit was, however, not quite the same as that between instincts and reflexes. This second antithesis asked whether all inherited forms of action are reflexes or whether instincts were at the same time inherited *and* different from reflexes. For Wundt (1894), even inherited instincts differed from reflexes, while for Lloyd Morgan (1912), instincts were just "complex reflexes, constituting adaptive behavior of the organism" (p. 56). Later on, as we shall see, the instinct-reflex antithesis led to modern motivational theory, but that tortuous path is traced later.

Finally, leaving aside inheritance and fixity, in the third antithesis instinct was contrasted with intelligence, especially after Darwin. The new belief in the continuity of mental evolution naturally directed attention to the adaptive behavior of animals. Was such behavior the sign of ratiocination in the animal or of some other, "instinctive," sort of adjustment? The struggle to clarify the relative contributions of deliberate reason, on the one hand, and something else, on the other, was doubtless the most elusive antithesis of all, though perhaps also the most revealing. To contrast instinct and intelligence meant facing the weighty issues of rationality, adaptation, the role of experience, the alternatives to reasoning as a basis for adaptation, and, finally, of how to gather scientific evidence to bear on such matters.

There was, by 1910, a sizeable literature about creatures doing something as adaptive and well suited to their goals as, for example, the golfer who carefully assesses the lay of the green before putting. But a digger wasp who evidently takes the measure of its paralyzed victim before adjusting the hole into which the prey will be deposited gets little credit for foresight. The golfer, the wasp, and the antithesis between intelligence and instinct absorbed the attention of C. S. Myers, C. Lloyd Morgan, H. W. Carr, G. F. Stout, and W. McDougall in a symposium held in London in the summer of 1910 (and published in *The British Journal of Psychology)*. But even these intellectual heavyweights could not control the unruly host of questions raised by their topic. Is instinct a matter of cognition, of action,

of both, of neither? Is intelligence a matter of foresight, or of conformity with one's past? Does intelligence imply learning? If so, is it the *outcome* of learning or the very *process* itself? Can there be intelligence without instinct, or vice versa, or both, or neither? Does *purposiveness* distinguish instinct and intelligence? Does consciousness? We may think ourselves lucky that we no longer feel compelled to answer such questions, but luck may be running out. Someplace in psychology there may need to be a concept to hold together those more or less adaptive, more or less innate, and more or less stereotyped movements innocently called instincts prior to the 1920's, even if called by another name. Unfortunately, in the post-Darwinian, pre-1920 literature, there were at least three distinct usages of "instinct," intersecting the three antitheses at odd points.

First, instinct was thought by some theorists, in addition to Morgan, to be merely concatenated reflexes. Having taken a bold position on one antithesis, they could circumvent the hazards of the other two. For them, the only possible difference between reflexes and instincts was the degree of complexity or degree of involvement of the entire organism, with the instincts being more complicated chains of reflexes than the familiar reflexes of tendons and joints. Herbert Spencer, the voluminous English philosopher of evolutionism, was a leading advocate of the *reflexive* conception of instinct (1855), as was —fifty-odd years later—the founder (or at least the prime publicist) of behaviorism, John B. Watson. In between came the analytical, mechanistic school of biology exemplified most relevantly for our purposes by Jacques Loeb. When Watson arrived for his graduate work at the University of Chicago just at the turn of the century, he was looking forward to studying pragmatic functionalism under John Dewey, but found Loeb's mechanistic reflexology far more congenial (Watson, 1936; Herrnstein, 1967).

Loeb had taken the botanical concept of tropism and applied it with vigor and eloquence to animal behavior. Just like plants, he argued, animals are trapped into "forced movements" by the operation of gradients of physical energy. Tropisms differed from classical reflexes only in their broader scope. While reflexologists had tended to study isolated "preparations," Loeb called for the study of "the organism as a whole" (the title of a book he published in 1916). As for instincts, Loeb agreed with Spencer. Instincts were just compound reflexes (1900), different from tropisms only taxonomically. Tropisms sorted out an animal's movements according to the stimuli—e. g., heliotropisms for light, geotropisms for orientations in the gravitational field, stereotropisms for tactual contact—instincts sorted them according to functional concepts—e. g., eating, reproduction, shelter. Instincts, Loeb acknowledged, often show the effects of learning and of general systematic conditions of the individual (such as hunger),

but he seemed confident that nothing more than ordinary physics and chemistry could handle those complications. In any event, Loeb's message got across to his energetic student, for Watson plunged into naturalistic studies of the behavior of "the whole organism" from a biological standpoint.

Nowadays, when Watson is remembered, it is for his radical environmentalism. And it is true that in his later years, when the ism in behaviorism became more important to him than the study of behavior, Watson denigrated the role of instinct and inheritance. But in his first book, *Behavior, an Introduction to Comparative Psychology* (1914), instinct, though significant and central, was indistinguishable from reflex. Here is Watson's own characterization:

> The student of behavior has come to look upon instinct as a combination of congenital responses unfolding serially under appropriate stimulation; the series as a whole may be "adaptive" in character (always adaptive from the Darwinian standpoint) or it may be wholly lacking in adaptiveness. Each element in the combination may be looked upon as a reflex. An instinct is thus a series of concatenated reflexes. . . . Such a series of reflexes, or an instinct, is best illustrated by the young bird's egress from the egg, and its later attempt at building a first nest; the first fighting responses in young animals, and in general the first attempts of young animals to capture, kill, and eat their prey. The act must be observed upon its first appearance if it is to be seen pure, i.e., without modification through habit. (p. 106f)

Watson acknowledged that instincts seem to be less definitely fixed in form than reflexes. But if they were really the same, how could the one be more variable than the other? Watson's answer appealed to a statistical argument—given the sheer multiplicity of stimuli bombarding the organism from within and without, the multiply-controlled instinct almost inevitably betrayed the variations in stimulation more than the simply-controlled reflex. Thus, he argued, "The organism is constantly changing. We may be able to control one element in the total system of stimulation of the animal at any given moment (e.g., the energy and wave-length of incident light) but we cannot control the internal physiological processes which are also essential parts of the total stimulation and which also affect markedly the state of the effector. The two factors, extra-organic stimulation and intra-organic stimulation, together determine the nature of the observable response" (p. 107f).

But the problem was not just random variability, as other writers of the era knew. It was variability with a point. Reflexes, as Sherrington had shown in his monumental work on the spine (1906), sometimes interacted so as to assure a suitable, rather than merely a fixed, response for a given stimulation. The dog's scratch reflex took in a network of facilitatory and inhibitory components that guided its leg, independent of initial position, to the point of stimulation. Not

merely *variable*, the response was *appropriate*. This could have been the model for Watson's instincts had he not thought that the integration of reflexes gets learned more often than inherited. Drawing on his observations of infant animals, he depicted the original mass of behavior as largely disorganized, unintegrated, and not particularly well suited to the survival of the infant. The jerks and jumps of the infant animal displayed its raw reflexes, thought Watson, and the smoothly coordinated movements of the adult come only after months or years of learning. Watson granted a few exceptional cases—where reflexes are genetically integrated—but he felt they had been overemphasized in the rush to confirm Darwin's theory by finding adaptation at every turn. Watson no doubt had a good point, but he overreacted, leaving too little scope in his theory for the inheritance of integrated movement.

Not just mechanists like Spencer, Loeb, or Watson believed in reflexive instincts. Here, for example, is William James, writing before either Loeb or Watson had started campaigning for the objective reform of psychology:

> *The actions we call instinctive all conform to the general reflex type;* they are called forth by determinate sensory stimuli in contact with the animal's body, or at a distance in his environment. The cat runs after the mouse, runs or shows fight before the dog, avoids falling from walls and trees, shuns fire and water, etc., not because he has any notion either of life or of death, or of self, or of preservation. He has probably attained to no one of these conceptions in such a way as to react definitely upon it. He acts in each case separately, and simply because he cannot help it; being so framed that when that particular running thing called a mouse appears in his field of vision he *must* pursue; that when that particular barking and obstreperous thing called a dog appears there he *must* retire, if at a distance, and scratch if close by; that he *must* withdraw his feet from water and his face from flame, etc. His nervous system is to a great extent a preorganized bundle of such reactions—they are as fatal as sneezing, and as exactly correlated to their special excitants as it is to its own. (1890, vol ii, p. 384)

There is no appreciable difference here between James and Watson, except in their prose styles. Like Watson, James tackled the question of variability and answered it by pointing to the diversity of causal agents, as a result of which even fixed reflexes may appear variable as they combine with each other. James was frankly impressed with the built-in adaptiveness of action—no doubt too much so according to Watson—but his account of it was staunchly mechanistic:

> At first this view astounds us by the enormous number of special adjustments it supposes animals to possess readymade in anticipation of the outer things among which they are to dwell. *Can* mutual dependence be so intricate and go so far? Is each thing born fitted to particular other things, and to them exclusively, as locks are fitted to their keys? Undoubtedly this must be believed

> to be so. Each nook and cranny of creation, down to our very skin and entrails, has its living inhabitants, with organs suited to the place, to devour and digest the food it harbors and to meet the dangers it conceals; and the minuteness of adaptation thus shown in the way of *structure* knows no bounds. Even so there are no bounds to the minuteness of adaptation in the way of *conduct* which the several inhabitants display. (p. 384, ii)

For James—as for the acknowledged devotees of the reflex—the total action of which the instinct was a part could be modified, elaborated, or suppressed by learning, but the instinct itself was innate, fixed movement and nothing more or less. Neither its variability nor its adaptiveness was persuasive counter-evidence. Variability in response just signified variability in stimulation, while adaptiveness emerged from the demands of natural selection. James was at pains to dissociate himself from theories that tried to see something more than evolutionary happenstance in adaptiveness:

> The older writings on instinct are ineffectual wastes of words, because their authors never came down to this definite and simple point of view, but smothered everything in vague wonder at the clairvoyant and prophetic power of the animals—so superior to anything in man—and at the beneficence of God in endowing them with such a gift. But God's beneficence endows them, first of all, with a nervous system; and, turning our attention to this, makes instinct immediately appear neither more nor less wonderful than all the other facts of life. (p. 384f, ii)

The quotation from James suggests the second usage of instinct. For some people, an instinct meant a special capacity for adaptation, differing from either intelligence or reflexes. The capacity usually was undefined, for with this usage, the account of an animal's behavior typically found its way to the marvels of adaptation and then trailed off into what James so aptly called "vague wonder." Being vague is apparently no shortcoming, at least by the standard of popularity. Nature movies in the Walt Disney style, with bears, birds, or beavers shrewdly going about their business epitomize the most abundant usage of instinct in our ordinary literature and in the common understanding. The wiliness of a fox, the tenacity of an ant, or the courage of a lion refer to not quite the same traits as in man, for they are "instinctive" in this second sense, which differs clearly from concatenated reflexes.

In a more scholarly vein, an example of the second usage is the work of Jean Henri Fabre, a French naturalist who enjoyed a measure of popularity at the end of the last century by writing on insect behavior. Not just a popularizer, he did his own experiments and used mostly his own data to make his points. An example is an experiment on homing by mason-bees. He marked with chalk a number of

these solitary bees, released them from an unfamiliar locale, and kept track of the flight times back to their nests. His conclusion:

> Of some twenty Bees who had seemed fit to make the long journey when I released them, fifteen at least had returned: two within the first hour, three in the course of the evening and the rest next morning. They had returned in spite of having the wind against them and—graver difficulty still—in spite of being unacquainted with the locality to which I had transported them. There is, in fact, no doubt that they were setting eyes for the first time on those osier-beds . . . which I had selected as the starting point. Never would they have travelled so far afield of their own accord, for everything that they want for building and victualling under the roof of my shed is within easy reach. The path at the foot of the wall supplies the mortar; the flowery meadows surrounding my house furnish nectar and pollen. Economical of their time as they are, they do not go flying two miles and a half in search of what abounds at a few yards from the nest. Besides, I see them daily taking their building-materials from the path and gathering their harvest on the wild flowers, especially on the meadow sage. To all appearance, their expeditions do not cover more than a radius of a hundred yards or so. Then how did my exiles return? What guided them? It was certainly not memory, but some special faculty which we must content ourselves with recognizing by its astonishing effects without pretending to explain it, so greatly does it transcend our own psychology. (1914, p. 54f)

Mason bees usually lay their eggs in cells that have been about two-thirds, or 10 mm, filled with honey, an observation that Fabre exploited in another experiment on a bee he caught in the act. Each time she put a little honey in any of the five cells she was preparing, Fabre swiped it out as well as he could, while the bee was off for new provisions. Sometimes his cotton swab got all the accumulated honey out of the cell, other times it got only a little. Occasionally, it left some cotton threads behind. The bee, he said, went about her business with apparent authority, adding honey and cleaning out any stray threads. And then, suddenly, she stopped, laid her eggs, and sealed the cells. Now Fabre opened up the cells to find out how much honey had actually accumulated—the standard 10 millimeters or something else. He found one egg on 3 millimeters of honey, two on one millimeter, and the remaining two on nothing but the glaze of honey left by his swab. Said Fabre,

> The inference is obvious: the Bee does not judge of the quantity of honey by the elevation of the surface; she does not reason like a geometrician, she does not reason at all. She accumulates so long as she feels within her the secret impulse that prompts her to go on collecting until the victualling is completed; she ceases to accumulate when that impulse is satisfied, irrespective of the result, which in this case happens to be worthless. No mental faculty, assisted by sight, informs her when she has enough, when she has too little. An instinctive predisposition is her only guide, an infallible guide under normal conditions, but hopelessly lost when subjected to the wiles of the experimenter. Had the Bee

> the least glimmer of reason, would she lay her egg on the third, on the tenth part of the necessary provender? Would she lay it in an empty cell? Would she be guilty of such inconceivable maternal aberration as to leave her nurseling without nourishment? I have told the story; let the reader decide. (p. 187f)

No need to criticize these experiments, for what counts here is the outlook, no matter how flimsy the basis for it. Fabre nicely exemplifies what can be called the *intuitional* approach to instinct. Fabre discounted reflexes and natural selection, as well as reason, to explain the wonders of animal adjustment. To ask how they are to be explained violates the spirit of the intuitional approach. In fact, the intuitional view is typically a set of denials held together by a sense of awe. Said Fabre about efforts to find in animal instinct some continuity with human adjustment: "The fashion will pass and the facts remain, bringing us back to the good old notions of the soul and its immortal destinies" (p. 39).

Judging from the historical record, it is easy to confuse the intuitional usage with the third one, whose leading advocate was William McDougall. To make a science of social psychology, he said in one of the first books to try to do so, demands a recognition that, "The human mind has certain innate or inherited tendencies which are the essential springs or motive powers of all thought and action, whether individual or collective, and are the bases from which the character and will of individuals and of nations are gradually developed under the guidance of the intellectual faculties" (1908, p. 19). Those innate tendencies were instincts, but neither in Spencer's reflexive sense nor Fabre's intuitional one. Like James before him, McDougall found popular writing on instinct intolerable: ". . . the actions of animals are popularly attributed to instinct, and in this connection instinct is vaguely conceived as a mysterious faculty, utterly different in nature from any human faculty, which Providence has given to the brutes because the higher faculty of reason has been denied them. Hundreds of passages might be quoted from contemporary authors, even some of considerable philosophical culture, to illustrate how these two words ['instinct' and 'instinctive'] are used with a minimum of meaning, generally with the effect of disguising from the writer the obscurity and incoherence of his thought" (p. 21).

But he also dissociated himself from the reflexive definition, which had been James's answer:

> In treating of the instincts of animals, writers have usually described them as innate tendencies to certain kinds of action, and Herbert Spencer's widely accepted definition of instinctive action as compound reflex action takes account only of the behavior of movements to which instincts give rise. But instincts are more than innate tendencies or dispositions to certain kinds of movement. (p. 26)

If instincts were "more than . . . dispositions to . . . movement," but less than "a mysterious faculty . . . which Providence has given," what were they? For McDougall they had something to do with striving—in modern parlance, with motivation:

> . . . every instance of instinctive behavior involves a knowing of some thing or object, a feeling in regard to it, and a striving towards or away from that object. (p. 26)

For him, the passage from stimulus to response in an instinct followed distinctively psychological pathways, not to be confused with the mechanistic reflex arc. The behavior of a broody hen was not merely elicited by some eggs, it was coordinated with respect to a definite outcome, like the chicks hatched:

> We may, then, define an instinct as an inherited or innate psycho-physical disposition which determines its possessor to perceive, and to pay attention to, objects of a certain class, to experience an emotional excitement of a particular quality upon perceiving such an object, and to act in regard to it in a particular manner, or at least, to experience an impulse to such action. (p. 29)

In McDougall's version of instinct, there was action until the outcome was achieved or the creature's inner state otherwise changed, for example, by exhaustion. Since he was at this point writing a social psychology, most of his examples were drawn from human behavior, but that was incidental. In fact, McDougall looked to lower animals for instincts in their "purest form" (p. 24), especially to insects doing the very things that Fabre had described but misconstrued, or so McDougall might have said. Among the higher animals—the vertebrates—instincts and experience blend into endlessly varied and idiosyncratic adjustments for each individual. But, said McDougall, an instinct is not perfectly malleable, for it has an invariant core:

> Now, the afferent or receptive part and the efferent or motor part are capable of being greatly modified, independently of one another and of the central part, in the course of the life-history of the individual; while the central part persists throughout life as the essential unchanging nucleus of the disposition. (p. 33)

The invariant "central part" is affective—the striving towards a particular outcome, like the spider getting its web spun or the tiger capturing its prey, which is why this third usage may properly be called "*hormic*," from the Greek word for urge. ("Hormic psychology" was the name suggested by McDougall after "instinct" had fallen into disfavor.)

McDougall's account of how the afferent and efferent channels adapt around the invariant central core constitutes an inchoate theory of learning. In one of his

examples, he noted that wild animals become unafraid of, i. e., do not flee from, a rushing railway train, even though the noise would scatter them if it were not for their prior experience. Presumably, the animals start with a non-specific tendency to flee from loud sounds, which becomes "specialized" according to circumstances. Railroad noises prove to be innocuous; consequently, they lose their power to alarm. Today, we would call this habituation or extinction. McDougall's second rule for modifying the sensory component of an instinct—the more important one, according to him—was simply association by contiguity. McDougall described birds on an unpeopled island who show no fear of men upon first contact, but later flee in evident alarm. Presumably, said McDougall, this is because something fearsome about people, for example, the sound of their guns, has been paired with their sight, with the fear transferring by straight contiguity. McDougall also discussed association by similarity. In all, McDougall outlined what in today's terminology would be called discrimination learning (including extinction), Pavlovian (or continguity) conditioning, and stimulus generalization—fair coverage of contemporary learning theory on the stimulus side.

For the efferent side, too, McDougall insisted on plasticity, although he did not characterize it very sharply. Motor changes, he said, are more frequent among the higher animals than the lower, reaching a pinnacle in man. In man, at any rate, the large movements of the limbs and trunk adapt more readily than what he called the "subsidiary movements," involving the visceral musculature. Wherever it occurs, however, it centers on helping the animal to achieve the end stipulated by instinct: "the creature strives to achieve its end under the driving power of the instinctive impulse awakened within it . . ." (footnote p. 40, the word was "instructive" in the first edition, but later changed to "instinctive"). This sort of talk about "ends" no doubt reminded his behavioristic contemporaries of a Fabre-like intuitionism, but styles have changed. These days even the most committed behaviorists can talk about striving, even if by other words, without being held guilty of believing in the ghostly promptings of Fabre, but that gets ahead of the story. For now, note that McDougall was here arguing that creatures, including man, learn to modulate instinctive impulses so as to make them conform to circumstances—social and otherwise—while still retaining the effectiveness of the action in achieving its goal. For McDougall it might still have been the instinct of "pugnacity," whether we are using our bare hands or a Sherman tank. Variable means converge onto a common goal, which is to satisfy the instinctive desire.

The hormic, no less than the reflexive, usage of instinct, could draw on biological support after Darwin. Wallace Craig, for example, on the basis of his

work on doves and pigeons (1918), argued for a new outlook on instinct. Like McDougall, he rejected pure reflexology, saying that instincts involve "appetite" or "aversion," whereas reflexes are supposedly free of affect or drive. An appetite is abated by getting an absent stimulus; an aversion, by the removal of a present stimulus. The animal often accomplished its goals by what Craig called "trial," which was to say, by means other than fixed patterns of movement. According to Craig, only the final "consummatory" act was fixed, the rest adapted itself to the circumstances. Even so basic an activity as drinking, Craig found, has to get built up by experience, at least in his doves. The only fixed ingredient appeared to be the swallowing reflex itself. The integrated act of drinking—bowing the head, opening the beak, etc.—as well as the recognition and approach to water, appeared by increments as the dove gained skill by practice (Craig, 1912). Nesting in male doves also revealed the inner motivational core, the fixed consummatory act, and the acquisition of environmental particulars:

> The first thing the observer sees is that the dove, while standing on his perch, spontaneously assumes the nest-calling attitude, his body tilted forward, head down, as if his neck and breast were already touching the hollow of a nest (incipient consummatory action), and in this attitude he sounds the nest-call. But he shows dissatisfaction, as if the bare perch were not a comfortable situation for this nest-dedicating attitude. He shifts about until he finds a corner which more or less fits his body while in the tilted posture; he is seldom satisfied with his first corner, but tries another and another. If now an appropriate nest-box or a ready-made nest is put into his cage, this inexperienced dove does not recognize it as a nest, but sooner or later he tries it, as he has tried all other places, for nest-calling, and in such trial the nest evidently gives him a strong and satisfying stimulation (the appeted stimulus) which no other situation has given him. In the next his attitude becomes extreme; he abandons himself to an orgy of nest-calling (complete consummatory action), turning now this way and now that in the hollow, palpating the straws with his feet, wings, breast, neck, and beak, and rioting in the wealth of new, luxurious stimuli. He no longer wanders restlessly in search of new nesting situations, but remains satisfied with his present highly stimulating nest. (1918, p. 98)

In the reflexive usage, instinct isolated a kind of movement, differing from habit on the one hand and voluntary action on the other, but identical with reflex except for complexity. In the intuitional usage, instinct designated a special kind of knowledge, differing from reason or intelligence on the one hand and mere mechanism, as in reflexes, on the other. Following upon the thesis of special knowledge and the antithesis of fixed movement, the hormic usage could be taken as virtually the Hegelian synthesis. For McDougall, Craig, and others in the hormic tradition, like Sigmund Freud and Edward Tolman, instinct occupied the core of virtually all behavior, energizing it and guiding it to certain objects or

goals. McDougall's social psychology, no less than Freud's psychic determinism, called for a tally of the human instincts, their common elaborations and variations, and their interconnections, forming the behavior of man in social contexts. No matter how complex the behavior, or how elevated above simple biology, the instinctive core remains indispensable, as McDougall noted:

> Take away these instinctive dispositions with their powerful impulses, and the organism would become incapable of activity of any kind; it would lie inert and motionless like a wonderful clockwork whose mainspring had been removed or a steam-engine whose fires had been drawn. These impulses are the mental forces that maintain and shape all the life of individuals and societies, and in them we are confronted with the central mystery of life and mind and will. (1908, p. 44)

McDougall's development of "instinct" marks the end of the line, for the assault on that word directed most of its fire at him and his followers, as he noted (1921) in an exhaustive but futile reply to his critics. As Figure 1 showed, there was some apparently fatal flaw in the concept of instinct, but what? The likeliest objection to instinct would have been the very ambiguities outlined here, but these seemed to bother virtually no one very much. The multiplicity of meanings was not unnoted. Zigler (1923) for example, spoke of three senses of instinct, approximately isomorphic with the present ones, but using the words "neurological," "biological," and "psychological," where I use "reflexive," "intuitional," and "hormic." Although the ambiguities may, in some sense, *explain* the objections to instinct, that was not what the objectors said. Anyone who has noted our field's capacity for confusion should not be surprised to hear that the trouble was at the same time less clear and more serious.

Instinct in Retreat

As an early and representative objection to instinct, let us take Knight Dunlap (1919). First, he granted instincts as reflexes, calling them "instinctive acts." Those pose no problem, he said, even the adaptive ones, for natural selection provides adequate grounds for well-designed behavior no less than well-designed structure. But "instincts"—the noun instead of the adjective—were flawed by their "teleological" use, as exemplified for him in McDougall's *Social Psychology.* It may be convenient, but it is not good psychology, said Dunlap, to classify a lion's pursuit of a deer as part of a predatory instinct and his pursuit of a female as part of an amatory instinct, if they comprise precisely the same movements. Given that instinctive acts are just reflexes, when two sorts of pursuit do not differ in

movement, they differ in nothing, except in the eye of a teleologically-tainted beholder. Dunlap made clear his disapproval of such muddlemindedness:

> Having posited a "pugnacious instinct," for example, one writer proceeds gravely to infer that war is forever a necessity, as the expression of this "instinct." Controversy over the hypothetical "moral instinct" is another illustration of confused procedure. As a matter of fact, there is or is not a "moral instinct," according to the plan of the author. If it is deemed useful to segregate, in the outer world, certain effects which are to be named "moral"—and this segregation can unquestionably be made—any unlearned tendencies which contribute to these effects, legitimately make up a "moral instinct." If the classification of effects as "moral" is not chosen, then of course there is, for the author choosing, no "moral instinct." Again; if it is advisable to distinguish between the mere process of copulation and the processes of conception and birth, there is an "amatory instinct," distinguishable from the "reproductive," otherwise there is not." (p. 309)

Dunlap was a professor at Johns Hopkins, a colleague of John B. Watson and, by Watson's own admission (1936), a major influence in the evolution of behaviorism. Dunlap's criticism centered on methodology rather than empirical substance. At this stage of its development, behaviorism was molecular—in principle physiological, if not in fact. A good psychologist, according to behaviorists, sought specific stimuli causing particular responses. The relative scarcity of well understood S-R connections was seen as an empirical, not a conceptual, deficit. The behaviorist's model for a scientific law was better adapted to the movements of the hands of a clock than to the temperature of a self-regulating oven, although both sorts of machines were around at the time. Hence, the methodological predisposition against McDougall and his ilk, with their emphasis on central affective states and purposes intervening somehow between stimulus and response and guiding the organism toward fixed ends. McDougall's persistent mechanist-baiting could not have helped. (One of his books, *Body and Mind* [1911], had as a subtitle the red flag: "A history and defense of animism.")

McDougall drew substantive fire too, not just methodological, but mainly from a rising school of social theory, exemplified by Luther Lee Bernard, a sociologist who had taken his doctorate at the University of Chicago seven years after Watson, in 1910. Like Watson or Dunlap, Bernard had no trouble with the reflexive definition of instinct. Some behavior, he admitted (1921), comprises inherited reflexes, although these need to be distinguished from habits. But that, he recognized, was not quite McDougall's usage, nor, for that matter, was it the usage of some of the other influential social theorists of the time. McDougall, along with Will Durant, Thorstein Veblen, Charles Sumner, and others, wrote about instincts as if they were classes of activity—e. g., parental, reproductive,

aggressive—rather than specific reflexes. Bernard argued vigorously that such activities, obviously learned to some degree in man, could not, by his definition, be instinctive since they are not reflexive. For reasons obscure to a modern reader, Bernard thought that Mendelian genetics proved conclusively that reflexes, and nothing more, could be inherited, certainly not the "tendencies" or classes of activity described by McDougall. Thinking he had outflanked his opponents with genetics on one side and learning on the other, Bernard assailed them with his most potent pejoratives, calling them "essentially Lamarckian and Galtonian, and even metaphysically vitalistic" (p. 109).

Like most other substantive attacks, Bernard's opposed instinct with environment. Given the reflexive usage, Bernard could sensibly say, "The influence of environment is cumulative in our lives and the decay of the influence of instinct is progressive" (p. 111). He could make such comparisons as: "Some men live lives which are relatively close to instinct, while other men build story after story of culture and sublimated interests until instinct is scarcely discernible in them in its original forms" (p. 110). Neither sentence made any sense as criticisms of the hormic usage, as McDougall's lengthy discussions of the interaction of instinct and culture implicitly prove. The nuance eluded Bernard, as it did the other substantive critics. Bernard's objection to instinct followed from his environmentalism, his belief that social institutions, rather than genetic endowments, were passed on from generation to generation. He, like a growing number of other social scientists of that time, believed in, and were determined to prove, man's susceptibility to his environment, for good or ill. They opposed fervently any theory that smacked of fatalism in human affairs, which explains their disapproval of instinct.

The apparent incompatibility with environmentalism cost instinct dearly, for environmentalism in the 1920's was on the upswing. It did McDougall little or no good that his usage was hormic, for environmentalists invariably first postulated the reflexive usage and then lambasted it. Scholarly controversies often do not get the issues straight the first few times around. For McDougall and Craig, the invariant part of instinct was not primarily inherited movement, but inherited drive or appetite or aversion. The adaptability of action was as crucial to the hormic theory as it was to the most radical environmentalism. Today, we have many environmentalists in good standing who believe in the reality of inherited drives, but that came later. In Bernard's time, when the reflexive theory of instinct was the straw man, instinct seemed at odds with environmentalism. Like other critics of McDougall, he first shrugged off the hormic definition. Said Bernard bluntly, "The isolated and unchanging central emotion of McDougall is a myth.

Instinct is action according to a structural action pattern or it is nothing" (1921, p. 116). The "structural action pattern" was the reflex, and so, with the spurious support of Mendelian genetics, Bernard thought McDougall, his instinct doctrine, and all its corollaries, were refuted.

At times, the behavioristic bent took odd turns. Consider, for example, W. S. Hunter, one of behaviorism's early major figures. While discussing (1920) instinct and its application to social psychology, Hunter grappled with the interplay of nature and nurture. He granted man instincts as reflexes, which, he said, are quickly modified by experience according to unspecified laws of learning. Next, he noted the adaptiveness of instincts, pure or otherwise, although he disclaimed anything preternatural:

> This statement [that instincts are adaptive] carries no implication that the purpose is a conscious one or that it has been instrumental in molding the behavior. The statement is a straightforward, scientific, objective formulation, implying nothing of vitalism or of other speculative interpretations of the place of purpose in nature. (p. 261)

But to depict instincts as, first, adaptive, second, variable in form, and, finally, based not on conscious purpose, reason, or instrumental experience seems to put Hunter in Fabre's company. If the variations in behavior came neither from reason, purpose, nor from learning based on adaptation, then they appear to be coming from just the sort of "special faculty" that Hunter no doubt wanted to scorn. What he needed instead was the hormic definition, for then he could account for adaptive variations in instinctive behavior as McDougall had, in terms of fixed goals and variable means. But prior intellectual commitments clearly prohibited Hunter, a leading behaviorist, from becoming one of McDougall's followers. General allegiances often outweigh particular merits in scholarly disputes.

The most extreme environmentalist assault on instinct must have been the one by Zing-Yang Kuo, a Columbia-trained Chinese psychologist who distinguished in the customary way between instincts as reflexes and as innate tendencies to action, and then denied them both. To handle apparently innate responses like the flight of birds, Kuo said, "Given a mature action system and given an environmental demand a definite reaction can be fairly predicted" (1921, P. 653). A disinterested reader might feel that Kuo had actually circumlocuted around, rather than circumvented, flight reflexes, but what Kuo really showed is less important for our purposes than what he and others thought he showed. The case of Kuo demonstrates that the environmentalist fervor could sometimes cast even reflexive instincts into doubt, ascribing them either to learning or to novel, if

vacuous, physiologizing. Action emerges from the environment; anything else is defined away:

> . . . there is no sex instinct in the sense that it necessarily involves coition between two opposite sexes. The fact that mating always takes place between the opposite sexes of the same species is because the members of the same species always live in the same community where the heterosexual habit is normally developed. . . . The point I am here driving at is this: that all our sexual appetites are the result of social stimulations. The organism possesses no ready-made reaction to the other sex, any more than it possesses innate ideas. (p. 657)

For this assertion, Kuo drew on studies (Whitman, 1919) showing that passenger pigeons raised with ring-doves attempted, when they matured, to mate with ring-doves rather than passenger pigeons. He interpreted these findings as evidence for what he called "elementary units of reaction" being woven into integrated behavior by learning. The learning process Kuo characterized, without acknowledgement, in roughly Thorndikian fashion, relying on the impact of reward and punishment.

The judicious Knight Dunlap was quick to answer (1922). After noting that Kuo had merely renamed reflexes, Dunlap reiterated his own objections to instinct, on methodological, not substantive, grounds. But there were really two groups attacking instinct, the methodological behaviorists like Dunlap and the radical environmentalists like Bernard, Kuo, and, in his later writings, John B. Watson. Union in a common cause masked their fundamental differences. Dunlap objected to the hormic use of instinct because its categories seemed to him psychologically vacuous. Whatever else he may have thought, the nature-nurture issue was immaterial on this count. His objections would have applied as well if categories like "pugnacity" or "acquisitiveness" described learned, instead of inherited, behavior. But the radical environmentalists, Dunlap's allies in the fight against instinct, often discussed just such categories of learned behavior, objecting only when inheritance was involved.

The environmentalist criticism came mainly from social theorists with a substantive interest; the methodological, from philosophers or, at any rate, the philosophically concerned. As a working psychologist, Dunlap was an odd case. The philosophical journals published much of the methodological criticism. For example, G. C. Field, in *Mind* (1921), compared instinct psychology to the old and discredited faculty psychology of an earlier era. Both, he said, were afflicted with circularity. It is all right, he granted, to call some particular action instinctive, for that merely says the action is innate and does not depend upon foresight of its end. But to go beyond that, to say, for example, that the action exemplifies a

"foraging" instinct or a "cell-building" instinct in a bee, adds nothing to the description. The outcome of the behavior is in the eye of the describer, not in the bee, said Field. For the bee, the action itself tells the entire story. Like faculty psychology, Field claimed, instinct psychology simply postulates what it needs to explain the behavior at hand. An instinct for gregariousness no more explains the herding of cattle than a faculty for mathematics explains the solving of an equation. Field found social psychology especially liable to perpetrate such vacuities because it rarely described behavior as specific movements, using the epistemologically dubious language of ends and goals instead.

Ayres (1921) had a similar complaint, facetiously blaming an instinct of "belief-in-instincts" for what he judged an excessive reliance on the concept. Instinct as a "definite series of complex acts that look ultra-rational but can be proved to be otherwise" (p. 561), Ayres could tolerate. But if, in extending the notion to human social behavior, the definiteness gets lost, the loss is fatal, said Ayres. As an end or outcome of behavior, instinct describes nothing, he said. We try to explain some behavior in terms of its ends, but if we know of its ends only from the behavior, we are saying just that people behave because they behave, according to Ayres.

Because the concept of instinct was in transition when it came under attack, different people found it objectionable, or defensible, for different reasons. Methodological thrusts drew substantive parries, and vice versa. Reflexologists allied themselves with environmentalists to attack purposivists who denied neither reflexes nor learning. Nevertheless, the course of the dispute may be briefly recapitulated by smoothing some of the jagged edges.

The pre-Darwinian concept of instinct was to some degree mystical or vitalistic, when it depicted animals as being capable of adaptive behavior without benefit of either experience or foreknowledge of outcome. This I have called the intuitional meaning of instinct. After Darwin, it occurred to people that behavior may seem to be adaptive when only merely reflexive. Since the struggle for survival selects for adaptive reflexes, at least some of the "inner wisdom" of animals could be discounted as inherited fixed action. At this stage belongs the reflexive definition of instinct. Soon, however, it became apparent that since fixed action was the exception, not the rule, something other than reflexes must be at work. Variability in instinctive behavior made the reflexive account unwieldy, if not utterly implausible. The answer set the next stage of the instinct doctrine. The hormic usage centered not on action itself but on the impulse to action, with the quality of the impulse distinguishing one instinct from another. Thus, an animal's sexual instinct impels or drives it towards sexual contact, rather than endowing it

with a fixed collection of prescribed sexual movements. Although some movements were more or less appropriate for sexual gratification, too much of the behavior clearly varied to allow classification in terms of reflexes. In contrast, there seemed to be a reasonably well-defined list of general categories among which virtually all movements could be sorted—sex, food, shelter, parenthood, etc. The hormic view seemed to raise the floodgates of reaction against instinct, although it was a good two steps removed from the intuitional conception with which some critics confused it.

The reaction was, as this review shows (perhaps too graphically), confused, but two main threads can be unraveled. First of all, there was nature-nurture. Because instinct had something to do with inheritance, it was bucking the tide of the time. And because McDougall's hormic conception deliberately covered more of man's behavior than the reflexive conception, it was that much more objectionable. The second problem was methodological, directed clearly at the hormic definition of instinct and essentially independent of nature-nurture. Isn't it, as Dunlap might have said, purely arbitrary to call the pouncing of a cat a "predatory" act, or does it, as McDougall might have answered, tell you something about a cat that you would not want to say about a chicken no matter how much it jumped about?

Instinct in Disguise

Whatever the merits of the case, the instinct doctrine was vanquished, driven from the American psychological literature for more than a generation and coming back recently only with trepidation and apologies. Writing in 1938, Lashley commented on the anti-instinct upsurge of the early 1920's:

> I am well aware that instincts were banished from psychology some years ago, but that purge seems to have failed of its chief objective. The anti-instinct movement was aimed primarily at the postulation of imaginary forces as explanations of behavior. It was only incidental that these had also been assumed to be constitutional. The psychology of instincts was a dynamics of imaginary forces and the anti-instinct movement was primarily a crusade against such a conceptual dynamism. Somehow the argument got twisted. Heredity was made the scapegoat and the hypostatization of psychic energies goes merrily on. Desires and aversions, field forces and dynamic tensions, needs and vectors, libidoes and means-end-readinesses have the same conceptual status as had the rejected instincts and, besides, lack the one tie to physiological reality which the problem of genetic transmission gave to the latter. The anti-instinct movement was a critique of logical method which failed of effect because it was aimed at a single group of concepts. Its history is a striking example of the lack of transfer of training or the futility of formal discipline. (p. 447)

Lashley's ironic complaint says that in its treatment of instinct, psychology threw out the baby and saved the bath water. According to him (as translated into this paper's terminology), the attack on instinct should have eliminated the hormic, not the reflexive, usage. It was the postulation of "psychic energies," not innate ("constitutional") mechanisms, that offended. But somehow, the inherited factors fell into disfavor while the hormic postulations were retained under new names: desires, aversions, field forces, needs, vectors, libidoes, and so on.

Lashley's lament, though correctly distinguishing between inheritance and inner drives, failed to credit the strength of environmentalism. As shown above, the substantive critics of instinct came down hard for the environment, arguing against instinct as the bankrupt central idea of the nativist conception of man. Those incipient learning theorists expressed their faith that human social institutions rested on the laws of learning *rather* than the laws of instinct. At this stage of the game, however, neither set of laws had been framed, or even articulated. The environmentalist argument could be paraphrased as: "Learning, however it comes about, surely accounts for more of man's social existence than instincts, whatever they are." Stated so generally, one can see how people with varying, and perhaps even opposing, views of both learning and instinct could readily agree on the predominance of the former. While Lashley was right that for the methodologist—mainly the philosophers—the use of instinct as explanation suffered from conceptual flaws bearing no relation to inheritance, for the environmentalists like Bernard, Kuo, Hunter, the later Watson, and many others, instincts themselves were the enemy. For about fifteen years, between the anti-instinct movement and the rise of articulated learning theories in the 1930's, it seemed that environmentalism had won a sweeping victory.

For the most part, learning theory, the conceptual underpinning of environmentalism, passed through an associationistic interlude during this period. John B. Watson seized on Pavlovian conditioning (1916) as the essence of how experience shapes behavior. Stevenson Smith and E. R. Guthrie (1921), in their elementary textbook, promulgated a version of contiguity conditioning that has since been tied to Guthrie's name. The verbal, rote learning school continued along the associationistic pathway laid out by its progenitor, Hermann Ebbinghaus (1885). Even Clark Hull, later to be the prime expositor of learning through reward and punishment, at this time (1929) still adhered to a contiguity theory. Only gradually did psychologists awaken to (see Herrnstein, 1969, for the dawning as regards avoidance learning) the crucial dichotomy between associationism and hedonism—between learning via sheer contiguity and via reward and punishment. In the wake of hedonistic theories inevitably followed the

study of motivation and drive, for what creatures "like" and "dislike" seems inescapably motivational. An example from the late 1920's shows how hormic concepts surreptitiously infused learning theory as it became hedonistic.

Leonard Troland's *Fundamentals of Human Motivation* (1928) is one of psychology's unjustly forgotten books, given its scholarship, originality, and timeliness. In the preface, Troland claims priority in using the word "motivation" in the title of a psychological work, and as far as I have been able to tell, his claim stands. What he meant by motivation, he acknowledged, verges on instinct, particularly in the hormic sense employed by McDougall and Craig. Writing a summary of his theory in 1930, Troland said:

> Some writers, such as McDougall, endeavor to ground the doctrine of motivation almost exclusively in instincts, whereas other writers, like Kuo, deny the existence of instincts altogether. One purpose which I have had in mind in formulating my own views concerning motivation has been to arrive at a resolution of this uncertainty and conflict regarding instincts. (p. 465)

Troland classified stimuli as "beneceptive," "nociceptive," and those which were neither, or "neutroceptive," a scheme that echoed ancient hedonism. Corresponding to these stimuli—and distinguishing among them—Troland proposed a 3-fold classification of receptors. The "beneceptors," "nociceptors," and "neutroceptors" generally signalled stimuli that were, respectively, beneficial, harmful, or neutral as regards the organism or its species. The ordinary stimuli of vision, hearing, and kinesthesis went into the neutroceptive class; into the other two went erotic, painful, and the other motivational agents of contemporary psychology. Troland chose to define these stimulus categories "biologically," rather than "psychologically." Instead of relying on introspection about pleasantness-unpleasantness, he argued that biological analysis should tell us which stimuli were beneficial, harmful, or neither—a tall order yet to be entirely filled.

Modern readers may care little for such old-fashioned taxonomies, but Troland carries his scheme substantially further, to the analysis of behavior. According to him, responding was facilitated by beneception and inhibited by nociception by a mechanism he named "retroflex action." The principle of retroflex action holds that "The strength of any . . . action tendency will . . . be proportional to the time integral of the affective intensities which have been correlated with the given form of response during the total life-history of the individual" (1930, p. 478). The "affective intensities" algebraically combine the positive and negative effects of beneception and nociception. In modern terms, then, Troland had outlined the role of reinforcement and punishment in regulating behavior. Choice, he noted, poses no special problems, since "it follows

that the choice of alternative lines of conduct in the face of a given stimulus will be determined by the greatest past affection" (p. 478, italics omitted). Anticipating the likely reaction of some of his readers, Troland carefully distinguished his theory from classicial hedonism, while granting the obvious affinity. "Earlier hedonisms have laid emphasis either upon anticipated (future) or present happiness, as the determinant of action; the present theory puts the whole burden of determination on *past* affection" (p. 473).

Troland saw the retroflex as the link between inheritance and environment. Any given creature's beneceptors, nociceptors, and neutroceptors were, of course, inherited, and so embodied its evolutionary history. However, the vicissitudes of an individual's life—its encounters with various beneceptive and nociceptive stimuli—created, through the principle of retroflex action, a unique configuration of behavior. By experience, the organism registers the "affective intensities" attached to its various forms of behavior. But it also acquires "secondary" (and still higher order) retroflexes, for Troland noted that neutroceptive stimuli may become beneceptive or nociceptive by being repeatedly paired with naturally beneceptive or nociceptive stimuli. Troland argued that his blend of nature and nurture could cope with the full range of behavioral data:

> Individuals of the same species, when placed in a generally similar environment, will be led to generally similar forms of response; and these forms will cluster about the more important retroflex schemes, such as those of alimentation, reproduction, and self-protection. *Generic systems of behavior, thus determined, take on the aspect of instincts*, because (a) they have evident biological functions, (b) they are comparatively constant throughout a given species, and (c) they are closely bound up with particular reflexes or groups of reflexes [such as alimentary reflexes]. As a matter of fact, however, if our theory is correct, they are largely products of *learning.* A radically different environment would produce unrecognizably different results. (1930, p. 477 first italics added)

The novelty of the system inhered in its scope, not its details, Troland said. The "stamping-in" or "stamping-out" by beneceptors, and nociceptors, he identified with Thorndike's positive and negative law of effect. The creation of higher-order retroflexes, he likened to Pavlovian conditioning. As for the tie to instinct theory, he was more inclined to see his contribution as a decided improvement upon, rather than a mere restatement of, McDougall's hormic usage: "It is to be feared that McDougall's general conception of an instinct is too vague to be of much logical value, although his applications of the concepts of specific instincts are most interesting."

Although McDougall, in turn, found Troland's "hedonism of the past" clearly inferior to his own "hormic" psychology, the continuity of thought between his "instincts" and the new psychology of motivation was not missed:

> Observation of animals of any one species shows that all members of the species seek and strive toward a limited number of goals of certain types, certain kinds of food and of shelter, their mates, the company of their fellows, certain geographical areas at certain seasons, escape to cover in presence of certain definable circumstances, dominance over their fellows, the welfare of their young, and so on. For any one species the kinds of goals sought are characteristic and specific; and all members of the species seek these goals independently of example and of prior experience of attainment of them, though the course of action pursued in the course of striving towards the goal may vary much and may be profoundly modified by experience. We are justified, then, in inferring that each member of the species inherits the tendencies of the species to seek goals of these several types.
>
> Man also is a member of an animal species. And this species also has its natural goals, or its inborn tendencies to seek goals of certain types. This fact is not only indicated very clearly by any comparison of human with animal behavior, but it is so obvious a fact that no psychologist of the least intelligence fails to recognize it, however inadequately, not even if he obstinately reduces their number to a minimum of three and dubs them the "prepotent reflexes" of sex, fear, and rage. Others write of "primary desires," or of "dominant urges," or "unconditioned reflexes," or of appetites, or of cravings, or of congenital drives, or of motor sets, or of inherited tendencies or propensities; lastly, some, bolder than the rest, write of "so-called instincts." For instincts are out of fashion just now with American psychologists; and to write of instincts without some such qualification as "so-called" betrays a reckless indifference to fashion amounting almost to indecency. (1930, p. 13)

McDougall thought the tide was turning in his favor:

> Fifteen years ago American psychologists displayed almost without exception a complete blindness to the most peculiar, characteristic, and important feature of human and animal activity, namely, its goal-seeking. . . . The laws of learning were the laws of frequency, of recency, and of effect; and, though the law of effect as formulated by Thorndike may have suggested to some few minds that the mechanical principles involved were not so clear as might be wished, the laws of frequency and recency could give rise to no such misgivings. . . . Now, happily, all this is changed; the animal psychologists have begun to realize that any description of animal behavior which ignores its goal-seeking nature is futile, any "explanation" which leaves it out of account, factitious, and any experimentation which ignores motivation, grossly misleading; they are busy with the study of "drives," "sets," and "incentives."
>
> . . . The terms "set," "drive," and "incentive," having been found indispensable in animal psychology, are allowed to appear in discussions of human problems, in spite of their anthropomorphic implications; "prepotent reflexes," "motives," "drives," "preponderant propensities," "impulses towards ends," "fundamental urges," and even "purposes" now figure in the texts. (1930, p. 3f)

Troland, too, thought he had unlocked the mystery of purpose:

> The apparent causal efficacy of purpose in the mental domain is attributable to its correlation with conditioners of retroflex action. We have seen that retroflexes act as *controlling agents,* inhibiting or facilitating forms of response in accordance with their bearing upon the continuation of the given retroflex processes. (1928, p. 352f)

The potency of hedonism impressed itself on Troland—as it had on his countless philosophical predecessors and psychological successors—as he ticked off a heterogeneous sample of instances. The pleasures of sports and games, of music, of love, as well as the compulsions of drug addiction and neuroses, to pick just a few examples, are all woven into the idea of retroflex action.

Troland, like McDougall, was explaining adaptive behavior—instinctive and otherwise—in terms of feedback ("back-kick," he called it in 1928, p. 216) from the environment, rather than in terms of inherited fixed reflexes or sheer associationism. There were, of course, others besides those two who had hit on similar accounts of animal behavior, from W. Preyer in describing behavioral adjustments in starfish (1896), through C. Lloyd Morgan (1891), H. S. Jennings (1906), M. F. Washburn (1908), and L**.** T. Hobhouse (1915) discussing " trial-and-error" learning up and down the phyletic scale, to E. L. Thorndike and the "law of effect" (1911). When "reinforcement theory" (to give feedback its contemporary psychological name) rose to preeminence among behaviorists with B.F. Skinner (1938) and C. L. Hull (1943), a rich heritage, both philosophical and empirical, could have been traced, but they both preferred to harken to their mechanistic origins, in the reflexology of Pavlov, Sherrington, Watson, and Cannon.

In Skinner's case, the line to psychological hedonism seems quite direct, for Troland was a member of the three-man committee that approved Skinner's doctoral thesis at Harvard in 1930. (Carroll Pratt and the physiologist W. J. Crozier were the other two.) The thesis had two parts: a history of the reflex as a behavioral concept (1931) and an account of the "reflexes" involved in eating (1932a, 1932b). It would have been easy for Skinner to relate his work to Troland and his retroflexes, for food clearly exemplified the hedonistic control of action, but the manifest evidence is that they were working independently, notwithstanding their proximity in location as well as thought. Skinner's work on the time course of eating in white rats ostensibly dovetailed with W. J. Crozier's reflexology more than with McDougall's or Troland's motivational theories, at least at first. It was a few years before the two lines of inquiry merged, and by then no one was thinking much about antecedents.

Crozier, Jacques Loeb's disciple at Harvard, arrived in 1925 to become chairman of the short-lived Department of Physiology. For about a decade, Crozier and his students showered the scientific community with paper after paper (mostly in the *Journal of General Physiology*, which Loeb had co-founded) on the simple "tropisms" of meal-worms, tent caterpillars, water scorpions, slugs, and other invertebrates. More complex response systems, in rats and mice, were depicted as the interaction of multiple sources of stimulation, but still conforming to the Loebian conception of mechanistic determination. Crozier seemed to be fleshing out Loeb's stark conceptual framework with quantitative, empirical fact. He contributed more than data, however, for his version of Loebian biology was more distinctive. In Crozier's hands, it became especially *mathematical* in the sense of functional relations between the physical measures of stimulus and response (1929, p. 45f), concerned more with *behavior as behavior* rather than as a manifestation of something else (such as a nervous system) (1929, p. 47; Crozier and Hoagland, 1934, p. 6), and *experimental* rather than statistical (1929, p. 82). More or less developed in Loeb's outlook, those attributes blossomed forth in Crozier's writings, and in the kinds of experiments that went on in his Laboratory of General Physiology.

The pertinent example, if not the most characteristic, is, of course, Skinner's early work on eating in rats. To treat the rate of consumption of food as a mathematical function of, for example, time since the beginning of a meal was fully compatible with the Crozierian outlook. Skinner has credited (1956, 1967) his antecedents—Ernst Mach, Henri Poincaré, and Percy Bridgman for a positivistic outlook on science in general and Bertrand Russell, I. P. Pavlov, Jacques Loeb, Charles Sherrington, R. Magnus, and John B. Watson for a substantive interest in behaviorism and reflexology—but the ambience of Crozier's laboratory permeates his early papers nevertheless. There is the detached mathematizing (eating is a power-function of time), the treatment of the behavior of the whole organism as a biological datum in its own right, and the emphasis on experimentally-imposed independent variables, rather than a naturalistic recounting of observations. Crozier's approbation became a matter of public record in 1934 (pp. 95-97). The line of behaviorist descent as regards actual research passes more conspicuously from Loeb *via Crozier* to Skinner, then *via Watson*, although such genealogical metaphors must be taken lightly or not at all. It may help understanding only a bit to think of Skinner as Watson's intellectual nephew rather as his lineal heir, with Loeb as grandfather.

Skinner, then, started off securely enmeshed in the reflexive tradition of instinct doctrine. Although the word "instinct" had vanished, Skinner's hardest

problem was the same one that dogged the early theorists. How to explain variability in behavior that was presumably purely reflexive? The banal fact that a rat sometimes eats when confronted with food and sometimes does not raised hob with a simple reflexive account of eating. Part of Skinner's answer was the traditional one—as noted above for James and Watson—adducing the multiplicity of stimulation. The opening sentence of his first published account (appropriately entitled "On the Conditions of Elicitation of Certain Eating Reflexes") of his research on rats in boxes establishes his continuity with his reflexological predecessors: "The behavior of an intact organism differs from the reflex activity of a 'preparation' chiefly in the number of its independent variables" (1930, p. 433). But Crozier had made the treatment of variability a specialty, carrying it beyond his predecessors. Crozier insisted (1929, 1934) that the presence of variability in behavior, if properly exploited, could be turned to the advantage of a mechanistic, deterministic analysis:

> The fact that the *variation* in response, at any one period of time, is described statistically, does not enable us to discuss parameters of the variation as a function either of the conditions of excitation or of any change in the organism.
>
> . . . To describe the *variability* of a response, then, it should be necessary to characterize not the mean value of the response as a function of something else, but the variation as such a function. In this way it may be possible to gain some insight into the nature of variation in particular cases. The variability of response might then be found to be in these cases far from lawless.
>
> . . . Upholders of the view that behavior is essentially unpredictable, that is, not directly a function of knowable conditions, inevitably trade upon the margin of variation supposed to be typical of organisms as contrasted with machines. If variability can be shown not to be lawless, the supposed logical foundation for this type of obscurantism disappears. (1929, p. 87f, italics in original)

The frontal assault on variability marked Skinner's approach too, as shown in the same early description of the proto-Skinner box cited above:

> In an adult rat [eating] is elicited by certain olfactory, visual or tactual stimuli. But these stimuli do not always evoke the behavior characteristic of them, and we say, among other things, that the rat eats only when it is hungry.
>
> The resulting variability of behavior is typical of the sort which has led to protestations of the inadequacy of the reflex concept. But variability in the observed as against the predicted does not question the validity of a law if the variability is itself lawful. In this instance, for example, it should be possible, once having determined a measure of the "strength" of these reflexes, to investigate the conditions under which this strength changes and to eliminate the variability by describing it in a further law. (1930, p. 434)

Though Skinner's case closely resembles Crozier's, the introduction of the concept of "strength" (which Skinner hedged in by quote marks at first) proved in time to be an ironic increment. Skinner pointed out (1932a) that even the simplest reflexes vary in strength, as shown by variations in latency, amplitude, threshold and so on. When we appeal to the rat's state of hunger to explain the variability in its eating reflexes, we are implicitly saying that reflex strength is some function of variables that might affect what the laymen calls hunger—such as time since feeding. All that remained, and which Skinner undertook to supply, were the specific, empirical functions controlling reflex strength in this case.

The two papers (1932a, 1932b) containing the full line of argument, plus some representative data on food-satiation in rats, were called "Drive and Reflex Strength I & II," which shows succinctly how Skinner infiltrated the mechanistic purity of Crozierian reflexology with "drive," the central concept, if not the actual terminology, of the hormic school of instinct.

It is worth pausing a bit over this amalgam, for it became the setting for psychology's most striking irony. Eating, according to the reflexive canon, was a chain of reflexes—approaching the food, grasping it, bringing to the mouth, chewing, swallowing, and on to the peristaltic movements of the alimentary canal. Each successive link in the chain turned up the stimuli for the elicitation of the next link. The subtlety and intricacy of adjustment bothered Skinner no more than it had William James 42 years before (see p. 80), although the almost certain compounding of error in such an open-ended mechanism might have alerted them to its implausibility. What did bother Skinner, however, was an *extra* factor, one not readily conceived of as merely another stimulus. The layman would say that the eating depended on "hunger"; the customary reflexive counterargument would have reduced hunger to various stimuli buried out of range of superficial observation, but no different in principle from any other stimulus. The muscular contractions of the stomach as hunger, the dryness of the mouth and throat as thirst (Cannon, 1929) typify the hidden stimuli of the reflexology of the time.

While not denying such hidden stimuli, Skinner added drives and, in doing so, acknowledged the incompleteness of a pure reflexology. Though he maintained both the vocabulary and allegiances of the reflexive school, he had defected. The variations that the layman calls "hunger," said Skinner (1932a), resemble reflex fatigue or refractory phase (although of opposite sign) more than the effects of varying stimulation. When rapid elicitation produces increased latency and decreased magnitude and probability in a reflex, classical physiology invokes the concept of fatigue, Skinner pointed out, not some unseen stimulus that inhibits the response. And so it is with hunger, argued Skinner. The "drive" is thus neither

stimulus nor response, but some intervening state whose presence or absence governs the strength (i. e., frequency) of the behavior—in this instance, eating.

But note that while fatigue or refractory phase applies to only a single reflex at a time (or, at most, a collection with some specific anatomical element in common), hunger applies to a large and varied collection, defined not anatomically but functionally. We know that the "reflexes" of grasping and swallowing share membership in the class of eating reflexes only because they rise and fall with the hunger drive. What sort of response class is this, in which membership arises not in common neural pathways but in common fate? Skinner's answer was the drive state, but another could have been McDougall's hormic instinct. Recall that for McDougall, each instinct was an intervening disposition favoring particular actions over others (i. e., their relative "strengths") according to their outcome. Had there been free and open exchange between McDougall and Skinner, McDougall would no doubt have claimed victory. Call it what you will, he might have said, but you now see that behavior is powered by inner states (like the instinct or drive of hunger). Furthermore, he might have noted, in addition to being *powered* by that inner state, it is also *directed*, as Skinner's experiments soon showed more elegantly than ever before.

In the second paper that year (1932b), Skinner reported the addition in the experimental chamber of a lever which the rat depressed to earn his food pellet and to simplify the recording of food consumption. Also, the lever pressing interested Skinner as an "arbitrary" response or "conditioned reflex." McDougall could have noted that when the rat pressed the lever and got fed, it revealed how new behavior depended on its relation to the inner state—the instinctive hunger. The hormic theory made easy sense where reflexology had to contort itself into new and uncongenial postures, adding a principle of learning that included drives. And, of course, Troland could have hardly found a better demonstration of the principle of retroflex action, as the food provided the beneceptive stimuli to stamp in lever pressing (or removed the nociceptive stimuli of hunger).

McDougall and Troland had both dwelt on the importance of learning in the expression of the primary urges or drives or instincts. And they both concluded that learning differed on the afferent and efferent sides. Adapting to life's circumstances meant learning the signalling stimuli for the various drives, but also learning the responses that served the drive's promptings. From environment to environment, both stimuli and responses were likely to change. Troland, as noted above, credited Pavlov for the principle of learning on the afferent side, and Thorndike for that on the efferent side. When Skinner hit upon the same, "two-factor" solution, he credited the acclaimed reflexologist Pavlov, but neither

McDougall, Troland, nor Thorndike (1935, after an incomplete formulation in 1932c). Like Hunter, he was implicitly showing the power of the general allegiance to the behaviorist alignments.

Skinner's version of the hormic theory may be paraphrased as follows (see especially Skinner, 1935). The innate behavioral endowment of an animal comprises reflexes and drives, the former being explicitly behavioral, the latter only indirectly so. The reflexes of a given animal (as a member of a species) are grouped according to categories corresponding to the drives, so that, for example, there are the *eating* reflexes, *drinking* reflexes, *sexual* reflexes, and so on. The adjectives correspond to the drives, just as they had earlier corresponded to the instincts. Membership in a particular drive's group implies that a reflex's strength (latency, threshold, magnitude, etc.) waxes and wanes with the state of the drive. A "hungry" animal is one whose eating reflexes are strong, a thirsty animal, one whose drinking reflexes are strong, and so on. On to this built-in, instinctive organization of behavior, learning provides the environmental correctives. A stimulus for one reflex could be tacked on to the response of another reflex. If, as was typically the case in Pavlovian experiments, the two reflexes "belonged" to different drives, the learning served to broaden the environmental scope of the drive on the stimulus side. In the second kind of learning, a response acquired membership in the group of some new drive. Thus, for example, pressing a lever might be merely an *investigatory* reflex for a rat to start with, but if it is instrumental in producing food, it becomes an (conditioned) *eating* reflex as well. Newly "shaped" responses, carved out of the tails of response-generalization distributions, similarly acquired membership in some drive's group. As he said, "Conditioning of Type I (i. e., operant conditioning) is really the becoming attached to a group of reflexes varying as a function of some drive" (1935, p. 69). Here, the broadening was on the response side. It hardly needs adding that Skinner's subsequent career, as well as the school named after him, has been largely dedicated to the voluminous tallying of adjustments in the efferent components of several drives, and to extrapolations of the findings to behavior in general.

Were it not for where the intellectual battle lines had been drawn, Skinner's splendid professional success would have seemed vindication of McDougall's claims for his theory in 1908. The dynamics of his instincts, it may be recalled, were supposed to provide "the bases from which the character and will of individuals and of nations are gradually developed" (1908, p. 19). And the dynamics of Skinner's "contingencies of reinforcement," which amount to approximately the same thing, are now offered to "supply a technology of behavior

appropriate to the ultimate utopian goal: an effective culture" (1969, p. 22). But the battle lines between the language of nativism and that of empiricism separate McDougall's theory from Skinner's, isolating them from each other and obscuring their essential similarity. Skinner, who speaks for *nurture* as opposed to *nature* in this generation, adheres to a theory of behavior promulgated in large measure by the last great systematic "nativist." Notwithstanding the similarities, McDougall's theory was shipwrecked on the same wave of environmentalism that Skinner's has ridden to preeminence.

Postscript

The theory, as distinguished from the practice, of Skinnerian behaviorism was essentially complete by 1935. He had by then identified two kinds of conditioning process—"respondent" and "operant" in his later terminology—and also the fruitful "three-term contingency," his later characterization of how behavior sorts itself in accordance with the prevailing conditions of reinforcement and punishment. He had also noted that operant behavior is keyed into certain drive states, like hunger, thirst, and so on. His later efforts (1953) to translate "drives" into deprivation and satiation do not alter his essential point that behavior falls into classes corresponding to internal states of the animal (such as food-getting in relation to hunger, in ordinary discourse).

The constancy of Skinner's theory may be taken as a sign of weakness, for being static, or as a sign of strength, for enduring. But in either case, its relevance here is that the overlap between McDougall's or Troland's theories and Skinner's would be no smaller had we taken Skinner's latest version instead of his earliest. From McDougall in 1908 to Skinner in 1971, there has been a conception of behavior comprising a set of inner states (instincts, needs, drives, tensions, appetites, deprivation states, etc.) and principles of modification regulating both stimuli and responding as they align themselves with the inner state's promptings. Each animal, by this view, is born with the potential for certain drives, and in that sense, the theory is nativistic. On the other hand, stimuli and responses are acknowledged to get hooked up to the various drives as a result of environmental vicissitudes, making the theory to that extent empiricistic. The din of the nature-nurture controversy has drowned out the essential continuity of thought as regards the principles of behavior.

Not everything is settled, to be sure. McDougall believed that each inner drive biassed the animal towards particular stimuli and responses. The sight or smell of food and its consumption seemed to him to have innate special status for a hungry animal. While experience could alter the relevant stimuli and responses,

the animal, for McDougall, started off with predispositions. In contrast, Skinner at first seemed to deny even those predispositions (by not discussing them), but lately (1966) he has found room for them in his discussion of the "phylogeny of behavior." The accumulating evidence for built-in stimulus (Seligman, 1970) and response (Segal, in press) biases has overtaken the simpler form of the theory, in which all the overt elements—the stimuli and responses—were acquired in experience, while only the inner states were innate. The essential theory can, of course, accommodate such complexities, as McDougall showed to begin with, but they come as a shock nevertheless to those who have grown up without them (Herrnstein, in press).

Other issues, too, await resolution. How many drives (or instincts) are there in a representative creature? Is it likely to be a short list, as McDougall thought, or a much longer one, as the modern ethologists (e. g. Lorenz, 1937) say? How do the drives interact? Is it merely in their separate effects on behavior or is there some more complex linkage among them? And, on the environmental side, what are the basic principles of modification? Are there two, as Skinner said, or three, as McDougall hinted, or one (Hull, 1943), or five or six (Pringle, 1951), or some other number? The longevity of the hormic theory may have more to do with how well it encompasses those (and many other) hard questions than with how well it answers them.

REFERENCES

Ayres, C. E. (1921). Instinct and capacity. 1. The instinct of belief-in-instincts. *Journal of Philosophy, 18*, 561-565.

Bernard, L. L. (1921). The misuse of instinct in the social sciences. *Psychological Review, 28*, 96-119.

Cannon, W. B. (1929). *Bodily changes in pain, hunger, fear and rage* (2nd ed.). New York: Appleton.

Carr, H. W, (1910). III. Instinct and intelligence. *British Journal of Psychology, 3*, 230-236.

Craig, W. (1912). Observations on doves learning to drink. *Journal of Animal Behavior, 2*, 273-279.

Craig, W. (1918). Appetites and aversions as constituents of instincts. *Biological Bulletin, 34*, 91-107.

Crozier, W. J. (1929). The study of living organisms. In C. Murchison (Ed.), *The foundations of experimental psychology* (pp. 45-127). Worcester, MA: Clark University Press.

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, MA: Clark University Press.

Diamond, S. (1971). Gestation of the instinct concept. *Journal of the History of the Behavioral Sciences, 4*, 323-336.

Dunlap, K. (1919). Are there any instincts? *Journal of Abnormal Psychology, 14*, 307-311.

Dunlap, K. (1922). The identity of instinct and habit. *Journal of Philosophy, 19*, 85-94.

Ebbinghaus, H. (1885). *Ueber das Gedächtnis.* Leipzig: Duncker & Humbolt.

Fabre, J. H. (1925). *The mason bees.* (A. Teixeira de Mattos, Trans.) New York: Garden City Publishing. (Original work published 1914)
Field. G. C. (1921). Faculty psychology and instinct psychology. *Mind, 30,* 257-270.
Herrnstein, R. J. (1967). Introduction. In J. B. Watson. *Behavior, an introduction to comparative psychology.* New York: Holt, Rinehart and Winston.
Herrnstein, R. J. (1969). Method and theory in the study of avoidance. *Psychological Review, 76,* 49-69.
Herrnstein, R. J. (1969). Behaviorism. In D. L. Krantz (Ed.), *Schools of psychology* (pp. 51-68). New York: Meredith.
Herrnstein, R. J. & Loveland, D. H. (1972). Food-avoidance in hungry pigeons, and other perplexities. *Journal of the Experimental Analysis of Behavior, 18,* 369-383.
Hobhouse, L. T. (1915). *Mind in evolution* (2nd ed.). London: Macmillan.
Hull, C. L. (1929). A functional interpretation of the conditioned reflex. *Psychological Review, 36,* 498-511.
Hull. C. L. (1943). *Principles of behavior.* New York: Appleton-Century-Crofts.
Hunter, W. S. (1920). The modification of instinct from the standpoint of social psychology. *Psychological Review, 27,* 247-269.
James, W. (1890). *The principles of psychology.* New York: Holt.
Jennings, H. S. (1906). *Behavior of the lower organisms.* New York: Columbia.
Kuo. Z.-Y. (1921). Giving up instincts in psychology. *Journal of Philosophy, 18,* 645-664.
Lashley, K. S. (1938). Experimental analysis of instinctive behavior. *Psychological Review, 45,* 445-471.
Loeb, J. (1900). *Comparative physiology of the brain and comparative psychology.* New York: Putnam.
Loeb, J. (1916). *The organism as a whole.* New York: Putnam.
Lorenz, K. (1970). Ueber die Bildung des Instinktbegriffes. In K. Lorenz (Ed. and Trans.), *Studies in animal and human behaviour,* Vol. 1 (pp 259-315). Cambridge, MA: Harvard. (Original work published 1937)
McDougall, W. (1908). *An introduction to social psychology.* London: Methuen.
McDougall, W. (1910). V. Instinct and intelligence. *British Journal of Psychology, 3,* 250-266.
McDougall, W. (1911). *Body and mind.* London: Methuen.
McDougall, W. (1921). The use and abuse of instinct in social psychology. *Journal of Abnormal Psychology and Social Psychology, 16,* 285-333.
McDougall, W. (1930). The hormic psychology. In C. Murchison (Ed.), *Psychologies of 1930* (pp. 3-36). Worcester, MA: Clark University Press.
Morgan, C. L. (1891). *Animal life and intelligence.* Boston: Ginn.
Morgan, C. L. (1895). Some definitions of instinct. *Natural Sciences, 7,* 321-329.
Morgan. C. L. (1896). *Habit and instinct.* London: Edward Arnold.
Morgan, C. L. (1910). II. Instinct and intelligence. *British Journal of Psychology, 3,* 219-229.
Morgan, C. L. (1912). *Instinct and experience.* New York: Macmillan.
Myers, C. S. (1910). I. Instinct and intelligence. *British Journal of Psychology, 3,* 209-218.
Myers, C. S. (1910). Instinct and intelligence. A reply. *British Journal of Psychology, 3,* 267-270.
Preyer, W. (1886). Ueber die Bewegungen der Seesterne. *Mittheilungen aus der Zoologischen Station zu Neapel, 7,* I. pp. 27-127; II. pp. 191-233.
Pringle, J. W. S. (1951). On the parallel between learning and evolution. *Behaviour, 3,* 174-215.
Segal, E. F. (1972). Induction and the provenance of operants. In R. M. Gilbert & J. R. Millenson (Eds.), *Reinforcement: Behavioral analysis* (Chapter 1). New York: Academic Press.
Seligman, M. E. P. (1970). On the generality of the laws of learning. *Psychological Review, 77,* 406-418.

Sherrington, C. (1906). *The integrative action of the nervous system.* New York: Scribner's.
Skinner, B. F. (1930). The concept of the reflex in the description of behavior. Unpublished doctoral dissertation, Harvard University.
Skinner, B. F. (1930). 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. (1935). Two types of conditioned reflex and a pseudo type. *Journal of General Psychology*, *12*, 66-77.
Skinner, B. F. (1938). *The behavior of organisms.* New York: Appleton-Century.
Skinner, B. F. (1953). *Science and human behavior.* New York: Macmillan.
Skinner, B. F. (1956). A case history in scientific method. *American Psychologist*, *11*, 221-233.
Skinner, B. F. (1966). The phylogeny and ontogeny of behavior. *Science*, *153*, 1205-1213.
Skinner, B. F. (1967). Autobiography. In E. G. Boring & G. Lindzey (Eds.), *A history of psychology in autobiography, V.* New York: Meredith.
Skinner, B. F. (1969). *Contingencies of reinforcement.* New York: Meredith.
Skinner, B. F. (1971). *Beyond freedom and dignity.* New York: Knopf.
Smith, S. & Guthrie, E. R. (1921). *General psychology in terms of behavior.* New York: Appleton.
Spencer, H. (1855). *The principles of psychology.* London: Longman, Brown, Green, & Longmans.
Stout, G. F. (1910). IV. Instinct and intelligence. *British Journal of Psychology*, *3*, 237-249.
Thorndike, E. L. (1911). *Animal intelligence.* New York: Macmillan.
Troland, L. T. (1928). *The fundamentals of human motivation.* New York: Van Nostrand.
Troland, L. T. (1930). Motivational psychology. In C. Murchison (Ed.), *Psychologies of 1930* (pp. 460-480). Worcester, MA: Clark University Press.
Washburn, M. F. (1908). *The animal mind.* New York: Macmillan.
Watson, J. B. (1914). *Behavior, an introduction to comparative psychology.* New York: Holt.
Watson, J. B. (1916). The place of the conditioned-reflex in psychology. *Psychological Review*, *23*, 89-116.
Watson, J. B. (1936). Autobiography. In C. Murchison (Ed.), *A history of psychology in autobiography, III* (pp. 271-281). Worcester, MA: Clark University Press.
Watson, J. B. & McDougall, W. (1928). *The battle of behaviorism.* London: Kegan Paul, Trench Trubner & Co.
Whitman. C. O. (1919). *The behavior of pigeons.* (Publ. No. 257, pp 1-161). Washington, DC: Carnegie Institution of Washington.
Wundt, W. (1894). *Vorlesungen über die Menschen- und Thierseele* (2nd ed.). In J. E. Creighton & E. B. Titchener (Eds. and Trans.), *Lectures on human and animal psychology*. New York: Macmillan. (Original work published 1892)
Zigler, M. J. (1923). Instinct and psychological viewpoint. *Psychological Review*, *30*, 447-460.