

*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.

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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 Teachers 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

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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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-

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 guid-

ance; 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 Joncich, 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 neo-behaviorists 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 Joncich, 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.



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