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THE STIMULUS-NEURAL CONTROL OF BEHAVIOR DURING AND AFTER LEARNING[1]

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IN the present paper I wish to discuss two classical psychological theories dealing with certain aspects of the learning process and to evaluate them in terms of experimentation. The first of these theories is to the effect that "consciousness" gradually disappears during learning, so that the completely learned response is made automatically and "unconsciously." An individual, for example, is said to be acutely aware of his piano playing in the beginning of the formation of this habit; but as learning proceeds, the playing is done more and more automatically until finally the subject can play all but "unconsciously," while giving his attention to other things and while simultaneously engaging in other activities. The second theory that I wish to discuss also deals with changes that are alleged to go on during the process of habit-formation. If we may continue to use the illustration of piano playing, this theory holds that originally such a serial action is controlled partly by visual and auditory stimuli, but that after playing has become perfected the stimulus control is turned over to proprioception. At this stage the stimuli from one arm-finger movement are adequate to produce the next arm-finger movement which produces new proprioceptive stimuli to arouse the next response, and so on. Thus the theory holds that the control of a perfected serial action is proprioceptive and that the process of the formation of such a habit involves, among other things, a shift from exteroceptive to proprioceptive control, making possible the automatic character of the final response. We shall first discuss the theory

[1] Address of the vice-president and chairman of Section I—Psychology, American Association for the Advancement of Science, Boston, December 29, 1933.

of shifting stimulus control of serial action and then turn to a brief analysis of the view that perfected habits are executed "unconsciously."

The theory that serial action is of the chained reflex type and that during learning the stimulus control shifts from the exteroceptors to the proprioceptors can be most fruitfully discussed on the basis of experimentation with the maze. The theory, however, was well established in American psychology before Watson, in 1907, wrote his monograph on the rôle of kinesthetic and organic sensations in the maze behavior of the rat.[2] The chief historical influence in establishing this theory was undoubtedly exercised by James in his chapter on "habit," a chapter that contains the still current diagram of the mechanics of serial action. The experiments reported in Watson's monograph involved the capacity of the white rat to learn the maze when deprived of various receptors, and the general conclusion drawn was that only proprioception was necessary for the learning and mastery of the maze habit, which was described as "a serially chained kinesthetic arc system." In 1914 Watson[3] held that the paper on orientation in the rat which he and Carr[4] had published offered "very definite and positive proof" that the above thesis was correct. And Washburn,[5] in 1926, endorsed the same theory on much the same evidence.

I shall not in the present paper again review the literature on the stimulus-neural control of the maze habit. It is necessary, however, to refer to certain studies in order further to clarify the problem of the possible shift of stimulus control during learning. In 1915 Vincent[6] published some important experiments, in which it was shown that the rat could utilize outstanding visual and olfactory stimuli during the learning of the maze. Animals that learned a black-white maze or an olfactory-trail maze were more irregular in the speed of their perfected responses than animals that learned the normal maze. This relative lack of automaticity in the responses Vincent attributed to the distractive influence of the olfactory and visual stimuli, respectively, the implication being that proprioceptively controlled responses would be more automatic than those controlled by other receptors. Vincent wrote as follows: "After the problem is learned, in the slow turning over to kinesthesis, when attention is freed, these [visual] sensory factors may still retain their potency in times of momentary distraction" (p. 24). After the above experiments were completed and the mazes learned, Vincent transferred the animals to olfactory and visual discrimination boxes in order to determine whether or not olfaction and vision were controlling the perfected maze responses or whether the control had shifted to kinesthesis. In both cases the results suggest that the exteroceptive senses were still involved in the control of behavior. There was no indication of a shift in stimulus control. In one experiment in the olfactory-trail maze, Vincent removed the trail after the maze had been mastered with the trail present. Under the changed conditions, the rats still ran perfectly. This, however, does not prove that the stimulus control of the behavior has been turned over to kinesthesis. It indicates rather that whatever stimuli were present after the olfactory trail was removed were capable of controlling a correct maze run.

The work by Dennis and Porter[7] (1932) clearly supports such a conclusion. They trained rats to run from the center of a circular platform to the periphery in order to secure food which was always located just beyond and below the end of a narrow metal strip running from center to periphery of the platform. A white square cardboard was placed at the food and within the rat's field of vision from the start of each run. The results show that when the card was removed, the metal strip controlled the rat's response and *vice versa*. In other words, there was a multiple stimulus control, even of the perfected response; and in the absence of a part of this complex stimulus, the remaining parts controlled the behavior.

In 1917 Carr[8] wrote "that the white rat learns the standard maze in tactual and kinaesthetic terms [and] that during the learning the control is gradually transferred from contact to kinaesthesis . . ." (p. 259). Carr's own extensive experiments were directed toward the analysis of the dependence of the maze habit upon the rat's total sensory environment. The results led him to conclude, rightly, I think, that "This act is dependent—both during and subsequent to its development upon a wider sensory situation of which it is a part" (p. 304). The two quotations from Carr are apparently contradictory. The first is partly a restatement of the psychological tradition that there is a shift in stimulus control during habit-formation and partly a conclusion based, probably, on the work of Bogardus and Henke[9] (1911) where it was found that normal (seeing) rats made many contacts with the

[2] J. B. Watson, "Kinesthetic and Organic Sensations," *Psychol. Rev.*, Monog. Suppl., 8, No. 33, 1907.

[3] J. B. Watson, "Behavior." New York: Holt, 1914.

[4] H. Carr and J. B. Watson, "Orientation in the White Rat," *Jour. Comp. Neur. and Psychol.*, 18: 27–44, 1908.

[5] M. F. Washburn, "The Animal Mind." 3rd ed. New York: Macmillan, 1926.

[6] S. B. Vincent, "White Rats and the Maze Problem," *Jour. Animal Behav.*, 5, 1915.

[7] W. Dennis and J. M. Porter, Jr., "Isolated Action of Compound Stimuli in a Locomotor Habit of Rats," *Jour. Genet. Psychol.*, 41: 127–135, 1932.

[8] H. A. Carr, "Maze Studies with the White Rat," I, II, III. *Jour. Animal Behav.*, 7, 259–306, 1917.

[9] E. S. Bogardus and F. G. Henke, "Experiments on Tactual Sensation in the Rat," *Jour. Animal Behav.*, 1: 125–137, 1911.

corners of the maze during learning, but later eliminated these. However, the more recent work by Dennis[10] (1929) indicates that while normal rats may make this change in behavior blind rats do not. The suggestion is, therefore, that vision played a rôle in Bogardus and Henke's experiments, and that kinesthesis was not alone involved in the completed habit.

Since 1917 many studies have appeared concerning the possible rôle of kinesthesis in the control of the perfected maze response. Rats have been trained to run a maze and then have been forced to swim the same maze, which they have done correctly in spite of the changed proprioceptive stimulations (Macfarlane[11]). Rats have been crippled either by muscular or neural operations and have still run the maze correctly (Dorcus and Gray;[12] Lashley and McCarthy[13]). Lashley and Ball[14] have interfered extensively with the spinal conduction paths, and still the rats could run the maze. Hunter[15] and Casper[16] have used mazes in which proprioceptive factors could hardly function to control a perfect response, and yet the rats learned the maze. Furthermore, Walton[17] has failed to find evidence that rats who form a visual maze habit come to depend upon proprioception. There seems to be no question but that kinesthetic cues are not necessary for the execution of such a serial response. *However, there is a great danger, already apparent in the literature, that the additional and unjustified conclusion will be drawn to the effect that proprioception is not normally involved in the control of serial action.* All that the evidence indicates is that, in some cases and perhaps in many more, proprioception can be altered or made to offer subliminal differences without disruption of the serial response. In such cases the evidence is that nonproprioceptive organs function as response controls. In the usual maze behavior and in such serial action as walking, as well as elsewhere, proprioception may occupy an important place both during and after learning.

We may, therefore, summarize the situation as follows: (1) No single receptor system is necessary for learning the usual maze habit or for executing it after mastery; (2) vision, olfaction, touch and proprioception are usually active in the control of the perfected maze habit, where experimental conditions permit; (3) there is thus no evidence that the control of this serial response is handed over to proprioception when and as learning is completed; and (4) if exteroceptors and proprioceptors are involved in the execution of the completed habit, there is no obvious reason for assuming that they were not involved earlier during the formation of the habit.

Some additional evidence on the problem of the shift in stimulus control has been gained by other than maze methods. While Watson was at work at the University of Chicago on the nature of the sensory control of the maze habit, the late June Downey was investigating, in the same laboratory, the control processes in handwriting. Downey[18] studied such highly automatized and over-learned responses as the signing of one's own name and other writing habits not so well established. Tests made with the subject blindfolded indicated that vision was partially effective in the control of the response. The nearest approach to evidence that the kinesthetic processes from the moving hand were sufficient to control the writing habit comes from certain cases where, with an effective distraction, the subject was still able to write correctly for several seconds, although even here the shaping of the various letters reveals the inadequacy of the response under such conditions. The general result of Downey's study was to show the complexity of the control processes for writing, with visual, auditory and vocal-kinesthetic factors operating. It was probably as a result of this work that Angell, when he came to discuss James' diagram and the conception of chain-reflex proprioceptive control of habit in his "Psychology" (1908), wrote as follows: "This description is probably roughly correct, but it presents far too simple and diagrammatic a picture to be taken literally. Experiments show that in such cases as writing illustrates, the sensory cues which are involved come from the eye and even the ear quite as often as from the muscles and joints" (p. 72).

The studies of typewriting which Book[19] performed at Clark University also give us some information on the stimulus control of serial action, although they were not primarily directed to this problem. The subjects who used the touch system of typing received no

[10] W. Dennis, "The Sensory Control of the White Rat in the Maze Habit," *Jour. Genet. Psychol.*, 36: 59–90, 1929.

[11] D. A. Macfarlane, "The Rôle of Kinaesthesis in Maze Learning," Univ. Calif. Publ. Psychol., 4: 277–305, 1930.

[12] R. M. Dorcus and W. L. Gray, "The Rôle of Kinaesthesis in Retention by Rats," *Jour. Comp. Psychol.*, 13: 447–451, 1932.

[13] K. S. Lashley and D. A. McCarthy, "The Survival of the Maze Habit after Cerebellar Injuries," *Jour. Comp. Psychol.*, 6: 423–433, 1926.

[14] K. S. Lashley and J. Ball, "Spinal Conduction and Kinaesthetic Sensitivity in the Maze Habit," *Jour. Comp. Psychol.*, 9: 71–106, 1929.

[15] W. S. Hunter, "A Further Consideration of the Sensory Control of the Maze Habit in the White Rat," *Jour. Genet. Psychol.*, 38: 3–19, 1930.

[16] B. Casper, "The Normal Sensory Control of the Perfected Double-alternation Spatial Maze Habit of the Albino Rat," *Jour. Genet. Psychol.*, 43 (in press).

[17] A. Walton, "Visual Cues in Maze Running by the Albino Rat," *Jour. Genet. Psychol.*, 38: 50–77, 1930.

[18] J. E. Downey, "Control Processes in Modified Handwriting," *Psychol. Rev.*, Monog. Suppl., 9: No. 37, 1908.

[19] W. F. Book, "Psychology of Skill," Univ. Montana Bull., 1908. (Also, New York: Gregg, 1925.)

direct visual cues from their hands, but they did read visually from copy and they may have received some visual stimulation in a peripheral manner from their hands. Along with the visual reading went, even in the expert stage, "incipient or actual pronunciation of the words, itself a form of the group 'spelling,' operative in the previous (word association) stage, that now initiated and directed the letter-making movements. The incipient or actual pronunciation of the words somehow directed and controlled the sequence of the letter-making movements" (p. 57). And again Book writes, this time in connection with the work by the sight system: "It was determined that a sort of half conscious incipient mental spelling occurred and was required for months after it seemed [to the subjects, as their reports indicate] that all traces of the spelling had disappeared" (p. 87). So far as Book's attitude toward the relation of "consciousness" and the performance of a highly perfected habit is concerned, we may quote the following: "As has already been pointed out in preceding sections, these processes need a minimum of oversight for a long time even after they seem completely self-regulative. In other words habits are perfected or sink to the realm of the unconscious very gradually" (p. 135). I shall come back later to a discussion of the "unconsciousness" of perfected habits; but at present I wish to offer a further analysis of the problem of shifting stimulus control, based upon suggestions derived from experiments on non-serial action.

In non-serial action of the conditioned response type a non-effective stimulus becomes effective through being paired, under certain conditions, with another stimulus which will elicit the given response. Thus if a sound is paired with shock under certain conditions, the sound will come to arouse the response (for example, finger withdrawal) which was formerly made only to the shock. Overtraining the subject does not result in a shift of sensory control from the sound to something else. According to Pavlov, the neural control involves the cerebral cortex, and so far as we know no amount of overtraining will switch the neural control to subcortical levels. If the non-serial action to be considered is of the type found in the usual visual discrimination experiment, analysis will show again that there is no shift in the stimulus control of the correct response, no matter how long the training continues. The experiment is so planned that visual stimuli will determine to which side of the apparatus the animal will go in order to escape and secure food. Once this response is connected with the light, it remains so connected irrespective of overtraining just so long as the experimental conditions are unchanged. The neural impulses set up by the light pass through the occipital cortex, and Lashley's work indicates that overtraining does not reduce them to a sub-cortical level, although his further work shows that the subcortical areas may function adequately for this behavior, if the animal is trained after loss of the cortical areas. Thus so far as non-serial action is concerned, training brings no shift in the stimulus-neural control of the correct response. One begins with a stimulus—neural-process—response and one ends the experiment with the same threefold phenomenon.

The analysis of the experimental literature on the maze habit shows that no shift in the stimulus control of this behavior during training has been found. Theoretically this is what would be expected. The rat in the maze will be affected by many stimuli both within and without the maze, both proprioceptive and exteroceptive stimuli. Certain of these stimuli will be sufficiently constant in relation to the correct pathway through the maze to serve as controls, or cues, for the maze-running behavior. It is true that, as the subject eliminates errors, certain responses no longer appear, and the stimuli for these responses no longer have their former effectiveness. However, the theory of the shift in stimulus control during learning does not refer merely to the fact that, when certain responses are dropped and certain others appear, a shift in stimulus control is usually involved. *The theory is that what are called the correct responses in the maze have first, let us say, a visual-olfactory-proprioceptive control whereas with additional training of the subject this control becomes solely proprioceptive.* For such a theory there is no experimental evidence. The only reasons why such a shift would appear, if it did appear, are (1) that proprioception is more invariably connected with the behavior of running the true path than are the other stimuli and hence with training it is favored more and more at their expense and (2) that proprioceptive stimulation is of a sufficiently greater prepotency to insure it the right of way over visual and olfactory stimulation.

If we are not justified in making the generalization that the learning of serial action necessarily involves a shift in stimulus control from exteroceptive to proprioceptive stimulation, neither are we justified in making a general denial that proprioception may at times be essentially involved in the control of completed habits. If exteroceptive forms of stimulation vary too much, or too little, to afford means of control, proprioception may carry the burden alone. If proprioception varies too greatly, or too little, then the control of the behavior is effected by the remaining forms of stimulation. Thus whether or not, and to what extent, proprioception controls the perfected serial action depends upon the particular situation in

which the responses are manifested and not upon a general law of the shift in stimulus control.

Let us turn now to a very brief analysis of the psychological theory that "consciousness" lapses during the process of learning and is absent from automatic acts. I introduce a discussion of this theory in the present context because of the implication in the theory that the stimulus-neural control of behavior changes during learning and over-learning in such a way that the subject's report (introspection) can no longer be elicited. We have already seen experimental reasons for doubting the existence of any significant changes in the character of the sensory-neural control of behavior as the automatic stage of performance is reached. Let us now see how defenders of the "lapsed consciousness" theory formulate their position. Wundt writes as follows in his "Outlines of Psychology" (1907): "This gradual *reduction of volitional to mechanical processes,* which depends essentially on the elimination of all psychical elements between the beginning and end of the act, may take place either in the case of movements which were originally impulsive, or in the case of movements which have become impulsive through the retrogradation of voluntary acts. It is not improbable that all the reflex movements of both animals and men originate in this way" (p. 214). Titchener writes in his "Textbook of Psychology" (1910) as follows: "The author, then, believes, with Wundt and Ward and Cope, that the earliest movements were conscious movements, and that all the unconscious movements of the human organism, even the automatic movement of heart and intestines, are the descendants of past conscious movements" (p. 452). In elaborating on the changes which action undergoes, Titchener says that "there is a tendency toward the simplification of movement, and the realization of this tendency is accompanied by lapse of consciousness" (p. 456). Angell's position in his "Psychology" (1908) can be indicated as follows: ". . . consciousness occupies a curious middle-ground between hereditary reflex and automatic activities upon the one hand and acquired habitual activities upon the other." When hereditary modes of adjustment fail to adapt the organism to its environment, consciousness immediately appears and "enters upon its characteristic cycle. At first of course its activities are vague and crude. But presently we find selected from out the masses of motor responses created by the sensory stimulations to which the sense organs are sensitive, those particular ones which issue in effective muscular control over the environment, and straightaway we are confronted with habits. As soon as these habits are firmly established, consciousness betakes itself elsewhere to points where habitual accommodatory movements are as yet wanted and needed" (p. 74–75). And again in his "Introduction to Psychology" (1918) Angell writes: "When [habits] have become sufficiently perfect, they are passed over almost entirely to the automatic control of the nervous system, leaving the mind itself free to go forward to the creation of other habits . . ." (p. 51). Similar quotations could be taken from the writings of many eminent psychologists, but the statements above given will suffice to indicate the general nature of the classical theory.

In support of the "lapsed consciousness" theory, or as a result of the theory, there is still a wide-spread tendency to comment upon the influence of training as a factor in permitting the organism to engage simultaneously in two or more activities. So long as both acts involve consciousness, their simultaneous activity is said to be difficult or impossible; but when habit has reduced one, or both, to the unconscious level, their simultaneous execution is possible. There is here a very genuine problem in the determination of the conditions under which two or more activities can be carried on by the subject; but the solution of the problem will not come, as it has not come, from speculations on the relation of attention and consciousness to habit formation. Rather the solution will result from an analysis of the stimulus—neural-process—response conditions which are necessary to permit the simultaneous and non-interfering occurrence of two forms of behavior. When such an analysis is experimentally made, it will reveal, I think, that the phenomenon is due to the independent organization of the two (or more) coordinations and not to any general factors of "lapse of consciousness" and "proprioceptive control" of serial action.

Oddly enough, there is little or no experimental support for the "lapsed consciousness" theory, although attempts have been made to interpret studies like those on reaction time in such a manner as to provide a basis for the theory. The mainstay of the theory is anecdotal evidence; and yet it would be relatively simple to study the problem in so far as such a problem can be formulated in scientific terms. Whatever differences may exist between mentalistic psychologists and behaviorists with reference to the existence and nature of consciousness, the fact is generally admitted that the only objective evidence of the presence of "consciousness" is the subject's report, usually verbal. The behaviorist, then, might state the problem as follows: How does the subject's report vary with the degree of training on any given response or series of responses? Since the subject's report is the behavior from which the mentalistic psychologist infers the existence of specific "consciousness," the lapse of "consciousness" should be indicated by the subject's inability to report during the final stages of habit

formation and after the responses have become automatic. So far as I know, no reports of such experiments have ever been published.

Some years ago I conducted a preliminary series of experiments on this problem, and the results are here outlined (briefly and rather inadequately) for the first time. In the first experiment the subject was instructed to name the objects that he saw. He was then shown tachistoscopically, exposure time 1/5 sec., a card on which were six colored squares and six lines. After each presentation of these stimuli, the subject was asked to name what he had seen. During the training his responses become more and more accurate until perfection was reached and the habit was established of responding to certain visual stimuli which were always the same. The subject was then greatly overtrained until both in time and accuracy his responses varied but little, *i.e.*, until the stage of automaticity was well established. After a given presentation and response the subject was then asked, "Did you see red?" (one of the colors shown) or "Did you say red?" (a response that he had made). In practically every case the subject reported correctly. Perhaps this was due to the fact that the same stimulus card was always used and, while the subject really could not remember what he had seen or said, he thought that he must have seen and said the usual thing. Therefore, certain control tests were made where a stimulus card was used which differed in some one particular from the usual card, let us say that the color red was changed to blue. When the new card was presented the subject immediately said "blue" in place of "red," although he did not know in advance that a new card was to be used. Apparently the visual stimuli were still effective and the control of the response had not been turned over to proprioception.

In a second experiment other subjects were to repeat a passage of prose first spoken by the experimenter. Trial after trial this was repeated until the subject was not only perfect but greatly overtrained. During the entire course of the experiment, the subject, during the time that he was reciting the prose passage, traced a star-outline under mirror drawing conditions. His instructions were to repeat exactly what the experimenter said and at the same time to work for speed and accuracy in the star tracing. After 100 trials the subject was tested, without warning, by changing some word in the prose passage and at other times by being asked to report what he had said. The results indicate that the subject was able to report with a high degree of accuracy, and that the words spoken by the experimenter in reciting the passage still controlled the subject's behavior.

A third experiment was then devised in which a greater premium would be placed on the accessory task. New subjects were used, and they too repeated a prose passage after the experimenter had finished reciting it. While listening to the experimenter and while reciting, the subjects performed the following tasks: They traced with a metal stylus a narrow black line which moved in a zigzag manner behind a slit. Whenever the stylus left the line, the subject received a slight electric shock through the stylus. Along the line and at varying intervals were certain numbers from one to five, each number corresponding to a digit of the left hand. The subject was to signal the appearance of each number by pressing a key with the appropriate digit. Ten dollars reward was posted for the best performance on this tracing-signaling problem in the hope that this would, for the subject, become the major problem and the recitation the minor one. After the 100th trial slight changes were occasionally introduced into the prose passage and the subject's recitation noted, after which he might be asked what he had said. In this experiment again, training brought no decrease in the subject's ability to report, and it brought no detected shift in the stimulus control from the auditory words of the experimenter to the proprioception of the subject.

It may be said that the training described in the above experiments was not continued long enough to bring about a "lapse of consciousness." Undoubtedly studies based upon more extended training are desirable, and yet it should be noted that the above experiments extended well beyond the beginning of automaticity of the response.[20] The general relationship between training and report which one would expect depends upon the nature of the instructions and is as follows: (1) If the subject is instructed to report his own movements, there will be a decreasing quantity of report during learning which will roughly parallel the actual decrease in quantity of response made by the subject. There is as yet no evidence that the subject would ever be unable to report his

[20] It should be noted that the theories of "proprioceptive control" and of "lapsed consciousness" deal primarily with automatic behavior without a clear definition of the term automatic. Unless one utilizes anthropomorphic implications of the absence of volition, automatic behavior is identified by its relative constancy, from trial to trial, in such characteristics as speed, accuracy and work accomplished. Such automatic behavior is found not only when the learning curve has reached the final level of attainment but also during the periods known as plateaus. Furthermore, all final levels of attainment are in reality incompleted plateaus to the extent that they are above the physiological limits of behavior and to the extent that further training of the subject may bring a change of behavior more nearly approximating the physiological limit. Therefore any theory that claims the presence of certain phenomena during the period of automaticity must make clear what period of automaticity is meant.

behavior, no matter how long he is trained, and thus there is no evidence for the psychologist to use as a basis for the inference that "consciousness" finally disappears. (2) If the subject is instructed to report not upon his movements but upon the stimuli which are presented unchanged from trial to trial, his report will either be accurate from the beginning of learning or it will increase in accuracy with training. There is no evidence that the subject will drop from accuracy to inaccuracy or from report to no report with overtraining, unless factors like fatigue are introduced.

These experiments, limited and inadequate as they are, nevertheless point the way toward future work which will more fully describe the relation between the subject's report and his degree of training in some specific response. I have said nothing in this discussion about such abnormal phenomena as dissociated personalities and automatic writing, because the theory of "lapsed consciousness" is a *general* theory of the relation of "consciousness" to learning. Experiment would undoubtedly confirm what anecdote has reported, to wit, the existence of some forms of behavior which were once reportable by the subject but which have ceased to be so as a result of some process of reorganization within the individual. This specific field of application for the theory would undoubtedly well reward the investigator bold enough and careful enough to till it!

For the present we have arrived at a general conclusion concerning the stimulus-neural control of behavior during and after learning to the effect that once stimuli and responses have been connected no amount of overtraining, under constant experimental conditions, will result in a necessary shift of the stimulus-neural control. This is the conclusion which would have been anticipated by investigators had they not been under the influence of the two psychological theories which we have discussed, one that "consciousness" lapses when the automatic stage of performance is reached and the other that the stimulus control of perfected serial action is proprioceptive in kind. If and to the extent that a shift in stimulus control occurs, the causal factors will probably be found to lie in the greater constancy of one form of stimulation or in its greater prepotency rather than in a general law that such a shift is inevitable and generally to be expected.

THE BIOLOGY OF HEAVY WATER

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As soon as it was found possible to prepare, with any desired degree of purity,[1] a new kind of water in which ordinary hydrogen, H^1, is replaced by its heavy isotope, H^2, it was interesting to ascertain what effect this heavy water would have upon living organisms. Several months ago the experiments were interrupted, and since there may be no immediate opportunity of resuming them it seems best to publish the somewhat sporadic results so far obtained.

On account of the very small amount of pure $H^2{}_2O$ available it was necessary to begin with small organisms. The first experiments dealt with the germination of tobacco seeds and a part of the results have already been published.[2] These seeds, which in ordinary water infallibly germinated within two days at 25° C., did not germinate at all in pure $H^2{}_2O$, as far as could be seen macroscopically. On the other hand, in water containing 50 per cent. $H^2{}_2O$ the seeds all germinated and developed about half as fast as in ordinary water. Later this experiment was carried on for one month, and although the development continued to be slow, the seedlings appeared to be perfectly healthy and normal.

The tobacco seeds which had remained three weeks in pure $H^2{}_2O$ without germinating, were then placed in ordinary water. At first it appeared that their power of germination had been completely destroyed, but after a week half of the seeds began to sprout, although in an abnormal way. The sprouts were extremely thin, and this sickly growth came to an end after a few weeks. There was no possibility of toxic impurities in the heavy water, as it had been very carefully distilled in a high vacuum at a temperature in the neighborhood of the freezing point. There is a remote possibility that some hostile organisms had been favored by the treatment with heavy water, but while this might conceivably explain the unhealthy growth of the seedlings, it could hardly account for the fact that one half the seeds did not germinate at all.

I believe that this whole phenomenon can best be explained by assuming that when the heavy water first began to permeate the seeds, it produced, together with the "bound water" already in the seeds, a medium in which germination could begin, but that as soon as the total water within the seed reached a

[1] Lewis, *Jour. Am. Chem. Soc.*, 55: 1297, 1933; Lewis and Macdonald, *Jour. Chem. Phys.*, 1: 341, 1933; Lewis and Macdonald, *Jour. Am. Chem. Soc.*, 55: 3057, 1933.

[2] Lewis, *Jour. Am. Chem. Soc.*, 55: 3503, 1933.