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Robert R. Bush

Later Career

EUGENE GALANTER

Columbia University, New York, New York 10027

AND

R. DUNCAN LUCE

University of California, Irvine, California 92664

NEW YORK 1956–1958

Bush accepted, in 1956, an associate professorship in applied mathematics in Columbia University's New York School of Social Work. His main teaching responsibility was statistics, but he spent much of his time exploring the problems of measurement in social science research. Interestingly, he returned to psychological measurement, to solve an applied problem, after returning to Columbia in 1968.

A key development in this period for all of us was our collaboration. He had met Galanter, who was then at the University of Pennsylvania, in the summer of 1953, and they worked on some experimental problems when Galanter spent academic 1955–56 at the Psychoacoustics Laboratory, Harvard. Bush and Luce had known each other slightly since 1951, but it was not until this first association with Columbia, where Luce was at the time, that they became friends. Our meetings began as pairs, but soon evolved into a three-way collaboration. The general topic was choice behavior —learning, psychophysics, and preference. On the mathematical side we focused on the derivable properties of stochastic models of choice, and on the closely related issue of parameter estimation. This interest was reflected in Bush's chapter "Estimation and Evaluation" in the *Handbook of Mathematical Psychology*. The conceptual concerns focused on the constraints imposed by the organism on its responses, on the invariance of estimated parameters from experiment to experiment, and on the interpretation of parameters as theoretical measures of subjective states. We also worked both on the design of experiments to test among classes of models which embodied different conceptual interpretations and on the thorny issue of how to study individuals in situations where their choice probabilities are changing.

These sessions occurred mostly on weekends, in surroundings conducive to Bush's intellectual well-being: an informal, smoky atmosphere accompanied by plenty of strong drink and good food.

During the middle of that year Luce accepted a position at Harvard University, which spread the trio out along the northeast coast. To continue our meetings as well as to run some of the experiments we were designing we needed financial support. A small grant from the American Philosophical Society made it possible for us to meet every third or fourth weekend during the next two years.

At least one of these meetings had interesting consequences. A train trip to Boston on Thanksgiving weekend 1957 afforded Bush and Galanter the leisure to discuss the problem that Pennsylvania's Department of Psychology was having in selecting a new chairman. It was then beginning to shift from its postwar concentration on training clinical psychologists towards a commitment to a strong experimental program in several fields of psychology, including psychopathology. By the time Bush and Galanter arrived at Luce's Cambridge apartment, they had hatched the idea of proposing Bush's name as chairman. Among its advantages would be the establishment of an Eastern haven where mathematical psychology could be fostered. The idea appealed to us, but the political realities were formidable. Just how realistic was it for an assistant professor to propose as chairman of one of the oldest departments of psychology in the United States a recently converted physicist who was then an applied mathematician in a School of Social Work, especially when one of his first proposed appointments would be an exmathematician, then a Lecturer on Social Relations? True, there was already considerably evidence from summer conferences that Bush possessed unusual administrative skill, but that was only known to a small group of specialists. Fortunately, he had spent some time at the Department in 1955, where he had impressed several senior members. Nevertheless, could any major unversity really be convinced that he was a suitable chairman of psychology?

PHILADELPHIA 1958–1968

Chairmanship

On July 1, 1958 Bush became Chairman of psychology at Pennsylvania. Great credit for this radical decision must be given not only to all of the members of the psychology department who pressed hard for the appointment, in particular Francis W. Irwin, Acting Chairman at the time, who saw the appointment as an intellectually valid choice, but also to key people in the central administration. Jonathan Rhoads, then Provost, the late Roy Nichols, then the Dean of the Graduate School, and Gaylord Harnewell, then President, all considered the idea meritorious. David R. Goddard, Chairman of Biology and later Provost of the University, and Eliot Stellar, of the

Institute of Neurological Sciences, now Provost, strongly supported the Department's proposal. The administration of the University had the wit to recognize that Bush was a wise though far-from-obvious choice, and they supported him strongly in his immediate attempts to attract outstanding faculty, to build new educational programs, and ultimately, to construct a new laboratory building for psychology.

The quality of Bush's chairmanship at Pennsylvania is attested to by most who were there. He was bold and incisive. He set high standards. He was student-oriented before it was fashionable, and yet he never allowed his regard for students to become a reason to demean the value of research. Above all, his own justified self-confidence in his ability as a scientist made him firm and effective in dealing with administrators. As Richard Solomon has written in a letter to F. Mosteller, "...he was undoubtedly the best chairman I have known, conversant with the work of his colleagues, paternal in encouragement and in practical assistance, forceful in getting support for good work."

It is difficult now to recover much of the detail of that five-year period. We recall it as one of excitement, promise, and tension; of sometimes delicate political balances in which one or two votes could be crucial; of successes in attracting senior people to the faculty (Philip Teitelbaum, Richard Solomon, Jacob Nachmias, Leo and Dorothea Hurvich, David Green, and Henry Gleitman, in that order), and of several failures to attract exciting psychologists, which failures were all the more frustrating because success seemed so close; of emergency evening meetings at Bush's house to plan the next day's strategy; and of plans for the nurturance of mathematical psychology.

Bush undertook a broader range of administrative duties than do many chairmen. In part, he was able to amplify his effectiveness by attracting to the department a most competent business administrator, Ada Katz. She served in this capacity first at the University of Pennsylvania and then later at Columbia when Bush went there. Throughout their collaboration, they remained good and loyal friends.

The excitement in the growing department was felt among all of its members, down to freshmen in the introductory course. The assistant professors under Bush's leadership worked 80-hour weeks to modernize and upgrade the instructional offerings in the department while maintaining a high level of reasearch output. The sense of excitement in Philadelphia made its way around psychological circles and, shortly, graduate students of the highest caliber began to arrive to take advantage of the renewed and reoriented department which Bush was forging. Regardless of one's theories about sources of intellectual growth, the Ph.D.'s who came from Bush's department comprise a sizeable fraction of today's list of distinguished psychologists at the associate and beginning full professor level.

Research

During the first year of Bush's chairmanship, we three continued to meet in Philadelphia, New York, and Cambridge. That academic year ended with two months

of close collaboration when we rented a house in Pigeon Cove, near Rockport, Massachusetts. We were joined for part of the time by Frederick Mosteller. There work was completed on four papers that appeared in Bush and Estes' *Studies in Mathematical Learning Theory* (Bush, 1959; Bush, Galanter & Luce, 1959; Bush & Mosteller, 1959; Galanter & Bush, 1959). That fall Luce joined the Pennsylvania faculty and further travel became unnecessary.

The daily tide of meetings, committees, and paper work rapidly altered the nature of Bush's intellectual activity. The long sessions of writing equations in his characteristic firm, round handwriting, or of rapidly operating an old-fashioned hand-crank calculator he kept at home (ultimately replaced by more modern ones) became less frequent during the academic year, although they continued for a number of summers. His intellectual life became increasingly centered on graduate students and on the creation of mathematical psychology as a distinct discipline.

Still, throughout his chairmanship, he supervised at least one Ph.D. student, and occasional papers resulted. He frequently elected not to attach his name to the work of his students although the mark of his approach was evident. His interests over this period centered on four main topics. First, he continued his investigations into properties of both the linear and nonlinear (but commutative) operator models. While still at Harvard, he caught the interest of a beginning graduate student, Saul Sternberg. This led not only to a joint paper in 1959, but to Sternberg's appointment at Pennsylvania. There he prepared, under Bush's critical eye, his widely acclaimed chapter "Stochastic Learning Theory" for the *Handbook of Mathematical Psychology*. Bush supervised the thesis of Laveen Kanal, a student of electrical engineering, on commutative operator models. Later, Eric Holman worked with him on properties of broad classes of operators. Bush also influenced his young colleague M. Frank Norman to consider this problem, which Norman has treated in great generality in his book *Markov Processes and Learning Models* (1972). Second, our interest in psychophysics stimulated Bush and Richard Rose to investigate the application of learning models to the decision criterion in detection and recognition of signals. They attempted to exploit sequential effects in the asymptotic process to estimate parameters for individual subjects. This work was frustrated by the small magnitude of these effects in the data, and Bush ultimately lost interest in it. In recent years, Donald Dorfman has been pursuing these ideas again with considerably greater success. Third, Bush worked on models for discrimination learning, attempting to formulate suitable linear operator models. This work is beautifully summarized in his chapter "Identification Learning" in Vol. III of the *Handbook of Mathematical Psychology*. Although Bush always thought that it was his interest in this problem that made him a psychologist, this chapter is the least known or referenced of his work. These ideas were pursued further by his student Elijah Lovejoy, resulting in his monograph *Attention in Discrimination Learning* (1968). Finally, Bush worked, again with Lovejoy, on the statistical question of the distribution of the last trial when a trials-to-criterion

procedure is used. They were able to show for a class of learning models that this distribution corresponds to a very wide range of response probabilities and that it is not, therefore, a very suitable measure of the degree of learning. This is of great importance, for example, for reversal learning studies. Unfortunately, the work was never published.

There is no question that Bush's interest in learning theory diminished rapidly toward the middle 1960s. As he never talked much to us about this change in attitude, we can only speculate about its bases. Several factors within the field seem important. In the early 1960s the Markov chain models being developed by Richard C. Atkinson, Gordon Bower, William K. Estes, Patrick Suppes, and their students at Stanford were having some notable successes not really matched by either the linear or nonlinear operator models. It is ironic that Bush initiated some of the Markov models, but their main development occurred at Stanford. Later, even these Markov models began to fall into disfavor, and the thrust of work shifted from probability learning and T-maze experiments to human memory and information processing. It is difficult to detail the exact reasons for this shift. Perhaps it was part of Zeitgeist of that period: the S–R approach to learning was being increasingly rejected by younger psychologists.

In Bush's case, however, we suspect that more specific influences were at work. There were two problems that he came to view as highly recalcitrant to his approach—the need for a natural explanation of the resistance to extinction after partial reinforcement, and the lack of parameter invariance under changes in experimental conditions. Stochastic models for learning never achieved a clean separation between the theory of learning for the organism and the boundary conditions imposed by the experimental situation. They were literally models for specific experiments, in which the parameters evidently combined aspects both of the organism and of the design, and so could not possibly exhibit any significant invariance over designs. The recognition of the difficulties of his approach to learning problems was, we suspect, a major emotional fact in his intellectual life at this time.

Judging by the subsequent events, we believe that he did not fully recognize how much his attitudes had changed when, in 1964, he gave up the Chairmanship and accepted a Guggenheim fellowship at Stanford to resume full-time research. He returned to Philadelphia from this year discouraged with his research in mathematical psychology and never pursued it again with any enthusiasm.

Development of Mathematical Psychology

Although mathematical models have existed in psychophysics since 1860 and in learning during much of this century, by the end of World War II the primary uses of mathematical ideas in psychology appeared in the statistical analyses of experiments and in the closely related area of psychometrics. The Psychometric Society and its journal, *Psychometrika*, were the center of activity. After the war, especially in the

Cambridge and Stanford areas, there began to coalesce a group of people, of mixed mathematical and psychological background, who differed sharply in approach from the psychometric group. Many were trained in fields other than psychology and had a correspondingly different idea about theories and models. They developed close ties to experimental psychology and, of course, they knew more and different mathematics than did most psychologists. The existence of this group was marked by various events in the early 1950s, including the publication of Bush and Mosteller's *Stochastic Models for Learning*. This book signalled the beginning of a new area: mathematical psychology.

Bush had a great deal to do with the founding and initial fostering of the new area. As Chairman at Pennsylvania, he quickly added a separate program in mathematical psychology for which graduate students were found, often with undergraduate training outside psychology. A training grant was secured from NIH, and young faculty were recruited. At the time, the only comparable programs were at Indiana, Michigan, North Carolina, Princeton, and Stanford. Because mathematical learning was not then well represented at the middle three and Estes had moved from Indiana to Stanford about that time, we always viewed Stanford as our intellectual opposition —one we happily joined during the summers for both the intellectual and physical climate. (Bush could not tolerate moderate, let alone warm, temperatures—one professor always brought an electric heater to meetings in his office). Bush was an active teacher in these summer programs, where he was known for clarity in expounding difficult ideas. This perspicuity extended to his interactions with nonmathematical colleagues, witness another of Richard Solomon's comments: "Bob Bush was one of my great favorites in psychology. Talking to him always made me more intelligent, at least so it seemed to me. It was always easy for me to explain something to him, and then he'd come back with the right, devastating question."

The three of us soon decided that the field of mathematical psychology needed to summarize its early accomplishments and to provide printed materials so that the methods and results could be taught in almost any department of psychology. At first, we thought in terms of an undergraduate text but concluded—probably incorrectly, judging by the success of the one by Coombs, Dawes, and Tversky (1970)—that it would be exceedingly difficult to prepare one with chapters on different topics written by different people. And we were not prepared to stop our other activities to write a complete text from scratch. Consequently, it seemed better to try to organize a more advanced survey-exposition of topics. Much time was consumed in laying out the plan for what ultimately became the *Handbook of Mathematical Psychology*.

Eventually, an outline with possible authors resulted. We found support both from the field itself and from Gordon Ierardi, that great and beloved editor of John Wiley and Sons, who was prepared to commit that publisher to a large and expensive enterprise of, at best, marginal financial prospects. Initially, we contemplated one volume, not three. And it was never really intended to be a "Handbuch" as found in the

physical sciences or even closely similar to Stevens' *Handbook of Experimental Psychology*. It was intended more as a substitute for a text. Our discomfort with the title was overcome by Ierardi's insistence on its sales value. That project—with its delays and late withdrawals, with manuscripts as much as three times planned length, and with the rewriting and editing—was rather more time consuming than any of us had anticipated.

As Mosteller (1974) has described, Bush also played a key role in organizing and running a number of the summer conferences and workshops on mathematical psychology supported by SSRC. He participated in the planning that led to the Mathematical Social Science Board. This board took over, and to this day maintains, the responsibility to promote conferences and workshops on mathematical approaches to behavioral and social science problems. These activities have had an effect out of all proportion to their cost.

Some of the research resulting from one summer workshop appeared in the volume edited by Bush and Estes, *Studies in Mathematical Learning Theory*. The appearance of this volume, of *Mathematical Methods in the Social Sciences*, 1959, edited by K. J. Arrow, S. Karlin, and P. Suppes, and R. C. Atkinson's *Studies in Mathematical Psychology* in 1964 reflected a problem that had become acute. There was no natural outlet for our products. Some of it could appear in the *Psychological Review*, *Psychometrika*, or the *Journal of Experimental Psychology*, but often there were problems. Much of the work was of a character alien to *Psychometrika*, whose subscribers did not include many of the people we wished to reach; much of it was too specialized or technical for the *Psychological Review*; and the editors of the *Journal of Experimental Psychology* refused to publish compact presentations of raw data, which we felt would be of use to other model builders, even though they would happily consume as much or more space in figures that partially destroyed those data. It became clear that we needed a new journal, and so the *Journal of Mathematical Psychology* was founded. Bush was very active in its birth in 1964, and the remained on the Board of Editors through 1970.

New York 1968–1972

A question often raised, one that many of us put to Bush himself, was why he did not move up the administrative hierarchy? He was so clearly a superb administrator and had reached a point in his career when research had lost its driving attraction that a position as dean, provost, or college president seemed an obvious direction for him. He pooh-poohed the idea whenever it came up, and we doubt if he ever let the process reach the point of a serious offer. He detested the demands that the ceremonial functions of these jobs place on one's social life. He was always jealous of the time he allocated for his personal life, and he devoted as much intensity to that aspect of his being as he did to his intellectual pursuits. But he found that the role of department

chairman did not make serious demands on the part of his time that he chose to keep for himself, and so in the winter of 1968, he accepted a second chairmanship, at Columbia University.

It is probably accurate to say that a central reason for his willingness to resume that role was his powerful desire to return to New York and the social opportunities that it offered. He loved Manhattan, mostly because it is the major center for ballet in the Western world. Ballet was his one artistic passion. He gave unstintingly of his time and his financial resources in its support; for example, he was the director of fund raising for the American Ballet Theater for several years. His colleagues recognized this enduring interest when, upon his retirement as Chairman at Pennsylvania, they gave him a painting of Nijinsky by Moura Chabor.

Bush's negotiations with the Columbia administration were completed by March, 1968. Although it is now widely recognized in retrospect that the enormous growth in institutional and federal support for science was ending at that time, the Columbia administration entered into commitments for new space and academic positions for the Department of Psychology which rested on their ability to raise outside funds. In April of 1968, the activities of student radicals following on the protracted student unrest in the University shook the foundations of academic life in a most spectacular way. The reordering of priorities, which may well be a valuable outcome of these events, resulted in shifts in the planned allocations of the central administration. These actions placed Bush in a most uncomfortable conflict.

He had always been most sympathetic to student needs and concerns, and with great patience and insight he would spend long hours with students urging the possibility of rational solutions to the problems that concerned them and him as well. The result was that he did not press the administration to maintain their timetable, as he might otherwise have done. This was personally frustrating to him and to the members of the department who, on the one hand, understood the reasons for the delays, but on the other, had legitimate objections to the termination of the promised physical and personnel reconstruction of the department. In the end, however, these efforts of Bush and others have prevailed, for the administration has improved the physical plant, has provided resources for several new appointments, and aided in the physical and intellectual amalgamation of Social and Experimental Psychology. This rejuvenated department remains a tribute to Bush's efforts.

His interest in the problems of students turned from his earlier concern with graduate students to a new-found interest in undergraduates. Among those features of undergradute life that seemed to him of deep significance was the growing recreational use of psychotropic drugs. In characteristic fashion, he translated his personal interest into a research interest, first by learning a good deal about the chemistry and physiology of drugs, and then by turning to the essential question of their psychological effects. He established an undergraduate seminar on drug research and served on the Dean's Council on Drug Use. His openness and honesty with students made it possible for

him to transmit to them the factual dangers associated with certain drug use. At the same time, he was unsparing in his criticism of those who, with no empirical facts, were prepared to punish, suppress, and otherwise attack any use of any drug at all except tobacco and alcohol.

His research directions in the study of drugs revolved around attempts, first, to formulate a lexical typology of the psychic effects of drug use and, second, to apply the method of magnitude estimation scaling to the reported experience of drug users. He was never satisfied with the empirical data, but did convince himself that his two-dimensional representation of the affective relations among various drugs captured something of the empirical phenomena. He did not publish this work because it was incomplete. He liked things squared off and cleanly terminated.

Personal Facts

Robert Ray Bush, born in Albion, Michigan, on July 20, 1920, came from an unpretentious background—his father was a butcher—and he remained unpretentious himself. In 1942, he received a B.S. in Electrical Engineering from Michigan State College at East Lansing, Michigan. As a graduate student, he studied physics at Princeton University, where he received a Ph.D. in physics in 1949. He was married briefly, but it was over by the time he reached Harvard. The major dates of his academic career have been given above. The last eight years of his life were increasingly plagued with physical ailments, which, however, did not seriously limit his activities; he taught until his death. He had a constant battle with weight—witness three distinct wardrobes—which, no doubt, contributed to his other illnesses. He died at his apartment on the night of January 4, 1972, at the age of fifty-one.

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