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REMINISCENCES

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The one incontrovertible fact of life is when you are asked to reminisce about the "good old days" you are thereby chronologically enhanced. When I was approached about reminiscing, my initial reaction was righteous indignation. However, after a brief reflection , I realized that if I should confuse facts, screw up dates, omit individuals' names, and generally distort the events of yesteryear, I can always take recourse in "What can you expect from an old man?" So, with this disclaimer in place, I shall begin my recall of the early years of what is now called behavior analysis.

My Introduction To Psychology

My introduction to psychology was through the master's program at The University of Pennsylvania immediately after WW II. I had not taken any psychology courses as an undergraduate (probably because I wasn't sure how to spell it). I am not sure that any of the 946 people in the town where I grew up had ever heard the word "psychology." I know I hadn't by the time I and my 22 classmates were graduated from high school.

At that time, having had no psychology courses as an undergraduate was not as disadvantageous for admission to graduate psychology programs as it is today. I entered the University of Pennsylvania because it was the one program that admitted me. I might add that the psychology department of the University of Pennsylvania was markedly different from the department today. I was having some difficulty with the program as I found the mentalism of structuralism to be incongruent with my general science background. One day I was walking across campus with Dr. Fernberger, a wonderful, charming, witty, and bright gentleman who had been a student of Urban, a leading structuralist of the day. Dr. Fernberger suggested that I might find the psychology at Indiana University a little more to my liking as they had a very bright young man there by the name of Skinner whose approach to psychology might be more palatable to me. I had never heard of Skinner, although I had heard of Indiana University.

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Psychology Department at Indiana University

I applied to IU and was pleasantly surprised when I was admitted to the program. I recall my first day at IU because when I walked up the steps to the Psychology Department office in old Science Hall, I was accosted by a graduate student who was wrestling with a desk he was moving into an office. He asked me if I were a new graduate student and I admitted that I was. He then proceeded to tell me to grab the other end of the desk and help him move it into one of the offices. I was also informed that an annual ritual at Indiana was for the graduate students to move the furniture of faculty members who changed offices. This student also asked me what I thought of gestalt. Having never heard of gestalt I decided to fake it. I told him that I thought he was ok, but I didn't agree with everything that he said. This student laughed uproariously, but did not let me in on the joke. Dr. Kantor subsequently took care of that.

Skinner was the chair of the department. His teaching was limited to one course in each semester. The first semester he taught a course called The Experimental Analysis of Behavior and in the second semester he taught a course called The Control of Behavior. The Experimental Analysis of Behavior course was essentially an animal course and there were frequent demonstrations of various concepts from operant psychology. Skinner had built equipment that enabled the students to observe the animal, a rat or pigeon, as the animal was subjected to various operations. On one occasion Skinner had described in rather elaborate detail how the animal, a pigeon, would behave as a function of a particular operation. When the pigeon was placed in the apparatus, it became apparent that the pigeon was not performing as advertised. Skinner maintained his cool in the face of this seeming setback to his systematic position. He simply commented that when you don't get what you expect to get, the next step is to figure out why. It didn't take too long to figure out why because it turned out that Skinner's assistant, a young graduate student who had no background in operant, had fed the animal shortly before class because he "felt sorry for the animal because it had not been fed for some time." Much to the surprise of most of us, this faux pas not only didn't cost the student his life, it didn't even cost him his continuance in the graduate program.

Since there were no vendors of laboratory pigeons operating at that time, it was necessary to have a supply source for the birds. The sources at IU were the traps that were placed on top of Science Hall to catch any errant pigeon that came along. Paul Fuller, the graduate student on the other end of the desk that I mentioned earlier, had the unenviable task of crawling out on the rather steep roof of the building to retrieve any trapped pigeon and reset the trap. Paul "earned" this job because he had raised pigeons for many years and knew more about pigeons than anyone in the department. The remarkable thing is that he not only survived the frequent visits to the traps but he never caught any diseases from these wild pigeons.

Skinner's Control of Behavior course dealt with a different behavior each spring semester. Thus, one semester it was concerned with the control of verbal behavior. The following year it was concerned with the control of sexual behavior. The control of sexual behavior was unquestionably prompted by the publication of *Sexual Behavior in The Human Male* by Kinsey, Pomeroy, and Martin in January, 1948.

Skinner lectured part of the time, asked questions about the subject matter, or requested comments and questions from the class. In the Control of Verbal Behavior class we became acquainted with the verbal summator, a device invented by Skinner. Some of the more clinically oriented students conceived of it is as a kind of verbal Rorschach Test. Surprisingly, Skinner didn't seem to take offense at that suggested comparison.

Skinner tended to consider his position as chair to be primarily concerned with the relationships between the department and the administration. Matters of concern to the individual graduate students were primarily the purview of a faculty member, Bill Verplanck. A student did not go to Skinner to find out what h/h standing might be with respect to obtaining an assistantship or progress toward the degree. First of all, Skinner was rarely in his office. To find him you would have to go to the animal laboratory. We sometimes suspected that Skinner spent a lot of time in the animal laboratory because it was the only air- conditioned room in the department. (That probably wasn't the reason.) And secondly, Skinner tended to remain somewhat aloof from the students, at least in comparison to many of the other faculty who tended to interact socially as well as professionally with the students. Occasionally Skinner came to the top floor of Science Hall where a lunch room was created each noon. Some of the students and faculty tended to convene there for lunch as meats, cheese, bread, etc. were available for purchase at a very modest price—a tremendous inducement—especially for the students.

Though Skinner was obviously the dominant operant psychologist in the department, there were other faculty members who were more or less interested in operant psychology. Bill Estes, who had obtained his PhD under Skinner's direction at Minnesota, was a member of the faculty. Bill had become interested in statistical models of learning and was relying more on the Guthrian than operant system. However, I always believed, and still do, that the operant and Guthrian systems are much more compatible in many ways than other learning systems.

Kantor taught a course called Systems of Psychology. He handed out material in which he listed various individuals under the system that he believed they represented. For example, Boring was listed under structuralism. Skinner, interestingly enough, was listed under interbehaviorism along with A. P. Weiss and Kantor.

At that time, all the operant laboratory equipment was designed by Skinner. (I suspect that everyone is well aware that Skinner was quite a gadgeteer.) That operant equipment was considerably different from today's operant equipment. Operant chambers were housed in ice chests usually purchased at the local Sears store. Since food-pellet-making equipment was unavailable, it was necessary to make the pellets in the laboratory. The pellets were usually made from ground Dog Chow biscuits. (Laboratory Chow and Rat Chow were still in the future.) Water was a frequently used reinforcer since it was much easier to use and required less preparation time than food pellets for the rats. Consequently, there were many operant chambers that were designed for dispensing water, although food-dispensing operant chambers were probably a bit more common. Some of the operant chambers were automated in that the bar press responses were reported automatically. However, some of the chambers required the experimenter to sit and count each response emitted by the rat. The

recording of pigeon responses was automated as the rate was frequently too high for manual counting.

Since the cumulative recorder (as we came to know it) was not available at that time, Skinner adapted a recorder used in the chemistry or physics lab to record the responses of rats or pigeons. Though these recorders were functional, they were certainly a far cry from the cumulative recorder that Skinner ultimately designed.

A strong, but friendly, rivalry developed between IU and University of Iowa while I was a student at IU. We were considered the hotbed of operant psychology, while Iowa was strongly influenced by Hull (through Kenneth Spence, one of Hull's outstanding students). This competition went so far as counting the number of papers presented at Midwestern Psychological Association (MPA) by the faculty and students of each university. Despite this competitive spirit, Iowa's psychology faculty invited IU's faculty and selected graduate students to participate in a joint discussion of psychology at Iowa; I was fortunate in being included. It was at this meeting that Bill Estes first publicly presented his statistical model of learning.

During our tour of the laboratories, an Iowa graduate student proudly pointed out their Skinner boxes, then complained that he had not been able to condition a rat to press the bar.

We inquired about his procedures so he brought in the rat and placed it in the chamber. The rat immediately assumed a prone position at the food cup. The Iowa graduate student explained that was about all he could get the rat to do. Upon further inquiry, primarily by Ben Wyckoff, one of our students, the Iowa student said that he had provided the animal with a rather large number of pellets in a relatively short time period. Ben suggested removing the rat and putting it in its home cage with water but no food. Ben added that we could return to the lab late in the afternoon and that he would show how to condition the rat to press the bar. Later that afternoon, we returned to the lab, the rat and the operant chamber. Ben had the animal pressing the bar in about 5 minutes.

Origins of Division 25

When Skinner assumed the chair at Indiana, several students from Minnesota also came to Indiana. George Collier, Norman Guttman, and John Haralson had been students at Minnesota and essentially followed Skinner to Indiana. They were certainly among the top PhDs produced by the Psychology Department at Indiana.

One very significant event that occurred in the mid-1940s was the creation of the annual meeting of faculty and students from Columbia and Indiana who were interested in operant psychology. The first meeting occurred at Indiana in 1947, I believe. The meetings alternated between Indiana and Columbia. They were held between the end of the second semester and the beginning of summer school. Visiting attendees usually were housed in a vacant dormitory. Papers were presented, although these presentations were much less formal than presentations at an APA or MPA. Discussions of various and sundry issues were conducted, again in a very informal manner, and research ideas were tossed out for discussion; research in various stages of completion was presented. Graduate students were encouraged to make presentations. The informality of these meetings made it possible for terrified

graduate students (such as I) to make presentations without fear of being ripped apart by someone hostile to the entire position. The Indiana-Columbia meetings were considered to be the forerunner to the establishment of Division 25, The Division of the Experimental Analysis of Behavior, of the American Psychological Association.

I recall that when Division 25 sponsored its first program at an annual meeting of the American Psychological Association Joe Brady made a very nice introductory speech in which he requested the audience to treat the presenters with dignity and respect. Many of these presenters were graduate students. Essentially, Brady was trying to create the same kind of environment that had prevailed at the Indiana-Columbia meetings.

Origins of Behavior Therapy

Skinner's departure to Harvard was a blow to the department, but it had been anticipated by a number of faculty and graduate students. Most people in the department had concluded that Skinner was not exactly enthralled with being a chair. Moreover, it was generally believed that Skinner was hoping to be "called back to Mother Harvard." In those days (and perhaps it is still true today), Harvard required its graduates to go out and make a name for themselves and then, perhaps, Harvard would call them to return as faculty. Although Skinner departed Indiana, there were other faculty members who, though not necessarily thoroughgoing operant conditioners, were sufficiently interested in operant conditioning to maintain the interest in operant that Skinner had initiated.

Paul Fuller's operant conditioning of a "vegetative idiot" in the summer of 1947 had an impact on some of us who were in what was purported to be the clinical psychology program. Paul's achievement, the first demonstration of operant conditioning of a human, provided some of us with the methodology to work with clients in the clinic. Though the clinic staff was not operant in its orientation, the presence of Sid Bijou breathed a behavioristic perspective into the clinic's operation. Bijou was still under the Hull-Spence influence from his Iowa days, but he tended to talk about behavior—what the client was doing.

I recall working with a 9-year-old boy who was a problem in the schools. In those days the IU clinic provided some psychological services to the Bloomington public schools. While I had been indoctrinated in the client-centered treatment advocated by Carl Rogers, I was unable to figure out how to reflect the feelings of a 9-year old boy, especially one who began each session by kicking me on the shins. After several sessions began in this fashion, I realized that I had to do something or I could be limping around for the rest of my life. I discussed the possibility of using operant procedures with Paul and he encouraged me to give it a try. I didn't bother to discuss this possibility with the clinic director as I suspected he might deny me permission. Operating on the premise that it might be easier to get forgiveness than to obtain permission, I proceeded to apply operant procedures to the behavior of this young man.

I found that a chocolate soda in the Student Union had great potential as a reinforcer for the behaviors we were trying to develop. The boy responded very well and progress began to be made. When I made my report to the clinic staff, which

included all of the graduate students who were in practicum, I expected to be awarded my degree on the spot. However, the reaction was considerably different. The clinic director called me into his office and informed me in no uncertain terms that clinic clients were not to be treated as rats and pigeons. That operant procedures seemed to be working vis a vis this boy's disruptive behavior was apparently irrelevant.

My continuance in the clinical program seemed to be in jeopardy until the principal of the school called and indicated that the school was very pleased with the service provided for this young man and wanted to refer a rather large number of additional children to the clinic. Paul Fuller continued to use operant procedures in working with clients and was subjected to similar reactions by the clinic director. Effectiveness seemingly was secondary to avoiding the appearance of treating human beings as rats and pigeons.

I survived this series of events and eventually was awarded the PhD My dissertation problem was concerned with the conditioning of verbal behavior. The genesis of this problem was Skinner's *The Control of Verbal Behavior*, although many people seemed to assume that I was interested in investigating whether the "mmm-hmm" of Rogerian therapy functioned as a reinforcer. I spent a lot of time trying to develop adequate procedures for investigating the problem. I probably spent 2 years trying out different procedures, none of which seemed to provide conditions that were comparable to the continuous response situation of the operant chamber. I finally decided to try asking subjects to say words individually. I seriously doubted that it would be possible to get subjects to emit words for 50 minutes, but I thought it was worth a try.

Although I found that subjects would say words for 50 minutes, I discovered along the way that they also responded in ways that were not conducive to investigating the problem of interest. Some subjects would simply emit sentences slowly so that they were seemingly saying words individually. Some subjects would count. Some subjects would emit large number of proper nouns such as the names of people. the states of the union, or the countries of the world. Eventually I zeroed in on the procedures and began the research.

At that time the tape recorder was a relatively new instrument and the longest playing tape was only 30 minutes. I didn't want to stop in the middle of the session and change tapes, so I had to resort to the wire recorder. The wire recorder provided a 60-minute recording surface. The wire recorder was unquestionably the creation of the devil. Only a frightfully perverse human being who hated other human beings could have created such an instrument. The fidelity was terrible. The wire would frequently become entangled, and it would be necessary to spend hours trying to untangle it.

I recall I was transcribing a recording on an early Sunday morning when the wire became entangled. I worked for several hours to no avail. Finally in exasperation I threw the wire spool against the wall, tearing a hole in the wall. At about the time I hurled the spool against the wall Bill Estes walked into the office. He made some cryptic remarks about graduate students throwing objects at faculty members.

APA Certification

When I was a graduate student at Indiana the American Psychological Association was asked by the NIMH and VA to assume the burden of approving graduate programs in clinical psychology to determine their eligibility for the programs in clinical psychology that were being instituted by both these federal agencies. I recall that O. Hobart Mowrer was one of the members of the team that came to examine the program at IU. The clinical program at IU was a relatively simple program. The clinical student was required to take all the courses required of the experimental students plus whatever clinical courses the clinical faculty deemed appropriate. I remember talking to Mowrer for a considerable period of time, but I don't recall his ever asking me any questions about the program.

When the report came in from APA about the program there was a recommendation that IU hire a psychoanalytically oriented psychologist as there seemed to be no representative of psychoanalysis in the department. I was a teaching fellow at that time and attended the departmental faculty meeting. J. R. Kantor reacted rather strongly to the recommendation and made his position very clear. Tell APA to go to hell. The faculty at IU would decide what was needed. However, the department bowed to the recommendation and hired a psychoanalytically oriented psychologist who surely must have felt like a bastard at a family reunion.

One of the subjects in my verbal conditioning research was a young lady who was regularly a student at Swarthmore College. Kohler, as well as other gestalt psychologists, were at Swarthmore at that time. This young lady indicated to Kohler that she would like to spend a year at a university whose orientation was different from the gestalt orientation there. Kohler suggested that she go to IU where, he apparently assured her, she would be exposed to a psychology that was at the other extreme from Swarthmore because of its strong behavioristic orientation. She said that Kohler was certainly right about that.

Life After Indiana University

Pomona College

My first academic position was at Pomona College where psychology was considered to be the waste basket department. If a student couldn't succeed in any major, he was recommended to try psychology. There was no lab or any other research facilities. We created an animal colony through the kindness of Irv Maltzman at UCLA. My father-in-law-to-be provided invaluable assistance with the construction of an operant conditioning chamber using water as a consequence. The students were quickly turned on by working with the rats in the operant chamber. We began to attract much better students to the program in psychology. The Keller and Schoenfeld book was a marvelous text for the introductory course. The Pomona students loved it and were eager to get into the lab. Some students with mechanical and electrical skills, such as Don Shearn and Conrad Nuthmann, constructed

additional equipment for the lab and the entire basement of the psychology building became a large operant lab.

One of the highlights of these efforts occurred on Science Day. Every year Pomona College invited high school seniors who were interested in science to visit the campus. Every science department was encouraged to develop exhibits that would tend to show off their departments. The students in the advanced learning course decided to go all out and make the psychology department exhibits the hit of the campus. And they did.

One of the major attractions was "Harwood Downs." Each student in the class was provided a rat and the opportunity to train the rat to run an oval "track". The students made little collar blankets with the name of the rat that fitted on each rat's back. On Science Day the rats were all placed at the starting gate and races were run all day. Phony odds were set up before each race. Don Shearn converted an old pin ball machine into an operant machine in which a rat had to push the pin ball up an inclined plane where it dropped through a hole that activated a dipper and provided the rat with water. This particular rat developed shoulders that made Mighty Mouse look like a pipsqueak. The rat race attracted so much attention that the LA Times sent out a reporter and photographer to take pictures of rats literally running a rat race.

Another event that occurred while I was at Pomona that contributed to a surge in the popularity of the "new psychology" was the appearance of the Breland's and their trained animals at the LA County Fair which was held in Pomona, only 7 miles from Pomona College. Many faculty and students attended the fair and observed the fantastic behavior sequences that the Brelands had developed in their animals. Students who had never been in the psychology building dropped by to talk to me about the Breland animal show. Enrollment in introductory psychology increased after the Brelands' appearance at the LA County Fair.

During my first semester at Pomona I taught Abnormal Psychology. Since I taught it strictly from a behavior analytic perspective, it was soon referred to as abnormal Abnormal Psychology. I began with 35 students in the class and at the end of about 4 weeks I had only 15 students. I was puzzled by the rather large drop in enrollment and asked the Registrar what the students were saying when they came in to drop the course. She said, "They say that you are crazy." So much for abnormal psychology.

While at Pomona I prepared a part of my dissertation for publication. At that time the *Journal of Experimental Psychology* (*JEP*) was the premier research journal in psychology. I recall talking to Nat Schoenfeld at an APA meeting which I think was held in Washington, D. C. in 1952. We began talking about the verbal conditioning research and I said that I was writing part of it for publication. He asked me if I planned to send it to the *JEP*. I said that I was. He then said that he was willing to bet me $5 that I could not get it published in the *JEP*. I was really surprised by his comment and asked him why. He was convinced that the *JEP* was anti-operant and would resist publishing any operant research. Despite his comments I sent it to the *JEP* and it was rejected.

Melton was the editor at that time and he sent me a letter containing all of the criticisms of the article. None of the criticisms was valid. For example, he said there

were no control groups. (There were, in fact, *several different* kinds of control groups.) It was his final comment, however, that convinced me that Nat was right. Melton said that he did not believe anyone would be interested in this kind of research anyway.

Ken MacCorquodale was on the Publications Board of APA at the time that Melton rejected my article on verbal conditioning. He raised the issue of the rejection of the article at a meeting of the Board. According to the story I was told, Melton claimed that the article was rejected because it was poorly designed and executed. Anyone familiar with the faculty at IU would find it hard to believe that a committee of that faculty would accept a poorly designed and executed piece of research for a doctoral dissertation.

After the rejection of the article by Melton and the *JEP* I didn't do anything with it. I was prepared to forget about publishing it until Ed Newman, who was the chair at Harvard, dropped by Pomona to talk to me about sending some of our students to Harvard. I had been pretty successful getting students into some of the better graduate programs in psychology, and I was flattered that he would come by to see me about sending students to Harvard. During the discussion he asked me what I was doing with the verbal conditioning research. I told him about my experience with Melton and the *JEP*.

Ed was an associate editor of the *American Journal of Psychology* (*AJP*) and he told me that if I sent it to the *AJP*, he would publish it. I sent him the same article that had been rejected by the *JEP* and it was finally published in the *AJP* in 1955. The irony of the situation is that a former student of mine, Joe Sidowski, conducted an experiment that was a variation of mine and it was published in the *JEP* before mine appeared in the *AJP*.

Florida State University

The next stop on the road was at Florida State University which had begun a PhD program 2 or 3 years before I came. It didn't take long for me to conclude that there seemed to be little or no organization to the program, especially the clinical program. When I mentioned this to the head of the department, he challenged me to produce a better one. It really wasn't that much of a challenge since almost any program would have been an improvement. Barbara Etzel was on the faculty when I joined the faculty. Unfortunately, Barbara jumped ship and went off to Kansas leaving me to wage the battle alone. I am quite sure that Barbara would have been supportive of what we were trying to do.

We finally created a clinical program that consisted of three 2-semester courses. The first course was called The Observation and Measurement of Behavior. This course covered theory of measurement, methods of measurement, and the construction of measurement instruments. The second 2-semester course was called The Experimental Analysis of Abnormal Behavior and was concerned with research on the development and maintenance of abnormal behavior.

The third course was called the Modification of Abnormal Behavior and included an examination of methods of changing behavior with major emphasis on behavior analytic methods. There were no traditional testing courses as we assumed

that the practicum and internship facilities would be able to train students in an environment and with the kind of clientele for which the tests were created. And, having been required to administered a number of psychological tests to the children of friends and neighbors, I didn't believe that I was qualified to administer these tests to individuals who were engaging in problem behavior.

The chief psychologist of the VA was very supportive of the program, although some of the station chief psychologists were not too happy. Some of the chief psychologists in the VA looked upon the training program as a serviceship rather than a traineeship. However, within a year of operation of the revised program the VA psychologists were very happy with the performance of the students.

As the director of the clinical training program I served as a consultant to a number of VA hospitals in the Southeast. I was visiting a VA on a Monday morning when the staff reviewed the 16 patients admitted over the weekend; these patients were being staffed and assigned to various kinds of treatment programs. The psychiatrist who chaired the staffing would announce the name of a newly admitted patient and one of the staff would ask if the patient was from a particular community. If the psychiatrist said he was, then one of the staff would request that patient be assigned to his or her group because he had been in the group before and had done so well. And so it went through the whole list of 16 newly admitted patients. Every one was requested to be assigned to a treatment program to which the patient had been assigned on previous occasions. Of the 16 newly admitted patients 15 had five or more previous admissions to that particular hospital and the other one had only three previous admissions.

When it was over the psychiatrist asked me what I thought. Being totally devoid of tact I remarked that I was appalled at the process. On further inquiry I said that it was apparent that the treatment programs weren't very effective since they all seemed to be returning to the hospital to receive the same kind of treatment. He asked me what I thought they should do. I suggested that they might try some behavior analytic treatment programs. It was quite apparent that I had pushed the wrong button. He told me that those behavior programs treated only the symptoms and didn't deal with the underlying problems. He also made it very clear that any one who suggested such programs had little or nothing to offer to their facility. I was not invited back for one and a half years when a new administrator took over the operation of the hospital.

Rise of Clinical Behavior Therapy

The publication of the Allyon and Michael article was a significant development for those of us who were attempting to introduce a behavior analytic approach into the clinical realm. It provided us with some research on individuals with serious behavior problems and the research demonstrated that behavior analytic techniques were applicable to serious behavior problems. Moreover, these techniques effected some behavior changes. The Allyon and Michael article ushered in a tremendous outpouring of research using behavior analytic techniques with a wide variety of behavior disorders. Like many others, I cited the Allyon and Michael article to support the application of behavior analytic techniques to behavior problems.

I was a consultant to the VA Hospital at Coral Gables, Florida in the mid-1950s and one of the staff psychologist was a bright young man by the name of Malcolm Kushner. On every trip to Coral Gables, Malcolm and I would have lengthy discussions about behavior analysis, which was called behavior modification at that time. Len Krasner is credited with using the term behavior modification. Malcolm was a traditional clinical psychologist and had serious reservations about the applicability of behavior analytic techniques to behavior problems, especially serious behavior problems. Although Malcolm had serious reservations about behavior modification, he was at least open to discussing the possibilities.

Eventually he used behavior analytic techniques on a man who had had a lingerie fetish for some 20 years. This man had undergone 5 years of psychoanalysis which had been very successful in eliminating a considerable amount of his financial resources but had done nothing for his fetish. While I consulted with Malcolm on this particular case, Malcolm deserved all of the credit for the subsequent outcome. The fetish was brought under control so that the man was no longer lifting lingerie items from department stores, off clothes lines, and even, perhaps, from ladies while they were still wearing it. An interesting iatrogenic side effect of this treatment for the fetish was amelioration of the impotence that the man had experienced. His impotence problem was solved—at least to the extent that his girl friend became pregnant.

After this success using behavior analytic techniques, Malcolm became a strong advocate of behavior analysis. When Len Krasner was working with Ullman on the *Casebook in Behavior Modification* he asked me if I had anything to contribute. I didn't, but I suggested that he contact Malcolm Kushner. Consequently, Kushner had at least one, possibly two, case studies reported in that book.

Problems With Behavior Modification

The tremendous surge in the popularity of behavior modification in the late 1950s and early 1960s made some supporters of behavior modification a little uneasy. Fantastic claims were being made for the effectiveness of behavior modification, even to the extent of emptying the mental hospitals. Some behavior modifiers thought that government officials should begin to consider what to do with old mental hospitals that would no longer be needed to treat patients with behavior problems.

Unfortunately the claims far exceeded the ability of behavior modification to produce change. I can recall a graduate student at Arizona State University saying that behavior modification could restore victims of severe strokes to full functioning. I seriously doubted it then and I still doubt it today.

In addition, there were some who questioned the desirability of calling it behavior modification. I recall talking to Bill Verplanck about that very issue. Bill said that he thought it was an unfortunate that these procedures were called behavior modification. He pointed out that there can be all kinds of procedures that can be called behavior modification which have nothing to do with the procedures used by behavior modifiers. Bill was quite prophetic on this issue.

Another factor that may have contributed to the downfall of behavior modification was the reliance on the two term contingency. The key phrase was "get the reinforcer." There were many who seemingly believed that if one could find the effective reinforcer, then success was ensured. Today we know that the situation is much more complex than that suggested by the two term contingency.

The awareness issue which, incidentally, was an after thought in my original verbal conditioning research, suddenly became a significant issue in the early 1960s. Whether the subject was able to state the contingency in effecting verbal conditioning seemed to become important. Numerous experiments were performed. Some showed that stating the contingency was a necessary condition for verbal conditioning. Other experiments showed that verbal conditioning could occur in the absence of being able to state the contingency.

Closer analysis of the entire issue indicates clearly that the entire issue is a non-issue. It is a non-issue because it may be that conditioning must reach a certain point before the subject can state the contingency. Secondly, even if the subject can state the contingency, it does not ensure that the frequency of the target response will rise perceptibly. In some instances, the frequency of the target response may actually decline.

Although the awareness issue was never satisfactorily resolved empirically, it provided ammunition to those who had serious questions and doubts about behavior modification with its rather rigid adherence to the extraorganismic environment as the sole source of controlling variables for the individual's behavior. The concept of awareness is not a behavioral concept. It is a cognitive concept; consequently, it is not too surprising that many of the advocates of awareness as a significant variable in accounting for human behavior were cognitively oriented. While the variables involved in the emergence of cognitive approaches to dealing with human behavior were probably many, by the end of the 1960s there were more and more references to cognitive variables as critical causal variables in human behavior. Behavior modification began a decline that, to a certain extent, continues to the present time. Cognitive approaches to human behavior are certainly dominant today.

For many years hospitals and other institutions were virtually immune from interference from legislatures and the courts. It is somewhat ironic that behavior modification should become the focal point of both legislative and judicial surveillance. But it did. I always thought that a significant factor in this development was that what behavior modifiers did was observable by anyone. This situation was in stark contrast to the talk treatment programs where it was not possible for anyone, perhaps even the treatment specialist, to observe what was happening. Behavior modification, with its emphasis on the manipulation of environmental variables, was wide open for observation by virtually everyone.

Moreover, there were people who were not behavior modifiers in the sense that they were applied operant psychologists began to do things that behavior modifiers could not do even if they had wanted to. Yet these procedures were being called behavior modification and they raised the hackles of the legal profession. Further, there were some behavior modifiers who were using techniques on human beings that were viewed rather disdainfully (at best) by many. These procedures usually involved some form of punishment, frequently using electrical sources such as cattle prods.

Suddenly behavior modification that had been viewed as the salvation of mankind was being charged with abusing people. Also, the widespread usage of deprivation, particularly food deprivation, evoked charges of denying human beings their inalienable rights. Court decisions and legislative actions terminated the use of many procedures that were commonly used by behavior modifiers. Behavior modifiers found themselves as the villain rather than the hero. Bill Verplanck's prophecy had been fulfilled.

Ft. Skinner in The Desert and The Beginnings of Personalized System of Instruction (PSI)

To resume the major theme of this presentation, I departed Florida State University to join the faculty of psychology at "Fort Skinner in the Desert," Arizona State University (ASU). Art Bachrach was the chair of the department and he made a determined effort to create an operant department. The department at ASU included some of the brightest names in operant psychology. Izzy Goldiamond was certainly a major figure in theoretical, experimental, and applied operant psychology. In addition to Izzy, the department included such notable operant psychologists as Jack Michael, Lee Meyerson, Stan Pliskoff, Thom Verhave, Aaron Brownstein, Gil Sherman, and the inestimable Fred Keller.

Fred was developing his PSI. Gil Sherman and he taught introductory psychology within the PSI system. Getting the administration of a state university to support PSI was a real tribute to both Fred and Art Bachrach. A strict application of PSI does not fit into the rather rigid time base as well as other characteristics of a state university. However, they pulled it off. The available evidence would suggest that the program was very successful.

Fred not only enhanced the status of the department with his vast knowledge of operant psychology, but also with his wonderful wit and charm. With Fred as the cornerstone, the department at ASU was certainly the dominant operant department in the country. Sometimes it is very difficult to maintain the high level that the department at ASU attained at that time. If a student was interested in operant psychology in the mid-1960s, ASU was obviously the place to be. During its brief period as the leader of operant psychology, the department turned out a number of students who have become important figures in behavior analysis today.

As members of the department departed for seemingly greener pastures, ASU's status as the leading operant department began to diminish. It is very difficult to determine just what factors were responsible for the downfall of the operant department. However, one should give credit to Art Bachrach for having assembled such an outstanding group of operant psychologists, even if the operant-based program itself was short-lived.

Temple Buell College

I departed ASU for a little known woman's college in Denver, Temple Buell College. By the late 1960s Temple Buell College probably had the most extensive

application of PSI of any college or university in the country. The combination of a private institution and a supportive administration made it possible to extend PSI into virtually every discipline in the college. I believe that our PSI program functioned in a way that Fred Keller would have approved.

A student could enter a course at any time and finish at any time. The traditional semester time frame was essentially abandoned. The traditional grades were abolished because the student had to attain the level of mastery of each unit before going on to the next unit. This condition meant that upon completing the course every student had attained the minimum level of mastery required by the course. The traditional classroom with its traditional lecture was also eliminated.

The PSI program encountered problems to be sure. Many of these problems arose from outside the College itself. One major problem occurred when a student tried to transfer to another college or university or made application to a graduate or professional school. The transcript would simply show a number of Ps, indicating that the student had passed the course. This P was markedly different from the "P for passing" that has become more popular in recent years. Our P meant that the student had attained a high level of mastery. If she didn't, she didn't get a grade in the course. It was necessary to develop a rather extensive cover letter describing the basis of the P grade to accompany each transcript.

Despite this many of our students found that they were not being accepted at graduate or professional schools although they may have demonstrated a level of performance that exceeded that of many students from more traditional institutions. Prior to the development of this problem, most students indicated that they liked the PSI program. However, as this problem persisted it began to have a serious effect on the students who became less and less willing to enroll in PSI courses. Eventually the program like the College itself folded.

We also developed an operant-based secondary teacher education program at the College. All education courses taken by the students in secondary education were combined into two education courses plus student teaching. The instructor for these courses was Bob Rothstein who was an enthusiastic supporter of the operant approach to classroom management. We quickly ran into some difficulty with the Colorado Department of Education that had to approve the teacher education program if the graduates of the program were to be certified to teach.

Otto Ruff, who headed the Certification Program of the Department of Education, expressed some reservations about the program—both in terms of its content and its methodological bias. He reluctantly approved the program but told me that he would be watching the program very closely. Two years later at a meeting of deans of schools and colleges of education that I attended Ruff informed these deans that if they wanted to know how to develop an outstanding teacher education program they should contact the people at Temple Buell College.

Bob Rothstein and I taught a 2-course sequence in the application of operant psychology in the classroom to public school teachers during the summer. The first time we offered the course, we had a relatively small number of participants because our tuition was so much higher than the state colleges and universities. The following spring the Colorado Federation of Teachers put out a blurb that contained comments from teachers who had taken various courses during the previous summer. The

comments about our two courses overwhelmed us. The most frequent comment was that they couldn't understand why they had to take so many education courses when these two courses had provided them with everything they needed to function effectively in the classroom. Our enrollment the second summer was much greater.

We encountered another problem with our operant-based teacher education program. The products of the program were too effective in the classroom and frequently made the supervising teacher look bad. I recall one supervising teacher who refused to sign the certificate that the student teacher had successfully completed the requirements for student teaching. During a lengthy meeting in my office the teacher finally admitted that the student teacher was so much more effective with the children in the classroom than she was and she resented it. She finally agreed to sign the necessary document. The success of the secondary teacher education program was a real tribute to the effectiveness of Bob Rothstein in explaining and demonstrating the important concepts and procedures of behavior analysis. He is still at it, but at a different institution.

University of Texas - Odessa

I recall a meeting on PSI was held at MIT around 1970 and I was asked to present a paper on the program at Temple Buell College. This meeting was the basis for Gil Sherman's edited book on PSI. At these meetings I met a man by the name of Harrisburger who was a dean at a newly formed unit of the University of Texas in Odessa. The University was scheduled to open in Fall, 1973, and it was going to be an innovative university. PSI was going to be the primary method of instruction. The University was to be an upper level and graduate university. Harrisburger indicated that they were looking for people who had a background in PSI and would be interested in this exciting new venture. He certainly made it sound as though it was the place to be if one were interested in PSI.

Subsequently another dean from the University visited with me in Denver. He also made it sound as though it was going to be the most exciting educational institution in the country. That the University was located in Odessa, Texas seemed to be somewhat puzzling. Though I was not too familiar with west Texas I had never heard of Odessa being in the vanguard of anything. When I visited, I must admit that I was impressed with the enthusiasm of the administrators who were the only personnel in place at that time. I accepted the position as chair of psychology.

Disillusionment set in rather quickly. At a meeting designed to train faculty in PSI, I was surprised that the major presenters did not appear to know very much about PSI. Moreover, I discovered that the administrators did not have the foggiest notion about some of the clerical-type problems that would arise and needed to be addressed before classes began. Though some of the faculty made a valiant effort to conduct their classes using some of PSI, if not the entire package, it soon became apparent that PSI had little chance of survival. Today it is as dead as a dodo bird at the University of Texas in Odessa.

Despite my disappointment in the demise of PSI at what was to be an innovative university, I stayed on because I was getting tired of moving.

Reflections

As I look back over almost 50 years in and around behavior analysis, nee operant psychology, I can detect many major changes. Some of the changes are for the better in my opinion and some of them aren't. The close relationship that existed between basic and applied operant in the early years seems to have weakened considerably. I am not sure why this occurred, but it certainly seems as though the basic and applied behavior analysts are moving on divergent paths. Perhaps the basic behavior analysts are not doing research which has any applicability to the problems encountered by the applied behavior analysts.

Maybe the preparation of basic and applied behavior analysts has diverged so that it is increasingly difficult for the applied behavior analysts to discuss their problems with the basic behavior analysts, and vice versa. If these two branches of behavior analysis are on divergent courses I think it behooves the leaders of behavior analysis to address this issue.

In the early years of behavior analysis we did not have to contend with the animal rights activists. This does not mean that animal rights activists are necessarily wrong or right. It simply means that animal rights activists raise issues that were not considered many years ago. I never saw any abuse of animals years ago, but, perhaps my concept of animal abuse is different from that of animal rights activists.

That there seems to have been some abuse of animals by operant researchers may mean that even those operant researchers who don't abuse the animals are suspect or guilty by association. Be that as it may, years ago it was possible to conduct animal research without having a committee review what you plan to do and grant approval or approval. There are certain facets of this change that may protect the behavior analytic animal researcher as well as the animals.

There have been tremendous changes in the conduct of research with human subjects. Years ago students in introductory psychology courses were the mainstay of the human subject research pool. They didn't have much choice in the matter as participating in experiments was frequently a requirement of the course. That is no longer possible. Students can no longer be required to participate in experiments.

The consent form did not exist 45-50 years ago. The student-subject was frequently provided no information about the experiment in which they were to participate much less having the option of being able to consent to participate in it. Moreover, we were not required to debrief subjects as we are now required to do. It doesn't make much difference whether we consider the changes to be good or bad. They are the conditions that prevail and will continue to prevail into the foreseeable future.

The committee that reviews the use of human subjects in research did not exist years ago. Today every university in which human subjects are used in research is required to have such a committee. This is another change that will be with us into the foreseeable future.

The rise of the computer as an important part of much of the research of both animal and human researchers in behavior analysis represents a tremendous change. As one who has resisted the use of the computer in research and has become a

reluctant user of it, I must admit that the computer has added numerous dimensions to the research capabilities, especially to behavior analytic research.

I can recall all too well the many hours spent at a desk calculator that took hours to do what a computer can do today in a matter of seconds. The computer still remains a major mystery to me, but I am willing to concede that it has changed the face of research methodology in behavior analysis.

Forty years ago there was little need for very complex methods of data analysis. The simplistic research problems of yesteryear, characteristic of the early years in most sciences, have been superseded by increasingly sophisticated research problems and research designs. One consequence of this development, a natural development in any science that is making any headway, is the growing need for increasingly sophisticated methods of data analysis. I can recall telling students at Pomona College over 40 years ago that operant psychology would be mathematized within 25 years and recommended that they obtain the mathematics background that would enable them to function in our discipline. I was a little optimistic in my prediction, but I certainly see a growing reliance on mathematical, even statistical, analysis in behavior analysis today.

My almost 50 years in behavior analysis have enabled me to observe behavior analysis, albeit under an alias, rise to great heights and then fall flat on its face. Today behavior analysis is attempting to rise from the ashes of the unbridled optimism of yesterday. I was optimistic about the future of operant psychology. I remain optimistic about the future of behavior analysis. However, I believe that we should proceed cautiously so that we can avoid the pitfalls of yesteryear. Let us not assume that we are farther along in the development of a science of behavior than we are. Despite the rollercoaster-like ups and downs of the past years, it has been a fun-filled ride.