

Reminiscences about Early History of Division 28 of APA

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My memories about the founding of Division 28 are fragmentary. I attended the first business meeting, probably at the 1966 APA meeting, after the Division was established by the APA. Murray Jarvik presided as the first president. I remember that he began the meeting by saying that Division 28 was born with a silver spoon in its mouth. This referred to donations of a hundred dollars or more from each of several pharmaceutical companies, as corporate sponsors.

I do not remember being active in the effort to establish the new APA division. My pertinent affiliations were with Divisions 6 (Physiological and Comparative Psychology) and 3 (Experimental Psychology). I was also a member of Division 10 (Esthetics). Division 6 had a good representation of behavioral pharmacology in its programs. I do not remember experiencing a craving for a new division of Psychopharmacology, but I willingly signed a petition for the establishment of the Division, I believe at an APA meeting.

I was previously acquainted with most or all of the early officers of Division 28. I had met Murray Jarvik at an ASPET dinner at a meeting in Atlantic City about 1958. Marty Adler was with him at the same table. I had known Bernie Weiss and Vic Laties for several years. Other early officers were Larry Stein, whom I met when he visited Yale several years before, and Pete Grossman, whose pre-dissertation research project I had supervised when he was a graduate student and I was a post-doctoral fellow at Yale.

I also knew Harley Hanson, who was an early officer of the Division. I was familiar with the name of Carl Scheckel, who I believe was the initial Secretary of the Division. My personal contact with him was slight. I believe

he was fairly tall, with light brown hair cut short, wore glasses, had a benign but serious facial expression. He died within a few years after Division 28 was founded.

I believe that the first APA meeting I attended was in 1956, and I have attended all but two of the subsequent meetings. Until a few years after my move to Pittsburgh, I also attended EPA meetings regularly, beginning in 1953, my first year in the Ph.D. program in Psychology at Yale. Many psychopharmacologists attended both meetings. I undoubtedly met most of those who were active then, but I have little specific memory about whom I met when.

Some behavioral pharmacologists probably felt hostile toward me because of my association with Neal Miller, who was a leading spokesman for a theoretical type of behaviorism, arguing against Fred Skinner's empirical behaviorism. Miller was the principal advisor for my Ph.D. dissertation, on effects of food deprivation on running speed of rats in a straight alley. My introduction to psychopharmacology was the continuation of my affiliation with Miller. He sponsored me as a post-doctoral research fellow 1957-59, funded by the Psychopharmacology Division of NIMH. I injected rats with drug or isotonic saline prior to putting them in a straight alley or other apparatus. I was subsequently non-principal co-investigator with Miller on a research grant and assistant professor at Yale 1959-61.

I attended what may have been the first official meeting of the Behavioral Pharmacology Society. I believe it was in 1958, at the EPA meeting. I reported on a test with a square runway, showing that amphetamine and caffeine increased the number of circuits by the rats but in a different way. Amphetamine increased the number of circuits in the same direction, thus decreasing number of reversals of direction, while caffeine increased both

circuits and reversals. There were about 15-20 people, most of them working for pharmaceutical companies. Some of them took notes on my report and seemed interested in it.

I met Peter Dews at that meeting. He talked with a strong English accent, and he seemed to be a leader of the group. At the end of the meal, I was asked to leave the room together with another newcomer, Ogden Lindsay, while the group had a brief business meeting. I believe the business included a vote on whether to elect Ogden and me as members. My impression is that he was elected. I know that I was not.

George Heise, Thom Verhave, and Francis Mechner were researchers in pharmaceutical companies whom I met early. There was an occasion when George Heise reported on Librium, which at the time was named methaminodiazepoxide instead of chlordiazepoxide. He reported the taming effect of the drug, especially in monkeys, and said that it did not cause ataxia or motor weakness. He showed a brief film. Sam Irwin said he detected signs of ataxia in the animal.

On another occasion, Francis Mechner gave a tour through his lab at Schering. He had developed concurrent automated control of a large number of operant conditioning boxes. Later, I believe at an APA meeting, Oakley Ray commented to me that he and some other behavioral scientists felt threatened by Mechner's expensive technology. If Mechner succeeded, everybody else would be required to use this high technology, which was computer intensive and a barrier to working with individual animals. If Mechner failed, it would be a setback to the whole profession.

An episode in 1959 or 1960 induced expressions of hostility toward Neal Miller and me by some psychopharmacologists. The background was, I believe in

1958, Neal Miller showed me a review article on psychopharmacology by Joe Brady. Miller was annoyed because it did not cite an article in Quarterly Journal of Studies on Alcohol in 1951 by John Conger. This reported on Conger's Ph.D. dissertation at Yale, directed by Miller. Alcohol decreased the avoidance component of an approach-avoidance conflict in rats, tested in a straight runway at the end of which the rat received a food pellet and sometimes also a painful electric shock. Miller said the article might be a thorough review of operant conditioning research but was incomplete because it missed Conger's article.

Miller seized the opportunity to retaliate in a footnote in a review article by Miller and me in Psychopharmacology published in 1960. A review of operant conditioning techniques by Murray Sidman was published in the same journal in 1959. Miller showed me his draft of a footnote to a citation of that article, stating that it was a useful review but in common with other Skinnerians the author suffered from a conviction that the only worthwhile research was with operant techniques, which conveniently curtails the literature to be surveyed and lowers the threshold for perception of ingroup originality. I agreed with his statement and admired the cogent criticism, though I generally avoid expressions of hostility toward individuals in my publications. I readily agreed with Miller when he said that he felt he should delete the last part of the passage, about the threshold for perception. In retrospect I have wondered if I should have insisted on deletion of the entire footnote, but I did not even consider doing so at the time.

After the review was published, Miller showed me several angry letters from psychopharmacologists. I believe one was from Peter Dews or Joe Brady,

beginning with the statement that he generally did not write to the author of an offensive statement such as this.

I remember having an extensive, friendly conversation with Thom Verhave at a psychology meeting. He argued vigorously that personal criticisms such as in that footnote did not belong in science. He or another colleague commented that our review was one of the best ever done in psychopharmacology but that it was marred by that comment.

Other memories are connected with that incident. When I met Murray Sidman at a meeting, we shook hands with exaggerated duration and vigor, and while doing so we looked at each other and laughed at our unusual behavior. In 1964, I started to work together with Henrik Wallgren, a physiologist in Finland, on a two-volume book that summarized scientific knowledge about actions of alcohol. He told me that he liked the review article with Miller but that he did not like the footnote about convenient curtailment of literature to be surveyed.

I felt glad and surprised that several people had read our review article attentively and taken our statements so seriously. I feel worried, however, whenever I believe that I have offended anybody. Now, many years later, I do not know whether I am overestimating or underestimating the degree of hostility toward Miller and me induced by that published footnote.

In the research grant with Neal Miller, we bought several operant conditioning boxes with programming equipment from Grason-Stadler Company. At one of the APA or EPA meetings, the BRS digibits were introduced. The salesman commented that all the psychopharmacologists seemed to know each other. He offered a free digibit component to anybody who could break one of the boards. He said Joe Brady was the only person who succeeded in doing so.

Neal Miller told me that Joe Brady had been very successful in persuading pharmaceutical companies to hire young psychologists. This may have been at the time Brady gave a colloquium at Yale, about 1959. Frank Logan urged me to attend it, saying Brady was an excellent speaker. Indeed he was, talking about stomach ulcers in the executive monkeys.

I remember an encounter between Neal Miller and Fred Skinner at an APA meeting, about 1959, when I gave a party in my room at the hotel. Besides these two famous psychologists, the others were all graduate students or very young Ph.D.'s like myself. Miller tried to get Skinner into a debate, continuing a discussion they had earlier that day in a session. Skinner did not want to debate then. Somebody suggested we sing college songs. Miller said he cannot sing well. The other people there listened attentively to both of them but said very little.

Skinner attended a paper I gave at APA in 1958 or 1959, on a procedure that involved effect of extremely high intensity of electric shocks delivered to the grid floor. The rats were trained to terminate the painful stimulus by turning a wheel mounted on one wall. I know that he did not like my topic and procedure, but I was pleased that he came to hear my talk.

I was aware that I was excluded from the Behavioral Pharmacology Society. Some members were friendly to me, especially Vic Laties. I do not remember my early meetings with him but I remember, several years later, he said to me that I seem strange without my mustache and asked me why I shaved it off. That surprised me because I wore a mustache for only about 6 months while I was at Yale, I think in 1960. I shaved it off partly because I was disappointed it did not grow into the big, bushy mustache my paternal grandfather had. It was a brief episode for me, and I felt surprised that it

was a prominent aspect of my appearance for a colleague. I understood Vic's reaction better several years later when I did not recognize Bob Balster on the first time I saw him after he shaved off his bushy beard.

I was elected to membership in the BPS about 1967. I had attended a couple of meetings as a guest beforehand, I believe invited by Vic Laties or Bernie Weiss. One of them wrote me the letter of invitation, which contained the statement this was an unusual society because there were no annual dues, but the members were expected to attend the annual meetings. I have attended every meeting since then except one year, when it was in Little Rock, Arkansas.

The establishment of Division 28 shortly before might have contributed to my election to the BPS. I was becoming increasingly acquainted with the active members of that division, many of whom belonged to the BPS. Also, I separated from Neal Miller and Yale in 1961, when I became an Assistant Professor of Psychology at the University of Connecticut. In 1963 I became a Research Associate Professor of Pharmacology at the University of Pittsburgh School of Pharmacy, where I was the only psychologist in a project "Analysis of Psychopharmacologic Methodology" funded by a large grant from NIMH. In 1966 the Journal of Pharmaceutical Science published a review article by me and Joseph P. Buckley on drug effects and the stress syndrome. This appeared to impress some psychopharmacology colleagues. It contained no footnotes criticizing any of them.

The most important influence on my early activity in Division 28 was research on discriminative drug effects. I initially became interested in the topic while with Neal Miller at Yale, prior to 1961. Some of my research there was on state dependent learning, and that was the basis for my awareness

of this possibility. I felt doubtful that it would be feasible to train animals to discriminate their drug states, and my desire might have been to expand the state dependency procedure rather than to train the drug discrimination. Neal Miller expressed no interest in research on this topic, however, so that I did not attempt to undertake that type of research.

At an APA meeting, probably 1958 or 1959, I met Lucy Gardner, a recent graduate of Oberlin College. I believe she was at McGill University, having worked as a research assistant for Girden or Culler, perhaps for Dalbir Bindra. She was interested in the possibility of working as a research assistant for Neal Miller prior to applying to graduate school. I flirted mildly with her, dancing cheek to cheek at the APA dance. I do not remember our conversation, but it probably included the topic of state dependent learning. She probably was acquainted with Don Overton.

Soon after the meeting I wrote a letter to her saying that I liked her and hoped she would come to Yale. She decided to go elsewhere. I speculated that my letter to her might have been improper and a mistake, deterring her from going to Yale.

When I started the job at Pittsburgh in 1963, I felt interested in studying state dependent learning. I met Ina Braden, who was then a Psychology graduate student at Pitt. Another student there was Don Posluns, who had been at McGill and knew Don Overton. I told Ina about my research interests, and at an APA meeting in 1963 or 1964 she said Don Posluns had a friend at McGill who was doing research on drug discrimination. I met that friend, who was Don Overton and had published his initial articles on the topic. This was the first of many discussions of the topic with him over a span of many years. I felt encouraged to proceed with research on this topic.

This became the main focus of my research in psychopharmacology.

One of the psychopharmacologists I met while at Yale was Bob Edwards. He was the research psychologist at Sterling Winthrop Co., and he did a study of analgesic effect of aspirin at our lab, using our device for measuring avoidance or escape of gradually increasing intensity of painful electric shock. A year or two later, he offered me a job as his assistant at Sterling Winthrop, but he told me there was a long-time assistant without a Ph.D. degree who expected to become his chief assistant and would be antagonistic toward the person appointed to that job. I felt flattered by the offer but was not interested. I wanted an academic career, and I felt uneasy about the prospect of him being my boss, especially with another assistant who would be certain to feel antagonistic toward me.

Bob Edwards several years later joined the staff at the Psychopharmacology Service Center of NIMH, which administered the research grant that employed me at the University of Pittsburgh. I had considerable contact with him, and much of it was adverse because the special committee that had established the "analysis of psychopharmacologic methodology" became increasingly disappointed with our progress. Bob Edwards participated with Don Overton in organizing a meeting at the Psychopharmacology Service Center to discuss drug discrimination. I had already begun research on that topic and was well acquainted with Overton. Edwards did not invite me to the conference. I believe I learned about it from Overton. Edwards was willing to let me attend but would not guarantee that my travel expenses would be reimbursed. I went by train instead of plane to minimize expenses. Murray Jarvik was especially cordial to me at the meeting. Bob Edwards was highly attentive to Jarvik, who was helping Edwards to get an academic job in

California. The conference was very worthwhile, and I was glad I attended. I believe that the NIMH did pay my expenses.

Within a few years after Division 28 was founded, I was nominated for offices. Harley Hanson told me that the nominations yielded several of the same names, and he decided which offices the nominees were to be assigned to on the ballot.

I became acquainted with Conan Kornetsky, who enjoyed talking with me at meetings and sent me numerous manuscripts to review for Psychopharmacology when he became editor in 1970. He recruited me to participate in a protest meeting he organized when Kenneth B. Smith in his APA presidential address proposed that political leaders be required to take anti-aggression drugs. I am not ordinarily a political activist but wanted to show support for Conan.

I was surprised when Conan told me he had recommended me as his successor as one of the Managing Editors of Psychopharmacology in 1974. This began a long-continuing and prominent role for me in psychopharmacology.

During a span of many years, Don Overton often organized a discussion group at APA and EPA meetings on drug discrimination and state dependent learning. They were stimulating, enjoyable experiences. My initial major paper on drug discrimination was with Kubena in 1969. Murray Jarvik, the Editor of Psychopharmacologia, said one reviewer had commented the paper contributed nothing beyond Overton's work, he discounted this because he had to wait three months for it and did not really agree with it.

This adverse opinion expressed the prejudice of many of the "Skinnerian" colleagues. I believe the concept of discriminative learning was too theoretical for their empirical biases. Pharmaceutical companies were reluctant to use the technique because it took too long to train animals.

Subsequently, the technique has become popular with Skinnerians and drug companies. I believe the decisive influence was the use of the fixed ratio schedule of reinforcement by Colpaert. This induces close to 100% correct choice. They find attractive a procedure that establishes effective control over the behavior, so that the drug effects can be demonstrated convincingly in a single animal.

I became more active in Division 28 when I was elected Secretary. The year that I began my duties in that position was one of the two years when I did not attend the APA meeting. It conflicted with a conference in Sweden in which I participated. This was a bad start to my position. I feel that subsequently I did the job tolerably well but throughout that time I was very heavily busy with other professional duties, so that was a severe limitation on my performance of that job.

Don Overton, Leonard Cook, and I served Division 28 as a committee to revise the bylaws of Division 28. Cook insisted that only the elected Executive Committee members should have the vote. This was adopted with the decisive support of Joe Brady.

Don and I advocated one instead of two years as the term for president of the division. The two-year term had been adopted a few years earlier. There was strong resistance to our proposal, and I thought it was doomed. But we persisted in advocating it. Then Joe Brady, who was president elect, was asked if he wanted a one or two year term. He said one year.

While I was President of Division 28, succeeding Joe Brady, I was heavily burdened with other professional activities and also the bereavement of the sudden death of Ina Braden on 2 April 1979. I therefore was not able to devote many hours to Division 28 business. This was probably not as much

of a detriment to my performance in that position as it had been in my function as Secretary. I feel that my most useful contribution was to help establish a precedent that the loser of the election as President is usually nominated and elected the next year. I had lost to Joe Brady the year before I was elected. I had also been the losing nominee several years earlier, when George Heise was elected.
