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What Psychology as a Science Owes Neal Miller: The Example of His Biofeedback Research

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Neal Miller did more to make psychology a science than any other investigator. His importance does not lie with any specific discoveries that he made, but rather with his way of doing scientific research, which involved pursuing a line of logic systematically through sequences of experiments, and paying attention to several alternate hypotheses that could answer each of the experimental questions addressed. His approach was a model of what has been called ''Strong Inference'' and that is characteristically used in the hard sciences. His biofeedback research is used as a case history of his method of approach.

Neal Miller was a great scientific thinker. He was arguably the most important scientific thinker in psychology of the 20th century. He did more to make psychology a science than any other investigator. The only other person who is in contention for this honor is B. F. Skinner. Important as Skinner was, he was not as important as Miller for reasons that I will outline below. Neal Miller is of greatest significance not for any specific discovery he made, but rather for his way of doing scientific research. This is less obvious than the contribution of specific discoveries; but because it changes our way of doing science, it usually makes a more long-lasting and general contribution than any specific discovery.

Neal Miller worked in many areas that were quite different from one another. The full range of these areas are described in large part by a companion piece to this article, appearing in this issue of *Biofeedback* by Edgar E. Coons and Sarah Leibowitz. These include human factors in World War II aviation, learning and behavior, an experimental analysis of psychoanalytic theory in behavioral terms, hypothalamic mechanisms of different drives such as hunger and thirst, central nervous systems of reward and intracranial self-stimulation, then biofeedback, and finally applications of biofeedback to ameliorate human pathology. He not only worked in these areas, but he also did definitive work in every one of them. Every time out he and his students rang the bell. This is a remarkable record. After all, the areas he worked in were widely divergent. What was the common thread that permitted him to do this? Neal Miller had great clarity of thought. His analyses almost always involve lines of logic that are transparent, simple, and pellucidly clear. However, this by itself is not the reason for his wide-ranging success; other scientific psychologists also had this. Neither does the element underlying Miller's success have to do with a fortunate choice of subject matter; Miller worked in many divergent areas that numerous other investigators worked in. Moreover, the different areas do not really have a commonality. The connection is in Neal Miller's extremely effective method of approaching and solving scientific problems. He is the prime example in psychology of a scientist who invariably and systematically used a method of doing research that has been called Strong Inference.

Method of Strong Inference

Strong Inference has a long and distinguished history. It is an approach to research that is characteristic of such hard sciences as molecular biology and particle physics; and as I see it, Neal Miller's main accomplishment was to bring this method into psychology. The approach derives directly from the thought of Francis Bacon during the English Renaissance, who formulated the idea of the experiment basically the way we do it now, with little change over the four intervening centuries. He did this most clearly in his work *Novum Organum* (Bacon, 1960), which translates as ''The New Instrument,'' by which he meant the experiment. It lay at the heart of his concept of the scientific enterprise before there was an experimental scientific enterprise, and it has come down to us today as the Accumulative Inductive Method of doing science. There has been just one important modification, by the American geologist T. C. Chamberlin at the turn of the 20th century (Chamberlin, 1897). The method was described by John Platt in a very influential lead article he wrote for the journal *Science* in 1964 (Platt, 1964). Platt said Bacon's method consisted of four steps, which Platt enumerated in his article. There are actually three additional steps that were omitted, but that are strongly implied. The method and in general the way in which Miller used it is as follows.

Seven Steps of Strong Inference

Step 1: Select a Phenomenon for consideration. It can be an observation that is made in nature or in the laboratory.

Next, Step 2: Ask a question about the Phenomenon. This is usually referred to as the Experimental Question. Miller was a master of selecting a centrally important Phenomenon and asking a crucially significant, and usually very simple, question about it. This comes under the heading of ''good taste.'' Neal Miller had very good taste. Most investigators are very good at criticizing previous research. We are all trained to do it. We all know how to identify what is wrong in other work. Miller certainly excelled at this, but he also had something more precious; He could identify what was good in other research. Even in weak experiments or diffuse theoretical structures, he could frequently identify whether there was something important that was worth pursuing further.

Next is Step 3, which is Platt's Step 1. Creativity in science is probably focused more in the first two steps than elsewhere in the research process. Neal Miller excelled here, and it is probably a hidden source of his remarkable repeated successes in so many diverse areas.

Step 3: Devise alternate hypotheses. Hypotheses are simply just answers to the Experimental Question; nothing more, and nothing less. According to Chamberlin, one should at the outset of the process, soon after asking an experimental question and before beginning work in the laboratory, establish as many alternate hypotheses or answers as one can think of. This he termed the Method of Multiple Working Hypotheses, which is the one important modification of Bacon's original method referred to above.

Neal Miller was a master of this type of approach. He had a most impressive ability to see the whole logical structure of a problem before doing the first experiment.

Step 4: Devise a crucial experiment (or several of them), with alternate possible outcomes, each of which will, as nearly as possible, exclude one or more of the hypotheses.

Step 5: Carry out the experiment or experiments so as to get a clean result.

Step 6: Recycle the procedure, making sub-hypotheses or sequential hypotheses to refine the possibilities that remain—and so on. Miller was also certainly a master of this. He always tried to nail things down. He never, or almost never, did single experiments on one subject, then drop it and move on to another subject. The principle was, as in the Method of Multiple Working Hypotheses of Chamberlin, that you always have to test the alternate hypotheses, because no matter how convincing the evidence is for the hypothesis or answer to the Experimental Question that you test first, which is probably your pet hypothesis, there remains the possibility that there is an unseen flaw in what has been done, or that an alternate hypothesis will also show up as being correct when tested by experiment; and it is this hypothesis or answer that either better fits the data or explains more of it.

This is the way that Neal Miller thought. He had the gift of seeing a specific experimental question as lying at the heart of a clear logical structure, which included its possible answers and also where they led. A number of scientists in fields of science other than psychology have done experimental research in this way. Miller, however, if I am not mistaken, was the first one in psychology to pursue the Accumulative Method of Inductive Inference or Method of Strong Inference in a systematic and disciplined way. And he always did it.

Finally, Step 7: Select for consideration a new phenomenon that emerged during the above process (e.g., a serendipitous finding, or an expansion of the original experimental question, or a new subject entirely) and repeat the procedure—and so on. That is the way you break out of the Baconian system before one's work becomes sterile.

This is Strong Inference, and it seemed very clear to me that this is what Neal Miller did, and, as I have noted, as far as I could tell he was really the only one in psychology to have done it. One day when I felt I knew him well enough, in the mid-1980s, I asked him whether he worked the Strong Inference system in his research and whether he had ever read John Platt's article. ''No,'' he said. He had certainly heard about the article, and had always meant to read it, but he never had. Well, had he ever read Francis Bacon's *Novum Organum?* He had certainly heard of that also, but no, he had never read it, and not only that, he had never intended to.

Though he never explicitly stated this in his writings or in our conversations, his former student Edgar E. Coons notes (personal communication, June 6, 2010) that he was probably influenced by the Pragmatic movement in philosophy represented by such thinkers as Charles Sanders Peirce and John Dewey, who considered Francis Bacon to be one of their early precursors, and by psychologist William James. In fact, Miller's father had studied with Dewey at the University of Chicago, and the Pragmatist position was very much in the air when Miller was a student. That, however, was clearly only Miller's point of departure. Prescriptions from the Pragmatist position do not account for the way he formulated experiments, his systematic reliance on establishing multiple working hypotheses to address an experimental question, or his characteristic method of chaining together or sequencing experiments to arrive at a firm conclusion, which is very much a part of Bacon's system as modified by Chamberlin.

Thus, Neal Miller, in all likelihood, started with the Pragmatist position. However, from there he had independently discovered Strong Inference. It had intuitively appeared to him that this was the correct or productive way of doing scientific research. He had discovered for himself what for many philosophers of science lies at the heart of the origin and continued development of the experimental or Baconian sciences. When I told him this, he was mildly pleased, but he really didn't care very much. The research was the research. It either uncovered new phenomena or it didn't. If it did, then that was what was important. For the rest—well, OK that was fine. I could get excited if I wanted to.

The Course of Neal Miller's Biofeedback Research

Neal Miller's biofeedback research can be taken as a case history of how his work in general embodied this approach. The point he started from was the following:

Phenomenon:

1. You can use classical conditioning to train both autonomic and somatic response systems.
2. You can use instrumental conditioning to train somatic response systems.

Experimental Question. Can you use instrumental conditioning to train autonomic response systems?

He thought this might be possible because there were many similarities in the nature of the phenomena that are associated with both classical conditioning and instrumental conditioning. These include: extinction; the delay of reinforcement effect or the weakening effect of delaying the reinforcement after the response is made in instrumental conditioning and delaying the presentation of the unconditioned stimulus after the conditioned stimulus in classical conditioning; the similarity of the effect of partial reinforcement schedules and the intermittent presentation of the unconditioned stimulus; the partial reinforcement extinction effect (which applies to both types of conditioning); and the fact that after the relationship of a discriminative stimulus and a reinforcement has been learned, the instrumental or operant conditioning situation appears to be just the same as a classical conditioned response paradigm, which can be easily seen if you diagram the situation—the main difference is in their history. In addition, although it was virtually an axiom in psychology that you could not operantly condition an autonomic response system, the actual evidence for this was extremely weak.

Canine Salivation. The first experiment addressed to this question was carried out with Alfredo Carmona (Miller & Carmona, 1967). They implanted a cannula just in front of the parotid gland so that the rate of salivary production could be measured in dogs. The animal's task was to either increase or decrease the amount of saliva production, which is normally under the direct control of the autonomic nervous system. Water reward was used; it was also a source of feedback as to the correctness of the response—biofeedback, if you will. The dogs did indeed establish instrumental control of their rate of salivation, but because they could move about in their harness, it was possible that their state of muscular activity in a part or all of their body mediated the self-regulatory control of their rate of salivation, a situation termed ''somatic mediation.'' If such were the case, then clearly there would have been no direct instrumental control of an autonomic response system, in which case the experimental demonstration, from a theoretical point of view, would have been of relatively minor significance. Indeed, it was found that the dogs trained to increase salivation had an electroencephalogram (EEG) characteristic of an aroused state whereas the dogs trained to decrease salivation showed a low arousal EEG pattern. In apparent confirmation, one observer, Edgar E. Coons, reports that the dogs tended to be alert and to stand fully erect in the increase situation and to slump in their harness in the decrease situation.

Curarized Rats. To deal with the problem of somatic mediation, Neal Miller switched to the famous paralyzed rat preparation with graduate student Jay Trowill (Miller, 1969; Trowill, 1967). The rats were paralyzed with *d*-tubocurarine and artificially respirated. Electrical stimulation through an implanted electrode of a part of the neural reward system located in the hypothalamus provided reward for the target behavior and feedback to the animal indicating whether variations in heart rate were in the desired direction. Different groups of rats learned to either decrease or increase their heart rate, but the magnitude of the changes were relatively small, on the order of 5% of total heart rate. This was not enough in Miller's view to completely resolve the theoretical issue, because overturning what had become virtually a fundamental axiom in the learning field, that you could not instrumentally condition an autonomic response system, was an extraordinary finding that required, as the scientific dictum prescribes, an extraordinary level of proof. The next ideas were conceptually simple but methodologically complex. In order to increase the experimental effect, thereby making the principle incontrovertible, the training technique termed shaping was used—that is, approaching a target behavior in small steps or ''successive approximations'' in which small improvements in performance are progressively required. Leo DiCara employed this approach in Miller's laboratory and reported extraordinary success (Miller & DiCara, 1967), achieving alterations in heart rate that were much larger than those reported by Trowill and Miller. There followed a series of studies using the same general methodology in which an attempt was made to manipulate a variety of autonomic response systems. Success was reported with blood flow in the rat's tail, systolic blood pressure (DiCara & Miller, 1968a, 1968b), rate of urine production (Miller & DiCara, 1968), gut motility with Ali Banuazizi (Miller & Banuazizi, 1968), having the blood flow in one ear increase (blush) while the blood flow in the other ear decreased (blanch) (DiCara & Miller, 1968c), and long-term retention of heart rate changes learned while paralyzed (DiCara & Miller, 1968d). The general conclusion was that many autonomic response systems can be instrumentally conditioned in the paralyzed rat preparation (where there is no possibility of somatic mediation).

The implications of this conclusion were far reaching, suggesting that humans could self-regulate their own internal environment through biofeedback and thereby self-correct a large variety of previously intractable pathologic conditions. This was a dream that even the namesake of the club sponsoring tonight's centennial celebration of Neal Miller, and the inventor of the term ''interior environment,'' Claude Bernard, did not conceive of.[1] Of course, there were others who were beginning to do or had done biofeedback research to exert instrumental control of autonomic response systems at the time, but none were as extensive and had the appearance of conclusiveness as the work coming from Neal Miller's laboratory. However, they did seem to lend support to the findings from the Miller laboratory.

Controversies over Instrumental Conditioning of Autonomic Function. A number of investigators did not accept the conclusions reached by Neal Miller and his students. Abraham Black, Larry Roberts, and two prominent psychophysiologists, Paul Obrist and Jasper Brener, were among the first to argue that the results from the Miller laboratory did not resolve the theoretical issue of whether operant conditioning of an autonomic response system was possible and/or that the results reported were artifacts (Black, 1974; Brener, 1974, 1987; Obrist, 1968; Obrist, Sutterer, & Howard, 1972; Roberts, 1974). Perhaps even more importantly, a number of efforts to replicate the curarized rat heart rate training results failed. Brener had his entire laboratory working on varying different aspects of the experimental procedure for several years, hoping to find the reason for the failures to replicate. One of Jasper Brener's graduate students, Hothersall (Hothersall & Brener, 1969), did succeed in achieving self-regulation of heart rate in a curarized rat, but after receiving his doctorate Hothersall left Brener's laboratory, and attempts there to replicate those results were negative. Similarly, Slaughter, Hahn, and Rinaldi (1970) reported replication of the DiCara and Miller (1968) results, but then Hahn (1974) could not replicate his own earlier findings.

Miller's Attempts to Find Reasons for Failure to Replicate Results. Neal Miller, in a heroic effort, sponsored a substantial number of different attempts at replication in his own laboratory at Rockefeller University, notably several by Barry Dworkin, using DiCara's shaping procedure (Dworkin, 1973; Dworkin & Miller, 1977, 1986; Miller & Dworkin, 1974). Again, the results could not be replicated. Before these events began to unfold, DiCara had left Neal Miller's laboratory for a position at the University of Michigan. To address the possibility that there was some subtle characteristic in the way DiCara had carried out the experimental procedure or some special skill he had in shaping the target behavior, Miller requested that DiCara come back to Rockefeller University and duplicate his first heart rate results. After much delay, DiCara returned, spent two weeks working on the experiment, failed to replicate the original results, and, citing urgent business that had to be attended to in his new laboratory, returned to the University of Michigan (Miller & Dworkin, 1974).

It should be noted that Miller and Dworkin's exhaustive efforts to replicate the curarized rat heart rate results,

[1] The present article originated as a presentation to the Claude Bernard Society, at the annual meeting of the Association for Applied Psychophysiology and Biofeedback, in San Diego, California, March 26. 2010.

though unsuccessful, were nevertheless a model of the use of the Strong Inference method of doing research. An impressive number of alternate hypotheses were evaluated, relating among other things to strain of rat used, including strain differences in response to *d*-tubucurarine, and type of artificial respiration procedure employed, especially gaseous exchange parameters. The array of alternate hypotheses examined were subtle and spun out with ingenuity; no explanation that could be thought of at the time was left unexplored. The entire last third of the article in which this work is first described (Miller & Dworkin, 1974) contains a discussion of several questions related to visceral learning in which a large number of alternate answers are presented in each case. This approach is simply a primary characteristic of the way Neal Miller thought, and it constitutes a textbook example of how scientific inquiry should be conducted. It is the work of a master.

Largely because of the repeated failures to replicate, most investigators aware of the situation were of the belief that DiCara and Miller's results were an anomaly and that it really was not possible to instrumentally condition an autonomic response system. There remained the demonstration of instrumental heart rate control by Trowill and Miller that had a small (but reliable) effect and the results of Hothersall and Brener (1969) and Slaughter et al. (1970), but adverse publicity had been so intense that a powerful negative halo resulted. Few people believed that the general principle that had presumably been demonstrated originally was correct.

It would have been understandable if Neal Miller had then dropped the whole subject and turned his attention to another problem, or retreated from this line of experimental analysis entirely and the general negative ambience it had acquired. However, Miller had not yet finished following the Strong Inference ''string.''

In many ways the paralyzed rat preparation is one of the worst possible settings in which to require an organism to learn anything. Consider the situation from the rat's point of view. There it is, lying on a table in an alien and clearly hostile environment. It suddenly has been totally paralyzed and now cannot move a muscle, nor can it breathe on its own. A cone is placed over its face and a gaseous mixture is forced in and out of his lungs. The rat would, of course, have to be completely terrified. Consider the state of any human subjected to similar conditions. Indeed, any vertebrate organism would be terrified. If the results were positive, that is, if instrumental learning had taken place, then all well and good; but if no learning took place, then looking at the situation from the rat's point of view, this is what would be expected.

Instrumental Learning of Blood Pressure Changes in Paralyzed Humans. In the next step Neal Miller did not attempt to modify a discredited experimental model that could probably only produce an equivocal reaction no matter how positive the results were. Instead, in a move demonstrating his exemplary flexibility and abiding focus on the conceptual basis of the inquiry rather than solely on attempting to correct methodological problems that would distract from this focus, he returned to the second Experimental Question in this sequence, ''Can instrumental conditioning be used to train an autonomic response system eliminating the possibility of somatic mediation using the paralyzed rat preparation?'' Was there some other way of achieving total somatic paralysis that would provide another avenue for approaching the conceptual issue, one that would have equivalent theoretical significance? By asking this question, an alternate pathway for addressing the central issue became almost obvious. Instead of paralyzing the somatic musculature of a rat with *d*-tubocurarine while leaving the autonomic nervous system capable of operation, one could achieve a similar result with humans by working with a patient with a severed spinal cord; somatic muscle activity would be abolished, but the cranial parasympathetic outflow would remain intact (Miller, 1979; Miller & Brucker, 1979; Pickering et al., 1977). The experimental question could now be rephrased as follows: ''Can instrumental conditioning be used to train an autonomic response system, eliminating the possibility of somatic mediation by employing quadriplegic humans with high spinal cord transections as subjects?'' Working with Bernard Brucker and Thomas Pickering, the answer was swift in coming (Miller & Brucker, 1979). Quadriplegic patients with high spinal transections were able to increase blood pressure using biofeedback from small, rapid changes in that parameter. Indeed, the control was sufficiently powerful to enable patients to overcome the orthostatic hypotension induced by the recumbent position in which these patients are normally maintained so that they could sit up.

This experiment was not completely satisfactory. In order to avoid the complexities of working with patients requiring artificial respiration and who typically had reduced longevity, patients were chosen with cervical transections below the level of the fourth cervical nerve, thereby leaving intact the phrenic nerve which is necessary for unaided respiration. Thus, the neck and very upper portion of the thorax still retained some innervation of the striated muscles. To deal with the possibility that self-regulatory control of blood pressure was being achieved through this small somatic remnant, patients were asked repeatedly to attempt to alter blood pressure by means of whatever striate muscle activity of which they were capable and/or changing their rate of respiration. Patients were able to alter blood pressure in this fashion, but it was only a fraction of what they were able to do using their skills learned through biofeedback. In addition, recordings of respiratory rate and in recordings from relevant muscles (electromyograph [EMG]) were made while these patients were exerting self-regulatory control of blood pressure; no correlations were found to exist. Caution was used in interpreting these results, which was perhaps wise given the prior history of this line of research. The title of the focal article was ''A learned visceral response *apparently* independent of skeletal ones in patients paralyzed by spinal lesions'' (Miller & Brucker, 1979, emphasis added). The results were taken as ''*strongly indicating* that these patients can learn unusually large increases in blood pressure and that this visceral response can be performed independently of skeletal responses'' (emphasis added). One possible reason for this caution was that Black (1974), Brener (1974, 1987), Obrist (1968; Obrist et al., 1972), and Roberts (1974) had formulated a widely discussed hypothesis to account for the visceral learning in curarized rats when the accuracy of the initial results was not yet being questioned. The reasoning went that though the rats were paralyzed, the learning may have been mediated by activity in central nervous system (CNS) somatomotor centers; if that were the case, then whatever visceral learning that occurred would not really be direct instrumental conditioning of an autonomic response system. There are at least three major problems with this argument: 1. No direct evidence was offered to support it. 2. There are no differentiable CNS signatures for the two types of learning; there are differences only in the training paradigms, which are operationally external to the organism. 3. Once inside the CNS the differentiation between somatomotor and visceromotor output is not at all clear. Consequently, once the critical mechanisms are placed inside the CNS and are not specified, the hypothesis becomes untestable at the current state of our knowledge. According to Popper's principle of falsifiability, such an hypothesis has no scientific value, at least at present. However, if one were to err on the side of caution, as might be advisable given the history of research on this problem, one might say that the central issue had not been conclusively resolved.

Taub's Thermal Biofeedback Experiments. This is not quite the end of the story. Beginning in the 1970s the author of this article carried out a series of thermal biofeedback experiments. The last part of this sequence was conducted in collaboration with Neal Miller, and that work extended the line of logical progression of Miller's experiments addressing the question of whether instrumental learning of autonomic responses without somatic mediation was possible. In the first part of the work, before collaboration with Miller began, it was shown that human subjects could self-regulate the temperature of their hands and other portions of the body using a visual information display (Taub, 1977; Taub & Emurian, 1973, 1976). The temperature of tissue is directly related to the amount of blood flowing through it (Taub, 1977; Taub & Emurian, 1973, 1976). Control of peripheral blood flow was presumably the method by which the temperature alterations were achieved, and this was demonstrated independently by photoplethysmography. Some of the characteristics of this self-regulatory control were explored in further work, and, among other things, it was unexpectedly found that when feedback was given from a single location on the hand, the temperature change was greatest at the feedback locus, and it progressively decreased with distance from that point, even though the subject's instruction was simply to alter the temperature of the whole hand (Slattery & Taub, 1976; Taub & School, 1978). The musculature responsible for moving the human hand is located mainly in the lower arm at a substantial distance from the hand. The intrinsic muscles of the hand are relatively small and weak and are mainly involved in spreading the fingers. If subjects could be trained to self-regulate temperature change in one half of the hand and not the other half, the most likely mechanism would be through direct control of the vasculature and not by patterning of isometric, unobservable contractions of the intrinsic muscles of the hand. Temperature-sensing elements, thermistors, were attached to five positions on the dominant hand: the web dorsum (located on the radial side of the metacarpus), the hypothenar eminence (located on the palmar surface of the ulnar side of the hand), and the palmar surface of the distal phalanges of digits 2, 3, and 4. Different groups of subjects were given feedback from the web dorsum only, the hypothenar eminence only, or the average of the three fingers only; but all were given the same instruction to raise (or lower) the temperature of the hand and had thermistors on the same five locations. All subjects learned to alter the temperature of the whole hand within six 15-minute feedback sessions, and after more training very clear anatomical differentiation of the temperature response emerged; there was a large response around the feedback locus, and much less or none at other locations.

Three subjects were given feedback from one side (radial or ulnar) of the index finger (D2) only and not from the thermistor on the other side of that digit or on either side of D3 or D4 or from the web dorsum or hypothenar eminence. Each showed a greater temperature effect on the fingers than on the metacarpus, but only one showed a greater effect on D2 than on the other two digits. However, that subject, after 43 sessions, showed a much greater temperature change on the ulnar side of D2 than on the radial side. For the last 13 days the difference was 5.1°F. On Day 44 the feedback, unknown to the subject, was switched to the radial side of the digit, and on Day 52 the feedback locus was switched back to the original side. In both cases the side showing the largest temperature response also switched over a period of days.

Taub and Miller's Thermal Biofeedback Experiment: Anatomical Specificity. At this point the author began collaborating with Neal Miller in planning the research. Eleven subjects were given feedback from either D2 or D4 with explicit directions to alter temperature at one or the other digit (in either an upward or downward direction) but not in the other digit (Taub, Miller, Morris, & Robinson, 1993). Eight of the subjects showed a significantly greater temperature response on the designated digit than at the other one. Lynch, Hama, Kohn, and Miller (1976) had reported similar differentiation of a temperature response between two digits of the same hand in one young subject. In the research from my laboratory no correlation was found between the temperature response and respiration or EMG recorded from the lateral or medial aspect of the forearm, the metacarpus at the interosseus muscles for D2 or D4, or the proximal phalanx of either D2 or D4. However, there was a strong correlation between blood flow in the two digits as measured by photoplethysmography and temperature change. The subjects were also kept under close visual observation through a one-way vision screen. Thus, the anatomical specificity results represented differential alterations in blood flow and were not due to an artifact or ''cheating'' by the subject and, in particular, not to somatic mediation involving muscle activity changes from any of the locations we recorded from. Further confirmation comes from data recorded from the nondominant hand, which had thermistors affixed to locations homologous to those on the dominant hand, which was the subject of the self-regulation instructions and feedback. Mirror responding or ''following'' by the nondominant hand developed in all 11 subjects that was two-thirds to three-quarters as great as in the instructed, dominant hand. The anatomical specificity effect occurred in each of the eight subjects in whom it was present in the dominant hand even though there was no contingent advantage for the subjects for that to occur.

This then brings to a close the work on the question of whether it is possible to instrumentally train autonomic response systems carried out in Neal Miller's laboratory or personally fostered by him. Like all work carried out along the lines of Strong Inference, its conceptual structure can be characterized by a logic tree. This is presented in Figure 1.

Before leaving the account of this line of research, it would perhaps be appropriate to mention the experiment of Ernst, Kordenat, and Sandman (1979), which had been virtually ''lost in the literature.'' They were able to train four dogs paralyzed with succinylcholine to decrease the blood flow through a branch of the main left coronary artery in order to escape or avoid electric shock to a hindlimb. The unconditioned response to shock is an opposite increase in coronary blood flow. The coronary blood flow increases were quite large. This result suggests the possibility, in conformity with speculation contained in a paragraph of Miller and Dworkin (1974), that there may be something idiosyncratic about heat rate, and that other response systems under autonomic control may be easier to train with instrumental conditioning procedures. Thus, the demonstrations in curarized rats reported by the Miller laboratory of instrumental conditioning of such autonomic response systems as blood pressure, rate of urine production, gut contractions, and peripheral vasomotion could still be correct; these response systems may have been more amenable to instrumental control than heart rate. The Ernst et al. experiment and its implications have not been included in the analysis and logic tree here since it did not involve Neal Miller directly. The objective in this paper was to analyze and exemplify the way in which Neal Miller carried out sequences of experiments to address an experimental question rather then to assess all the evidence concerning whether instrumental training of autonomic response systems is possible. However, the hypothesis suggested by the Ernst et al. experiment is worth considering in future research.

Discussion

What is the importance of this work? Although the answer to the original question is reasonably compelling, it is not absolutely conclusive. At the time this line of investigation was begun, the question addressed was one of the critical issues in the field of learning. Learning, in turn, was considered to be the central process underlying the understanding and, more importantly, the control of behavior, a position it had held since the advent of the dominance of behaviorism in psychology more than 30 years earlier. However, by the time the work in the laboratory of Taub described briefly above was done, the day of

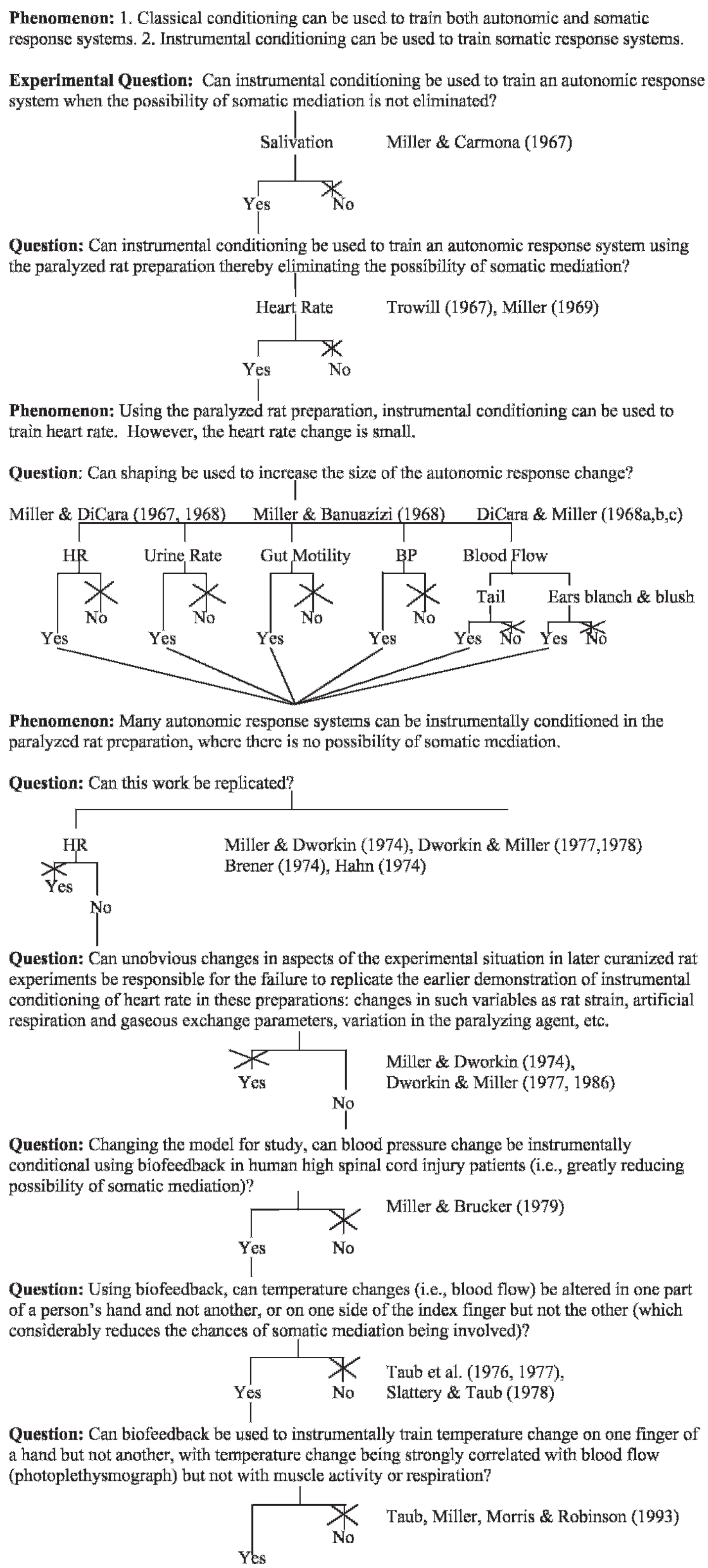

Figure 1. Logic tree of Neal Miller's biofeedback research.

behaviorism's dominance had passed. Cognitive processes had become the center of conceptual and experimental attention. Not many investigators were interested in the original question any longer. It was enough to know that visceral learning could take place and to have available procedures by which this could be accomplished. The question of whether this could be achieved by instrumental learning or classical conditioning no longer had salience.

Was the work then unimportant? This was clearly not the case on a practical level. The experiments produced a storm of interest and were reported extensively in the media in this country and worldwide. In the process, the work from Miller's laboratory probably did more to generate interest in the nascent field of biofeedback than any other body of work. However, the true significance of Miller's work in biofeedback did not lie in the content of the questions he asked or in their answers. Their importance lies in the fact that they are a model for the use of Strong Inference in addressing scientific questions in what had formerly been almost universally considered a ''soft'' area of scientific inquiry. Neal Miller used this approach not only in his biofeedback experiments, but also in virtually every area he worked in over his long career. It was his characteristic mode of approach, and it had arguably not been done before in as systematic a fashion in the field of psychology. This is a method of carrying out research that had previously been employed in such ''hard'' experimental sciences as particle physics, chemistry, and molecular biology. It harkens back to the formulation of the experiment and the experimental method by Francis Bacon, and its use was responsible for a great deal of success in these fields. Its systematic use by Neal Miller has been subtly influential in promoting this type of approach by many other investigators, and by so doing has brought psychology into the mainstream of the scientific enterprise. It is in this way that Neal Miller has done more than any other individual to make psychology into a science.

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