Analysis Verbal Behav (2016) 32:324–335
DOI 10.1007/s40616-016-0054-6

Jack Michael's Appointments at the University of Houston and Arizona State University: Reflections from a Former Student

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Published online: 29 April 2016

Abstract Jack Michael was an early enthusiast for what is now called applied behavior analysis. His many seminal contributions were through early publications in applied behavior analysis and the work of the students he trained (e.g., T. Ayllon, M. M. Wolf). His close mentorship of students earned him acclaim as a teacher along with his many theoretical contributions to the literatures on verbal behavior and motivation, and behavior analysis in general. This paper is a series of personal reflections about Michael's time and contributions at the University of Houston and Arizona State University, which preceded his lengthy tenure at Western Michigan University, where he spent the remainder of his career and is now an emeritus professor.

Keywords Behavior analysis · Jack Michael · Verbal behavior

University of Houston

In the summer of 1957, I took the last of my undergraduate courses in psychology at the University of Houston (U of H); one of them was in intermediate statistics. Although I received an "A" in my first statistics course, it had been one of those "cookbook" courses using a text by Henry Garrett, a staunch defender of southern segregation and who in many articles used statistical arguments against "miscegenation." John L. (Jack) Michael, my instructor for intermediate statistics, was somewhat unconventional in dress for U of H faculty. His views of psychology and his later views about race and similar subjects were also quite unconventional. Jack was always slender and, at the time, his hair was cut as close as a Buddhist monk, which gave him a somewhat ascetic appearance. He also wore sandals on his feet, which certainly distinguished him from

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other faculty members at the university. His sandals were later referred to as "Jesus boots" because he quickly began to attract a following among the students.

His approach to statistics was serious and he tried to make use of probability theory which was a little advanced for many of us following the very basic introductory statistics course. In 1957, Jack was already talking about studies involving the behavior of rats at a choice point in a "Y" or "T" maze, a typical situation for employing probability. A major emphasis in the earlier statistics course had been significance tests and correlation coefficients, which were ubiquitous in psychology and education. Jack was more concerned with confidence limits and at some point, possibly later, referred to significance tests as "bastard" use of the field of interval estimation. As I look back, it was a continuing effort to justify his choice of a Skinnerian approach to science against the current odds. Statistical hypothesis testing he later characterized as "making the best of a bad thing" rather than the quantitative excellence that it was regarded as in psychology at that time.

I graduated with my Bachelor of Science degree in August of 1957, at the same time having acquired a family in the midst of an economic recession, without prospects for much employment. Jack, for whatever reason, remembered that I had an interest in white rats from his class. He invited me to join him and others (usually only one other, sometimes none) to a "journal reading group." We had our meetings at Jack's apartment close to the U of H. The *Journal of Experimental Psychology* (*JEP*) and the *Journal of Comparative and Physiological Psychology* (*JCCP*) were our major diet. In our reading group, we analyzed actual published experiments on animals, mostly rats but also monkeys, which was a departure from the readings to which I was exposed during my undergraduate years. In the following December (1957), I still had not turned up any employment opportunities and Jack asked if I would be interested in enrolling in graduate school in the spring of 1958 and taking an assistantship with him and Lee Meyerson. That year the *Journal of the Experimental Analysis of Behavior* (*JEAB*) published its first volume. This created a lot of excitement because JEP and especially JCCP, both APA journals, were not famous for publishing cumulative records of individual subjects, or studies which did not contain a reference to "statistical significance."

A grant that Lee and Jack had gotten required a lot of equipment with which none of us were eminently familiar. To assist in learning about the equipment, we were allowed to help an ongoing project by Dan Sheer, which was aimed at brain stimulation with Rhesus monkeys. Sometime in the summer of 1958, I was able to make an observation which was submitted to *JEAB* as a note (Mabry 1960), where I described a saga of shaping a response by using the opening of the door to the experimental room.

I took my first learning course in the spring of 1958 with Jack as the instructor. The text was, I believe, *Theories of Learning* (Hilgard 1948). During the course, Jack made report assignments; mine consisted of a number of Murray Sidman's publications which appeared in JCCP and the *Journal of the New York Academy of Sciences*. I think my assignment must have been the most difficult but also was rewarding in some respects. Although our class was about theories of learning (Hull and Tolman chiefly, among others), Jack did not emphasize the importance of the various controversies. At one point, either then or later, he characterized the major sticking points between them as "whether rats were more like people (Tolman) or people were more like rats (Hull)." For those trained in later years, the major empirical contests at the time revolved around "whether

reinforcement was necessary for learning" (the latent-learning experiments) and the "response versus place learning" controversy. Skinner (1950) had written *Are Theories of Learning Necessary*? mainly aimed at the uselessness of some of the issues when looked at from an operational analysis. The issues went back and forth and never came to any resolution and the contentions from both sides eventually died out. Nevertheless, the argument involving latent learning was mentioned in Chomsky's (1959) critique of Skinner's (1957) *Verbal Behavior*, as a criticism of Skinner and the book.

Jack found time in the class on learning to talk about potential uses of the operant conditioning paradigm and Skinner's interest in applications. At that point in time, these would have been speculative; only some clinical studies theoretically modeled on Pavlovian conditioning, such as desensitization and of course, Watson and Rayner's "little Albert" had appeared pre-1958. There were few, or perhaps no, applied operant studies in the literature. I, for one, was especially attentive when Jack referred to contingencies between parent and child as "the squeaking wheel gets the grease syndrome." This was described as a situation where the mother engaged in other activities (e.g., telephone conversations, cleaning) while her toddler wandered from her sight before being attended to in any way. Jack cited the situation as an example of inadvertent shaping, which often resulted in child behaviors such as breaking things, being in contact with dangerous cleaning materials, or wandering into a busy street. The alternate label was simply "the busy mother syndrome." Jack's interest, and of course Skinner's, in practical application of laboratory results was obviously important in the birth of applied behavior analysis. It is probably significant that the first editor of the *Journal of Applied Behavior Analysis* (*JABA*) was a student of Jack's from that period: Montrose (Mont) Wolf. As I will describe later, Mont eventually went from rats and pigeons to a career that emphasized the strong role of social reinforcement for children and in training parents.

For me, this allusion to possible applications in child rearing was rather pivotal. In a later published article (Mabry 1996), I described several small experiments in child rearing (with my daughter) which were mostly successful and revealing. One had to do with keeping our child from running into the busy street or the driveway surrounding each set of apartments. We took her to the park where there was an un-trafficked street and let her play; ignoring her occasional steps toward the curb, while clapping and shouting when she went to the curb and turned away. That it worked was somewhat astounding, as was the fact that it was seen to carry over to the street in front of our apartment building. I was equally amazed by the fact that I had planned it that way. For me, I had learned that there was something in psychology that had practical value which I had not seen in my undergraduate courses. For toilet training, I baited the potty chair's tray and popped "out of nowhere" whenever she could be found close to and looking in the direction of the chair. There was a progression from that to reinforcing her for sitting on the chair, in training pants, followed by training in pushing down the pants and so on. Except for praise, it was without instructions (a silent method that did not require speech or instructions from me). I suppose I was a bit of a fanatic about shaping. When I told my father what I was doing he reacted by saying "we just spanked your fanny." Which, of course, was the reason I was looking for other ways to discipline.

Jack most often gave credit to Skinner for his assertions, but the wording as above of "the squeaking wheel gets the grease syndrome" and the "busy mother syndrome" were all Jack's as I recall. Other students such as Sam Toombs and Patricia Corke were also

busy with projects at the time. Chiefly among them was a grant that Jack received (Michael 1980). It involved a variety of potential subjects ranging from preschool children to children with intellectual or sensory disabilities. The purpose of the grant, which was due in part to our association with Lee Meyerson, was to look for solutions for various handicapping conditions. For my part, I tried to invent (with the help of two engineering students) a light driven by sound (now very common) to convert the sounds of the mother in tending the child and the sounds of the child into a common medium so that sound production could be maintained, an early version of stimulus-stimulus pairing. I remember Jack being interested in a "vocoder" type of application applied to the back of an individual with a hearing impairment. The grant project itself proceeded fairly directly from the era of intensive research in stimulus control involving rate-based assessments of *generalization gradients* principally of visible hue distinctions of wavelength. The era or much of it was identified with the early Guttman-Kalish studies and Hansen's peak-shift phenomena. Mont's thesis and dissertation studies were based on much of that literature. There was much more work in the area, of course, but the interest in rate-based measures largely faded in the early 1970s. An article by Honig and Urcuioli (1981) summarized much of the results (sans, however the combined S^D work of M. Wolf and S. Weiss).

The Blough-Bekesey procedure, introduced by Don Blough (1958), had made much of that interest possible. It was often referred to as an improvement in animal psychophysics because it was such an important application of operant procedures. It had been the model for our grant-supported research on hearing for hard-to-test individuals. Jack (Michael 1980) expressed regret that the technology of apparently useful stimulus-control concepts such as blocking and stimulus shaping were no longer to be researched or applied. The issue of transferring control from one stimulus element to another was encountered in this research with children. We initially trained children to switch levers when a tone was turned off, a light over that lever disappeared, and the one over the other lever was illuminated. We first tried lowering the intensity of each light (fading) but the children continued to follow the light by holding their eye in close proximity to the source. When the light was fully extinguished, the tone alone would not produce the appropriate shift. We then tried a flashing light where the illumination was gradually decreased and that allowed a smooth transition to the tone being absent so that control by the tone alone was evident. Much later after Mont Wolf had graduated from Arizona State University (ASU) and was at the University of Washington, he was involved in a classroom project with intellectually disabled clients at the Rainer School. In teaching them to sign their names, they started by having them trace a signature on a light box. They also faded the light intensity with similar results as above, but then switched to a flashing light with shorter and shorter duty cycles as above, which was successful. I am not aware of any communication of our technique to Mont or others on the Rainer project. It was the signs of the times that in the exhilaration of pursuing useful knowledge that produced some simultaneous inventions.

Jack, at the U of H, was also carrying on a correspondence with a clinical student who was on an internship in Saskatchewan, Canada, Ted Ayllon. I had met Ted in the spring semester, informally, with Jack. Their collaboration became an important, even seminal, study. Ted originally was from Bolivia, wrote his dissertation with Jack, and with Jack authored an article that appeared in *JEAB* (Ayllon and Michael 1959) titled *The Psychiatric Nurse as a Behavioral Engineer*. As Ted recalls, the relationship at the

U of H began over ping pong and a mutual interest in jazz music. He had originally met Jack at the University of Kansas and also knew Lee Meyerson from there. What I personally knew about Ted's relationship with Jack, was Jack's tutoring Ted's dissertation project at the psychiatric hospital in Canada, by mail. The dissertation involved, as most know, altering the reactions of the nursing personnel to their psychiatric patients. The behaviors investigated and treated included excessive "entering the nurses' office," psychotic talk, lying on the floor, hoarding of magazines, and messy eating, some of which were cited in the psychiatrist's evaluation or diagnosis of patients. By today's standards, the plan was simple: establish a baseline of the target behavior, then a treatment, such as ignoring the psychotic talk for "entering the nurses' office." The article included individual graphs depicting baseline and treatment conditions for each participant, which again would be a novel contribution for the time. The article was in many respects a seminal event in the founding of applied analysis of behavior. It was subsequently reprinted in a number of collections as a "first." Moreover, it was published in the same year that Noam Chomsky (1959) criticized Skinner's (1957) entry into human behavior with the publication of *Verbal Behavior*. Ted graduated from the U of H with Jack as his committee chair; Jack was the only nonclinical member of the committee.

In addition to Ted Ayllon, Mont Wolf also entered the graduate program as a student of Jack's. Later at ASU, he and his wife Sandra (Wolf) both acquired degrees with Jack as their advisor. I had first met Mont as an undergraduate in a creative writing club meeting. Roland Tharp was also part of the creative writing scene, and he was to be influenced by Mont when they met again at University of Arizona. Roland later coauthored a book with R. J. Wetzel (Tharp and Wetzel 1969) titled *Behavior Modification in the Natural Environment*. Nate Miron, a clinical student, during this time at the U of H began coming around in order to make some derisive comments to me about behaviorism, emphasizing the *-ism*. Mont and, occasionally, I would often engage Nate in informal debates conducted in the hallway of the psychology building. The topics were usually on the importance of behavior as a dependent variable or the inappropriateness of the current use of averages in psychology. Nate later became converted and wrote a very nice book about how to be an effective parent. Sam Toombs and Pat Corke formed part of our solidarity movement (against the likes of Nate Miron). Sam continued for many years in practice of behavior analysis usually with developmentally disabled persons. Another clinical student who later wrote a book directed at clinical psychology was Larry Simkins. He was another late convert to behavior analysis, credited to Jack's teaching. Another two students who were interested in Jack's accounts of behavior were Lloyd Brooks and Jerry Short. Jerry, I believe, was an industrial-organizational psychology student. Both were later recruited by Jack for a start-up company in programmed instruction in Tempe Arizona (Learning Inc.). Marvin Dailey, Bob Harris, and Maddie Weiss (later Michael) can be added to the list of persons influenced by Jack. Of course, add to the list Lee Meyerson and Nancy Kerr, both of whom gravitated to ASU where Lee helped fund graduate students. Lee's influence was also apparent while initially skeptical of the approach; he was early to see its practical value to disabled persons in keeping with his own interests.

My master's thesis was conducted in a unique manner, at least for that time. To protect graduate students, the policy was that a student must first submit a formal proposal to his committee containing a hypothesis that may or may not be confirmed.

This, of course, was right in line with the current emphasis on "hypothesis testing" involving statistical procedures. However, my thesis was an outcome of attempts to obtain maintained responding from the subjects of the grant project described earlier. With Jack's support, I never had to submit a proposal, then or later, at ASU. Jack was clearly following Skinner's lead, but without Skinner's seniority, to buck the trend in psychology. I always thought it took a certain amount of "chutzpah" to push the issue with the more traditional faculty members.

In or out of class, Jack would recommend *Science and Human Behavior*, Skinner's 1953 effort to operationalize many then-current psychological notions, including Freud's ego-defense mechanisms, with a less mysterious discussion of behaviors and contingencies. In addition, while at U of H, *Verbal Behavior* (1957) had just been published and *JEAB* was the brand new official journal of the society. In 1958, Skinner gave his newly unclassified "Project Pigeon" speech at the summer meeting of APA when receiving a distinguished scientist award along with awards to Paul Meehl and Frank Beach. I was there!

Funds from our grant were used to convert the department's perception room, which held an array of demonstrations of visual illusions: the Ames frame and rod illusion, phi phenomena equipment, and a small duplicate of the well-known distorted room, frequently illustrated in beginning texts in psychology. Behaviorists, and others, had been eager to debunk the notion that the illusions were proof of the brain's inherent organization; instead they had manipulated the cues in a number of studies to show the possibility of learning.

From the perception room, carpenters built a small suite of rooms containing two cubicles: one for the subject and an observation room with an alley behind both to accommodate electromechanical equipment. I suppose that Lee and Jack cooperated in the design. There was an area in front of the suite to greet and check-in subjects. The equipment alley served a double purpose for several of us, including Jack, who were learning the ropes, first hand, of operant conditioning. A homemade rat chamber was alternately used by Jack or I and later by Mont Wolf for his master's thesis which began at the U of H and concluded at ASU. In 1957, *Schedules of Reinforcement* (Ferster and Skinner 1957) had been published and we all were eager to put a rodent or two through their paces. One notable experiment that Jack tried was a tandem schedule consisting of multiple 5-s segments where responses had to be at least 5 s apart (differential reinforcement of low rates or DRL). It resulted in an animal pressing the lever and engaging in several strange behaviors such a throwing itself on its back and or turning in tight circles between lever presses and similar. Longer DRL intervals in the behavioral literature had shown no tendency to produce stereotyped behaviors, but the shortness of the interval with only infrequent eating seemed to do the trick. Another attempt was to use light aversion by suspending a strong lamp above the chamber, but instead of pressing the lever the rat huddled in one corner with its hands covering its eyes.

There was little additional classroom contact for me with Jack, with the exception of a course in advanced statistics where Jack taught the various analysis of variance (ANOVA) designs which were becoming popular as supposedly sophisticated science. We used Walker and Lev as a text for that course. We studied and memorized the various experimental designs first proposed by R.A. Fisher in 1925. Jack was a well-versed but rapid-fire speaker who analyzed

material while lecturing. It was well advised to prepare for sessions by carefully organizing the material beforehand; taking readable notes was difficult. Jack's contribution to the course material came from his study of interpreting results, which otherwise left students in a state of confusion not resolved by the text. Jack's contributions are remembered in several caveats which he mentioned in class. One was the necessity to do an additional test once a significant difference was found between multiples means. The means had to be lined up in order of size and then a line drawn between the two with the greatest distance. The test was named after the statistician John Tukey who wrote at least two books urging the visual examination of data before submitting them to the statistical procedures. Among other quotes, he was credited with "Far better an approximate answer to the right question, which is often vague, than an exact answer to the wrong question, which can always be made precise." Always a confusing issue was how meaning could be assigned to a significant result. The decision theory concerning significance tests was always confusing and was stated in terms of logical type I and type II errors which one could commit; the size or magnitude of effect was not well explained by or illustrated by power curves, usually presented in terms of standard error and without reference to the initial data under discussion. Jack tried to clarify such confusing issues and referenced alternate views such as one proposed by Jerzy Neyman and Egon Pearson, naturally called the Neyman-Pearson hypothesis. At Jack's suggestion I tested out of a required "Experimental Design" course, which I did, I think, with some margin coming from Jack's course.

A recollection from my time with Jack at U of H was his knowledge of opposing views in the philosophy of science. The major contending learning theories each had its own philosopher who argued for a particular position. It was obvious that the Hull-Spence hypothetical-deductive system was largely patterned after the attempt by Russell and Whitehead to capture the totality of human knowledge entirely through symbolic logic in *Principia Mathematica*, a title they borrowed from Isaac Newton's own. Jack's interest stemmed from his earlier studies at University of California at Los Angeles (UCLA) and the obvious contrast with his more recently acquired interest in Skinner's research and his very lean approach to science. The philosophers' view of science according to Jack was often couched in terms of what could be referred to as the "Great Man" theory and to which Jack added "Princes and Kings" theories of science. Like most behaviorists of the time, a more incremental, less episodic view of science seemed appropriate; where the philosophers had ignored the contribution of many hands in favor of pointing to the emergence of an Einstein or Newton. The opposing view, which was clearly that of Skinner, contained in many writings the Machian view that the foundations of modern science were the product of many craftsmen or artisans. Ernst Mach himself asked to be apprenticed to a cabinet maker and was the rarity among physical scientists for building his own equipment (e.g., to photograph the "Mach Effect" of a bullet fired from a gun). The Great Man theory put great faith in man's inherent rationality and "thought experiments" over empirical observation. This last was the basis later in the Chomskyan position against associationists' and behaviorists' views. Jack's position was clear when he recommended a history of technology where the growth of technology was clearly demonstrated over the usual histories of science.

Verbal Behavior and *Schedules of Reinforcement* were both published in 1957. In addition to the previously published *Science and Human Behavior* (1953), they both became prominent in our readings. I personally had some difficulty with *Verbal Behavior* for its absence of discussion of traditional grammatical notions that I had encountered at home and in secondary education. I tried consulting other psychology texts on "language" such as Brown's *Words and Things* (1958) but without much success in resolving the differences. The dictates of formal education had considerable status and grammar was everywhere in education and commerce. Psychology had often given "language" a special status where it was separated as "verbal' versus "motor" behavior in many discussions and was supposed to obey different laws when those were discovered (someday). There were some empirical studies of verbal behavior as such (e.g., Esper 1973; Pronko 1946), but for the most part little that could resolve the issues. It was very early that Jack became interested in Skinner's treatment and had less confusion than I did on what was correct. I did not have much of a chance to discuss the book until later at ASU when Jack offered a seminar on verbal behavior and several of us attended.

In 1960 or before, Jack received an offer from ASU (later nicknamed "Fort Skinner of the Desert") which was just beginning a doctoral program in psychology. It had been a normal school preparing teachers but had only recently been upgraded by the legislature to university status. Two former graduate school colleagues of Jack's at UCLA, Arthur and Caroline Staats were there and had recommended Jack as a faculty member. Both had been trained in a behavioral learning theory tradition that was common at the time. When Jack accepted the offer from ASU, Mont had not completed his master's research and decided to follow Jack and complete his doctoral degree there when it was finally established. I had just completed my masters in May and after some deliberation and completing a language requirement in German (which was later accepted at ASU), that summer I followed suit. This required packing my small family into a tiny French car and driving to Tempe, Arizona.

My daughter Linda was growing and no longer needed as much of my active participation. Either then or earlier I used what we were calling "backward chaining" to teach her to tie her shoes in a rather rapid fashion. As a child, my own shoes were always coming untied so I counted her as a success. Only much later did I read where Murray Sidman (2011) recounted his own (happy) experience with the method of what he called "backward training" in teaching shoe tying. Before leaving for Arizona I concluded my instruction to her in reading, using the Dr. Seuss books. I would read them to her frequently and then read with her as we re-read the books together. She actually had memorized them all and my teaching consisted of slowing her down and causing her to actually respond to the text before proceeding. I credit the regular orthography of the stories for a smooth start; Linda was reading before kindergarten, English spelling has been regarded as a horror at least from the time of Ben Franklin and Noah Webster. The frequency of "dyslexia" of English speakers is the worst on record among major languages.

Arizona State University

Jack was busy during his first years at ASU in planning and equipping a student lab for psychology majors based on the undergraduate lab at Columbia University under

professors Keller and Shoenfeld. Jack soon wrote a manual containing various exercises in shaping, stimulus discrimination, chaining, conditioned reinforcement; all of the tricks of the trade. The beginning course for psychology majors (Psych 112) used James Holland and B. F. Skinner's (1961) *The Analysis of Behavior: A Program for Self-Instruction*, a programmed text which covered much of the same material. The text for the beginning course for non-psychology majors (Psych 100) remained Keller and Shoenfeld's *Principles of Psychology* (1950), which was still a staple behavioral textbook. The lab which accompanied Psych 112 was taught or monitored by graduate students who in turn were monitored and coached by Jack. Carl Cheney made the following note about this course:

> I "taught" several sections of that student lab and cared for the rats as part of my assistantship. Jon Bailey was in one of my sections. We had great commercial rat chambers (I forget who built them) but they were round plastic and used Noyes pellets. I consider the experience that students get in such a lab (handling, weight mgt, shaping, discrimination training, etc.) the most valuable exposure to operant conditioning they could get. All teachers in training (everyone actually) should have such experience so as to come to "understand" the factors involved in learning. I sat in on most of Jacks classes and then ran some labs. I used his lab manual (from Lever Press) when I started teaching a similar class at Eastern Washington. I continued to offer a two or three week rat lab for undergraduates throughout my 45 years of teaching. (personal communication, September 2014).

My first year in Arizona consisted of teaching elementary psychology classes and working for Art Staats, while waiting for the PhD program to begin accepting students. As one of several assistants to Art and Caroline, I had less chance to shape young humans' behavior than I had in Houston. The Staats pursued a more conventional style of research, which was compatible with my statistics background at U of H. At the end of a study with children, I calculated the results of a Treatment by Subjects design (same subjects, repeated measures). There was some distrust that as a dedicated single-subject person I might mishandle the results the group design. Caroline Staats replicated my calculations and, with a questioning look, she confirmed my findings.

Dr. Israel Goldiamond was teaching a course on perception according to a more behavioral perspective. Signal detection was advocated as replacing the threshold theories of traditional psychophysics. At one point Goldiamond invited Dr. Tanner of "Tanner, Swets and Birdsall" fame, to talk with many of us. Most of his informal talk was in engineering parlance and left the assembled students rather dumbfounded. A simplified version was presented in class by Dr. Goldiamond and years later I was able to read my way through to some understanding of an article by the same three authors published in *Science*.

Mont Wolf had taken my place with the Staats and was able to modify their research approach. Dr. Goldiamond had a grant to build a psychophysical booth within a former classroom and I was one of his hired assistants. Most of my time was spent with an illuminometer making the illumination even on the forward wall. Ed Crossman, another student of Jack's, also worked on the project.

Carl Cheney wanted his child to enjoy the benefits of a Skinner-style baby tender. I was enlisted to wire the thermostats which controlled the heating element and an alarm buzzer. Carl was involved, along with Jerry Short and Lloyd Brooks, both of whom had been at U of H, in *Learning Inc.* which produced small programmed instructional materials for Coronet Films, an educational branch of the old Coronet magazine. Jack was a consultant to the enterprise. Pat Mabry, my wife, also worked for the firm editing the programs as they were produced. Jerry, who had an intense interest in programmed instruction along with some considerable skills in writing, was the major director of the effort. Lloyd Brooks was developing a way of assessing the errors, frame by frame, of each of the programs being produced, involving an adaptation of Skinner's cumulative recorder. Subject matter experts were recruited to supply the material. I was told much later that the corporation was disbanded because as a tax write-off for some unnamed investors, it had become too profitable. Ah, progress! Carl later expressed what most of Jack's students would confirm:

> It is hard to mention things about Jack that you don't already know. He was very charitable with me in terms of his stat course and my incompetence. He seemed much more interested in mentoring us, his groupies, than paling around with faculty buddies. The time spent at his house in "seminars" made a big impression on me anyway and I believe on the other students as well. It wasn't so much the content of articles but the passion for reading journals and discussing ideas and defending positions and always being engaged with the science that was impressive. I stayed at his house when I defended my dissertation in 1966. He was important in my passing and I have tried to be as sensitive to students in my 44 years teaching. After that I mainly saw Jack at conferences. We served together with Scott [Wood] on the BFS Foundation board and he was always creative, helpful and a lot of fun. You and Mont also supplied a lot of support, were role models and encouragement for the other students (personal communication, September 2014).

For my own part, being Goldiamond's assistant gave me limited access to yet another relay rack of equipment and allowed me to collect data which became my dissertation. In that year I had three projects, two with rats and one with pigeons. Jack suggested that I select one to finish, as I was hogging all the available equipment. I selected the pigeon experiment on Goldiamond's relay rack and again avoided having to submit a research proposal and hypothesis. Mont had extended his master's research on combined S^Ds and finished his dissertation and defense before I did and went on to gainful employment, before the graduation ceremony.

Since Mont's was the first doctoral defense to be held at ASU, the president of the university at that time attended. One member of his committee had asked what his experiments would do for *belle lettres* (a term that means "fine writing")—this was a critique of the research Mont had conducted. The same member complained about Mont's pronunciation of the term "experiment" taking it to be spearmint, the flavor, by asking "What did chewing gum have to do with his thesis?" In contrast my non-departmental member was an engineer so questions were technical. We (Mont and I)

were officially awarded our doctoral degrees at the same time, May 1963, and were also officially among the first four PhD's awarded by ASU. One other, a female, was a clinical psychology graduate, and the fourth PhD, the first African American, was awarded a PhD in chemistry. At the ceremony, only three of us were represented, Mont having gone to employment and was working on authoring (with others) some significant social reinforcement studies with children (Harris et al. 1964; Harris et al. 1964; Wolf et al. 1964). Ultimately at KU, he was responsible for the Teaching Family Model and of similar activities at Father Flanagan's Boys' Town, both successful and highly documented work with delinquent children. Jack, of the graduation ceremony, said only that he had been afraid that I would miss the steps and fall off of the stage. Fooled him!

Of course, graduating from ASU was not my last contact with Jack; I had become dependent on his knowledge of both the behavioral field and in statistics (which I taught at Illinois Wesleyan from 1965 to 1967). I continued to ask questions by mail or phone about the significance of IRT (inter response time) distributions on the one hand and received much of his later source material from statistics while he was still at ASU. There never had been quite enough time while in school to exhaust his store of knowledge and scholarship. He was still, as I had met him, perusing journal articles. My contacts with Jack and his many students were my chief support when helping to compile and edit the series of "Control" volumes (Ulrich et al. 1966).

Concluding Remarks

If I were asked to comment on what was the major excitement of my graduate years, it was what I was learning from Jack (and Skinner) about parenting, especially those earliest experiences with my daughter as a toddler. There was first: not going into busy streets. Second was toilet training, shoe tying, reading, and "crying spells" especially when returning from the grandparents. Also there was bedwetting, thumb sucking, etc., curing the eternal problem of "crawling into mommy and daddy's bed" at three in the morning and a last, last drink of water. What was most amazing was the fact that no punishment was used and yet it worked. Most of the above was started or accomplished before I had much experience with monkeys, rats, pigeons, and other children. Shaping became a way of life, at least toward my small charge and in the confines of our hearing lab.

In many ways, it was the learning a "bag of tricks" like shaping, fading, stimulus shaping, and what we called "backward chaining;" intermittent and delayed prompting, the use of interspersal techniques and similar. And of course what later came to be called "catch them being good," as an injunction to teach alternative behaviors. I never thought of this as an inferior form of knowledge but rather like Archimedes Screw, or the peg-like teeth in early gears, or the "shot towers" and other early technologies which gave way to the science of mechanics and the physics of motion. If they have been discarded or disused, it may be to the sorrow of the field.

Concluding, I think it fair to say that Jack Michael exerted a powerful influence on the field of behavior analysis and of those of us who were his students. As one former KU graduate said of Skinner, I could also say of Jack—"He taught me to think."

Acknowledgments I appreciate the contributions to my memory from the following: Jon Bailey, Carl Cheney, Ed Crossman, Brian Jacobson, Ed Hanley, Scott Lawrence, Garry Martin, Carl Minke, J. Grayson Osborne, Rick Shull, and Sandra Wolf.

Compliance with Ethical Standards Human or animal participants were not employed for this manuscript, so informed consent was not necessary.

Conflict of Interest The author declares that he has no conflict of interest.

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