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Francis Mechner

The Interview
Dr. Francis Mechner

By Erik Arntzen

Francis Mechner was born in Vienna, Austria, in 1931 and came to US in 1944. Mechner holds a Ph.D. in Experimental Psychology (1957) from the Columbia University Graduate Faculties of Pure Science. His professors at the university were F. S. Keller and William N. Schoenfeld. Mechner was affiliated at the faculty there for several years. He has published a number for articles within the areas of basic experimental research in the field of behavior, behavior technology, learning, education, and theory. He is best known for having developed and introduced a notation system of behavioral contingencies. In late sixties he founded a Paideia School in New York. The pedagogy it this school was founded on personalized instruction. Today he is the directory of the Queens Paideia school.

Erik Arntzen (EA): You were in the Columbia University psychology department in the 1950s. Can you tell us how that influenced your career in behavior analysis?

Francis Mechner (FM): Many of the towering figures of that era had formative influences on me—Professors Fred Keller, Nat Schoenfeld, Ralph Hefferline, Clarence Graham, Ernest Nagel, Conrad Mueller, Lofti Zadeh, and B. F. Skinner. Schoenfeld's practice of questioning widely held beliefs resonated for me with what my childhood painting teacher, Solomon Lerner, had taught me when I was eleven: always to watch out for preconceptions, my own as well as those of others. Not just in painting—in everything. It was at Columbia that these influences led me to understand that the reason it is so difficult to override one's preconceptions is that we perceive mainly what we have learned to perceive. In short, I was turned into a maverick.

EA: What do you mean by "maverick"?

FM: A tendency to do things in unorthodox ways—to ignore how others do it or think about it. I have this dangerous habit of trying to solve every problem my way rather than the traditional way. We know that one hundred years from now much of what we believe today will be considered wrong. I don't want to wait one hundred years.

EA: Did this maverick tendency affect your work in behavior analysis?

FM: Probably. For example, while still at Columbia, I questioned the widely held assumption that "steady state behavior" existed. I believed, instead, that all behavior streams change continuously as a function of previous behavior, due to such factors as learning, automatization, and resurgence, and that a behavior stream appears to be in a steady state only so long as it is not examined too closely. I thought that the concept of steady state behavior was a misleading metaphor, probably inspired by electronics, and that applying it to biological systems deflects research attention from important ways in which behavior streams change.

EA: Did you have in mind any particular way of observing changes in behavior streams?

FM: I wanted to develop a technique that would enable one to examine the internal structure and non-criterial properties of individual occurrences of operant responses, rather than viewing operant occurrences simply as all-or-none digital events. I suggested that a sequence of switch closures that terminate with a specified behavioral event could be considered a single operant occurrence—I called it a "multi-response operant"—and cited my "counting schedule" as an example of such a multi-response operant unit.

EA: What happened to that idea?

FM: About 30 years later, in 1989, Sigrid Glenn encouraged me to write a monograph on it for publication by the Cambridge Center for Behavioral Studies. I called it "the revealed operant." In that monograph, which Dr. Glenn did a superb job of editing, I discussed various potential applications of that concept, including ways to examine my suggestion that reinforcement may work by impacting behavior changes rather than individual occurrences.

EA: Why did you decide to leave academia? You obviously chose a very different career path than most of your colleagues.

FM: I had many ideas for lines of research and projects that were simply too radical for the 1950s. With the folly of youth, I concluded that it would be easier for me to fund my ideas by making the money than by begging for it.

EA: How did you expect to make the money? Did you have a business background?

FM: No, and even worse, I didn't realize how much this mattered. Many people warned me that I would surely fail, and they would have been right had I not been lucky over and over.

EA: Did you find the transition from academia to business difficult?

FM: Somewhat, because I had loved my five years of teaching two sections of my five-point experimental psychology laboratory course in the Columbia psychology department, of working with wonderful scientists during my years of doing basic research on Professor Keller's projects, and my research at Schering Corporation where I had built a computerized psychopharmacology laboratory. But I also found it exhilarating to feel that no one would ever again be able to block me from proceeding with something that I thought could make a significant contribution. I found it freeing. Little did I realize what I was letting myself in for.

EA: How did your colleagues view your decision to leave academia?

FM: Most thought I was crazy, and many thought I was abandoning behavior research, even when I explained that it was really the opposite. I used to argue that the most effective way to ensure the long-term survival of scientific ideas and technologies was to bring them to the service of society, and that the American free enterprise system provided an environment in which this can be done. I wanted to show how corporations can serve as vehicles for perpetuating promising ideas and technologies.

EA: Is that why your first company, Basic Systems, commercialized Skinner's programmed instruction idea?

FM: Yes. I thought that to make programmed instruction viable, it would have to be made independent of teaching machines, Skinner's emphasis on the teaching machine notwithstanding. It was actually Skinner himself who referred my first investor to me (Appleton Century Crofts). I believed that the long-term viability of programmed instruction would depend on the quality of the programs created, which in turn would require starting the development of instructional programs with a specification of the behavioral objectives and a behavioral analysis of the concepts and skills to be learned. Those were the innovations responsible for Basic Systems' great success. After five years, in 1965, we sold it to Xerox Corporation, which resold it in 1971 to Learning International for $117 million! This encouraged me to start and build 10 more companies over the following decades.

EA: How were you able to do that without a business background?

FM: Fools rush in where angels fear to tread, as the saying goes. I made many mistakes, and it was only after the first few companies that I began to know what I was doing. My role was usually to come up with the original concept, recruit and install the management, provide or procure the initial financing, and then continue to provide general guidance. It didn't work every time, but the six or seven time it did were sufficient to fund my behavior research work over a 50-year period.

EA: What did these companies do?

FM: Five of them innovated in some aspect of education or educational technology, but all of them introduced innovative products or services and made, what I believe, were significant technological contributions in their respective fields.

EA: Some of your work has focused on education and more effective ways to organize and operate schools. Can you tell us about that?

FM: In 1968, I thought that if the educational approaches that behavior analysts like Keller and educators like John Dewey had described were combined, the result would be a completely new and far more effective type of K-12th grade school. I founded and operated such a school from 1968 to 1973, and now again since 2008. I called the system "Paideia Personalized Education." The essential feature of this PPE system is personalization—every student proceeds along a customized learning plan made up of learning objectives drawn from a large database. Equal attention is devoted to academic objectives and to non-academic ones like self-management, interpersonal behavior, and learning skills. A small number of 30 students aged 5-17 share a common space while receiving continuous feedback and coaching from several learning managers. The educational effectiveness of this system is quite remarkable. I am planning to demonstrate that when a number of such small schools operate as modular self-contained units that share administrative and other resources, the resulting economies of scale make the system financially viable as a practical approach to large-scale school reform.

EA: You published a paper in the *Journal of the Experimental Analysis of Behavior* in 1959 with the title *A notation system for the description of behavioral procedures.* What is your present view of this work?

FM: It was gratifying to me that Professor Jack Michael and many others adopted it as a tool for teaching behavior analysis, and that it was also used in several textbooks and papers. But I always believed that the system's real significance lay in its potential as a general formal symbolic language for advancing behavior analysis.

EA: Did you ever try to demonstrate that potential?

FM: Starting in 2008, I published several papers that presented a generalized form of the original 1959 language and showed how the generalized version could be used to codify complex multi-party situations and their controlling contingencies in any field that involves human behavior—economics, environmental issues, child-rearing, education, health, law, conflict, religion, sociology, business, public affairs, and so on. I felt that such a formal behavioral contingency language could provide behavior analysts with a way to extend their impact by applying their science to other fields. I tried to present the rationale for the language and the functions it can perform in a 2011 EJOBA article entitled, "Why behavior analysis needs a formal symbolic language for codifying behavioral contingencies."

EA: You are an accomplished pianist. Has this had any influence on your work in behavior analysis?

FM: It led me to write *Learning and Practicing Skilled Performance*. I parsed the cue sources on which any skilled performance depends—kinesthetic, tactile, verbal, visual, auditory, and mental cues—and suggested ways to design the learning and practicing process in a way that makes each of these cue sources redundant and sufficient, thereby making the resulting performance error-free, ultra-stable, and resistant to disruption. This approach has since been extended to many of the performing arts.

EA: You used to have a master rating in chess. Has your chess career had any influence on your work in behavior analysis?

FM: Perhaps. By observing and analyzing chess skills, I was able to identify and introduce some of these into the PPE system's database of learning objectives. On the more theoretical side, blindfold chess chess helped me to understand the relationship between sensory perception (like seeing or hearing) and mentalization (like visualization or internal hearing), and to propose a novel quantitative method for measuring proficiency in any task that consists of making choices at a certain rate (as in chess).

EA: What do you think behavior analysis has to offer in the analysis of concepts?

FM: The bright beacon is still Keller and Schoenfeld's brilliant definition of concepts as "generalization within classes and discrimination between classes." The discipline of equivalence relations, charted by Murray Sidman, followed by Thom Verhave, Lanny Fields, you, and others, may well be the basic science of concept formation and an important foray into the territory of human cognition. My own work in the field of concept formation has been limited largely to applications, like the development of strategies for learning concepts and thinking skills in the context of training and education.

EA: What do you think are the biggest challenges for behavior analysis in the future?

FM: As in any discipline, the overarching challenge is to identify the frontier issues—the problems that present opportunities for making advances. It has often been pointed out (e.g., Ernst Mach, Eric Kandel) that these issues are usually found at the boundaries of disciplines, where the formulations and conceptualizations of one interact with those of the other. For example, the work on equivalence relations is beginning to make contact with neurobiology, cognition research, and education (as in the learning of thinking skills), and I have tried to show how my behavioral contingency language can be applied in various adjacent disciplines, like economics, law, and environmental science. Another great challenge in any field is to demonstrate how its concepts and methods can deliver benefits to society.

FM: But the farthest-reaching challenge is recruitment of the next generation. Max Planck pointed out that new theories and paradigms are rarely adopted because of their superiority or correctness, even when heavily supported by data; they are adopted and survive because a new generation espouses them.

EA: Thank you.