JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1999, **72,** 425–428 NUMBER 3 (NOVEMBER)

THORNDIKE'S LEGACY: LEARNING, SELECTION, AND THE LAW OF EFFECT

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This introduction to a symposium on the centennial of Edward L. Thorndike's 1898 monograph on animal intelligence briefly considers the origins of his law of effect and the influence of Darwin's selectionism. It also provides the background for an unfinished book review by William W. Cumming of a biography of Thorndike. The review places in historical context Thorndike's position both on psychology as a science of behavior and on the vocabulary of that science.

Key words: E. L. Thorndike, *Animal Intelligence* monograph, law of effect, learning theories, associationism, Charles Darwin, selectionism

A symposium celebrating the centennial of Thorndike's monograph, *Animal Intelligence* (Thorndike, 1898) was held in San Francisco on Saturday, August 15, 1998, at the annual meeting of the American Psychological Association. We shared our celebration with others who recognized the significance of the anniversary: This journal published a commemorative review by Lattal (1998), and the *American Psychologist* devoted a special section to Thorndike at about the same time (Dewsbury, 1998).

All four original participants in the symposium have contributed their papers to this special section of the *Journal of the Experimental Analysis of Behavior* (*JEAB*) dedicated to Thorndike's seminal work. The symposium included Paul Chance's discussion of Thorndike's experiments with puzzle boxes and other research reported in the monograph, Eliot Hearst's summary of Thorndike's later contributions to psychology, John A. Nevin's treatment of some implications of stimulus–response bonds, and John W. Donahoe's selectionist account of Thorndike's legacy. Our symposium was conceived and organized by Raymond C. Pitts, cochair of the Division 25 Program Committee, who graciously asked me to serve as chair rather than chairing the symposium himself. That invitation gave me the opportunity to include a posthumous participant, William W. Cumming, whose contribution took the form of excerpts from a book review of a Thorndike biography that he did not live to complete.

The monograph celebrated by this symposium has been a focus of discussion not only for its substantive content but also for its proper citation. Lattal (1992) points out the great variability in the ways it has been cited. The issue is particularly important because monograph supplements are not always bound in with the regular volumes of the journals of which they are a part and therefore may be difficult to locate even when they are in a library collection. Following Lattal's lead, let us therefore set that part of the record straight. Identical text appears on the speckled gray front cover and on the first inside page. As shown in Figure 1, the heading at the top reads *The Psychological Review.* An editorial listing indicates that the journal is edited by James McKeen Cattell of Columbia University and J. Mark Baldwin of Princeton University, "with the co-operation of" 10 others. The citation information then follows, with the publisher's name and address appearing at the bottom of the title page. The back cover gives some information about *The Psychological Review* and *The Psychological Index*, and then lists two volumes, each consisting of four monographs. Thorndike's is listed as number VIII in Volume II: "Animal Intelligence: Edward L. Thorndike. Pp. ii + 109. $1.00." Subscription information appears at the bottom of the page.

One topic that will be touched on by more than one presentation is the dating of the origin of Thorndike's law of effect. The law is certainly implicit in the 1898 monograph. The name came later, but in its early uses it

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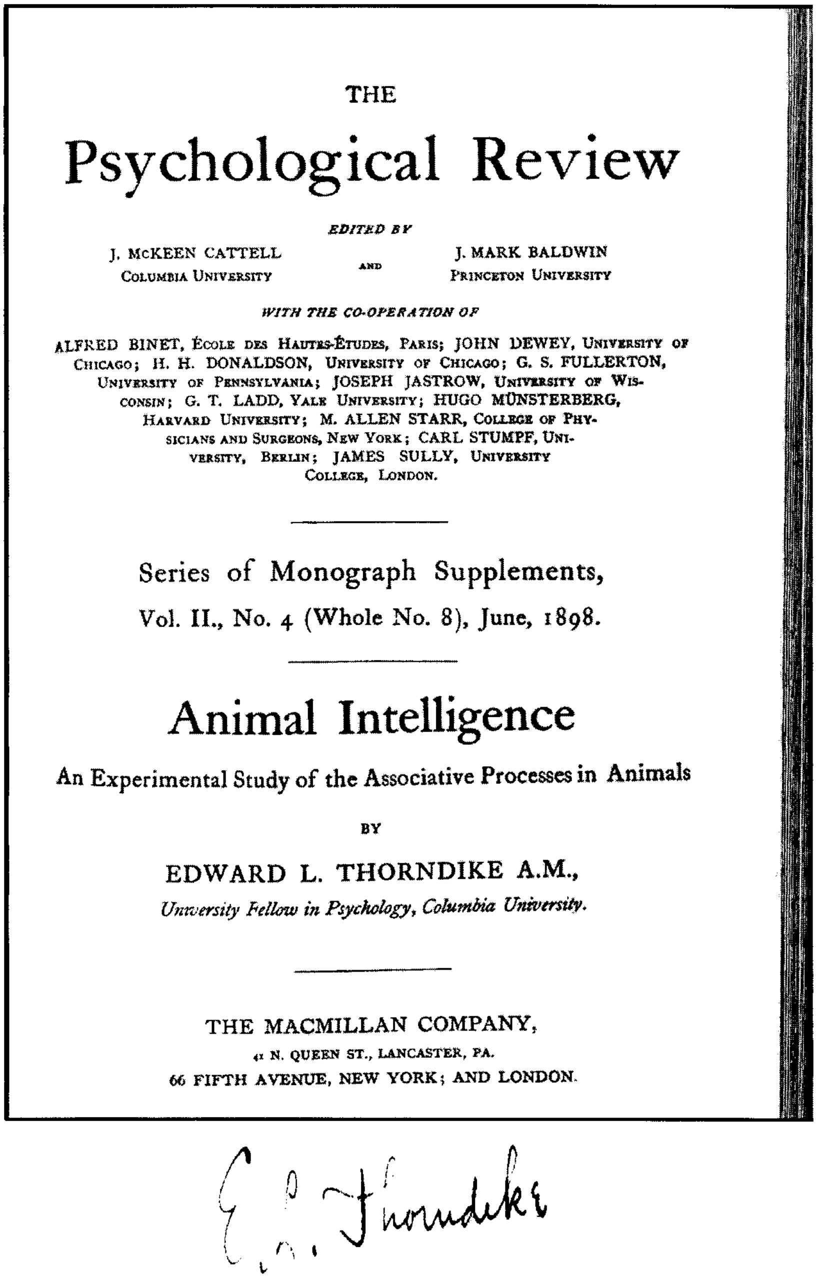

THE

Psychological Review

EDITED BY

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Series of Monograph Supplements,

Vol. II., No. 4 (Whole No. 8), June, 1898.

Animal Intelligence

An Experimental Study of the Associative Processes in Animals

BY

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University Fellow in Psychology, Columbia University.

THE MACMILLAN COMPANY,

41 N. QUEEN ST., LANCASTER, PA.

66 FIFTH AVENUE, NEW YORK; AND LONDON.

Fig. 1. The cover page of Thorndike's 1898 *Animal Intelligence* monograph. This page is reproduced from a custom-bound volume of Thorndike's that included a copy of his own monograph together with reprints of works by C. Lloyd Morgan, Willard S. Small, John B. Watson, and others. Thorndike's signature from the front of the volume appears below the cover page.

was applied to inferred neural changes rather than to changes in behavior. For example, consider the following (Thorndike, 1907, p. 166):

Stating the law in terms of connections made between cells, we could say: *Connections between neurons are strengthened every time they are used with indifferent or pleasurable results and weakened every time they are used with resulting discomfort.* This law includes the action of two factors, frequency and pleasurable result. It might be

> stated in a compound form as follows. (1) *The line of least resistance is, other things being equal, that resulting in the greatest satisfaction to the animal*; and (2) *the line of least resistance is, other things being equal, that oftenest traversed by the nervous impulse.* We may call (1) the *Law of Effect* and (2) the *Law of Habit.*

Our conclusions about date of origin will depend on whether we are interested in the word *effect* or its definition, and on whether we count definitions in terms of physiology as equivalent to those in terms of behavior. It should be evident that Thorndike's verbal behavior was itself a product of selection, and it is often difficult to place a date on the origin of an evolved or evolving form.

It is fitting to place this symposium and Thorndike's law in the context of a discussion of evolution by selection, because Thorndike's position was Darwinian from the start. The following is from Thorndike's *The Human Nature Club,* written only a short time after the publication of his monograph: ''The method of learning by the selection of successes from among a lot of acts is the most fundamental method of learning'' (Thorndike, 1901, p. 38). Even then, Thorndike meant selection by the consequences and not selection by the organism (in speaking of selection, it is crucial to specify what gets selected and what does the selecting).

> The gradual increase in success means a gradual strengthening of one set of nerve-connections, and a gradual weakening of others. This method of learning may be called the method of trial and error, or of trial and success. . . . The cause of such strengthening and weakening is the resulting pleasure in one case and discomfort in the others. (Thorndike, 1901, pp. 38–39)

In the later book version of his monograph, Thorndike treated learning itself as a product of selection:

> The most important of all original abilities is the ability to learn. It, like other capacities, has evolved. The animal series shows a development from animals whose connection-system suffers little or no permanent modification to animals whose connections are in large measure created by use and disuse, satisfaction and discomfort. (Thorndike, 1911, p. 278)

It has been suggested that Thorndike was more of a Darwinian early in his career than later (Jonçich, 1968). Whether or not that was the case, Thorndike wrote about Darwin and evolution (Thorndike, 1909) at a time when Darwinian selection still seemed to be in jeopardy as a legitimate account of the mechanism of evolution. Those involved in the dispute acknowledged evolution, but at issue was whether it was a product of natural selection (Catania, 1987). In this context, it may be of interest that the role of selection by consequences received explicit treatment only relatively late in Skinner's work, roughly half a century after Thorndike had virtually begun his career by treating learning in terms of selection (Catania, 1995; Skinner, 1953).

Now let us turn to the participant whose name did not appear on the program. Early during my editorship of the *JEAB* book reviews, I received a letter from H. S. Terrace. The letter said that Bill Cumming had suggested the review in *JEAB* of a biography of Thorndike by Geraldine Jonçich (1968). The letter went on to suggest that Cumming be invited to write that review (H. S. Terrace, personal communication, January 22, 1969). I extended the invitation and Bill Cumming accepted it, but he did not live to complete the review. He died early in 1970. His family made the working manuscript available, and I forwarded it to Bill's colleague, John A. Nevin, after having discussed with him the possibility of finishing the review: ''I enclose the materials for Bill Cumming's projected review. . . . I have made a photocopy, if it should be that we can find some way to complete the review. But there is so much still to be done that I can't feel optimistic'' (A. C. Catania, personal communication, April 28, 1970).

Cumming's working manuscript included multiple revisions of early sections, miscellaneous brief sections in both typed and handwritten form, and various odd notes and references. There was not enough to show how Cumming planned to continue and end the review, and we eventually dropped the project. But much of what Cumming had finished was about Thorndike's monograph and his law of effect, which had taken up only a chapter or so in the biography. When Ray Pitts invited me to chair this symposium, that provided a perfect opportunity to make Bill Cumming's unfinished work available, because the papers of the other participants would extend to the material that he had not covered and therefore would at last provide closure.

At the 1998 symposium, I read and commented on only a few passages from the manuscript, so as not to constrain the time available to the other participants. Here it is possible to allow William W. Cumming's unfinished *JEAB* review of Jonçich's (1968) biography of Thorndike to speak for itself. Although what follows has waited roughly 30 years for publication, the writing still seems fresh. From what was, we can guess at what might have been.

REFERENCES

Catania, A. C. (1987). Some Darwinian lessons for behavior analysis. A review of Peter J. Bowler's *The Eclipse of Darwinism. Journal of the Experimental Analysis of Behavior, 47,* 249–257.

Catania, A. C. (1995). Selection in biology and behavior. In J. T. Todd & E. K. Morris (Eds.), *Modern perspectives on B. F. Skinner and contemporary behaviorism* (pp. 185–194). Westport, CT: Greenwood Press.

Dewsbury, D. A. (Ed.). (1998). History of psychology: Commemorating E. L. Thorndike. *American Psychologist, 53,* 1121–1152.

Jonçich, G. (1968). *The sane positivist: A biography of Edward L. Thorndike.* Middletown, CT: Wesleyan University Press.

Lattal, K. A. (1992). Where is ''Animal Intelligence''? *The Behavior Analyst, 15,* 85–87.

Lattal, K. A. (1998). A century of effect: Legacies of E. L. Thorndike's *Animal Intelligence* monograph. *Journal of the Experimental Analysis of Behavior, 70,* 325–336.

Skinner, B. F. (1953). *Science and human behavior.* New York: Macmillan.

Thorndike, E. L. (1898). Animal intelligence: An experimental study of the associative processes in animals. *Psychological Review Monograph Supplement, 2* (4, Whole No. 8).

Thorndike, E. L. (1901). *The human nature club: An introduction to the study of mental life* (2nd ed.). New York: Macmillan.

Thorndike, E. L. (1907). *The elements of psychology* (2nd ed.). New York: A. G. Seiler.

Thorndike, E. L. (1909). Darwin's contribution to psychology. *University of California Chronicle, 12,* 65–80.

Thorndike, E. L. (1911). *Animal intelligence.* New York: Macmillan.

JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1999, **72,** 429–432 NUMBER 3 (NOVEMBER)

A REVIEW OF GERALDINE JONÇICH'S THE SANE POSITIVIST: A BIOGRAPHY OF EDWARD L. THORNDIKE

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This book review, unfinished at the author's death, examines in historical context Thorndike's law of effect, his *Animal Intelligence* monograph of 1898, and related works on learning and behavior.

Key words: E. L. Thorndike, *Animal Intelligence* monograph, law of effect, learning theories, associationism, impulse, emission

Very little in the published histories of psychology prepares us for the giant that emerges from the pages of Geraldine Jonçich's *The Sane Positivist: A Biography of Edward L. Thorndike* (1968). In his history of experimental psychology, Boring (1950) devotes two pages to the man, sandwiched in among other notes on ''Columbia's Functional Psychology.'' But Thorndike himself had been almost equally laconic in his autobiographical sketch in Murchison's (1936) *History of Psychology in Autobiography III.* Out of his busy schedule, he managed to free enough time to write seven pages on his own life. Thirteen years later, in the year of his death, he provided an additional two pages, concluding with ''It should perhaps be noted that I have spent much time and thought on educational science proper, as shown in various monographs and articles, most of them factual.'' Jonçich remarks: ''He told us less of himself than is necessary, certainly much less than we would wish to know'' (''One's first duty, then, is modesty,'' Thorndike had observed in *The Human Nature Club* in 1901).

Jonçich does not share this stinginess. In 591 pages (plus a 21-page ''essay on sources'') she examines the 75 years between 1874 and 1949, and the man whose boundless energies filled those years and left a more profound mark on psychology and education than many of us are aware. As B. F. Skinner remarked in a letter to Thorndike on February 7, 1939, while apologizing for not having acknowledged Thorndike in *The Behavior of Organisms*: ''I seem to have identified your view with the modern psychological view taken as a whole.'' Since so much of Thorndike's view became modern psychology, it was a reasonable confusion.

[The following quotation from Tolman (1938) was on a separate manuscript page; it is reasonable to guess that if Cumming had used it in his review it would have appeared about here:

> The psychology of animal learning—not to mention that of child learning—has been and still is primarily a matter of agreeing or disagreeing with Thorndike, or trying in minor ways to improve upon him. Gestalt psychologists, conditioned-reflex psychologists, sign-gestalt psychologists—all of us here in America seem to have taken Thorndike, overtly or covertly, as our starting point.]

The title of this book is its most regrettable feature. What Robert S. Woodworth had said of Thorndike in his own autobiography was: ''His sane positivism was a very salutory influ-

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Editorial note: I have lightly edited the manuscript and placed my notes or comments in brackets. Where more than one version of a passage was available in the manuscript, I included only the one that appeared to be Cumming's most recent and most edited version. Breaks separate text that appeared in different groups of manuscript pages. I omitted some material peripheral to issues of learning and behavior and some quotations from Thorndike and others that were unaccompanied by Cumming's own text. I shortened some extended quotations from Thorndike, especially where the same material is considered by other participants in this symposium. The manuscript did not include a reference section, and I was not able to confirm all of Cumming's references and quotations. For example, the 1907 second edition of Thorndike's *Elements of Psychology* is easy to find but not the 1905 first edition that Cumming cites. The complete working manuscript from which these excerpts have been drawn and correspondence related to the manuscript have been deposited with the Archives of the History of American Psychology at the University of Akron.—A.C.C.

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ence for a somewhat speculative individual like myself." With equal justice and less misdirection, Jonçich might have entitled her volume *Freak of Nature,* citing William James's reply when Thorndike sent him a $100 check to offset what he feared was a reduction in royalties for James's *Briefer Course* caused by the publication of Thorndike's *Elements of Psychology.* "Seriously, Thorndike, you're a freak of nature. When the first law of nature is to kill all one's rivals (especially in the school book line), you feed them with the proceeds." The check was returned with James's express hope that Thorndike's wife had not heard of his magnanimity "for she *ought* never to forgive you."

Freak of nature or eighth wonder, Thorndike was clearly a phenomenon of energy unequaled in prenuclear times. When the *Teachers College Record* compiled his bibliography at the time of his death, the list stood at 507 publications (about 50 of them books), but the returns on titles in press were still coming in. In judging this prodigious publication rate, one must remember, as Jonçich notes, that Thorndike slowed down after his retirement in 1940, reducing his mean number of publications per year to about seven through the next decade.

It is more than 70 [now 100] years since the first embryonic form of the law of effect emerged from the smelly and unventilated attic of Schermerhorn Hall. The publication of a biography of Edward Lee Thorndike seems an appropriate occasion for a review of the circumstances surrounding the birth of that profoundly important concept and the controversies which gave it meaning in its early development.

It is often worthwhile to examine the history of a concept in some detail, for it is only in historical perspective that we can see why a law took a particular form, how it was shaped by the intellectual forces present during its development and the reasons for the salience of particular aspects of the data being emphasized at the expense of others. The law of effect is no exception.

> That Thorndike comes to the Schermerhorn attic with intellectual baggage is clear. That he also comes with preconceptions of what empirical tests would reveal is as proper as it is inevitable: it is always man who frames the questions asked of nature, and the questions asked inevitably depend on prior theoretical considerations. (Jonçich, 1968, p. 143)

It is perhaps unfortunate that the 1898 dissertation *Animal Intelligence* is often unread today and in its place the more readily available book by the same name (1911) is often read and assigned to students. The issues in 1911 were not the issues of 1898. The law of effect appears clearly described by 1911 but was only hinted at in 1898, for the great central thesis which was buttressed by data in the dissertation was the inadequacy of classical associationism. Ideas were not associated, but rather an "impulse" (read "response") was associated with a situation. Associationism would never be the same again and the way would be prepared for the law of effect, which would develop in Thorndike's thinking through the following decade.

It is surprising today to rediscover just how much is in Thorndike's dissertation of 1898. There are the first vague gropings toward the concept of the operant. The dissertation attacked the notion that what was learned was an association of ideas. It had been the position of C. Lloyd Morgan that the *idea* of the act was associated with the idea of a previous pleasant experience. It was this doctrine that Thorndike attacked with vigor. The central principle of the dissertation, if indeed there can be said to have been a central principle, was placed in italics: "*No cat can form an association leading to an act unless there is included in the association an impulse of its own which leads to the act.*" In the language of modern theory: The response must be emitted before it can be reinforced.

"The association evidently concerned what it had *done,* what it had an impulse for, . . . not what it remembered, had a representation of." This is the great message of the doctoral research. Animals do not learn by imitation or by being shown how to make the response. They make the response by their own impulse.

To be sure, the law of effect in its early primitive form is there. Not stressed greatly, but there:

> The cat does not look over the situation, much

less *think* it over and then decide what to do. It bursts out at once into the activities which instinct and experience have settled on as suitable reactions to the situation "*confinement when hungry with food outside*." It does not ever in the course of its successes realize that such an act brings food and therefore decide to do it and thenceforth do it immediately from *decision* instead of from impulse. The one impulse, out of many accidental ones, which leads to pleasure, becomes strengthened and stamped in thereby, and more and more firmly associated with the sense-impression of that box's interior. Futile impulses are gradually stamped out. The gradual slope of the time-curve, then, shows the absence of reasoning. They represent the wearing smooth of a path in the brain, not the decisions of a rational consciousness.

Thorndike's dissertation struck boldly at the associationistic doctrines of George John Romanes [1892] and Conway Lloyd Morgan [1894]. There is an unfortunate tendency to regard Morgan as an early kind of behaviorist because of his "canon," which is avidly taught to introductory students and has even begun to distort our history books. In fact, Morgan held that animals learn by associating *ideas*. "The kitten has an impression of the ball with which it is playing, and the hungry dog may have an idea of a nice meaty bone," he says at one point (Morgan, 1894). "It would not be difficult to fill several pages with examples of association in animals but it is better to leave the reader to draw upon his own experience for supplementary cases. . . . Of course it is only when the idea suggested through association expresses itself in action that we can obtain evidence of its existence." It is true that Morgan stopped short of Romanes' conclusion that animals could "reason" (he did not deny it but simply felt that there was insufficient evidence).

Thorndike's use of the word "impulse" in this early writing is curious and no doubt is a vestigial intellectual remnant of his immersion in the thought of William James. *Impulse* is the conscious act.

> The word *impulse* is used against the writer's will, but there is no better. Its meaning will probably become clear as the reader finds it in actual use, but to avoid misconception at any time I will state now that impulse means the consciousness accompanying a muscular innervation *apart* from *that feeling of the act which comes from seeing oneself move, from feeling one's body in a different position, etc.* It is the *direct feeling of doing* as distinguished from the *idea of the act done* gained through the eye, etc. For this reason I say "impulse *and* act" instead of simply "act." Above all, it must be borne in mind that that by impulse I never mean the *motive* to the act. . . . Anyone who thinks that the act ought not to be thus subdivided into impulse and deed may feel free to use the word *act* for *impulse* or *impulse and act* throughout, if he will remember that the act in this aspect of being felt as to be done is in animals the important thing. (Thorndike, 1898, pp. 14–15)

While this distinction may seem odd to the modern reader, the reason for it is clear in the dissertation. Manual manipulation of the cat's paw so as to "demonstrate" the release mechanism of the puzzle box is insufficient to produce learning. This is the act stripped of its impulse. Similarly, the *act* can be observed in others (as in imitation) but no learning occurs for the impulse is not observed.

Out of the welter of mentalistic concepts Thorndike was struggling to put into words an embryonic notion of response emission. "The response must first be emitted to be reinforced" we glibly tell our introductory students, forgetting how much excess verbal baggage had to be dropped before this streamlined version could be emitted. Thorndike, like his cats, learned quickly which of his own responses were crucial, and by the time of the publication of *The Human Nature Club* in 1901 it is clear that he regards the word *act* as alone sufficient to convey his meaning.

The important thing to be noted in this intellectual history is that the concept of spontaneous emission had to precede the concept of effect. How does the response get there in the first place? Thorndike seems impatient with the question as of little pragmatic consequence. "Instinct," he says, and adds just to make sure that the reader understands that this is a shorthand for ignorance, "Anyone who objects to the word may substitute 'hocus-pocus' for it whenever it occurs. The definition here made will not be used to prove or disprove any theory, but simply as a signal for the reader to imagine a certain sort of fact.'' What sort of fact? That a response has occurred—''Any reaction to totally new phenomena.''

[The following, based on Herrnstein (1967), appeared in the first paragraph of an early draft of the review, just before the reference to Boring] ''Why, then,'' asks Herrnstein in his introduction to Watson's *Behaviorism*, ''do we call Watson a behaviorist, and not Thorndike?'' The answer to this rhetorical question is supplied in terms of the priority of Thorndike's eminence (''that virtually prohibited him from changing his affiliations'') and in Watson's refusal to deal with the law of effect.

[The next paragraph, from a handwritten page of the manuscript, quotes the 1905 statement of the law of effect cited elsewhere in this symposium; the brackets below are Cumming's] Postman (1947) cites Thorndike's 1911 book *Animal Intelligence* as the earliest statement of the law of effect. Actually, a law going by the name ''effect'' had been stated as early as 1905 in Thorndike's *Elements of Psychology*:

> Connections between neurones are strengthened every time they are used with indifferent or pleasurable results and weakened every time they are used with resultant discomfort. The line of least resistance [to the transmission of a nervous impulse] is, other things being equal, that resulting in the greatest satisfaction to the animal.

A law sounding somewhat closer to the later law of effect was also promulgated at that time and called the *law of habit formation*: ''Any act which in a given situation produces satisfaction becomes associated with that situation, so that when the situation recurs the act is more likely to recur.''

REFERENCES

Boring, E. G. (1950). *A history of experimental psychology* (2nd ed.). New York: Appleton-Century-Crofts.

Herrnstein, R. J. (1967). Introduction to *Behaviorism: An introduction to comparative psychology* by John B. Watson. New York: Henry Holt.

Jonçich, G. (1968). *The sane positivist: A biography of Edward L. Thorndike*. Middletown, CT: Wesleyan University Press.

Morgan, C. L. (1894). *An introduction to comparative psychology*. London: Walter Scott.

Murchison, C. (Ed.). (1936). *A history of psychology in autobiography. Vol. 3.* Worcester, MA: Clark University Press.

Postman, L. (1947). The history and present status of the law of effect. *Psychological Bulletin, 44,* 489–563.

Romanes, G. J. (1892). *Animal intelligence*. New York: D. Appleton.

Thorndike, E. L. (1898). Animal intelligence: An experimental study of the associative processes in animals. *Psychological Review Monograph Supplement, 2* (4, Whole No. 8).

Thorndike, E. L. (1901). *The human nature club: An introduction to the study of mental life* (2nd ed.). New York: Macmillan.

Thorndike, E. L. (1905). *The elements of psychology*. New York: A. G. Seiler.

Thorndike, E. L. (1911). *Animal intelligence*. New York: Macmillan.

Tolman, E. C. (1938). The determiners of behavior at a choice point. *Psychological Review, 45,* 1–41.

JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1999, **72,** 433–440 NUMBER 3 (NOVEMBER)

THORNDIKE'S PUZZLE BOXES AND THE ORIGINS OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR

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The year of Thorndike's dissertation on animal intelligence, 1898, may mark the beginning of the field that eventually became known as the experimental analysis of behavior. The dissertation began a major shift in thinking about animal and human learning, provided important methodological innovations, and carried the seeds of later research and theory, particularly by B. F. Skinner. Although Thorndike was an associationist in 1898, the dissertation began the systematic search for fundamental behavioral processes, and laid the foundation for an empirical science of behavior.

Key words: E. L. Thorndike, puzzle boxes, history, animal intelligence, learning, problem solving, associations

> When the crude beginnings of this research have been improved and replaced by more ingenious and adroit experimenters, the results ought to be very valuable. (Thorndike, 1898, p. 30f)

Attaching dates to the origin of a field of study is bound to start an argument, but a case can be made for placing the beginning of the experimental analysis of behavior in 1898. That was the year E. L. Thorndike published his famous dissertation, *Animal Intelligence: An Experimental Study of the Associative Processes in Animals.*

It was a banner year for Thorndike. In January he spoke before the New York Academy of Sciences. In June, a summary of his experiments appeared in *Science,* and *Psychological Review Monograph* published his dissertation in its entirety. Finally in December he described his work in an invited address at the seventh annual meeting of the American Psychological Association.

Although Thorndike was only 23 years old as the year opened, his research caused quite a stir. It provoked immediate criticism from members of the old guard. Wesley Mills (1899), an eminent animal psychologist at McGill University, suggested that Thorndike's findings were the result of the contrived nature of his experiments. C. Lloyd Morgan (1900), the leading figure in comparative psychology at the time, also complained about the artificiality of the experiments, and added that Thorndike's animals were less his subjects than his victims. Yet the appearance of the dissertation was widely recognized, as Thorndike's biographer Geraldine Jonçich (1968) would later write, as ''an undeniably important event'' (p. 148).

Jonçich may be guilty of understatement: The dissertation helped to establish comparative psychology as an experimental science; began a major shift in thinking about both animal and human learning; provided important methodological innovations in behavioral research; and carried the seeds of later research and theory by many others, most notably B. F. Skinner. I believe it also provides the starting point for the field that would become known as the experimental analysis of behavior. On this 100th anniversary of the dissertation, it is appropriate that we examine the experiments and theory it describes.

Experiments

Thorndike's general experimental method was ''to put animals when hungry in enclosures from which they could escape by some simple act'' (p. 6; all page references are to Thorndike, 1898). Thorndike typically placed food in view outside of the enclosure and observed how the animal behaved. He also recorded the time that elapsed before the animal escaped. He repeated this procedure over and over again and noted the change in the animal's behavior and in escape times.

The most famous of the dissertation experiments are those with cats in ''puzzle box-

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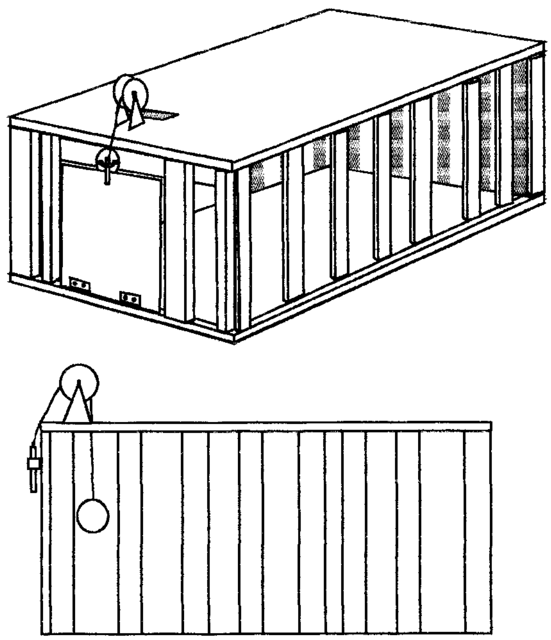

Fig. 1. Box A. Pulling on the loop (lower figure) released a bolt and the door fell open.

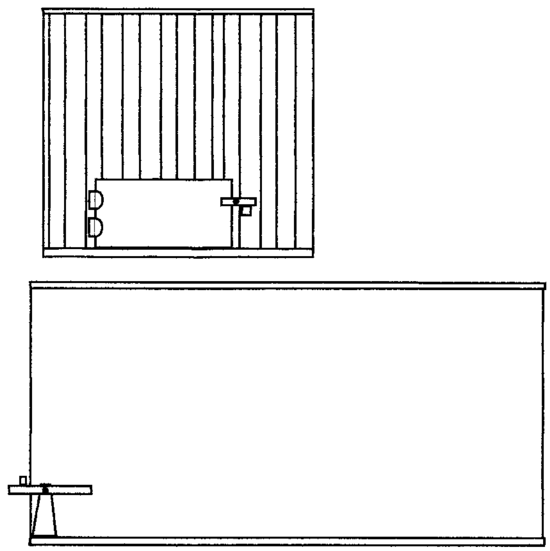

Fig. 3. Box I. Depressing a lever (lower figure) raised a bar that pivoted, allowing the door to swing open. (In the lower figure, the solid side of the box has been removed to reveal the lever.)

es.'' There were 15 of these boxes, and they were constructed mainly of wooden slats and hardware cloth. Each box contained a door that the cat could open by manipulating some device. A cat could open the door to Box A, for example, by pulling on a wire loop suspended six inches above the box floor (see Figure 1). The door to Box H could be opened by pushing it aside (see Figure 2). Cats opened the door to Box I by pressing a lever (see Figure 3). (The cat that first escaped from Box I may well deserve a place in history for being the first in a long line of lever-pressing animals.) Box K, the only box depicted graphically in the dissertation and the one that is most familiar to behaviorists, required the performance of three distinct responses: The cat had to depress a treadle, pull on a string, and push a bar up or down before the door would finally fall open (see Figure 4).

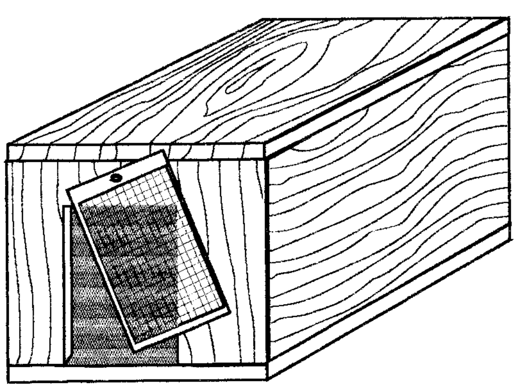

Fig. 2. Box H. The door pivoted on a screw and could be pushed aside to the right or left.

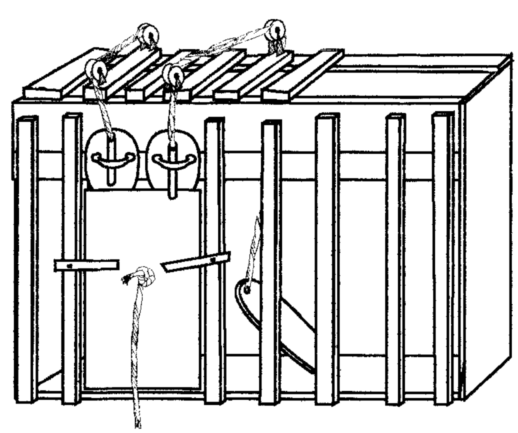

Fig. 4. Box K. The door is held in place by a weight suspended by a string. To open the door, a cat had to depress a treadle, pull on a string, and push a bar up or down. (After Thorndike, 1898, Figure 1, p. 8.)

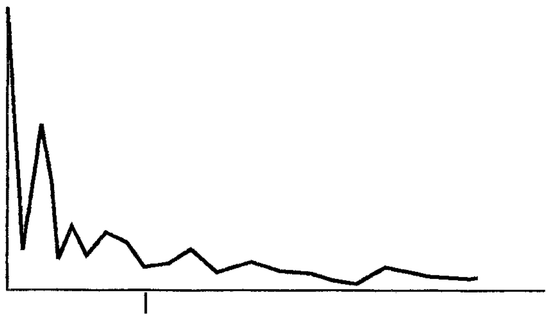

Fig. 5. Performance of Cat 12 in Box A. Box A required pulling on a loop. Trials are depicted on the abscissa, escape times on the ordinate. In this instance, escape times varied from a high of 160 s to a low of 6 s over 24 trials. The short vertical line on the abscissa indicates an interruption in training of about 24 hr. (From Thorndike, 1898, Figure 2, p. 18.)

Thorndike notes that a cat placed in such a box typically

> tries to squeeze through any opening; it claws and bites at the bars or wire; it thrusts its paws out through any opening and claws at everything it reaches; it continues its efforts when it strikes anything loose and shaky; it may claw at things within the box. It does not pay very much attention to the food outside, but seems simply to strive instinctively to escape from confinement. The vigor with which it struggles is extraordinary. For eight or ten minutes it will claw and bite and squeeze incessantly. (p. 13)

Thorndike found that when he put a cat into a given box again and again, the whole demeanor of the animal changed. At first the cat's behavior appeared to be almost random, one might even say chaotic. Gradually, however, it became more orderly, more deliberate, more efficient. ''The cat that is clawing all over the box in her impulsive struggle will probably claw the string or loop or button so as to open the door. And gradually . . . after many trials, the cat will, when put in the box, immediately claw the button or loop in a definite way'' (p. 13).

In addition to noting gross changes in behavior, Thorndike recorded the time required for the cat to escape and plotted these data on a graph to yield a ''time-curve.'' The time-curve for Cat 12 in Box A, which required pulling on a wire loop, shows a rapid and fairly steady decline in escape time and is typical of the performance of cats in boxes that require a single response (see Figure 5). The time-curve for Cat 4 in Box K, which required three distinct responses, shows slower and more erratic progress (see Figure 6).

Thorndike was interested not only in the

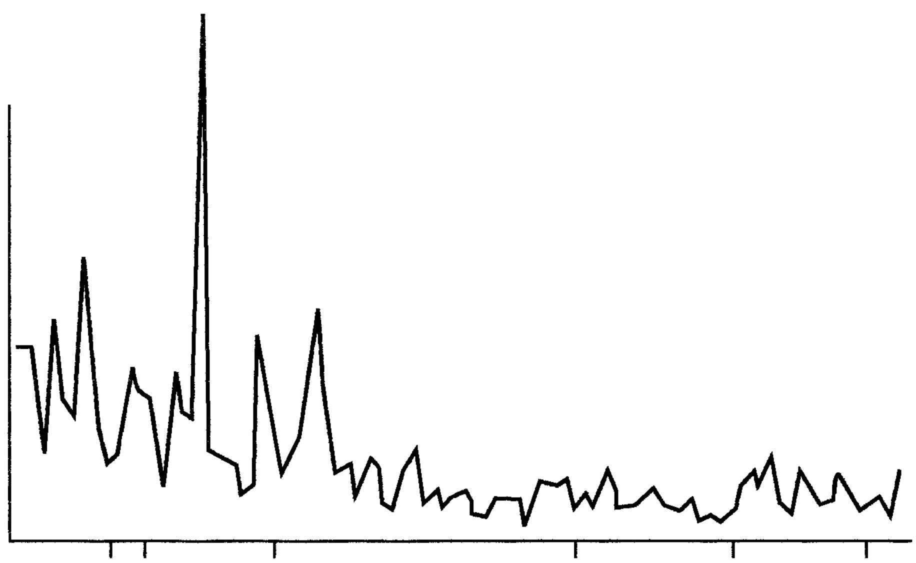

Fig. 6. Performance of Cat 4 in Box K. Box K required three distinct responses. The figure shows escape times on approximately 117 trials over a 7-day period. Progress was slow and erratic. (After Thorndike, 1898, Figure 10, p. 26.)

change in escape times, but in the *rate* of the change: He wrote that "we may take the general slope of the curve as representing very fairly the progress of the association" (p. 16). It was the slope of the curve, in other words, that showed the rate of learning. This interest in curve slope anticipates Skinner's (1938) use of the slope of a cumulative record.

Thorndike also did puzzle box experiments with 3 small dogs of unspecified breed. The nine puzzle boxes for dogs resembled those used for cats. The experimental procedure was also similar except that the period of food deprivation for dogs was shorter. This was because of the "practical necessity that the dogs should be kept from howling in the evening" (p. 32). The dogs showed the same sort of changes in gross behavior noticed in cats, with effective behavior persisting and becoming smoother and more efficient, and ineffective behavior dropping out. The time-curves for dogs also resembled those for cats.

Thorndike did experiments with chicks that showed the same patterns seen in dogs and cats. In some experiments a chick could escape confinement by stepping on a platform, pulling on a string, pecking a door, or pecking a tack. In other experiments the birds escaped from pens, some of which were constructed of books stood on end. To escape from one of the more complicated pens, a bird had to climb a spiral staircase, go through a hole in a wall, walk across a horizontal ladder, and jump off a ledge to reach food and the company of other chicks. Of course, today such a sequence of acts would be called a response chain.

Thorndike found that a confined bird typically "runs back and forth, peeping loudly, trying to squeeze through any openings there may be, jumping to get over the wall, and pecking at the bars or screen" (p. 36). Eventually the ineffective behaviors died out and the bird performed the act required for escape as soon as it was placed in the enclosure.

In most of the dissertation experiments, the animals escaped confinement by manipulating a device that caused a door to fall open. In some experiments, however, escape required Thorndike's intervention. For example, in one experiment Thorndike removed a chick from a box whenever it preened its feathers. In other experiments, Thorndike opened the door of a box when a cat licked or scratched itself. These efforts were generally successful. The chick that escaped a box by preening, for example, eventually "would whirl his head round and poke it into the feathers as soon as dropped in the box" (p. 27). However, these experiments generally produced greater variability in performance and shallower time-curves (slower rates of learning) than when the means of escape involved manipulating some part of the enclosure (see Figure 7).

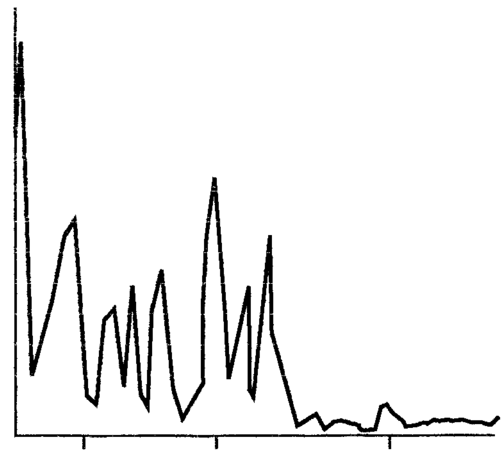

Fig. 7. Cat 5 in Box Z. The cat escaped from the box when it licked itself. The graph depicts about 64 trials over 4 days. The time-curve shows greater variability in performance and slower learning than when escape resulted from manipulation of some part of a box (cf. Figure 5). (After Thorndike, 1898, Figure 6, p. 22.)

Thorndike conducted some experiments on generalization, although he did not use that term. After an animal had learned to escape from one box, Thorndike put it into a different kind of box. He found that the animal took what it learned, as it were, to the next problem. "A cat that has learned to escape from A by clawing," he wrote, "has when put into C or G a greater tendency to claw at things than it instinctively had at the start, and a less tendency to squeeze through holes" (p. 14).

Other experiments involved discrimination. Thorndike noticed that some cats climbed the wire netting in their home cages when he was about to feed them. Could he get control over this behavior by systematically manipulating the environment? Thorndike tested this idea by saying, ''I must feed those cats!'' just before feeding them. At other times he announced, ''I will not feed them,'' and then did not provide food. Thorndike recorded whether the cat climbed up to the wire netting of its pen after each of the statements. He recorded error data in two frequency graphs, one showing the failure to climb to the netting at the first signal, the other showing climbing the netting at the second signal. The animal learned to respond appropriately to what would now be called the S^D (''I must feed those cats!''; i.e., a stimulus correlated with obtaining food by climbing) in 60 trials, but learning *not* to approach at the S^Δ (''I will not feed them''; i.e., a stimulus correlated with no food) took much longer—380 trials.

Thorndike also did a number of experiments on observational learning. His basic procedure was to allow a chick, cat, or dog to observe as another of its species escaped from an enclosure, and record the observer's tendency to imitate the model's successful action. For example, a box might have two compartments separated by a wire screen. A cat could watch a model escape from confinement and reach food by pulling on a string. The observer could then escape confinement by pulling a string, but typically it did not. Instead, it went through the same gradual learning process seen in cats that had not had the benefit of a model. In the end, Thorndike concluded that, at least for animals below the level of the primates, ''we should give up imitation as an *a priori* explanation of any novel intelligent performance'' (p. 62). Later experiments would prove him wrong on this point (e.g., Herbert & Harsh, 1944; Miller & Dollard, 1941).

Theory

Although the dissertation's empirical contributions are of major importance in the history of behavior analysis, the theoretical work, outmoded though it is today, may have been equally important to the development of the field. In Thorndike's day, most people, including scientists devoted to the study of comparative psychology, believed that higher animals such as birds, dogs, and cats learned through ''the association of ideas.'' This meant that they understood, in some primitive way, the logical relationships among events and used those ideas to reason their way through problems. In accounting for a cat opening a door by manipulating a latch, for example, George Romanes (1882) wrote, ''First the animal must have observed that the door is opened by the hand grasping the handle and moving the latch. Next she must reason, by 'the logic of feelings'—'If a hand can do it, why not a paw?' Then strongly moved by this idea she makes the first trial'' (quoted in Thorndike, p. 41).

The support offered for this theory, even in scientific circles, consisted largely of anecdotal evidence and casual observation. Thorndike argued that this led to a bias in favor of evidence supporting animal reasoning. ''Thousands of cats on thousands of occasions sit helplessly yowling,'' Thorndike wrote, ''and no one takes thought of it or writes to his friend, the professor; but let one cat claw at the knob of a door supposedly as a signal to be let out, and straightway this cat becomes the representative of the cat-mind in all the books. . . . In short, the anecdotes give really the . . . *super-normal* psychology of animals'' (p. 4f). There was, Thorndike said, no solid evidence that animals grasped ideas or learned through reasoning, and he set forth his arguments against the theory: First, the behavior of animals in an enclosure is impulsive and apparently random, not systematic and logical. The behavior of a cat in a puzzle box is initially ''just a mad scramble to get out'' (p. 43); the animal shows no sign of contemplation or thoughtfulness.

Second, the change in an animal's behavior is gradual, not abrupt. A cat becomes more and more likely to step on a treadle when put into the box, and less and less likely to scratch at the slats. If the animal understood the relationship between the pressing of the treadle and the opening of the door, the time-curve would show a sudden fall in escape times. The dozens of time-curves Thorndike produced ''show no such phenomenon. . . . The gradual slope of the time-curve, then, shows the absence of reasoning'' (p. 45).

Third, the animals show no sign of understanding the relationship between action and consequence even *after* they have learned to escape from a box. Thorndike found, for example, that a cat that had learned to escape a box by pulling on a loop would paw at the air where the loop had been, even when the loop was no longer there.

Fourth, animals learned only if they performed the necessary act themselves. As noted earlier, Thorndike saw no evidence that animals benefited from observing the performance of a model. He also found that putting the animal's limb through the required motion did not produce learning.

> For example, a cat would be put in B . . . and left two minutes. I would then put my hand in through the top of the box, take the cat's paw and with it pull down the loop. The cat would then go out and eat the fish. This would be done over and over again, and after every ten or fifteen such trials the cat would be left in alone. . . . The results . . . show that no animal who fails to perform an act in the course of his own impulsive activity will learn it by being put through it. (p. 68)

Thus, the actual performance of an act, not merely the *idea* of an act, was essential.

Having thus demolished, so he thought, the dominant theory of animal learning, Thorndike then offered an alternative. He proposed that animal learning has nothing to do with reasoning or the association of ideas; it occurs as a result of "trial and accidental success," a phrase that gave a new emphasis to the role of actions and their consequences. (Although Thorndike is typically said to have studied "trial-and-error" learning, he preferred the phrase "trial and success.")

Thorndike theorized that a cat is innately equipped with various "action impulses" when placed in a box. It has, for example, the impulse to bite the bars and to scratch at objects within the box. If pulling a loop results in escaping from a box, the impulse to pull on loops is strengthened; if the impulse to scratch at bars does not result in escape, that impulse is weakened. There is no association of ideas, said Thorndike, only the association of sensations, such as the sight of a loop, and the impulse to perform certain acts, such as clawing at things. Thorndike suggested a mechanical "stamping in" of these associations.

As this summary of the theory implies, Thorndike distinguished between impulse and act. An impulse was the *feeling* of performing an act, as distinct from the act itself. Although Thorndike would later speak of responses rather than impulses, in the dissertation it was the *feeling* of acting, and not the act itself, that concerned him. He wrote that the important thing was "the feeling of the doing"; the act itself was "a secondary affair" (p. 15).

The resulting view of learning requires no ideas in the head of the animal:

> The cat that is clawing all over the box in her impulsive struggle will probably claw the string or loop or button so as to open the door. And gradually all the other non-successful impulses will be stamped out and the particular impulse leading to the successful act will be stamped in by the resulting pleasure, until, after many trials, the cat will, when put in the box, immediately claw the button or loop in a definite way. (p. 13)

It may surprise some readers to learn that this statement is as close as Thorndike gets to a formal expression of the law of effect in the dissertation. In fact, the phrase "law of effect" never appears in this work.

Thorndike anticipated that his theory would be challenged on the grounds that some instances of learning are too remarkable to be the result of an essentially random process. The behavior of a cat that opens a latched gate, for example, seems too complicated to be the result of accidental success. "The whole substance of the argument vanishes," Thorndike wrote, "if, as a matter of fact, animals do learn those things by accident. *They certainly do*" (p. 40). If animals in carefully controlled experiments can learn, through trial and accidental success, to perform an act, then there is no reason to suppose they acquire the act in any other way in more natural settings.

Assessing the Dissertation

Anyone who is interested in the study of behavior as a natural science must recognize Thorndike as a seminal figure: He replaced isolated anecdotal reports with repeated experimental observation; introspection with systematic description; and subjective impressions with numerical data. He was probably the first to study the process of learning in a truly systematic way, and was one of the first to capture learning in the slope of a line graph. At a time when others speculated about animal cogitations, Thorndike showed that a particular cat in a particular box took a certain number of seconds to perform the precise act required for escape.

Thorndike began the search for fundamental behavioral processes, and noticed that those processes seemed remarkably similar in different species. The chicks learned more slowly than the dogs and cats, but ''the biggest part of the difference . . . is not referable so much to any difference in intelligence as to a difference in their bodily organs and instinctive impulses'' (p. 38).

Thorndike recognized not only what would today be called reinforcement, but extinction, generalization, discrimination, and response chaining. He realized that effective responses are ''selected by success'' (p. 14), and understood that three elements are essential to the description of behavior: the situation, the act, and the consequence. He introduced the lever press and the disk peck as dependent variables (although the disk was a tack suspended by a string), and emphasized individual rather than group data.

Thorndike rejected Cartesian dualism, and issued this challenge: ''To the interactionists I would say: 'Do not any more repeat in tiresome fashion that consciousness *does* alter movement, but get to work and show when, where, in what forms and to what degrees it does so' '' (p. 104). This challenge is still being issued to dualists today.

In all of these contributions, and others, we can see the roots of *The Behavior of Organisms* (Skinner, 1938) and of much of the work that followed it. Yet it would be wrong to leave the impression that Thorndike was a Skinnerian 6 years before Skinner was born. He was not. He was as much an associationist as Romanes, except that in his view it was not ideas that were associated, but sensations and impulses. And despite his criticism of the subjectivism of others, he was himself, at least in 1898, a mentalist concerned with the ''inner nature'' of the animal's associations; the facts ''as observed from the outside'' were of limited interest to him (p. 38).

He was also guilty on occasion of the same sort of careless reasoning that he criticized in other animal researchers. Thorndike tells us, for example, that he repeated his experiments with a given animal ''until the animal had formed a perfect association between the sense-impression of the interior of that box and the impulse leading to the successful movement. When the association was thus perfect, the time taken to escape was, of course, practically constant and very short'' (p. 6). This is clearly circular: There was no evidence of any association between sense-impressions and impulses except the ''practically constant and very short'' escape times from which it was inferred.

We can also see, with the telescopic lens provided by a hundred years of research, that Thorndike failed to appreciate the significance of some of his own findings. He was puzzled, for example, by the fact that when escape was the result of licking or scratching, there was ''a noticeable tendency . . . to diminish the act until it becomes a mere vestige of a lick or scratch. . . . The licking degenerates into a mere quick turn of the head with one or two motions up and down with tongue extended. Instead of a hearty scratch, the cat waves its paw up and down rapidly for an instant'' (p. 28). Later research by others would suggest that the behavior ''degenerated'' only because the requirement for reinforcement degenerated.

Similarly, although Thorndike recognized the selective effects of consequences, and knew that animals could perform complex response chains, he seems to have had no idea that an experimenter might shape behavior or build chains by carefully arranging consequences.

Finally, if we judge Thorndike's dissertation against current standards, we must admit that it would be unlikely to win a doctorate in behavior analysis today, or even in general experimental psychology. His experimental methods, his data analysis, his theoretical arguments—all major contributions of his day—would be unacceptable.

Were Thorndike alive, I do not believe he would take offense at this assessment. Indeed, he anticipated it. He wrote in the dissertation, ''When the crude beginnings of this research have been improved and replaced by more ingenious and adroit experimenters, the results ought to be very valuable'' (p. 30f). Whether there have been any more ingenious and adroit experimenters than Thorndike is a matter for debate, but it is clear that we should not hold him to a standard arrived at after a hundred years of the experimental analysis of behavior. Instead, we should recognize that it was his great contributions that laid the foundation for that new science.

REFERENCES

Herbert, M. J., & Harsh, C. M. (1944). Observational learning by cats. *Journal of Comparative Psychology, 37,* 81–95.

Jonçich, G. (1968). *The sane positivist: A biography of Edward L. Thorndike.* Middletown, CT: Wesleyan University Press.

Miller, N. E., & Dollard, J. (1941). *Social learning and imitation.* New Haven, CT: Yale University Press.

Mills, W. (1899). The nature of animal intelligence and the methods of investigating it. *Psychological Review, 5,* 262–274.

Morgan, C. L. (1900). *Animal behavior.* London: Edward Arnold.

Romanes, G. J. (1882). *Animal intelligence.* New York: Appleton.

Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis.* New York: Appleton-Century-Crofts.

Thorndike, E. L. (1898). Animal intelligence: An experimental study of the associative processes in animals. *Psychological Review Monograph Supplement 2* (4, Whole No. 8).

JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1999, **72,** 441–446 NUMBER 3 (NOVEMBER)

AFTER THE PUZZLE BOXES: THORNDIKE IN THE 20TH CENTURY

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From the beginning of this century, following the publication of his dissertation, Thorndike made many significant contributions to psychology, some related to animal and human learning and others to various areas of educational psychology. This paper concentrates on the former and mentions some of the latter, in the context of personal and professional aspects of his life.

Key words: E. L. Thorndike, connectionism, law of effect, law of exercise, reward versus punishment, spread of effect, learning without awareness

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After completing his dissertation with the puzzle boxes, Thorndike had one overriding goal: to find a steady job or a good postdoctoral position, which is not so much different from the situation facing many new, excellent PhDs today. He lost out for openings at New York University and Wesleyan (his alma mater), and possibilities at Columbia's Teacher's College were dubious. He considered obtaining a second doctorate, this time at Harvard, when he got an offer from the Normal School in Oshkosh, Wisconsin, at $1,800 a year. This option did not seem too attractive to him, but he soon received and accepted an offer from the College for Women of Western Reserve University in Cleveland as a Special Lecturer in Education for only $1,000 a year. His brother was also going there in 1898, to join the English Department, and the Ohio city was a metropolis compared to Oshkosh. At Western Reserve Thorndike said he didn't enjoy teaching and felt ''only like doing experiments.'' It all ended happily because within a year Columbia's Teachers College invited him to return to New York (which he had earlier denigrated as ''that treeless city''). Although he did not plan on remaining at Columbia for more than a few years, he stayed for the rest of his official academic career (41 years). All reports indicate that the satisfying effects of his activities at Teachers College were mainly responsible for this long connection (see Jonçich, 1968; Thorndike, 1936).

Thorndike and many others realized that his 1898 studies marked the first deliberate and extended application of the experimental method to animal learning. When Pavlov later heard of Thorndike's work, he admired it and credited him with having started objective research on animal learning a few years before his own studies of conditioning.

Thorndike's trailblazing research with animals ended a few years later and he did not return to nonhuman experiments for about 30 years, and then only briefly. Nevertheless, the basic principle of his so-called connectionism—that is, that the satisfying consequences of an action in a situation lead to a bond between the situation (S) and the behavior (R)—became a view that dominated theoretical interpretations of learning, animal and human, for 30 to 40 years. This was true even though Thorndike never presented a tightly organized systematic approach anywhere in his writings. He did not bother to give the principle a specific name, the law of effect, until more than 5 years after his dissertation was published (Wilcoxon, 1969, p. 10, states that the name first appeared in print in Thorndike, 1905).

Because psychologists and students often do not fully grasp the point, any discussion of Thorndike's ideas should make it clear that for him the so-called ''effect'' (i.e., satisfaction or annoyance) was not part of the association that was established during learn-

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For their aid and inspiration, I thank the other speakers at the American Psychological Association's symposium celebrating the centennial of Thorndike's dissertation. This event took place in San Francisco in August of 1998 and was the time when the original version of this paper was presented. Raymond Pitts was responsible for organizing the symposium and bringing the participants together; without his work the symposium would not have been as successful as it was.

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ing. Only S-R bonds (''sarbons,'' as they later were jokingly referred to) were formed, with the consequent effect being the mechanism or catalyst for the stamping in or stamping out of these bonds. Subjects did not form stimulus–reinforcer or response–reinforcer associations, as we might call them today; they did not come to ''expect'' anything in the situation; they did not associate ideas of the situation with ideas of satisfaction or discomfort. Speaking loosely, they basically came to ''feel like'' making or not making a particular response in a particular situation, and this outcome was the result of an automatic, mechanical (and for humans, often unconscious) process. Thorndike eventually came to think that most animal and human learning could be explained without too much more theoretical baggage than the law of effect and the law of exercise—the belief that mere repetition of a response in a situation could also strengthen a bond between the S and the R.

In the 20th century Thorndike's interests were incredibly diverse (he ended up with more than 500 publications, many of them lengthy books), and some of these interests deserve mention before I focus on his experimental research comparing the law of effect and the law of exercise. His presidential address to the American Psychological Association in 1912 (see Thorndike, 1913; the address has also been reprinted more recently in Hilgard, 1978, pp. 105–117) was on the topic of ideo-motor action, in which he ridiculed the notion that the idea of a response could produce that response. Can the idea of sneezing cause a sneeze, and does thinking ''Wake up!'' lead directly to one's getting out of bed? Certainly not, he claimed; it was more like a type of magic or superstition, and if anything like ideo-motor action really occurred it was best explained as an example of habit formation through S-R bonds and the law of effect.

In educational psychology Thorndike may be best known for his development of various mental tests. He believed in a strong role for genetic factors in the determination of intelligence, although he thought intelligence was composed of several different abilities or skills, as opposed to Spearman's more ''general'' conception of it. He wrote and worked on such topics as: aspects of vocational guidance; wants, attitudes, and social engineering; vocabulary lists and dictionaries (for which he counted thousands of different word frequencies, obviously without a computer and even without a calculator to aid him); the psychology of arithmetic, for which he recommended practical, real-life problems (not like one he had seen: ''Mary had just cut out 35 paper dolls when the wind blew 16 away. How many were left?'' In real life you'd never know the number blown away but would merely count those remaining); analyses of school dropouts; the construction of valid entrance exams for selective schools; handwriting studies; urban sociology (his surveys rated Pasadena at the top for ''general goodness of life'' and therefore one of the most pleasant cities in the United States to inhabit); and especially the famous set of experiments (many of them done with Robert Woodworth; see chap. 12 in Jonçich, 1968, for an extensive discussion) concerning transfer of training, research that was instrumental in weakening the prevailing attitude that, for example, Greek and Latin were essential to an academic curriculum because they disciplined the mind and helped learning in such fields as mathematics and logic. The outcome of these studies strengthened his belief in the specificity of learned responses, as well as elicited his warning that transfer of skills from one field to another might be possible but should not be taken for granted. At any rate, from all the work they produced it is clear that Pavlov and Thorndike shared another characteristic: Although Thorndike's overall interests were much more varied, they were both workaholics.

Now let us concentrate on Thorndike's experimental work, mostly with humans, that is pertinent to his two original laws, to the comparative efficacies of reward versus punishment, and to the automaticity with which the law of effect supposedly acted. In both animal and human learning, Thorndike's views anticipated important contemporary issues and raised significant questions that are still not clearly resolved. Around 1930 he returned with gusto to the topic of learning itself, which he believed was the most important area in psychology and the one to which basic research could make the greatest practical contributions. Of course the typical reader of this journal would agree with him.

His new experiments led him to admit frankly, ‘‘I was wrong.’’ What was he wrong about? First of all, he performed a variety of studies with human beings to investigate whether mere practice or exercise of a response actually increased the likelihood, accuracy, or stereotypy of that response (see Postman, 1962, for a review). For example, in probably the best known of these experiments, he asked what must have been very compliant (and surely very bored) subjects to draw hundreds and hundreds of 4-in. lines while blindfolded. The subjects received no feedback on their performance and he did *not* find, as the law of exercise would presumably predict, that the initially most frequent line lengths drawn by subjects became more and more numerous or that their drawings usually became more stereotyped as trials progressed. With specific feedback about the *actual* length of lines drawn on each trial, subjects did improve, as the law of effect would predict. So, Thorndike concluded that the mere exercise of a response may play some role, but is not very important in linking Ss and Rs; the emission of a response in a situation was primarily needed only to allow the law of effect to operate.

Thorndike’s other revision of his original views came from animal and human research on the relative effectiveness of reward versus punishment. According to the law of effect, S-R bonds are strengthened by satisfying consequences and weakened by annoying or discomforting consequences. Were these outcomes symmetrical, though in opposite directions? To study this question, Thorndike (1932) brought young chicks back into his life and that of his family. A typical study allowed chicks to choose among three alleys, one of which led to exposure for a minute to a large compartment in which other chicks were gathered and there was grain scattered all over the floor—what he called ‘‘freedom, food, and company.’’ Choice of any of the other two alleys led to the chick’s being confined for 30 s in a small area. Obviously, choice of the large compartment was considered satisfying and either of the other two punishing. His ways of evaluating preexperimental choice preferences and of establishing baselines to assess response biases, as well as the kinds of preliminary training he gave to overcome such biases, were criticized, but Thorndike was well aware of these problems and discounted them because so many different kinds of his experiments pointed to the same conclusion: namely, that reward always strengthened a choice whereas punishment had little, if any, effect on the subsequent occurrence of a choice.

In a biographical sketch (R. L. Thorndike, 1991) about his famous parent, Robert L. Thorndike recalled that he ran many of these studies for his father and that, right after punishment, chicks were often reluctant to enter any alley on the next trial. These trials were scored as a ‘‘nonresponse’’ and were not counted. Clearly, this method of scoring the data would lessen any measured effects of punishment, as Robert Thorndike himself suggested. And, besides, the big issue is whether the relative magnitudes of reward and punishment had been fairly equated. Isolation in a small space for 30 s is not a very powerful form of punishment and, anyway, how can one justifiably contrast this procedure with 1 min of freedom, food, and company?

Thorndike performed numerous human experiments directed at the same question (see Postman, 1962, for a review), most of which employed symbolic rewards and punishments, such as announcing ‘‘right’’ or ‘‘wrong’’ after a response in situations involving very long lists of words to which subjects had to respond with a number or another word. Conclusions turned out the same as in the animal work: Subjects were likely to repeat a response that they were told was ‘‘right’’ and did not appear to decrease the likelihood of a response that was announced as ‘‘wrong.’’

Therefore, Thorndike rescinded the second half of the law of effect and declared that punishment was nowhere near as effective in reducing the probability of a response to a stimulus as was reward in increasing it. If punishment had an effect, it was indirect; punishment might possibly increase the variability of behavior and allow a response to emerge that could be rewarded. Once again, the question arises as to whether he fairly equated reward and punishment in his human studies. ‘‘Right’’ gives you definite information about the appropriateness of a certain response, whereas ‘‘wrong’’ tells you only that a particular choice or response was incorrect and nothing specific about which one is correct. At any rate, Thorndike's slogan became ''spare the reward and spoil the child'' and, even though the official revision of his original statement did not come until all his own children were adults, his family reported that he had never used force or punishment to influence their behavior.

Readers of this journal, especially, will have recognized that Thorndike's views on reward versus punishment matched Skinner's early conclusions. From research reported in *The Behavior of Organisms* (1938) and in parts of William Estes' doctoral thesis under Skinner at Minnesota (Estes, 1944), Skinner concluded that punishment had only temporary weakening effects, compared to the long-lasting, strongly strengthening effects of positive reinforcers. When it later became clear that relevant conclusions depended on the severity of the punishment or the magnitude of reward, Skinner admitted that punishment could be quite effective in weakening behavior. Still, he advised that its use ought to be avoided, because of its various emotional effects, influences on social interactions, and so forth.

The final set of experiments that are worthy of special mention in connection with Thorndike's contributions to the field of learning relate to what he considered to be the automatic, mechanical action of the law of effect; its simple operation did not involve reasoning or logic or conscious deliberation. He believed that his discovery of the spread of effect (Thorndike, 1933) was the most significant finding of his career. The spread of effect refers to situations in which human subjects are arbitrarily told ''right'' for, say, guessing any number from 1 to 10 in response to a particular word on a list, say the fifth one, and also told ''wrong'' for guessing any number to surrounding word items. He found that the frequency of later repetition of responses to items previously labeled ''wrong'' depended on how close that item was to the one labeled ''right.'' He obtained a gradient of decreasing response repetition both before and after the serial position of the ''right'' item. Thorndike argued that there is no logical reason why one wrong response should be repeated more often than any other, and yet the effect of saying ''right'' for a guess to an item spreads automatically to other items, even ''wrong'' ones, depending on their proximity to it.

The amount of research inspired by Thorndike's results on this topic was huge (see the deep analysis of this work in Postman, 1962). Some studies were attempts to establish better baselines from which to assess positive and negative effects, and other work examined alternative explanations for the phenomenon. Today, empirical results justify our acceptance of a spread of effect for ''wrong'' items immediately *following* the one labeled ''right,'' but not for *preceding* items. Currently the gradient effect is usually explained in terms of stimulus generalization of serial positions within a list (see Estes, 1969), but this is not the place to delve into the intricacies of that type of analysis, except to say that Thorndike had thought of it and believed he could rule it out.

Two other topics of continued contemporary interest emerged from other work of Thorndike that was also directed at showing the automatic, unconscious effects of reward; he declared that the process was ''as natural in its action'' as a stone falling from the sky or a hormone being secreted into the blood. The first line of research concerned the possibility of ''learning without awareness'' and the second ''unintentional or incidental learning'' (once again, Postman's 1962 chapter provides a scholarly review of this work). With respect to the former, for example, he would ask subjects to choose the one of two cards that had longer lines on it; but in actuality the lines were of equal length on both cards. Nevertheless, he would tell the subjects ''right'' if some presumably irrelevant feature like a small inkblot appeared somewhere on the card they chose, and ''wrong'' if it did not. Thorndike eliminated subjects who caught on to the significance of the inkblot, but an examination of results for subjects who insisted that they were choosing solely on the basis of line length revealed that they still displayed a great improvement in their scores, which could be based only on the presence versus absence of the feature.

Thorndike's son, the ever-faithful Robert, helped him with several of these experiments (see R. L. Thorndike, 1991), in one case buying for the research hundreds of different Christmas cards, a good number of which had the color gold somewhere on them. In a series of simultaneous choices supposedly tapping the esthetic appeal of the different cards, the experimenter would then ask subjects which of the two cards they liked best. If they chose the one with gold on it he would say, "Yes, I like that one, too." Even though the experiment was not set up in a right versus wrong arrangement, many subjects came consistently to select the golden one. Determining whether the subject was aware of the occurrence and basis for the experimenter's favorable comments is a tricky business, and contemporary researchers seriously question the existence of learning without awareness in many situations like those Thorndike first devised.

This work is also pertinent to the second line of study just mentioned above, that is, how much learning can occur that is incidental to the main task assigned the subject. Presumably, there is no intent to learn about supposedly irrelevant but still salient or predictive aspects of stimuli, because learning via reward or punishment may be explicitly given on the basis of some other property of the stimuli.

The present article was not designed to exhaustively criticize and analyze the details of any specific experiments Thorndike performed. What remains impressive is the large number of topics he studied and the alternative explanations he considered, many of which retain interest today. His views on the need for action and behavior on the part of the subject had obvious influences on neobehaviorists like Hull, Guthrie, and Skinner. Readers may be particularly interested to learn that Skinner was justifiably criticized by Hilgard (1939) in a review of *The Behavior of Organisms* for ignoring Thorndike's much earlier work on instrumental conditioning. Skinner's book included only two sentences directly relevant to the topic: "A conditioned response of Type R does not prepare for a reinforcing stimulus, it produces it. The process is very probably that referred to in Thorndike's Law of Effect" (1938, p. 111).

Skinner wrote Thorndike a letter in 1939 stating that

> Hilgard's review of my book . . . has reminded me how much of your work in the same vein I failed to acknowledge. In searching my soul to learn why the acknowledgments were never made I get only this far: (1) I have never seen an advertised and promoted "system" under your name and, (2) I seem to have identified your point of view with the modern psychological view taken as a whole. It has always been obvious that I was merely carrying on your puzzle box experiments, but it never occurred to me to remind my readers of that fact. I don't know why I mention this, because I can't imagine that it bothers you in the least.

Thorndike's gracious response to Skinner was typical of the man whose initial studies inspired the collection of articles about him in this issue of *The Journal of the Experimental Analysis of Behavior.* He wrote to Skinner: "I am better satisfied to have been of service to workers like yourself than if I had founded a 'school' " (the Skinner–Thorndike exchange can be found in Jonçich, 1968, p. 506). Considering that I have had to omit, among other things, mention of Thorndike's views on brain size and learning ability or intelligence, his evolutionary emphasis, his interest in biological constraints on learning and what he called "the helping hand of instinct," his work on imitation and artificially putting a subject through the desired response, and the possible relationships of his pioneering approach to contemporary connectionistic theories in cognitive psychology and neuroscience, Thorndike's response to Skinner seems far too modest; his contributions were monumental. His "service" to psychological scientists in some of these other respects is developed in the articles by Donahoe and Nevin. However, many of today's practicing experimental psychologists fail to appreciate the relevance and importance of his numerous studies, findings, ideas, and interpretations, and my hope is that they will spend some time learning about or reconsidering them. It will be time well spent.

REFERENCES

Estes, W. K. (1944). An experimental study of punishment. *Psychological Monographs, 57* (3, Whole No. 263).

Estes, W. K. (1969). Reinforcement in human learning. In J. T. Tapp (Ed.), *Reinforcement and behavior* (pp. 63–94). New York: Academic Press.

Hilgard, E. R. (1939). Review of B. F. Skinner's *The Behavior of Organisms. Psychological Bulletin, 36,* 121–125. (Reprinted in *Journal of the Experimental Analysis of Behavior, 50,* 283–286, 1988)

Hilgard, E. R. (1978). *American psychology in historical perspective: Addresses of the presidents of the American Psycho-*

logical Association, 1892–1977. Washington, DC: American Psychological Association.

Jonçich, G. (1968). *The sane positivist: A biography of Edward L. Thorndike.* Middletown, CT: Wesleyan University Press.

Postman, L. (1962). Rewards and punishments in human learning. In L. Postman (Ed.), *Psychology in the making: Histories of some selected research problems* (pp. 331–401). New York: Knopf.

Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis.* New York: Appleton-Century.

Thorndike, E. L. (1905). *The elements of psychology.* New York: Seiler.

Thorndike, E. L. (1913). Ideo-motor action. *Psychological Review, 20,* 91–106.

Thorndike, E. L. (1932). Reward and punishment in animal learning. *Comparative Psychology Monographs, 8* (4, Whole No. 39).

Thorndike, E. L. (1933). A proof of the law of effect. *Science, 77,* 173–175.

Thorndike, E. L. (1936). Edward Lee Thorndike (autobiographical sketch). In C. A. Murchison (Ed.), *A history of psychology in autobiography* (Vol. 3, pp. 263–270). Worcester, MA: Clark University Press.

Thorndike, R. L. (1991). Edward L. Thorndike: A professional and personal appreciation. In G. A. Kimble, M. Wertheimer, & C. White (Eds.), *Portraits of pioneers in psychology* (pp. 139–151). Washington, DC: American Psychological Association.

Wilcoxon, H. C. (1969). Historical introduction to the problem of reinforcement. In J. T. Tapp (Ed.), *Reinforcement and behavior* (pp. 1–46). New York: Academic Press.

JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1999, **72,** 447–450 NUMBER 3 (NOVEMBER)

ANALYZING THORNDIKE'S LAW OF EFFECT: THE QUESTION OF STIMULUS–RESPONSE BONDS

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The stimulus–response bond postulated by Thorndike's (1911) law of effect is not required in a functional account of behavior in relation to its consequences. Moreover, the notion of a bond has been challenged by the findings of several experiments. Nevertheless, it remains viable in the light of reanalyses of those findings. Thorndike's suggestion that the strength of the bond depends on the magnitude of satisfaction is consistent with current research on resistance to change.

Key words: law of effect, S-R bond, reinforcer devaluation, resistance to change

> Of several responses made to the same situation, those which are accompanied or closely followed by satisfaction to the animal will, other things being equal, be more firmly connected with the situation, so that, when it recurs, they will be more likely to recur; . . . The greater the satisfaction . . . , the greater the strengthening . . . of the bond. (Thorndike, 1911, p. 244)

Although Thorndike's law of effect is cited in virtually all textbooks of learning and conditioning, it is usually the empirical or functional aspect of the law that is emphasized. For example, Chance (1994) states that "Another way of saying the same thing is 'Behavior is a function of its consequences' " (p. 104). Here, I want to concentrate on some theoretical aspects of Thorndike's law, and will address only its positive side, which is quoted above (the omissions refer to discomfort and weakening of the bond).

As described in some of the accompanying papers, one of Thorndike's experiments involved a cat in a puzzle box, where pulling a wire loop was followed by escape from the box and access to food. Over a series of trials, the frequency of ineffective responses, such as clawing at the sides of the box, decreased and loop pulling came to occur rapidly and reliably. An empirical way of describing the result, based on a parallel to Darwinian notions of evolution, is that effective behavior was selected by its favorable consequences out of the variety of responses the cat initially made in the box.

Thorndike's theoretical statement of the law explains how this selection process might work. The situation (S) evokes a variety of responses; one response (R) happens to be followed by satisfaction (S^R); the satisfier stamps in a connection or bond between the situation and the response; and as a result, when the same situation is presented, the response is more likely to occur. In diagram form, $S{:}(R \rightarrow S^R) \rightarrow$ [S-R bond] $\rightarrow$ increase in $p(R|S)$. Clearly, the theoretical bond, in brackets, is superfluous for a functional account. If it is deleted, $S{:}(R \rightarrow S^R) \rightarrow$ increase in $p(R|S)$, which simply asserts that the situation sets the occasion for responses to be followed by reinforcers, leading to an increase in response probability.

However, the theoretical bond is important for two reasons. First, it proposes a mechanism for translating the organism's history of reinforcement over previous trials in the experimental situation into an overt response on the next trial. In addition, as stated in the final sentence of the above quotation, the strength of the bond may be a function of that history, including experimental variables that determine satisfaction: [S-R bond] = $f[S{:}(R \rightarrow S^R)]$. The strength of the theoretical bond cannot be inferred from current responding without circularity. Instead, the bond must be evaluated by inference from the effects of some sort of test in which responding is examined under altered conditions.

The most popular tests have involved reinforcer devaluation. In what may be the first such experiment, Elliott (1928) trained two

This article is based on a symposium presentation at the meetings of the American Psychological Association, August 1998. Some of the ideas presented here are also discussed by Nevin and Grace (in press).

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groups of hungry rats, one trial per day, in a 14-unit maze. A control group received sunflower seeds in the goal box throughout training; the experimental group received bran mash for the first 9 days and then was switched to sunflower seeds. Bran mash was the more effective reinforcer, in that errors decreased more rapidly over the first 9 days for the experimental group; but after the switch to sunflower seeds on Day 10, this group immediately exhibited an increase in errors to levels greater than those of the control group. Thorndike's law suggests that the experimental group should at least have maintained its preswitch level of performance because the pattern of correct turns in the maze had been stamped in by previous reinforcers; and one could argue that performance should continue to improve at the same rate as the control group. Thus, the increase in errors is evidence against the S-R bond. Mackintosh (1974) reviewed a number of related experiments and concluded that "The implication, then, is that the role of reinforcement in instrumental learning is not to strengthen antecedent responses; reinforcers do not increase the strength of an association between stimulus and response; they are themselves associated with those responses" (p. 216).

There are some more recent within-subject versions of the Elliott (1928) study that support Mackintosh's conclusion (see Williams, 1997). For example, Colwill and Rescorla (1985b) trained rats to press a lever for food pellets and to pull a chain for liquid sucrose on identical variable-interval schedules in successive sessions (for half the subjects, these contingencies were reversed). They then removed the manipulanda and devalued one reinforcer by delivering it according to a variable-time schedule, paired with illness-inducing lithium chloride (LiCl) injections; the other reinforcer was presented similarly, in alternated sessions, but was not paired with LiCl. During a subsequent 20-min extinction test session, both manipulanda were concurrently available, and there was significantly less responding on the manipulandum whose reinforcer had been devalued than on the alternative. This response-specific devaluation effect appears to be inconsistent with expectations based on S-R bonds, which should have been equally strong for the two responses before one of the reinforcers was paired with LiCl. Instead, the result suggests that each response had been associated with its particular reinforcer.

As shown in Figure 1, responses were not entirely eliminated after their reinforcers had been devalued, even though the subjects did not consume the devalued reinforcers during a separate test. In a later study, Colwill and Rescorla (1985a, Experiment 3) showed that the rate of this "residual" responding was greater than that observed after response-independent reinforcer presentation. Therefore, this residual responding depends on the history of response-dependent reinforcement before reinforcer devaluation—in Thorndike's terms, the S-R bond. Presumably, the S-R bond would be stronger for whichever reinforcer gave greater satisfaction.

There is some evidence in the Colwill and Rescorla (1985b) data that sucrose was more satisfying than pellets. At the end of training, sucrose reinforcers maintained a slightly (but not significantly) higher rate of responding, and during extinction, there was significantly more responding on the manipulandum that produced sucrose during training when it had not been paired with LiCl (see Figure 1). If sucrose was in fact more satisfying than pellets, and thereby established a stronger S-R bond between the experimental situation and sucrose-reinforced responding, then after these reinforcers had been paired with LiCl, the response that had been reinforced with sucrose during training should be more resistant to extinction than the response that had been reinforced with pellets. Colwill and Rescorla's (1985b) data are consistent with this expectation. As shown in the right panel of Figure 1, the level of responding after sucrose was devalued, relative to the group for which sucrose had not been devalued, was consistently greater than for pellets. Thus, at least some aspects of their data provide evidence for S-R bonds that differ in strength.

Length of training is another variable that should affect the strength of the S-R bond. In two related experiments, Colwill and Rescorla (1985a) examined the effects of extended training in several ways—for example, by training the lever press for one session and the chain pull for 13 sessions with the same reinforcer, while a nose-poke response received 14 sessions of training with a different

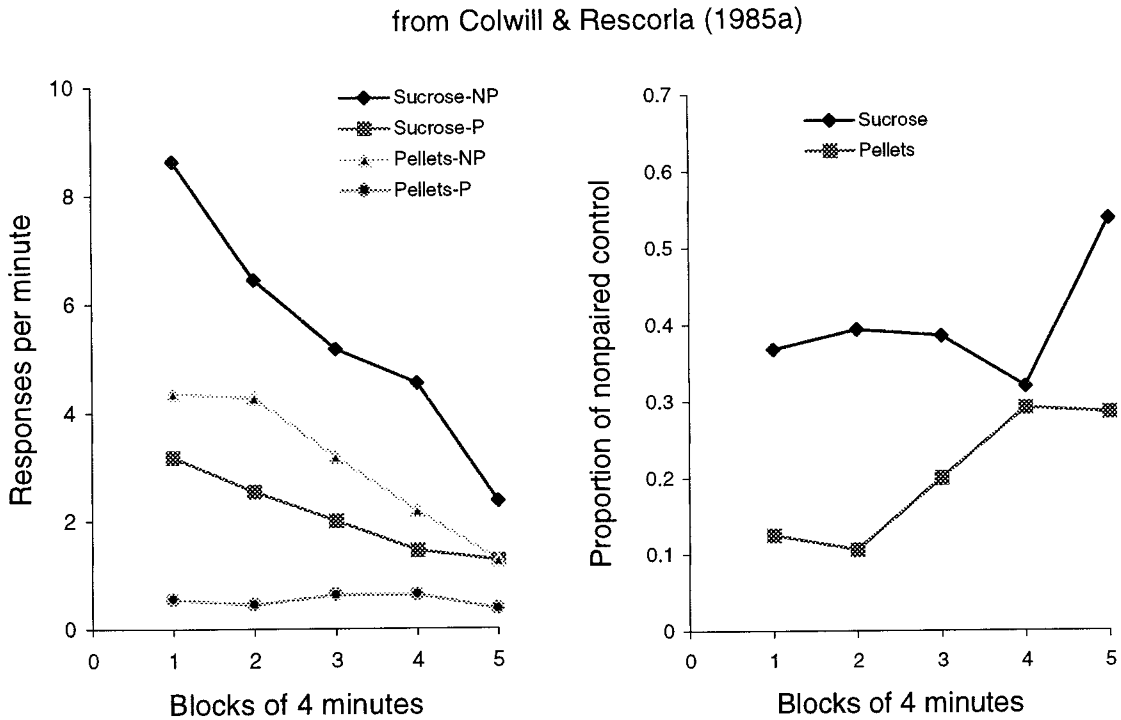

Fig. 1. In the left panel, data from Colwill and Rescorla (1985b, Experiment 1) are replotted to show response rates during a 20-min extinction session after training with sucrose or pellet reinforcers that had been either paired (P) or not paired (NP) with lithium chloride. The right panel shows the rate of a response previously reinforced by sucrose or pellets after pairing with lithium chloride (functions labeled P in the left panel) relative to nonpaired control response rate (functions labeled NP in the left panel) throughout the course of extinction. Proportions of nonpaired control rates were greater for sucrose than for pellets.

reinforcer. Then, one reinforcer was devalued, either for the target response or for the nose poke. The question, for Colwill and Rescorla, was whether the response-specific devaluation effect would survive extended training; but in relation to the notion of an S-R bond, the question is whether extended training increased residual responding relative to unpaired controls. There was a clear response-specific reinforcer devaluation effect with both one and 13 sessions: Responding based on a devalued reinforcer was consistently lower, during extinction, than responding based on a reinforcer that had not been devalued. At the same time, the magnitude of the devaluation effect, relative to nonpaired controls, decreased with extended training. Both aspects of the results were repeated in a second experiment with four different responses, two reinforcers, two levels of training, and two kinds of tests—choice or only a single response available during extinction. These results, which are summarized in Figure 2, confirm the suggestion that resistance to reinforcer devaluation is related to experimental variables in a way that reflects the strength of an S-R bond.

The findings of Colwill and Rescorla (1985a, 1985b), together with those of Adams and Dickinson (1981), led Dickinson (1994) to suggest that "instrumental training established lever pressing partly as a goal-directed action, mediated by knowledge of the instrumental relation, and partly as an S-R habit, impervious to outcome devaluation" (pp. 51–52). Colwill and Rescorla (1985a) had suggested a similar interpretation. Donahoe (1999) shows how a connectionist model with feedback from conditioned reinforcer-elicited activity (cf. Trapold & Overmier, 1972) can simulate both Dickinson's "goal-directed action" and Thorndike's "S-R habit."

In more empirical terms, training with response-contingent reinforcement both selects

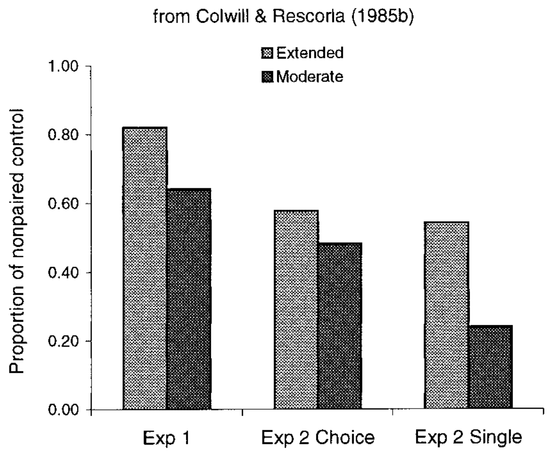

Fig. 2. Data from Colwill and Rescorla (1985a, Experiments 1 and 2) for responding in the first block of extinction after either moderate or extended training with reinforcers that had been either paired or not paired with lithium chloride. The data are expressed as response rates following pairing relative to nonpaired control response rates. In Experiment 1, responses were extinguished singly in successive sessions. In Experiment 2, both responses were available during an initial extinction session (choice), followed by single-response extinction as in Experiment 1. Proportions of nonpaired control rates were greater following extended training.

the response and, separately, makes responding more resistant to change in the training situation (for a review of research on resistance to change, see Nevin, 1992; for discussion in relation to the law of effect, see Nevin & Grace, in press). Response-specific reinforcer devaluation both reflects the current reinforcer value and tests resistance to change based on the predevaluation history of reinforcement. In Thorndike's terms, resistance to change reflects the strength of the S-R bond.

As described above, Thorndike's law of effect links the selective effects of reinforcement to the strengthening of an S-R bond. Subsequent analyses suggest that these processes are separable, not that Thorndike was wrong to invoke the S-R bond as a way to capture what happens during learning.

REFERENCES

Adams, C. D., & Dickinson, A. (1981). Instrumental responding following reinforcer devaluation. *Quarterly Journal of Experimental Psychology, 33B,* 109–121.

Chance, P. (1994). *Learning and behavior.* Pacific Grove, CA: Brooks/Cole.

Colwill, R. M., & Rescorla, R. A. (1985a). Instrumental responding remains sensitive to reinforcer devaluation after extensive training. *Journal of Experimental Psychology: Animal Behavior Processes, 11,* 520–536.

Colwill, R. M., & Rescorla, R. A. (1985b). Postconditioning devaluation of a reinforcer affects instrumental responding. *Journal of Experimental Psychology: Animal Behavior Processes, 11,* 120–132.

Dickinson, A. (1994). Instrumental conditioning. In N. J. Mackintosh (Ed.), *Animal learning and conditioning* (pp. 45–79). New York: Academic Press.

Donahoe, J. W. (1999). Edward L. Thorndike: The *selectionist* connectionist. *Journal of the Experimental Analysis of Behavior, 72,* 451–454.

Elliott, M. H. (1928). The effect of change of reward on the maze performance of rats. *University of California Publications in Psychology, 4,* 19–30.

Mackintosh, N. J. (1974). *The psychology of animal learning.* London: Academic Press.

Nevin, J. A. (1992). An integrative model for the study of behavioral momentum. *Journal of the Experimental Analysis of Behavior, 57,* 301–316.

Nevin, J. A., & Grace, R. C. (in press). Behavioral momentum and the law of effect. *Behavioral and Brain Sciences.*

Thorndike, E. L. (1911). *Animal intelligence.* New York: Macmillan.

Trapold, M. A., & Overmier, B. J. (1972). The second learning process in instrumental learning. In A. H. Black & W. F. Prokasy (Eds.), *Classical conditioning: Vol. 2. Current research and theory* (pp. 427–452). New York: Appleton-Century-Crofts.

Williams, B. A. (1997). What is learned? Revisiting an old issue. *Journal of the Experimental Analysis of Behavior, 67,* 255–258.

JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1999, **72,** 451–454 NUMBER 3 (NOVEMBER)

EDWARD L. THORNDIKE: *THE* SELECTIONIST *CONNECTIONIST*

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From the very outset of his work, Thorndike allied himself with the Darwinian proposition that complex phenomena can arise as the cumulative effects of a selection process, here the process envisioned by the law of effect. Thorndike's selectionist approach, when combined with his connectionism, laid the foundation for a synthesis of behavior analysis and neuroscience.

Key words: E. L. Thorndike, selectionism, connectionism, response-outcome associations

Edward L. Thorndike believed that complex behavior could be understood as an emergent product of the cumulative action of relatively simple processes, notably those summarized by what he came to call the law of effect. ''Complex as human life is, it is at bottom explainable by a few principles'' (1905, p. 316). More pointedly, ''it has been shown that in great measure the intellects and characters of men are explainable by a single law [the law of effect]'' (1905, p. 318). Thus, he endorsed a selectionist approach to behavior from his earliest work (cf. Galef, 1998). Thorndike was also a connectionist. That is, he believed that the strengths of connections—what we now call synaptic efficacies—changed as the result of the biological mechanisms that implemented the law of effect. The importance that he ascribed to these mechanisms led him to a neural restatement of the law of effect as the ''law of acquired brain connections'' (1905, p. 165). With his commitment to selectionism and connectionism, Thorndike allied himself with the resurgent Darwinism of his time and, in so doing, foreshadowed the biobehavioral approach of our time. After documenting Thorndike's selectionist views, I close by noting his prescient comments on a topic of central interest in current associationist accounts of animal learning—the nature of the associations inferred to underlie instrumental learning (i.e., operant conditioning). Thorndike was an associationist as well as a selectionist and connectionist, but his associationism differed from contemporary versions.

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Selectionism

A selection process (see Figure 1) consists of three interrelated steps—variation, selection, and retention (see Dennett, 1995; D. L. Hull, 1973; Mayr, 1988; Sober, 1984). Variation provides the raw material upon which selection operates. It is the source of whatever novelty arises from repeated iterations of the three-step process. Variation is undirected (Campbell, 1974) in the sense that the factors that affect variation are not correlated with those that affect selection. Selection by the environment favors (or disfavors) some variations over others, and confers whatever direction is apparent in the process. Of course, selection is not truly directed because its trajectory is utterly dependent on the environment. When the environment changes, the direction of selection changes. Only the relative constancy of the environment permits the illusion of direction or purpose. Finally, the third step—retention—enables favored variations to endure long enough to contribute to the variation upon which future selection operates. Without retention, the effects of selection could not accumulate and the possibility of complexity would not exist (see Donahoe & Palmer, 1994; Palmer & Donahoe, 1992).

Variation. Thorndike was explicit that whatever creativity or novelty emerged from the process of selection was dependent on the pool of variation upon which the selecting environment acted. ''The first necessity of mental progress is fertility in response. Unless the baby does something, it can learn nothing'' (1905, p. 209). He recognized that the initial variants upon which selection acted were largely the reflexive relations provided by natural selection (i.e., respondents). ''The start-

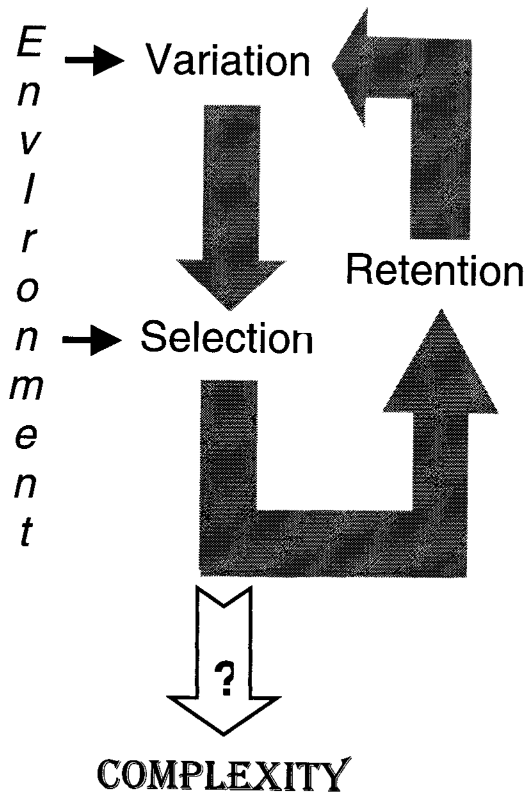

Fig. 1. The three-step process through which the repeated action of relatively simple processes has the potential to produce complex outcomes, as in the emergence of complex behavior through the cumulative effect of reinforcement.

ing point for the formation of any association . . . is the set of instinctive activities'' (1898, p. 13). Thorndike also acknowledged the contribution to variation made by nonelicited behavior: ''Progress was not by seeing through things, but by accidentally hitting upon them'' (1898, p. 106). A fuller appreciation of the role of nonelicited behavior awaited Skinner's (1938) conception of the operant. Thorndike understood that variation was undirected with respect to the selecting factor. ''The one impulse, out of many accidental ones, which leads to pleasure, becomes strengthened'' (1898, p. 45). His designation of the selecting factor as ''pleasure'' or, at other times, as ''satisfaction'' sounds quaint to modern ears, but his conception of undirected variation has a contemporary ring.

Selection. The parallels between selection by ''pleasure'' (i.e., by reinforcement) and natural selection were apparent to Thorndike.

> The development of human mental life may be likened to that of the animal kingdom as a whole. The present animal kingdom is the result of the extinction of those which did not fit the environment. . . . Any man's intellect and character are the results of the existence in his past of many connections, the elimination of those which did not fit their environment so as to bring satisfaction. (1905, p. 317)

Most important, the population of variants on which selection operated was the behavior of an *individual* organism. The focus upon the behavior of the individual was an enduring characteristic of Thorndike's thinking, both his early animal research and his later educational research. It is one of the chief characteristics that differentiates Thorndike and Skinner from their fellows. Even those who otherwise embraced Darwinian thinking, such as Clark Hull (1943), sometimes inadvertently acted as if selection operated on variations in the behavior of *different* organisms. How else to explain the use of group experimental designs that Fisher had correctly devised to measure the effects of natural selection (cf. Sidman, 1960)? An analysis of variation produced by individual differences is appropriate in the study of natural selection but not of selection by reinforcement. Thorndike's focus on the single organism was apparent in the graphs of the behavior of individual animals that he used to communicate his findings (see Chance, 1999) and is explicit in his writings. ''The process is . . . simply the selection of the . . . movement from amongst the many sorts made because of its relatively greater amount of resulting satisfaction'' (1905, p. 204). The foregoing suggests that the focus of selection was a ''movement'' (i.e., behavior). However, other more complete statements indicate that Thorndike considered the unit of selection to be an environment–behavior relation, not behavior alone (cf. Donahoe, Burgos, & Palmer, 1993; Donahoe, Palmer, & Burgos, 1997). To wit, ''The one impulse, out of many accidental ones, which leads to pleasure, becomes strengthened . . . and more firmly associated with the sense-impression of that box's interior'' (1898, p. 45). And, ''any act which in a given situation produces satisfaction becomes associated with that situation, so that when the situation recurs the act is more likely than before to recur also'' (1905, p. 203). Finally, Thorndike was sensitive to the fact that selection produces complexity only by dint of variation. ''Purposive thinking equals spontaneous thinking plus selection'' (1905, p. 264).

Retention. Thorndike also appreciated the essential contribution of retention to the emergence of complexity from a selection process. The behavioral repertoire initially included only ''instinctive activities'' and other ''movements,'' ''but this is the starting point only in the case of the first box experienced'' (1898, p. 14). In subsequent boxes in which his subjects were tested, the behavioral repertoire included the environment–behavior relations that had been selected in prior chambers. The critical role of the accumulation of prior selections was especially apparent in complex human behavior: ''Selection and survival of the fit thoughts . . . are the essentials of purposive thinking'' (1905, p. 265).

Like Darwin before him, Thorndike did not know the biological mechanisms that enabled retention and upon which selection acted. Nevertheless, Thorndike believed that the full development of his approach would require the discovery of these mechanisms.

> How the satisfaction following upon a connection strengthens it . . . must be left [an] unanswered question. Neither psychology nor physiology has yet anything much better than a guess to offer this, the most fundamental question of the mental life of man and the animal kingdom as a whole. All that can be said is that the original satisfiers are as a rule events useful for the survival of the species . . . ; consequently any means by which the[y] . . . could reinforce the connections causing them . . . would, when evolved, be maintained by natural selection. (1905, p. 316)

(Note the use of the term *reinforce* in this statement.) ''Everywhere we have to seek for the physiological basis of mental facts and connections'' (1905, p. 323). The developing modern synthesis of behavior analysis with neuroscience—a biobehavioral approach—would be welcomed by Thorndike as it would by Skinner. ''The experimental analysis of behavior is a rigorous, extensive, and rapidly advancing branch of biology'' (Skinner, 1974, p. 255).

> The physiologist of the future will tell us all that can be known about what is happening inside the behaving organism. His account will be an important advance over a behavioral analysis, because the latter is necessarily ''historical''—that is to say, it is confined to functional relations showing temporal gaps. . . . It will make the picture of human action more nearly complete. (Skinner, 1974, pp. 236–237)

Skinner's earlier reservations about forays into physiology stemmed from pragmatic considerations—the absence of the requisite neuroscience—not from principled objections to such a synthesis. Behavior analysts such as Jack Michael recognize that the present situation is quite different: ''I would strongly urge anyone starting a research career in behavior analysis in the late 1900s to include extensive training in the neurosciences. And I would also urge extensive training in computer science sufficient to understand computer modeling'' (Michael, 1998, p. 160).

The Nature of the Selected ''Association''

Consistent with Michael's admonitions, Thorndike's ''most fundamental question'' is currently being pursued by integrating the experimental analysis of behavior and neuroscience using neural networks (e.g., Donahoe & Palmer, 1989, 1994). The interconnected ensemble of units that constitutes a neural network may be regarded as a much-mutated descendant of Thorndike's connectionism. It is ironic that simulation via neural networks has recently been brought to bear on a matter of contention between Thorndike's early views of the law of effect and current statements of associationism, that other branch of the Thorndikian tree. The issue is the nature of the association inferred to underlie operant—or instrumental—conditioning. Present-day associationism generally takes the position that an instrumental response occurs because ''the reinforcer is encoded as a consequence of the response'' (Rescorla & Colwill, 1989, p. 291) or, stated in other terms, ''instrumental learning leads to the development of response-outcome associations'' (Colwill, 1994, p. 31; see also Colwill & Rescorla, 1990). Concerning this view, Thorndike asked: ''Do they [animals] ever conclude from inference that a certain act will produce a certain desired result, and so do it? . . . Although it is in a way superfluous to give the *coup de grace* to the despised theory that animals reason, I think it is worthwhile to settle this question once for all'' (1898, p. 39). ''The commonly accepted view . . . is that the sight of the inside of the box reminds the animal of his *previous pleasant experience after escape and of the movements* which he made which were immediately followed by and so associated with that escape'' [i.e., a response–outcome association] (1898, p. 65). Thorndike disagreed: ''This view has stood unchallenged, but its implication is false. It implies that an animal, whenever it thinks of an act, can supply an *impulse to do* the act'' (1898, p. 66). ''The groundwork of animal associations is not the association of *ideas,* but the association of . . . sense-impression with *impulse*'' (1898, p. 71). In short, Thorndike rejected the notion that is implicit in the concept of response–outcome association—that of a response initiated by an autonomous organism. Consideration of the discriminative effects of conditioned respondents provides an interpretation that is more congenial to Thorndike's views. The behavior that fostered inferences about response–outcome associations can be interpreted as the joint control of operants and respondents by the environment, with feedback from the respondent modulating the strength of the operant (Donahoe & Palmer, pp. 108–109; cf. Trapold & Overmeir, 1972). The law of effect, when implemented by the neural mechanisms sought in the law of acquired brain connections, supports Thorndike's views (and Skinner's as well; see Palmer, 1998) that selection by reinforcement changes the environmental guidance of behavior, a conclusion that is not well characterized as the formation of response–outcome associations.

REFERENCES

Campbell, D. T. (1974). Evolutionary epistemology. In P. A. Schlipp (Ed.), *The philosophy of Karl Popper. The library of living philosophers* (Vol. 14-1, pp. 413–463). LaSalle, IL: Open Court Publishing.

Chance, P. (1999). Thorndike's puzzle boxes and the origins of the experimental analysis of behavior. *Journal of the Experimental Analysis of Behavior, 72,* 433–440.

Colwill, R. M. (1994). Associative representations of instrumental contingencies. In D. L. Medin (Ed.), *The psychology of learning and motivation: Advances in research and theory* (Vol. 31, pp. 1–72). San Diego, CA: Academic Press.

Colwill, R. M., & Rescorla, R. A. (1990). Evidence for the hierarchical structure of instrumental learning. *Animal Learning & Behavior, 18,* 71–82.

Dennett, D. C. (1995). *Darwin's dangerous idea: Evolution and the meanings of life.* New York: Simon & Schuster.

Donahoe, J. W., Burgos, J. E., & Palmer, D. C. (1993). A selectionist approach to reinforcement. *Journal of the Experimental Analysis of Behavior, 60,* 17–40.

Donahoe, J. W., & Palmer, D. C. (1989). The interpretation of complex human behavior: Some reactions to *Parallel Distributed Processing,* edited by J. L. McClelland, D. E. Rumelhart, and the PDP Research Group. *Journal of the Experimental Analysis of Behavior, 51,* 399–416.

Donahoe, J. W., & Palmer, D. C. (1994). *Learning and complex behavior.* Boston: Allyn & Bacon.

Donahoe, J. W., Palmer, D. C., & Burgos, J. E. (1997). The S-R issue in behavior analysis and in Donahoe and Palmer's *Learning and Complex Behavior. Journal of the Experimental Analysis of Behavior, 67,* 193–211.

Galef, B. G., Jr. (1998). Edward Thorndike: Revolutionary psychologist, ambiguous biologist. *American Psychologist, 53,* 1128–1134.

Hull, C. L. (1943). *Principles of behavior, an introduction to behavior theory.* New York: Appleton-Century.

Hull, D. L. (1973). *Darwin and his critics.* Cambridge, MA: Harvard University Press.

Mayr, E. (1988). *The growth of biological thought: Diversity, evolution, and inheritance.* Cambridge, MA: Harvard University Press.

Michael, J. (1998). The current status and future directions of the analysis of verbal behavior: Comments on the comments. *The Analysis of Verbal Behavior, 15,* 157–161.

Palmer, D. C. (1998). On Skinner's rejection of S-R psychology. *The Behavior Analyst, 21,* 93–96.

Palmer, D. C., & Donahoe, J. W. (1992). Essentialism and selectionism in cognitive science and behavior analysis. *American Psychologist, 47,* 1344–1358.

Rescorla, R. A., & Colwill, R. M. (1989). Associations with anticipated and obtained outcomes in instrumental learning. *Animal Learning & Behavior, 17,* 291–303.

Sidman, M. (1960). *Tactics of scientific research: Evaluating experimental data in psychology.* New York: Basic Books.

Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis.* New York: Appleton-Century.

Skinner, B. F. (1974). *About behaviorism.* New York: Knopf.

Sober, E. (1984). *The nature of selection: Evolutionary theory in philosophical focus.* Cambridge, MA: MIT Press.

Thorndike, E. L. (1898). Animal intelligence: An experimental study of the associative processes in animals. *Psychological Review Monograph Supplement, 2* (4, Whole No. 8).

Thorndike, E. L. (1905). *Elements of psychology.* New York: A. G. Seiler.

Trapold, M. A., & Overmier, J. B. (1972). The second learning process in instrumental learning. In A. H. Black & W. F. Prokasy (Eds.), *Classical conditioning: Vol 2. Current research and theory* (pp. 427–452). New York: Appleton-Century-Crofts.