SELF-EXPERIMENTATION: A CALL FOR CHANGE[1]

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Few behavioral psychologists spend much time experimentally analyzing their own behaviors. Perhaps they study animals in operant chambers, or people in laboratories, classrooms, or hospitals, but rarely, if ever, do they study themselves in quite the same manner. To the contrary, they generally do what others do, layman and scientist alike, and strive to lead a good and effective life, try to raise their children well, and hope things will change for the better. Some even fight with spouse or colleagues and attempt to solve problems, at least for the moment, in alcohol, orgasm, or acid.

Although scientists by profession, behavioral psychologists generally leave their science at work, in the laboratory or the office. Nine-to-five scientists, they neither bring science home nor turn it on the substance of their ongoing lives. The main point of this paper can be phrased as an hypothesis: if experimental psychologists applied the scientific method to their own lives, they would learn more of importance to everyone, and assist more in the solution of problems, than if they continue to relegate science exclusively to the study of others. The area of inquiry would be relevant to the experimenter's ongoing life, the subject would be the experimenter, and the dependent variable some aspect of the experimenter's behavior, overt or covert.

Self-experimentation has a long history, especially in the medical sciences, resulting in significant discoveries (Altman, 1972). For 30 years in the late 16th century, Sanctorius of Padua weighed himself before and after meals, weighed all foods he ate, and weighed his excrements, and then attempted to account for the differences in weights. His was an early attempt to study the energy expended by a living organism. Anton Storck, in 1760, drank hemlock to determine its therapeutic effects. To study how digitalis affected his vision, Purkinje ate foxglove. John Hunter, attempting to determine whether syphilis and gonorrhea were separate diseases, inoculated himself with matter obtained from the penis of one of his patients. W. Forssmann placed a catheter through the vein of his arm into his own heart, demonstrating the feasibility of this important medical

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procedure. Lazear died from his self-experiment with yellow fever. Serturner, after isolating morphine, swallowed a large dose and made the significant observation that his toothache was relieved. Helsted experimented on himself with cocaine. Hoffman discovered and experimented on himself with LSD. Henry Head cut the nerves in his arm to study the regeneration of pain. And an anonymous scientist (Anonymous, 1970), while a temporary recluse on an island, weighed the hair he shaved from his face each day as an index of testosterone production.

Experimental psychologists, too, have a heritage of self-experimentation. The introspectionist tradition of Wundt and Titchener relied on the subject's, and often the experimenter's, evaluation of his or her experiences, and this tradition has been influential. For example, E. G. Boring (1915), Titchener's student, placed tubes into his mouth or anus and reported on the sensations caused by different temperatures, electric shocks and the like. Early volumes of the *American Journal of Psychology* and *Psychological Review* show numerous cases in which the experimenter was the sole or major subject in his or her experiment. Lombard (1890) examined the effects of fatigue on muscular contractions, using his own muscles. Angell and Pierce (1891) were their own subjects in an elaborate series of experiments attempting to decide between the Wundt and James competing theories of attention. Edward Thorndike (1900) served as a primary subject in his series of experiments on mental fatigue. Of particular interest to behaviorists, Dressler (1891) explored his own response rates as functions of time of day and of physical and mental exercise. He found that the rate of his tapping on a Morse-code key was related by a U-shaped function to the time of day; that response rates decreased with prior physical exercise; and that response rates increased after prior vigorous mental exercise. Anticipating contemporary authors, Dressler wrote proudly of the large number of responses emitted in the study, more than 120,000, and of his newly invented cumulative recorder where response rates were seen directly from the slopes of the curve. At about the same time, Stratton (1897) passed eight days with inverting lenses in front of his eyes. And, in the most notable case of self-experimentation in psychology, Hermann Ebbinghaus (1913) explored his own memory in an extensive series of experiments lasting for more than two years. Ebbinghaus's findings have been influential until this day.

A number of my students have attempted to follow the above tradition. For example, Ned Connor recorded aspects of his own behavior for more than four years. Ned kept a small sheet of paper and a wrist stopwatch with him at all times and recorded the time spent at each major activity, such as when he slept, when he was in the laboratory, when he was socializing and with whom, time spent working, relaxing and exercising, what foods he ate and when, and how many sentences he wrote. In one study, Ned constructed cumulative records of number of sentences written each day over the course of 2½ years, and identified reasons for changes in response rate: he identified fixed-interval scallops, with sentences increasing as each school term drew to a close; living with another person caused a decrease in writing rate; writing in a single isolated room increased rate; and sleeping during the day and working at night also resulted in a marked increase in rate. Ned also analyzed his sleep cycle (Weber, Cary, Connor, and Keys, 1980) and found that over more than two of the four years analyzed, he maintained a greater than 24-hour sleep-wake cycle. Although this effect is commonly found when people are isolated in environments containing no obvious time cues, e.g., while living deep in a cave, the effect is noteworthy for a sighted individual in an everyday environment.

I kept a similar but less detailed account of my daily activities for about one year and, in addition, recorded "ideas," thoughts about experiments, about courses I teach, papers I'm writing, etc. Although I maintained the daily-activity records for only a few weeks at a time, with many weeks intervening when daily records were not kept, I tried to record the "ideas" throughout the year. I had, thus, an unplanned A-B-A design, with number of ideas the dependent variable and the recording or absence of recording of daily activities as the independent variable. I found that when I kept the daily activity record – what I ate, when I slept, etc. – I recorded more than three times as many ideas as when I was not keeping the daily records. There are a number of possible explanations, each of which can be tested, but the one I favor was suggested by Professor B. F. Skinner who advised his students to catch birds on the wing. If you have an idea, he would say, write it down immediately or it will fly away. I hypothesize that recording daily activities increased not the genesis of ideas, but the probability of catching those ideas on wing and transcribing them.

Another student, Susan Duncan, hypothesized that her food intake was related to her need for sleep: the more she ate, the more sleep and vice versa. Using an A-B-A design, she compared how many hours she slept per night as a function of two levels of caloric intake, a normal level of 2,000 to 2,500 calories per day and a low level of about 1,000 calories per day, i.e., half of her normal food intake. The phases were as follows: 12 days normal calories, 6 days low calories, 30 days normal, 35 days low, 24 days normal. Each day, upon waking, she marked the time she fell asleep (that being closely related to the time she went to bed) and the time she awoke. Throughout the 107 days of this experiment, she otherwise maintained her normal routine – that of a senior in college – and used the alarm clock when necessary to wake for classes, did without the alarm on weekends, etc. Figure 1 shows that when she ate 1,000 calories per day, she slept fewer hours, an average of 6.6 hours per night, than when she ate normally, an average of 7.7 hours per night. She reported being more alert and better able to study during the low calorie phase, and also waking earlier, generally before the alarm was set to ring, which was unusual for her. However, she also reported becoming physically tired and weak, e.g., unable to run as far as usual, toward the end of the second block (35 consecutive days) of the 1,000 calorie per day phase.

As part of the introductory Psychology course at Reed College, I asked students, mainly freshmen, to do self-experiments, to choose a question of personal interest, design an experiment, record and analyze data – all with-

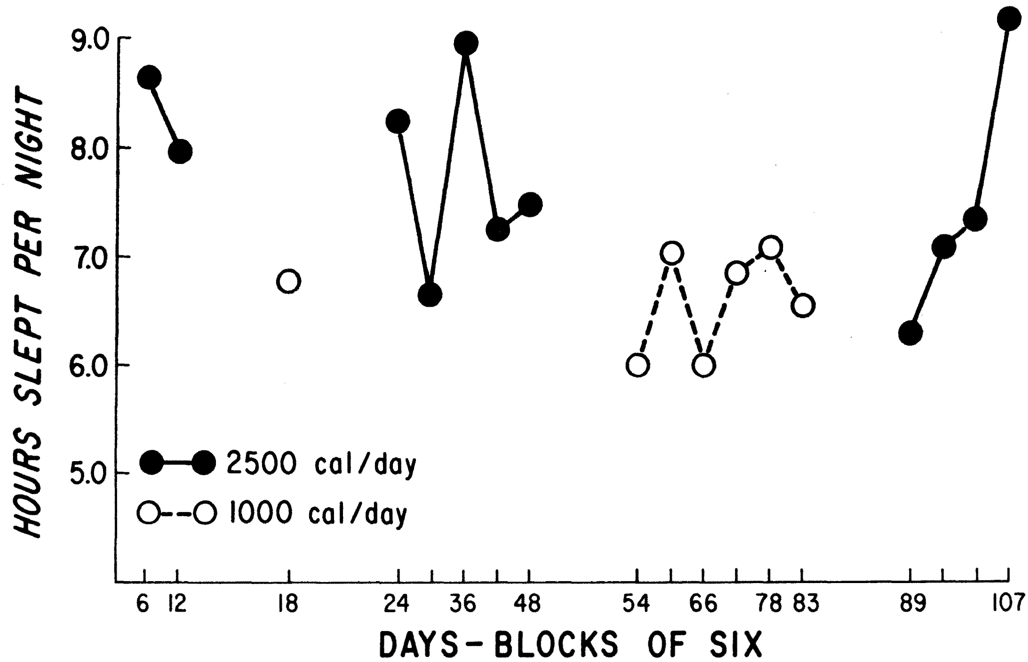

Figure 1. Number of hours slept per night as a function of calories eaten in the preceding day. Closed circles indicate normal caloric intake of about 2,500 calories per day; open circles indicate about one-half the normal caloric intake. Data points are arithmetic averages over 6 days.

in a period of two weeks. These studies, although neither sophisticated nor extensive, illustrate the possibilities of brief self-experiments in nonlaboratory settings and I shall describe a few.

Ann Jenkins began her report, "For many years my mother has been telling me that standing on my head and having the blood rush to the brain creates an awakening sensation." Ann decided to test this "old mother's tale" by comparing her ability to solve "jumble" word problems after standing on her head for 5 minutes with her ability to solve these problems after normal relaxation. She administered a test twice daily, once after head standing, and once after the normal rest. The times of the day were constant, 7:30 and 9:00 p.m., and the order randomized. She did 10 trials in each condition and found that it took an average of 117 seconds to solve each word puzzle after she had rested normally, and an average of 98 seconds after she did a head stand.

A student whom I'll call J. wrote, "My interest springs from the debilitating nature of my constant need to urinate." Her experiment attempted to determine whether the frequency of urinations was correlated with the amount she drank during the day – during the experiment she kept exact records of ounces drank – or correlated with the amount of stress she was undergoing. She (a) estimated four times per day her stress level on a subjective 1-5 scale, (b) recorded events that occurred each day in order to assure herself of the accuracy of her subjective reports, and (c) had a friend attempt to estimate her degree of stress. She found little correlation between her stress levels and the number of urinations. One unexpected result: on days when her stress was high, she drank considerable less fluid, 45 oz., than on days when her stress level was low, 59 oz. The causal direction is not clear, and additional experiments are required, but she has a beginning.

Laura Crosslin compared the number of smiles she received from friends and strangers when she wore contact lenses versus when she wore glasses. She tried to dress similarly in the two conditions and to look equally directly at the other person. She found that she was smiled at more frequently when she wore contacts.

G. compared the effects of smoking marijuana just before going to bed versus no smoking on a short-term memory task the next morning. He hypothesized large effects but found none.

Sally Snyder noted that when she returned from a few days' camping trip, her skin would be clearer than before her vacation. She wondered whether that was due to her not washing during those trips. In a repeated A-B-A design, she compared periods of three days of normal face washing with three days of no washing. Her results were important to her: her skin cleared when she was not washing and again broke out during the washing phase.

Marie Price sought to prove that she was not dependent upon caffeine by having a friend place caffeinated instant coffee in a jar marked "A" and non-caffeinated instant coffee, identical in appearance, in another jar marked "B." Marie drank only coffee from jar "A" for two days, then from jar "B" for two days, and so forth. She writes, "The results were staggering." She found that on decaffeinated days she slept longer at night and was more tired during the day, occasionally falling asleep while studying (something she rarely did prior to the experiment). I can't help quoting from Marie's conclusion: "My unfounded reasons for drinking coffee as a pleasurable sensation might have continued if I had not tried this brief experiment. I now realize the effects of coffee and with that knowledge I am willing . . . to (try to) make changes in my diet that will better my health. And that's what (self-) science is all about, bringing it into one's life on a more personal level for greater self understanding."

I have performed two more extensive series of experiments on myself, one concerning the effects of physical activity on what I loosely call "intellectual" tasks, the other on my ability to behave randomly. I will describe the activity research here and randomness at a later date. As a graduate student at Harvard, I studied late into the night while pacing around the long hallway circumscribing the basement of Memorial Hall where the Psychology Department was housed. Occasionally my perambulations would intersect with George von Bekezy, the Nobel prize winning sensory physiologist, who would likewise be pacing there. On one occasion, von Bekezy stopped me and said something to the effect that, "To have big thoughts, one must move through big spaces." I agreed with gusto. More recently, I took an introductory modern dance class from Judy Massee, an extraordinary teacher at Reed College. Together with about 50 other bodies, all of whom were undergraduates, I learned the means and importance of moving my body. von Bekezy and Massee motivated the four experiments in this series. The basic hypothesis was that I could think and learn better when moving around a space than when sitting quietly at a desk.

In Experiment 1, I attempted to test directly the hypothesis by comparing the number of "novel, good, or interesting" ideas generated while sitting at my desk versus while moving around a room. There were ten comparison trials, each trial consisting of one Sit condition and one Move condition, with the order of the two conditions randomized. During the Sit condition, I sat quietly at my desk; during the Move condition, I paced up and down my room, swung my arms, swayed and performed dance-like movements. Each condition began with me starting a stopwatch; when an "interesting" idea occurred (during some of the trials ideas about any topic would do, but during the majority I selected particular topics to think about, e.g., an experiment I was performing), I stopped the watch and wrote the idea on a sheet of paper. Upon completion of the writing, I restarted the stopwatch and continued this procedure until the allotted time period had elapsed. This time was 15 minutes in 7 trials (7 Sit and 7 Move conditions) and 5 minutes in the remainder. The main dependent variable was the number of ideas generated. The results: in nine of ten trials, I generated more ideas while moving than while sitting. The average number of ideas per minute while moving was 1.05; the average while sitting was 0.72. A Wilcoxon matched-pairs signed-ranks test (Siegel, 1956) showed the difference to be statistically significant at the .01 level, 2-tailed. (Note that statistical tests are used here to indicate something about the present population, i.e., myself, and not other populations, see Edgington, 1967.)

The major problem with the just-described experiment is the subjectivity of the definition of "interesting idea." Several days after completion of all trials, I attempted to magnitude estimate the "novelty, goodness, or interest" of each of the ideas generated. In eight of the ten trials, the average "novelty, etc." was greater in the Move condition than in Sit. But this attempt to evaluate the ideas was itself highly subjective. I therefore next attempted to look at other more objectively defined tasks.

In Experiment 2, I compared my speed of reading in Sit versus Move conditions. During Sit, I again sat at my desk as I normally do when I read; during Move I paced slowly up and down a room, or sometimes moved slowly within a small radius, while holding the book in my hand. In both conditions, I assessed comprehension immediately after the reading (passages were selected from study books with test questions). My intent was to keep comprehension as high as possible while reading as rapidly as possible in the two conditions. There were twenty trials, each trial consisting of one period of reading in the Sit condition and another in the Move condition, with order randomized. (Data from one trial were lost, and analysis is therefore based on 19 trials.) Reading speed was higher in the Move condition in 15 of the 19 trials. The difference was statistically significant, using Wilcoxon, at the .05 level, two-tail. Average speed during Move was 254 words per minute and during Sit was 235 words per minute, a difference of 8 percent. The comprehension scores did not differ significantly, but were slightly higher during Move. Thus, in each of the first two experiments, an "intellectual" task appeared to be improved by movement. The next experiment suggests that these results were not due solely to the experimenter's expectations, hopes or anticipations.

In Experiment 3, with high hopes for further demonstrating the same effect, I attempted to solve Miller Analogy problems in Move and Sit conditions. There were 12 trials, each trial consisting of 25 Miller Analogy problems solved in the Sit condition (again sitting at my desk) and 25 problems solved in the Move condition (again moving around the room, swaying, pacing, etc.), with the order of the two conditions randomized. Counter to my expectations, performance was better in Sit than in Move. In 10 of the 12 trials, a higher percentage of problems was solved while sitting (significant at .02 two-tail, Wilcoxon). Overall, 12 percent more problems were solved in the Sit condition than in Move. At present, I cannot account for the difference in results between this experiment and the previous two. However, quantitative analogies were particularly troublesome during Move and, indeed, I would often stop moving in order to solve these.

The last experiment in the series showed that I learned to identify lists of pictures faster when I moved around than when I sat at my desk. There was one major change in procedure; for the Move condition, in addition to moving during the experimental period itself, as in the above experiments, I exercised – by running 2 miles or swimming 20 lengths of a 75-foot pool – approximately 30 minutes prior to the experiment. In Sit, I again sat at my desk after not having exercised during the few preceding hours. The experiment employed 400 3 inch by 5 inch cards: on one side of each card was the face of a worker from a large corporation; on the other side was the individual's complete name. Cards were randomly divided into 20 sets of 20 cards each. Ten of these sets were randomly designated to be learned in the Move condition, the other ten sets in the Sit condition. One set of these cards was learned each day, with Move and Sit conditions alternating every other day.

The procedure for learning to identify the pictures was as follows. I scrambled the set of 20 cards, picked up a card, and looked at the picture. I then attempted to identify the name of the individual, immediately following which I turned over the card and saw the correct name. I then picked up the next card and repeated the procedure until all twenty cards were reviewed. Upon completion, I recorded the number of correct identifications, rescrambled the cards, and repeated the procedure until all cards were perfectly identified during three consecutive repetitions of the deck. The main datum was the number of repetitions before I learned the cards perfectly. The results showed clearly that I learned more rapidly in the Move condition than in Sit: it took an average of 7.5 repetitions of the deck to learn perfectly in the Move condition and an average of 9.7 repetitions to learn in the Sit condition. This difference was statistically significant at the .05 level (Wilcoxon, two-tailed). Figure 2 shows the two average learning curves.

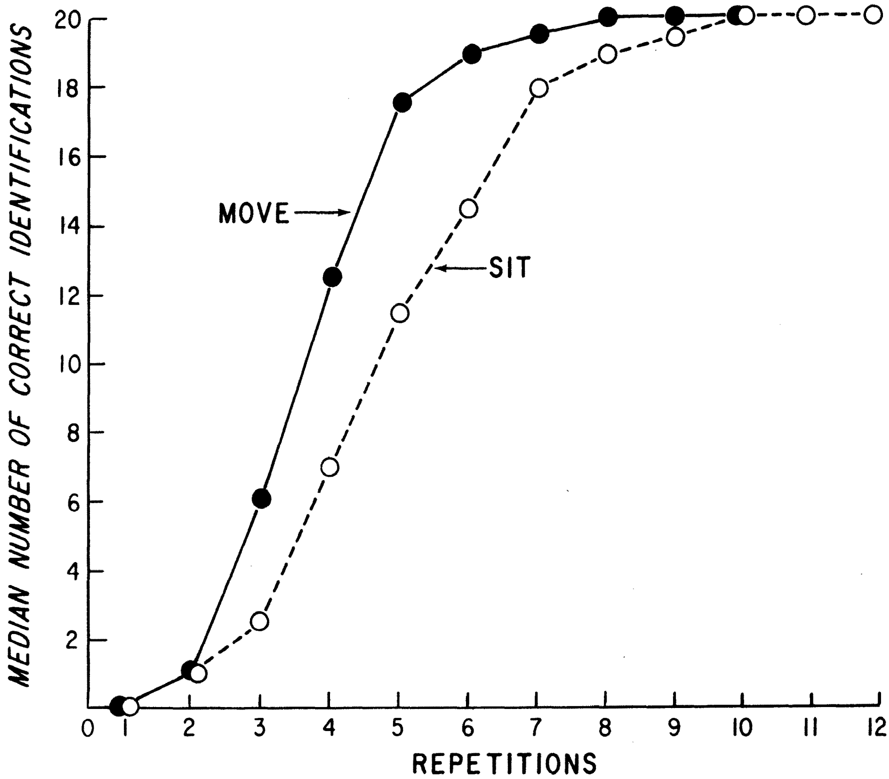

Figure 2. Learning curves for the Move and Sit conditions showing average number of faces correctly identified (in a list of 20 faces) as a function of the number of times the list had previously been reviewed.

This research is ongoing. Others are attempting to see if the Move versus Sit effect is valid for them, and I am continuing the study in different ways. I use exercise now and believe that I am more productive after exercising, especially if it comes late in the day when I normally am tired. However, the literature on the effects of exercise is inconsistent and conflicting (e.g., Gutin, 1973). Future research must explore whether the confusion is due to the rather arbitrary nature of the exercise often studied in experiments on "others," e.g., squeezing a dynamometer or pressing a pedal; to differences between subjects in these "other" experiments, with different subjects requiring different types or amounts of exercise for positive effects; or to the wide variety of tests employed to assess the effects of exercise.

There are major problems with doing research on one's own behaviors, problems which might keep some from making an attempt. I will discuss a few of these. First, in self-experimentation, there is only one subject (although replication by others is possible and often desirable) and how much of a general nature can be learned? The answer is sometimes a lot, as in Skinner's research with single organisms and Ebbinghaus's research on his memory, and sometimes very little. But that is the case with all research: no matter the number of subjects, some experimental contributions yield more general insights and engender wider applications than others. It is the continuous nature of science – the testings, checks, and challenges – which weeds out the useful from the false. That is true in self-research as in any other research.

Second, when doing research on yourself, there is the unavoidable problem of experimenter expectations. I advise my students to read Karl Popper (1972) and attempt to *disprove* their hypotheses, thereby hopefully counteracting hopes and expectations. But, as with all scientists, this is sometimes difficult to effect. Again, the checks and balances of the scientific process will weed out expectation-determined results from environmentally-controlled outcomes. Furthermore, in cases where the expectancy of an outcome is sufficiently powerful to engender that outcome, then expectation becomes an important variable to study in its own right, as done by Rosenthal (1966) and Bandura (1977).

Third, when doing research on oneself, there is much less control over environmental and hereditary variables than when doing research with white rats. However, because science sometimes progresses from simple, carefully controlled preparations to complex, it is not the case that scientific progress *must* proceed in that direction. Yes, Mendel, Sherrington, and Pavlov worked with relatively simple preparations and these had relevance to the much more complex human case. But Copernicus and Darwin worked within extremely complex and uncontrolled subject areas, and they too helped to make scientific progress. We must beware of choosing examples only to support our present practices.

A fourth objection often raised by students is that if one spends time collecting data, performing experiments, and analyzing one's behavioral results, there will be insufficient time for life's pleasures; further, doing science takes away from spontaneity. I tell students that, as with sports, there is a time for inquiry and practice and another time for letting go and playing or living freely. If members of the champion team never practiced – never tried different combinations, experimented and analyzed – they would not have won many games. But if they attempted to experiment and analyze carefully each move during the championship games, again they would not have won.

Finally, a most significant problem is that we are not surrounded by models who demonstrate the possibilities and rewards of engaging in the scientific analysis of our own behaviors. It is difficult to do research: time consuming, tension-producing, often disappointing. The difficulty is exacerbated by our lack of experience in formulating questions about our own behaviors in a rigorous, empirically testable manner. Perhaps we don't do science of self partly because it simply is not yet done by others.

Some of these problems are serious, and I do not mean to make light of them. But scientists before us persevered despite the seeming impossibility of their task. For us wisely to decide whether to exert our energies to overcome these problems, it might be helpful to compare the potentials of self-experimentation with the present state in which behavioral analysts find themselves.

The experimental analysis of animal and human behavior has helped us to understand and assist people. There are the extraordinary advances of the modification of institutionalized patients and of classroom activities, to name two of the most notable ways in which operant conditioning has contributed. But the experimental analysis field is not doing what I, as a student of that field for almost twenty years, hoped of it, and I'd like briefly to mention some criticisms.

First, although the analysis of animal behavior has, in a number of cases, led to advances on the human level, by far the majority of animal research has not been directed at human concerns, and, at least at the present time, has little relevance to the concerns of most people. Increasingly, research reported in the *Journal of the Experimental Analysis of Behavior* and comparable journals is highly technical, which may be a euphemism for ingrown, or incomprehensible to most.

Second, a number of writers, for example Donald Campbell (see Cook and Campbell, 1979), have objected to the highly controlled nature of operant conditioning studies, and to the difficulty of predicting from such controlled environments to the uncontrollable world in which most of us live. Similarly, Barry Schwartz and co-authors (Schwartz, Schuldenfrei and Lacey, 1978), offered that the controlling effects of reinforcement, while powerful, derive from the highly controlled environments in which they are studied. According to Schwartz et al., although behaviors can come under the control of powerful reinforcement contingencies, that does not necessarily mean that behaviors generally *are* under such reinforcement control in natural environments. In short, both Campbell and Schwartz suggest that studying behavior under highly controlled circumstances may not effectively help us to understand unconstrained behavior in natural situations. Many ethologists and comparative psychologists would concur.

The third criticism is a personal one, but perhaps one shared by others. I became an experimental psychologist partly because, in Helen and Scott Nearing's terms, I wanted to live the good life, or at least, the best life possible. But, sigh, I have no evidence that I live a better life than my business friends, my athletic neighbors, or my religious colleagues. Am I wiser than the philosophy professor who does only Gedankin experiments at best? Why do I share the traumas of love, jealousy and anger – and become as incapacitated by them – as those who know nothing about concurrent schedules of reinforcement? Simply put, I cannot demonstrate that my personal life has significantly profited by my association with operant conditioning.

Fourth, the experimental psychologist, as the natural scientist, seeks to acquire and generalize knowledge. Knowledge is power, and thus the more we know, the more powerful we become, or so goes the basic presumption. But when the focus of attention is an *individual's* life, it may be time to re-evaluate the old saw. There is much more known today, i.e., published and housed in library stacks or computer banks, than can possibly be used by any individual. Furthermore, even when an individual has objective knowledge, for example, can describe a set of laws or contingencies, that knowledge does not necessarily lead to action or to changes in behavior (see Skinner, 1969, pp. 146-171). The areas of medicine and self-control are filled with examples of an individual knowing that some behavior is maladaptive, knowing the possible methods of change, but not changing. The question thus becomes: is it sufficient for us as experimental psychologists to continue in our attempts to gain and publish information, or, if our concern is to help our fellow men and women, should we not emphasize the utilization of knowledge. Perhaps the old saying should be changed to: "Knowledge *utilized* is power." The act of doing science inexorably involves utilizing previous work and discoveries. It may be, therefore, that when a person does self-experiments, that person would more likely utilize the fruits of previous and present research than if he or she spent the same time passively listening to expert advice. At any rate, this is a testable hypothesis.

The final criticism is basic. Findings from the animal or human laboratory can serve only as a suggestion, as a hypothesis, for our own behavior. Skinner argued well that behavior must be understood and explained in its own terms and on its own level, and not by attempting to reduce behavior to physiology. Analogously, if my interest is ultimately in my own behavior, I must test *on myself* any hypothesis offered about me by the experimental analysis of animals or other people. In all sciences, any law is a not-yet-confirmed hypothesis, and each attempt to use the law is a possible case of disconfirmation. The possibility of disconfirming "laws" is all the more critical with respect to human behavior in natural environments, mainly because of the extraordinary behavioral diversity of people. No organism learns as much as the human; this learning translates into variability, with different people responding differently to the same contingencies. No self-control or behavior modification technique is effective for all human subjects. I am reminded of the technique used by Professor Skinner to control his study and writing behaviors: he reported sitting at a desk and having a light go on to get his academic work under stimulus control. Only when he was working productively would he sit at that desk; daydreaming, random conversations, and pleasure reading would be done elsewhere. I have often recounted this to students, and a number of them have reported positive results of following Skinner's example. But there are others for whom the technique appears not to work; and, indeed, some report that they work best, study hardest, when they continually vary their study environments. A simple behavioral "law" obtained from animal or human laboratories may be relevant for some people some of the time, but rarely will the law be relevant to all, all of the time (see, e.g., Bem and Allen, 1974).

These criticisms do not imply that the experimental analysis field is unimportant, but rather that self-experimentation may help to strengthen that field: an interaction between self-experimenters and animal experimenters will profitably direct both areas of research towards a common useful goal; in attempting to utilize results obtained in the operant laboratory, self-experimenters will define cases where controlled-environment research is relevant to contemporary real world concerns, and where not; self-experimenters will most probably deal with issues directly relevant to their own lives and, no doubt, will thereby cause "other"-experimenters to study similar issues; and self-experimentation will help to overcome the problems of intersubjective variability. In short, self-experimentation is compatible with the experimental analysis field and, indeed, can be viewed as the next step in the evolution of that field: from the experimental analysis of the behavior of rats to the behavior of psychotic people to the behavior of normal people to one's own, ongoing behavior.

In 1953, in *Science and Human Behavior,* Skinner argued for utilizing science to understand human behavior. But as far as I can tell Skinner rarely travels the path himself. *Science and Human Behavior* has little science *of* human behavior in it: it is replete with what are at best testable hypotheses and at worst fiction. However, the book, as well as Skinner's other writings, points the way to a science of self. To understand, predict and control our own behaviors, we need a functional and experimental analysis of these behaviors and Skinner and his followers best point that way. To study the behavior of a single organism, of me, read *Behavior of Organisms* (Skinner, 1938) and *Schedules of Reinforcement* (Ferster and Skinner, 1957) for some necessary techniques. To do scientific analyses of our behaviors, formulate ideas and hypotheses in empirically testable form, and who better than Skinner (*Verbal Behavior,* 1957) can guide us to avoid the obfuscation of language, especially the pseudoexplanatory mentalist's language.

Indeed, Skinner and his followers have made direct contributions towards a science of self. A chapter in *Science and Human Behavior* on self-control anticipated the great interest in this topic today, as evidenced by the articles, behavior modification techniques, and self-control books, both technical and lay, being published at an increasing rate. The technology of self-control provides an important step towards a science of self. If one looks to the early days of modern science in the 1400s and 1500s, one sees the importance of a technology in some ways analogous to the self-control technology now being developed. The increasing demands made on technology – for better armaments, for better navigational tools – generated the procedures, mores, and concepts of a modern science (see Azrin, 1977, for a contemporary analogy). The demands made upon teachers of self-control are grist for the mill of self-science. Indeed, self-control strategists, such as Mahoney (1974), have described the need for a personal science in which subjects learn techniques to evaluate their own progress, and modify their self-control contingencies accordingly. Thus self-experimentation is offered as a method of self-control.

But the consequences of rigorous self-experimentation go beyond solving common self-control problems and I shall describe some of these consequences. First, to do self-experimentation, a person necessarily increases the *directed variability* in his or her life. There can be neither evolution nor learning without a variable substrate – in one case genetic, in the other behavioral. It is not always appreciated that variability tends to be highly adaptive. In ecology, for example, the stability of a biological community increases as the number of links in the food web increases. That helps explain the many different species of animals and the diversity of most ecological communities. Recent advances in biology show that in most natural populations of plants and animals genetic diversity is maintained because a genetically diverse population is most successful at exploiting its environment. With respect to behavior, too, the shaping process depends critically on variability of behavior (see, for example, Staddon and Simmelhag, 1971). But many aspects of our lives tend to be highly stereotyped. Most of us sleep in the same bed, read in the same way, take the same path from one place to another, eat similar types and amounts of foods day after day, work at the same job, interact in the same ways, and so forth. What indeed are the effects of reading while slowly walking rather than sitting or lying? How might our sleep differ if we slept at different angles, or for different blocks of time? What foods might we be somewhat allergic to and what might be the effect of reducing normal caloric intake by 20 percent? Science of self demands systematic variation – variation which breaks our present behavior niches and thereby suggests solutions to future as well as present problems.

Second, self-experimentation, like other forms of science, depends upon the acquisition and recording of data. Graphs of one's behaviors, descriptions of environmental contingencies, notes to oneself, diaries, autobiographies – all are forms of recordkeeping, of course, and their importance must not be under estimated. While keeping systematic notes concerning my health, I've learned that I am sick more often than I would have liked to believe, that changing from my isolated country home to my city job is often correlated with headaches, that my headaches generally can be overcome by jogging two miles or swimming 20 laps, and so forth. And my notes about interactions with Martha, my primary cohabitor, provided historical context and long-forgotten "facts" when we were working through a difficult period in our relationship. All organisms have what might be called a behavioral history: whatever happens to the organism might influence its behavior at some future date. Thus, if you were slapped at the age of two in the presence of a red balloon, you may thereafter feel vaguely uncomfortable whenever balloons are nearby. But the possibility of maintaining a written record appears to be unique to the human. Imagine that your mother was a compulsive recorder and she wrote in your diary that you were slapped at a party, that toys and balloons were present, etc. Now, at the age of 47, you read about the slap and the balloons and therefore deal more effectively with your problem. Records become significant proximal stimuli and thus help us to modify contemporary actions.

Third, when doing science on our lives, we necessarily begin to focus on the *process* of discovery rather than solely on the products we usually emphasize. The solution to many of our problems is not a pill, not some product, but rather a continuous process of discovery and change. Experimental psychologists attempt to transmit results; we might profit by an emphasis on transmitting process, that is, giving to others the means of acquiring knowledge. If you teach a person a behavioral law, that person has but one law (and, as argued above, that law might not be relevant). But if you teach how to partake of the process of experimental self-discovery, then the person has a lifetime guide for action. (Goethe provides a nonscientific example: for he emphasized, and in his own life exemplified, the importance of continual change and discovery, see Kaufman, 1980.)

A fourth consequence of self-experimentation is that it permits the functional analysis of private or covert phenomena in a way shown highly successful for overt phenomena. I might doubt your toothache but I refuse to doubt my own. Let us indeed reject vague mentalisms from our science but not reject the age-old belief that psychology can study private events. We image, talk to ourselves, daydream and the like, and these private events are much more than epiphenomena or artifacts. Science generally looks first to proximal causes to establish relationships, and the private event, that rumination or thought, is oft times the significant proximal stimulus.

A science of self permits the individual to study functional relationships between such private events and other events, private or public. One then can report the relationship found. For example, I sat each day for a period of time before a vase. By my side were two buttons attached to microswitches, a cumulating timer, counters and a polygraph. A trial would start with me pressing the first microswitch button and gazing at the vase. Whenever a thought or image intruded, I would press the second microswitch. My dependent variable was the rate of intruding thoughts while I attempted to focus on, or meditate upon, the vase. The independent variable was the method used to try to minimize these intrusions. In one case I tried simply to concentrate for as long as possible; in a second case, I interrupted bouts of attempted concentration with frequent rest periods; in the third, I put concentration under stimulus control – I'd say aloud "concentrate" and would begin to gaze, and "stop" and remove my gaze – and I then tried slowly to increase or "shape" concentration times. The results are preliminary, but it appears that the third method was most effective. This is one type of functional relationship concerning a private event which can be publicly transmitted.

Fifth, a science of self has the somewhat paradoxical consequence of causing us to be less self-important while increasing the importance of our lives. In premodern times, each philosopher-scientist wrote as if his system were perfect, containing final understanding of the world. Only later did scientists consider their works as tentative stepping-stones on the gradually evolving path to better knowledge. The notion of scientific progress implies a realization of the fallibility of any concept. If my life become grist for the mill of science, then although clearly marred and inadequate, my life becomes part of the lasting scientific process. There is another side to this paradox: *self*-experimentation increases significant interactions with *others*. For in self-experimentation, as in science generally, progress depends upon the interaction between individual experimenters: to generate background information and hypotheses, to check results, to provide missing pieces for the puzzle (see Zilsel, 1957).

Sixth, for those experimental psychologists who are, in truth, closet philosophers, self-experimentation fosters the formulation of an experimental ethic. Most ethical statements of the kind, "you ought to do such and such," or "it is good or desirable to do such and such," can be reduced to an implicit if-then contingency statement (see Reichenbach, 1957). For example, the Golden Rule can be translated into, "If you treat other people well, they will treat you well." The problem with ethical statements is that, while we often make and respond to them, the if-then contingencies are not specified clearly. Sometimes those contingencies were once functional but are no longer so; sometimes there was never any valid evidence for the contingency; sometimes knowledge of the contingency would be a more helpful guide to action than the vague "oughts." Science of self results in personal if-then contingency statements: if I eat broccoli at night, I wake with an upset stomach the next morning (and that's what I can tell you; not, "you ought to not eat broccoli at night"); if I am nice to you, there is an increased probability that you will smile at me; if I exercise for at least one hour per day I am less likely to catch colds, and so forth.

I can imagine self-experimentation becoming as common and accepted as attending church, going to a ball-game, or seeing a therapist. The average American, rather than watching TV for 6 hours per day, would engage in self-experiments. And groups would form in which self-experimenters assisted one another with data collection, literature searches and analyses. Perhaps groups would form around subject matter, such as self-experimental health groups, self-experimental child-rearing groups, self-experimental runners, self-experimental artists, and self-experimental citizens for social change.

Poets and novelists could turn to a science of self to at last figure out causes of action, and valid explanations of falling madly in love. Novels and poetry books are filled with well-wrought hypotheses, but very few clearly established functional relationships, relationships which the poet-scientist might finally clarify.

I can imagine all members of society, no matter their position, viewing their lives as important, for they contribute to scientific progress. For all members would acquire data and thus contribute, in some small manner, to the evolution of knowledge and to effective change.

And I can imagine that instead of the often depressing "How are you?" people would greet one another with "What experiments are you doing?"

But despite the preceding, I do not know how best to do science of self. How many experiments can be performed concurrently by an individual? Seven plus or minus two? What types of portable data collecting devices are feasible? What are the important dependent variables? How can I keep myself doing my self-experiments?

I therefore request assistance. Help me to turn science on my life, on my everyday behaviors and problems, and on my most intimate concerns. Help me by doing research on that subject most important to us all – yourself.

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