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A short history of psychological theories of learning

Learning remains an elusive topic, despite the endless research lavished on it. And what we mean by it, of course, is shaped by how we choose to study it. Concentrate on how children master their native language and you arrive at a very different conception of learning than had you researched how undergraduates memorize nonsense syllables. Does learning to finger a Bach cello sonata tap the same learning processes as learning to trace your way through a finger maze? Is all learning alike, reducible to a common set of principles?

Two learning tasks are said to be alike if mastering one makes mastering the other easier – the so-called transfer criterion. But what is transferred? Is it responses? Rules? Or do we simply learn how to learn, as when with enough practice we become exam-wise or taxform-wise? How do we learn the lay of the land? How do we learn to concentrate our attention?

And then there are questions about differences in how learning occurs. Do all species learn in the same way and do the bright and the dull go about it in like manner? And what about external inducements, rewards, and punishments? Are all learning situations comparable?

I used to give the star performers of the experiments I'd just completed to my young daughter. These rats seemed to develop a more open curiosity under her magnanimous care. What, indeed, does domestication do to an animal's approach to learning? Were those insights achieved by Wolfgang Koehler's pampered chimpanzees – their figuring out how to rake in an out-of-reach banana by putting two sticks together, for instance – simply the result of the leisurely tutelage they received on that German island of Tenerife?[1] It used to be said, only half jokingly, that Yale stimulus-response-reinforcement learning theory was different from more cognitive California theory because Clark Hull in New Haven taught his graduate students that rats "should get on with

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1 Wolfgang Koehler, *The Mentality of Apes* (New York: Harcourt Brace, 1926). This was originally published in German in 1917.

it," while Edward Tolman counseled his at Berkeley that rats need time enough to pause at the choice points in a maze.

And finally, do we learn for learning's sake, or must we be extrinsically motivated to do so? Assuming the latter, the Yerkes-Dodson law tells us that too much or too little motivation reduces learning. I checked that out once myself and got a surprise. I found that very hungry and just moderately hungry rats learned to find their way through a succession of pairs of doors. The correct path through was marked redundantly in two ways: follow a left-right-left-right path, or just choose the darker door at each choice point. The hungry rats learned only one of the two cues; the moderately hungry rats learned both. The less hungry rats had a more open curiosity – like my daughter's pets.

Given all this, it is natural enough that scientists would want somehow to simplify what we mean by 'studying learning.' And, of course, the standard way of doing that is to agree on some paradigm that would make it possible to compare results. That is exactly what happened at the very start of learning research. But, as often happens, rival paradigms came into existence and, alas, this research soon became a war of would-be paradigms. Indeed, the learning theory wars that resulted came to dominate the psychological research scene from the latter nineteenth century until a decade after World War II, with various 'schools' devising clever experiments to demonstrate how well their paradigm worked, or how poorly rival ones fared.

There were two competing paradigms from the start, each with its variants. The principal one, a child of its times, was molecular associationism, a metaphoric extension of the atomism of nineteenth-century physics. (As the quip goes, psychology is forever subject to physics envy.) The atomism of learning theory embodies the notion that learning consists of the association of ideas, memories, sensations, whatever; at its heart is the conception of the associative bond, the linkage that co-occurrence or spatial proximity produces between two sensations or ideas. While associationism is of ancient provenance, it had more recent philosophical adherents as well – not only Aristotle, but Locke, Berkeley, Hume, and pére et fils Mills. Indeed, by the mid-nineteenth century, philosopher-psychologist Johann Friedrich Herbart had proclaimed the associative bond as the keystone of the new psychology.

This paradigm found further, if indirect, support in the newly burgeoning brain physiology of those times. As the nineteenth century entered its last quarter, the older phrenology of the days of Gall and Spurzheim was reformulated in terms of newly discovered cortically localized 'centers' in the cerebral cortex, each dedicated to a particular function. Perhaps the most compelling localization study was the one conducted in 1870 by the German physiologists Fritsch and Hitzig. In their study, electrical stimulation of different spots in the medial-lateral cortex produced particular, quite finite motor responses: stimulating one spot produced flexion of a monkey's forearm, another would turn his eyes upward, still another would turn them downward.[2] If the brain were organized in this localized punctate way, psychologists asked, why not the mind as well? One needs to remember that the prevailing philosophical view among those

2 The classic article was Gustav Fritsch and Eduard Hitzig, "Ueber die elektrische Erregbarkheit des Grosshirns," *Archiv der Anatomie und Physiologie* (1870): 300 – 332.

scholars was psychophysical parallelism, which held that mind and brain move along parallel tracks.

Their critics, however, championed another model – that of molar configurationism. This paradigm took as its major premise that mind and brain alike operate as integral systems controlling the functioning of component parts. Like its rival, it too rested its case on brain physiology, for there was already plenty of evidence that overall cortical processes controlled localized centers – the neural 'mass action' holism represented by the renowned Pierre Flourens.

The brain's mass action was analogous to the phenomenology of everyday life – that ordinary experience transcends its bits and pieces. The 'urban scene,' after all, is more than just a collection of taxis, buildings, pedestrians; its properties as a whole shape the elements that make it up. Gestalt psychology was, of course, the most direct expression of this view, and it had much to say about how learning was a matter of overall organization rather than of local associative linkages.

Consider now the rise of the associationist paradigm. That closing quarter of the nineteenth century was a time of many new studies of learning – mostly concerned with the memorization of lists of words or pairs of words to be associated. But it was the nonsense syllable principally that gave associative bonding its scientific flavor. Hermann Ebbinghaus used nonsense syllables in order to rule out past experience and 'meaning' in explanations of learning. Ebbinghaus's 1885 *Ueber das Gedaechtnis* is a tedious account of learning lists of nonsense syllables (with Ebbinghaus himself as the subject of most of the experiments). His findings – for example, that nonsense syllables in the middle of the list are more slowly learned than ones at the beginning or end – are easily reproducible.[3]

But the associative bond, even between nonsense syllables, soon came to seem mentalistic, too fragile to suit the scientific taste of the times. So by the turn of the century it was replaced by Pavlov's more scientifically solid 'conditioned reflex.' Pavlov's paradigm physicalized associationism, turning its content into something more measurable while preserving its associative form intact. All his paradigm required was linking and relinking stimuli and responses: a salivary reflex, once produced by food, was now evoked by a bell signaling the coming of food. Pavlov's Nobel Prize in physiology seemed to clinch the triumph of physicalism. But Pavlov himself was not altogether pleased, as we'll see later.

Now turn to configurationism, which had no shortage of psychologists to support it, dubious as many were of associationism's abstractness and its remoteness from ordinary experience. Configurationism had the support of brain research as well, with the holistic neurology of the indomitable Flourens still very much in vogue. Also in those fin de siècle times there was a rising tide of interest in how language and culture shaped mind, with figures like Emile Durkheim and Max Weber in the neighboring discipline of sociology urging that culture – not just individual encounters with the world of physical nature – also forms mind.

3 Ebbinghaus's 1885 classic is available in English only in brief, but representative excerpts may be found in Wayne Dennis, *Readings in the History of Psychology* (New York: Appleton-Century-Crofts, 1948), 304 – 313. Interestingly enough, Ebbinghaus's original monograph was published in its entirety in English translation in 1913 by Teachers College, Columbia University – very much in keeping with the then dominant emphasis on rote learning in American education. It has long been out of print.

Gestalt theory was the prime exemplar of the configurationist trend in those early years, though it hit its full stride only after World War I. Its credo was that all systems – physical, biological, and mental – have the intrinsic character of controlling the local elements that compose them. Field theory in physics was its model, and its proclaimed maxim was "The whole is greater than the sum of its parts," which the Gestaltists proceeded to confirm with a steady stream of clever studies on human perception. The Koehler chimpanzee studies on Tenerife were intended to make the same point where learning was concerned: There was no way in which those chimpanzees could turn a pair of sticks into a reaching tool by the simple 'association' of elements. It took an act of insight to do so, a way of configuring the whole situation.

Koehler had a deep belief in the ubiquitousness of configurationism in all of nature. He launched one of his first major attacks on associationism by arguing the insufficiency of atomism, in a book bearing the forbidding, if telltale, title *Ueber die physische Gestalten im ruhe und im stationaren Zustanden* (On physical configurations at rest and in stationary states). If atomism was insufficient even in physics, Koehler asked, how could it serve as a paradigm for psychology?[4] He applied a phenomenon in visual perception to make an analogy that would drive home his point: When two nearby points of light are briefly flashed one after the other, the eye perceives pure apparent movement, not the light points moving. The whole, then, is indeed different from a sum of its parts.

Now as it happens, Pavlov himself came to advocate a kind of linguistic configurationism. How does the conditioned response square with an ordered phenomenon such as language? Does language change how stimuli are interpreted, how a conditioned stimulus is substituted for an unconditioned one in the case of human beings? Troubled by such issues in his later years, Pavlov proposed a Second Signal System whose stimuli were not raw physical inputs, but language imbedded in codes and categories. Thus linguistic synonymy influenced stimulus substitution in ordinary conditioning.

Some say that Pavlov was driven to his new views by communist ideologues with prematurely Gramscian leanings, but in fact his Second Signal System was quite in keeping with the European tradition of human studies, *Geisteswissenschaft*, rather than with *Naturwissenschaft* – a well-revered tradition among the Russian intelligentsia. Still, structuralism was virtually the hallmark of the lively Russian literary and linguistic scene of Pavlov's day, and the Second Signal System was certainly, to some degree, a response to that scene. I recall flying to Moscow from Paris in the 1960s with the celebrated Russian emigré linguist Roman Jakobson. He laughed when I told him about Pavlov's later turn and about the accusation that he had knuckled under to the *nomenklatura*. "No, no, Jerry, communist ideologues weren't needed, just being Russian was enough. And being a Russian intellectual besides! Not even Pavlov could live with the idea that language makes no difference, that people learn like dogs!"

Small wonder that cultural theorists like Vygotsky and Luria took over after

4 For Koehler's philosophical allegiances, see Mary Henle, ed., *The Selected Papers of Wolfgang Koehler* (New York: Liveright, 1971). Perhaps the best and most accessible account of Gestalt psychology's empirical accomplishments (mostly before Hitler's rise to power) is Kurt Koffka, *Principles of Gestalt Psychology* (New York: Harcourt Brace, 1935).

Pavlov and that many of the post-Pavlovian young studied Gestalt psychology at the Institute of Psychology in Berlin in the years after.[5]

The climax of the rivalry between associationism and configurationism came in America in the years before World War I. Nourished by the imposing Edward Lee Thorndike of Teachers College, Columbia University, the associative paradigm had flourished in the United States. Thorndike had been a postdoctoral student at one of the major centers of associationism in Germany. On his return to America (and Teachers College) he popularized practice and repetition as the routes to proficient school learning: practice and repeat as you would were you memorizing nonsense syllables.[6]

But the associationist research program soon changed in America under the influence of Pavlov. J. B. Watson, the founder of American behaviorism, popularized Pavlov and gave his findings an American twist, by stressing how all learning occurred through stimulus and response. I sometimes wonder whether it was Watson's oversimplifications that eventually drove American associationist learning theorists to their zealous rigor in exploring Pavlov's ideas. It was the energy and determination of their research that made America for half a century the home of later Pavlovianism, a half century dominated by the likes of Walter Hunter, Clark Hull, Edward Guthrie, B. F. Skinner, Kenneth Spence – all distinguished, self-professed stimulus-response learning theorists.

Their forte was the well-designed animal experiment: maze running, discrimination learning, operant conditioning à la Skinner box, and the like – mostly with rats as subjects, but sometimes pigeons, and occasionally monkeys. Undergraduates were used as well, but again, mostly in rote learning experiments – in what was referred to in my graduate student days at Harvard as 'dustbowl empiricism.' It was in these days that Pavlov's dog became a metaphor for American know-nothing anti-intellectualism.

The burden of the behaviorists' findings, taken collectively, was that repetition of a task, with suitable reinforcement for completing each trial, improved performance. There were subtleties, to be sure – like the deleterious effects of massing trials rather than spacing them, creating interference by setting positive and negative reinforcement in a conflicting relationship, and the like. But the overall outcome of the work, where ordinary everyday learning was concerned, was, I believe, much as I've stated it. I'll return to this matter later.

But, as in Europe earlier, a contrarian configurationism soon came into being. Partly it was influenced by Gestalt theorists, now in America and sparking the opposition, but it had American roots as well, nourished particularly by Edward Tolman, who was sympathetic to the work of Koehler and was a close friend of Kurt Lewin, a latter-day leader in the Berlin Gestalt group. Tolman's brother Richard, moreover, was a distinguished nuclear physicist and shielded him well from old-fashioned atomistic notions – and, indeed, from physicalistic temptations. Tolman, from the start, was a cognitivist.

5 Lev Vygotsky, *Thought and Language* (Cambridge, Mass.: MIT Press, 1962); Aleksandr Romanovich Luria, *The Role of Speech in the Regulation of Normal and Abnormal Behavior* (New York: Liveright, 1961).

6 Edward L. Thorndike's classic is the three-volume *Educational Psychology*, which appeared in 1913 – 1914.

Tolman's first major book appeared in 1932 and it quickly gained adherents among the discontented, and there were plenty of them. His students – notably David Krech, but many others as well – also joined the battle against associationism. By World War II, there was virtually open conflict in America between configurationist and associationist learning theorists – the first holding that learning is principally a task of organizing knowledge from the top down, the second insisting that it is accreting it from the bottom up. The configurationists, though still a minority, had been officially well received on the American scene when they fled Hitler's Europe. Koehler was invited to deliver the William James Lectures at Harvard, and Kurt Lewin became virtually a cult figure in social psychology. The displaced members of the old Gestalt group were soon well placed in leading American universities. They made commonsense phenomenology seem commonsense rather than arcane, an achievement given the hold of behaviorist American psychology. Learning began to be understood as grasping things in context, not in bits.

Take Edward Tolman's research as an example. He taught that learning is like mapmaking and that to learn is to organize things in the light of their utility for achieving ends. In "Cognitive Maps in Rats and Men," his still renowned Research Lecture to the Berkeley faculty in 1947, Tolman claimed that trial and error is not so much acting out habits to discover which are effective, but rather a looking back and forth to get the lay of the land in order to construct a solution. That is why he urged his graduate students not to rush their rats through the maze.[7] He believed that our cognitive maps are not mirrors of the happenstance of our encounters with the world, but a record of our strivings and what has proved relevant to their outcome. His views in this sense were basically pragmatist, perhaps because of his years of exposure as a psychology graduate student to Harvard's pragmatist philosophers, particularly C. I. Lewis, whom he greatly admired. Following Tolman's lead, David Krech went to the extent of proposing that learning is hypothesis driven, not just passive registration. Even rats, Krech tried to show, generate hypotheses.[8]

It's revealing to compare Tolman with the leading, perhaps most radical associationist behaviorist of the same period, B. F. Skinner. Skinner was surely as compelling in defense of operant conditioning as Tolman was of cognitive map theory. His central concept was the operant response – an act not initially under the direct control of some particular feature of the immediate environment. An example of an operant response is provided by a starting pigeon in a Skinner box whose pecking of the button on the box's wall either produces or fails to produce a reinforcement (a grain of seed, say). Any reinforcement increases the likelihood of the operant response occurring again, the level of likelihood depending upon whether the reinforcement always follows the response or does so only sometimes, and whether it does so regularly (periodically) or irreg-

7 Tolman's most influential book was *Purposive Behavior in Animals and Men* (New York: Century, 1932). His Berkeley lecture was later elaborated in his International Congress address, "Cognitive Maps in Rats and Men," *Psychological Review* 55 (4) (1948): 189 – 208.

8 Krech's bold study (written under his original name) is I. Krechevsky, "'Hypothesis' versus 'Chance' in the Presolution Period in Discrimination Learning," *University of California Publications in Psychology* 6 (1932): 27 – 44.

ularly (aperiodically). Partial aperiodic reinforcement, for example, evokes a rather more persistent response than one might expect, though Skinner would scoff at interpreting such persistence as hope springing eternal. Learning, in Skinner's austere terms, is under the sole control of schedules of reinforcement: reinforcement can only be positive; punishment does not affect learning. And that is about it. As Skinner would sometimes say, a bit ironically, learning scarcely needs a theory.[9]

Not all behavioral associationists, to be sure, shared Skinner's disdain for theory. Clark Hull at Yale, indeed, elaborated his theory into a highly refined set of axioms about what constitutes positive and negative reinforcement, what makes a conditioned stimulus generalize along a certain gradient, how organisms anticipate reinforcers, and the like – all in rather exquisite and specialized detail. His first books – the 1943 *Principles of Behavior* and the more triumphally titled 1952 *A Behavior System* – bristle with tables and idealized learning curves and with abstract formulae for relating those findings to his central axioms – perhaps a prophetic effort to devise a mathematical model of learning, the preoccupation of computational psychologists a generation later.[10]

The conflicts between Hull and Skinner, and between both of them and Tolman, were the last battles of the learning theory wars. Learning theory in the classic sense died around 1960 – though there are still Skinnerians who stalwartly continue to publish operant findings, mostly for each other. I know of no more Tolmanians or Hullians.[11]

It was the cognitive revolution that brought down learning theory or, perhaps, focused attention elsewhere. After 1960, say, stimulus-response learning theory seemed quaintly stunted, hemmed in by its own self-denial. As for more molar, cognitive learning theories, many of their ideas were restated and absorbed into general cognitive theories such as Newell and Simon's on problem solving, or Bruner, Goodnow, and Austin's on thinking, or Miller, Galanter, and Pribram's on planning.[12] By the latter 1960s, learning was being translated into the concepts of information processing, with no compulsion to elevate one kind of learning over another in terms of its 'basic' properties. Certainly, the old wars were over. And so, interestingly, were the old rat labs and their ubiquitous mazes.

As I reflect on the transition period, I think that it was the study of language and particularly of language acquisition that precipitated learning theory's decline. Language use and its acquisition are too out of reach of piecemeal S-R learning: efforts to bring them into the fold soon become absurd, and linguists have mostly dismissed them as such.

9 B. F. Skinner, "Are Theories of Learning Necessary?" *Psychological Review* 57 (1950): 193 – 216.

10 Clark L. Hull, *Principles of Behavior* (New York: Appleton-Century-Crofts, 1942) and *A Behavior System: An Introduction to Behavior Theory Concerning the Individual Organism* (New Haven, Conn.: Yale University Press, 1952).

11 The most detailed and authoritative volume on the classic learning theories is Ernest R. Hilgard, *Theories of Learning,* 2d ed. (New York: Appleton-Century-Crofts, 1956).

12 Alan Newell and Herbert A. Simon, *Human Problem Solving* (Englewood, N.J.: Prentice Hall, 1972); Jerome Bruner, Jacqueline Goodnow, and George A. Austin, *A Study of Thinking* (New York: Wiley, 1956); George A. Miller, Eugene Galanter, and Karl Pribram, *Plans and the Structure of Behavior* (New York: Holt, Rinehart, and Winston, 1960).

The contemporary linguistic assault on associationist learning theory began with Noam Chomsky's gloves-off critical review of Skinner's *Verbal Behavior*.[13] But the mentalist, problem-solving emphasis it introduced has now expanded beyond language as such. One now asks whether cultural codes are learned in some language-like way. Neither psycholinguistics nor cultural psychologists think of learning in the old-fashioned learning-theory way.

I think it would be fair to say that, under this new dispensation, more has been learned during the last three decades about language acquisition than in any prior century – more, indeed, than in all of them combined. And it's well to remember that the flood of research that made this possible was precipitated by the linguist Chomsky, not by a learning theorist.

The turn to language, moreover, has shifted learning-related research away from many of the older, artificial experimental paradigms – mazes, paired-associate word lists, nonsense syllables, and the rest. Let me give an example: the prediction that children must be so early tuned to the structure of their native language that they pick up its phonemic distinctions in parental talk even before they learn to understand or talk the language proper. It is a prediction that grows out of linguistic and developmental theory. And you can test it in context *directly* – by seeing whether childrens' prelinguistic babbling has a higher frequency of native-language phoneme sounds than of foreign ones. And so it does: French babies babble in French, Spanish in Spanish, etc. With such experiments, one tests in context, not in a maze, and knows without extrapolation whether the experiment has any bearing on real learning by real people in real life.

Shall we conclude, then, that three-quarters of a century of warfare between associationist and configurational learning theories taught us little or nothing about the real nature of learning? That would be a mistake.

Both Pavlov's dogs and Koehler's chimpanzees did, in fact, learn, though in different ways and in different circumstances. And we have ample reason to suspect that neither of their approaches can be reduced to the other. In the next turn of things perhaps we will figure out how to put them together. But of one thing at least I am quite convinced. You cannot strip learning of its content, nor study it in a 'neutral' context. It is always situated, always related to some ongoing enterprise. Perhaps there is no such thing as 'learning in general' – and perhaps that is what we should learn from Pavlov's dogs, Koehler's chimps, and the disputes over learning that they once symbolized.

13 B. F. Skinner, *Verbal Behavior* (Cambridge, Mass.: Harvard University Press, 1947). Chomsky's structuralist-mentalist views were first published in his *Syntactic Structures* (The Hague: Mouton, 1957). His frontal attack on Skinner's *Verbal Behavior* (in *Language* 35 [1959]: 26 – 34) only two years later came as a rather unexpected, though scarcely a conceptual, surprise.