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Robert R. Bush

Early Career

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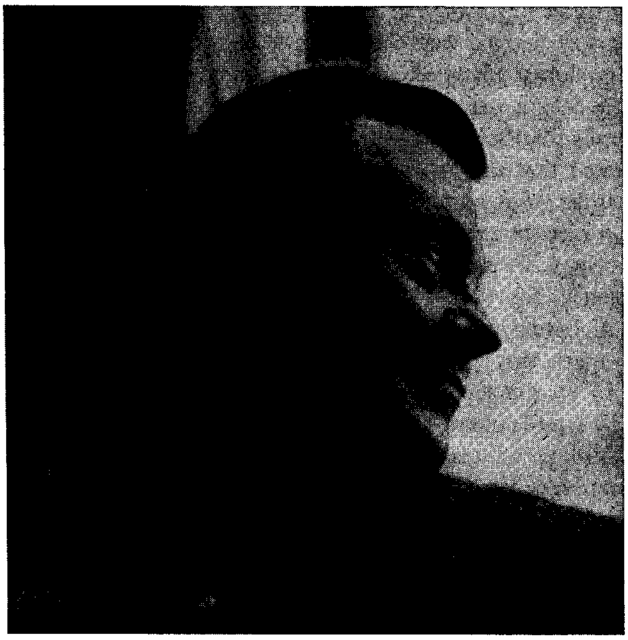
Robert R. Bush left a successful career in physics and became a psychologist. His research contributions in psychology fell primarily into the area of mathematical learning theory, and his teaching and expository writing were mainly mathematical, but his acquaintance with the whole of psychology helped him make a contribution as a departmental administrator.



This first paper describes the beginnings of his psychological career and a large part of his research effort, both of which occurred at Harvard, and the second paper by R. Duncan Luce and Eugene Galanter describes the major administrative and expository contributions he made while chairing the psychology deparments of first, the University of Pennsylvania, and second, Columbia University. On leaving Harvard in 1956, he first went to an appointment as Associate Professor of Applied Mathematics at the New York School of Social Work, Columbia University, where he considerably broadened his knowledge of the uses of sociology and psychology. Teaching was important to him throughout his career.

Early Days

Beginning his academic career at Michigan State University, Bush studied electrical engineering (B. S., 1942). From Michigan State, he went to work in the RCA Laboratories at Princeton, New Jersey (1942–1947, overlapping with his graduate work). There he participated in a project to develop a frequency modulated magnetron. Harry Fulbright, Bob's thesis director, recalls that this RCA work stood Bob in good stead when later they worked together at Princeton University (1946–1948) on the redesign of the frequency modulated cyclotron, a project directed by Milton White. Their subgroup was charged with the design of an oscillator and a rotating condenser. Bob suggested a slotted plate which was ultimately used, though the hardware and the details of the design were carried out by others.

Fulbright recalls that Bob paid little attention to his thesis topic until it had actually been demonstrated that the experimental apparatus (a camera inside the cyclotron) worked, but at that point he took over and with his usual boundless energy quickly completed the research entitled, "The inelastic scattering of protons from light nuclei." He received his degree in 1949 and was appointed instructor in physics at Princeton University for 1948–1949.

About 1948, Bob, together with other physics students, organized a seminar held about once a month throughout the year at the Institute for Advanced Study (in Princeton, N. J.) on the topic "Are the methods of physical sciences applicable to social sciences?" I understand that such stars as Margaret Mead, Gregory Bateson, and Abraham Kardiner addressed the seminar. Although this sort of student-produced seminar may seem a commonplace today, it was a first at Princeton at that time and foreshadowed Bob's move into the social sciences.

At the close of World War II, many physicists were in personal turmoil over some of the military uses of scientific research, Bob among them, though later he did not talk much about it. He decided to try to move into the social sciences. Several friends have the impression that J. Robert Oppenheimer, a friend and hero of Bob's, helped him think this step out and somehow smoothed the way. The National Research Council and the Social Science Research Council jointly gave postdoctoral fellowships in 1949 for social scientists and natural scientists who wanted additional training in the other field, and they awarded Bob one of these which, when renewed, lasted two years in all. Since Oppenheimer was on the committee supervising the program, he may have called this opportunity to Bob's attention. The fellowship program attracted top-notch people, but only five awards were made in all its years. And so it died for lack of flesh not funds, a peculiarly successful failure.

Postdoctoral Study

Although we do not know how he decided to come to Harvard's three-year-old Department and Laboratory of Social Relations, it was an exciting and stimulating place in 1949 teeming with clinical and social psychologists, sociologists, and anthropologists interacting vigorously and productively. The research ran from totally nonquantitative, nonempirical social philosophy through wide-open anthropological field studies, to the tightest laboratory experiments; from completely empirical studies, to totally theoretical mathematical ones; and from research in hospitals and in mental institutions to research in work camps and on psychodrama. Bob found it easy to join in seminars and courses with graduate students and in the discussions of the faculty. By participating in many of the informal student–faculty study groups, he learned a lot of psychology and social science and quickly made a reputation for himself that ultimately led to his appointment to the staff.

In those days, preliminary tiptoeing explorations for a special visitor included making sure that someone on the grounds would be responsible for making the visitor welcome and facilitating his work, and in some cases the possibility of working together was raised. As a recent Princeton Ph.D. in mathematics myself and a member of the Department of Social Relations with some ties to the Social Science Research Council, it was natural that these inquiries and suggestions would drift my way.

When reporting orally on his quality, his recommenders gave the underselling forms of high praise very talented people reserve for other very talented people: "not at all bad," "better than average," and "pretty good." With such gilt-edged assurances and the fact that Bob was already coauthor of four articles in physics, it seemed reasonable that he would want to start research as soon as he arrived. He did, and this was the start of our seven or eight years of extensive congenial collaboration. He came alone, as he and his wife were separated and later divorced.

As a postdoctoral fellow, Bob's primary project was to do a good deal of reading and to attend lectures and seminars, which he did with a will. On the research side, I suggested three possible areas with a view to our working together on one of them. They were (1) the study of problem solving in small groups (related to departmental work in progress by R. Freed Bales, earlier my office-mate), (2) finding relations between various psychological scaling methods through theory and experiment (based on a course I had given in psychometric methods), and (3) developing mathematical models for learning (based on some data on the relief from successive doses of analgesics brought to my attention by Dr. Henry K. Beecher of Massachusetts General Hospital). As soon as Bob came, we spent a few days looking into these problems.

As for the first, we couldn't see how to get a sharp mathematical wedge into the small-group area though later David Hays and Bob (Hays & Bush, 1954) wrote a paper using learning theory on a study of small groups. For the second area, in the light of S. Smith Stevens's later sustained innovative work on scales of measurement and their relations, it may amuse the psychological reader that he advised us that the field was settled and so there was nothing left to do. We were not put off by this advice. Bob didn't care for the scaling problem even though it looked tractable both mathematically and experimentally—he said he wanted something more social. Again this is amusing because Stevens's later work was directed exactly to the social, or at least societal, uses of scaling. It all belongs to the New Yorker's "Department of the Clouded Crystal Ball." As for the third area, we both saw ways to start on probabilistic models for learning. And so in a matter of three days, we chose and began to work on a problem that turned into years of effort.

The speed and specificity of the decisions were rather characteristic of Bob's "let's do something, and let's do it now" attitude toward work and play. Wherever possible he liked to try alternatives out and see what worked, rather than argue or even think very hard about what alternative was preferable. Being quick, well organized, and so directly to the point, he was able to make this approach work, where others might not. In the instance under discussion, if the learning work had not been productive, we could readily have turned to one of the other two areas, because for both of them we had sounder preparation than for the one we chose.

When I knew him, Bob started the day with a "grocery list" of what was to be accomplished that day, more or less in the order planned for doing them; for example,

prepare course for Thursday
get laundry
write Madow
teach course
prove theorem on asymptotes
call about typewriter
see student about thesis
fix door
write section about asymptotes

He expected to complete his grocery list each day (many of us don't), and when he finished, he felt entirely free to play. He planned ahead for this as well, but he did not feel comfortable until the grocery list was checked off, and more than once I remember him working very late at a party at his own house, long after the guests arrived.

With his fellowship, Bob was free most any time and my duties during his first few years at Harvard were only moderate. We were frequently able to spend half a day at a time together on research and then half a day apart. Memoranda were written very rapidly. Long phone calls were frequent. We had the aid of a well-trained psychologist and mathematician, Doris Entwisle, as mathematical assistant. By 1965, she was teaching in both the Departments of Social Relations and Electrical Engineering at The Johns Hopkins University. Bob pointed out to her as she launched on her own independent research career that it was going to be hard for her to get the kind of assistance we had had.

When we first worked on learning theory, we were much helped by repeated sessions with well-known psychologists; among these were Ernest Hilgard, Carl Hovland, William O. Jenkins, George A. Miller, R. R. Sears, Fred Sheffield, and John Whiting, who is also an anthropologist. I saw Clark Hull just once and had a most encouraging discussion. As Bob learned about conditioning and reinforcement, he longed to try it out on a human, and that was why my two-year old son used to race up to Bob, whenever he saw him, shouting, "subset, subset," his first serious word, painstakingly taught by Bob visit after visit.

After Bob took a teaching post in the Department of Social Relations, his time was not quite so free, and because I was acting chairman in 1953–1954 we had a terrible time finishing our book before I went on sabbatical, but we managed, mainly by working very late at night. In writing the book, we usually worked out the theory and the statistical analysis together and then one of us would take the pieces and draft a chapter; the other would later revise it.

Teaching at Harvard

In all of his five formal teaching years at Harvard, Bush gave a seminar in mathematical models in the social sciences, the first such seminar, I believe, at our university. In addition to models for learning, he drew also, for example, on growth models, game theory, kinship structure, and simultaneous differential equation models. He taught experimental social psychology each year. One year, together with Ray Hyman, he taught quantitative methods in the social sciences, our second-semester course in statistical methods. Beyond these courses, however, he gave many reading courses, participated in a variety of extra seminars, and directed or participated in the direction of a number of doctoral dissertation. He was generous of his time with students and he enjoyed working with them. Several of my correspondent have mentioned that from the first he had an attitude of working *with* students, as nearly as equals as he could arrange. Among the graduates of the Department of Social Relations expressing in prefaces appreciation to Bush for his help with their doctoral dissertation where: Bernard P. Cohen, Jacqueline M. Jarrett Goodnow, David G. Hays, Saul Sternberg, and Thurlow R. Wilson.

He had an especially good deep voice which had had some training in operatic singing. He also had a great sense of humor. These together with very legible handwriting and well-organized blackboard behavior helped a lot in Emerson Hall at Harvard where the blackboard were entirely inadequate for mathematical work.

Letters from people who were graduate students and worked with Bob in courses, as project assistants, or in summer institutes emphasize his warmth, the feeling of excitement he gave a class, the seriousness and honesty with which he appraised his own work and that of others. More than one emphasizes the encouragement of independent work by the student—encouraging personal responsibility—but not leaving the student in the cold. The letters tell concrete stories of Bob's work to help students—helping one to get a very inexpensive room because his stipend was so low, helping another to get reimbursed for a stolen typewriter on the grounds that it was to aid project work. It pleased students that he was happy to argue with them on social science theory and practice as well as on details of mathematical models and statistics. He criticized written material with thoughtful care and encouraged high standards. He believed in writing things up as quickly as possible and encouraged students to do the same.

Malcolm Arth, then a graduate student in the Department of Social Relations, says of this period:

> Bob was very much a Social Relations Department person. Although his strongest ties were with the social psychologists, his own experience with psychotherapy and his sympathies with psychoanalytic theory also allied him closely with the clinical psychologists, and not just experimentally oriented ones. He also had a strong bond with anthropologists and sociologists. His understanding of the culture concept, and the importance of cross-cultural research for all the social sciences, was deeper than one normally encounters in a non-anthropologist. . . .
>
> I recall as a young graduate student in 1953 that most of us entered with great expectations for the ambitious interdisciplinary goals of the Department of Social Relations. Some of the zest and optimism of the first years of the Department had already peaked, but in 1953 there remained enough viable interdisciplinary teaching, research, and theorizing to make the Department seem often what is was supposed to be. Some members of the faculty had already become fairly isolated and specialized, but many were still committed to building bridges and cutting across traditional lines. Bob, again typically, understood both groups and yet always seemed to me to be in the latter camp. Certainly he was when talking and working with people like me who had that sort of commitment.

The National Scene

In writing a history (Mosteller, 1974) of the role of the Social Science Research Council (SSRC) in the rise of mathematical methods in the social sciences, I was impressed with the extent of Bush's contribution to this development. He taught in the first summer training institute at Dartmouth College in 1953 for the SSRC's Committee on the Mathematical Training of Social Scientists. He was well liked there as a teacher, indeed everywhere as far as I know.

After the 1953 institute, the teachers and participants reviewed the work carefully, and Bush wrote a section of a paper "Mathematics for Social Scientists" (Bush, Madow, Raiffa & Thrall, 1954a). In this he suggests first that the content of mathematics courses for social scientists might well be based on an empirical assessment of those methods that had already been especially useful. Second, the movtivation of social science students is to strengthen their work in their own field, and therefore mathematics courses for such students should have social science content. Third, since mathematicians were not then adequately educated in social science applications of mathematics, some new educational effort was required.

The students' questionnaires supported his second point, and the 1955 summer training institutes were taught with more social science content. Since more applied materials were needed for the 1955 seminar he was to teach in, Bush, Robert Abelson, and Ray Hyman (1956) prepared special problem materials in the summer of 1954.

His third proposal was found immediately attractive by the director of SSRC, and a large summer institute to train collegiate mathematics teachers in mathematical applications in the social sciences was held in 1957.

Bush was a member of the SSRC's Committee on the Mathematical Training of Social Scientists in 1956–1957.

He taught summer school at Harvard in 1956 and then directed a 1957 SSRC summer institute on applications of mathematics. That was the last of the SSRC summer training institutes, but there were further SSRC research training institutes and research institutes, and Bush led and participated in these in the summers of 1961 1962, 1963, 1964, and 1966 under the auspices of either the SSRC or the Mathematical Social Science Board (MSSB).

The MSSB was developed about 1963–1964 partly by SSRC committees and partly by others. Bush together with W. K. Estes, R. D. Luce, and P. Suppes largely developed the proposal leading to the organization. The MSSB in turn has been very influential in broadening the use of mathematics in such social sciences as anthropology, history, and political science where it was rarely used before 1950, and in extending its use in the more traditional areas of economics, sociology, and psychology.

After Bush went to the University of Pennsylvania, we did little more together as we both were chairing departments, though in 1964, he and I committeed together under the chairmanship of John Kemeny for the Panel on Biological, Management, and Social Sciences of the Committe on the Undergraduate Program in Mathematics of the Mathematical Association of America. As I recall, Bush's contribution to that work was, by intermittent urging, to press Samuel Goldberg, Geoffrey Watson, and me to design a statistics curriculum for the Panel. Looking back on his work, I am astonished at how many committees Bush was *not* on. For example, among 30 members of the MSSB, he never appears. I think he was happy to do things but not pleased with the time spent in committee debates. Perhaps at this later period in his life he preferred, as some have suggested to me, to concentrate his efforts on psychology rather than on the social sciences, generally, a reversal of his attitudes while at Harvard.

Scientific Work

In the models for learning that Bush and I developed, the fundamental representation was that prior to a trial an organism has a vector of response probabilities. A stimulus corresponded to a mathematical operator that replaced the organism's current vector by a new probability vector. In the models in Bush and Mosteller (1955), the effects of previous responses were summed up in the current vector, independent of the path to the present state. The operators had a linear form, so that if p is a vector of probabilities $(p_1, p_2, \dots, p_k)$ and Q is applied to p, the new vector is

$$Qp = \alpha p + (1 - \alpha)\lambda$$

where λ is also a probability vector $(\lambda_1, \lambda_2, \dots, \lambda_k)$ and α is a scalar, $0 \leqslant \alpha \leqslant 1$. If Q is repeatedly applied, the limiting vector is λ, when $\alpha \neq 1$.

When there were only two responses, say R and $\mathbf{R}$, if instead of a vector we take p to be the first coordinate so that $p = \text{Prob}\{R\}$, $1 - p = \text{Prob}\{\mathbf{R}\}$, then we can get along without vectors by merely following the courses of p as various operators are applied as in

$$Q_i p = \alpha_i p + (1 - \alpha_i)\,\lambda_i\,.$$

For example, in a T-maze the animal's turning right and being rewarded might have the operator Q_1 with $\lambda_1 = 1$. Turning left and being rewarded would by symmetry give $\alpha_1 p = Q_2 p$ (because $Q_1 q = \alpha_1 q + (1 - \alpha_1)$ and the new value of p is $1 - Q_1 q = \alpha_1 p$). Turning right and not being rewarded might increase the probability of turning right a little, according to $Q_3 p = \alpha_3 p + (1 - \alpha_3)$, or might decrease it, say according to $\alpha_3 p$. This is regarded as an empirical question that might depend on the problem. The paradise fish investigation mentioned below deals with this question in one context.

The particular form of the linear models has an attractive interpretation in terms of conditioning whose essence has been described by Estes (1950). At the start some elements of the space S are conditioned and some are not. As a result of the trial a random set A, whose measure is m (this use of a general measure aids in generalizing Estes's idea of counting elements), is drawn and all those elements not conditioned are now conditioned. And a set B is also drawn and all those elements in it already conditioned are deconditioned, overlaps in the sets A and B being unchanged. If a and b are the measures of sets A and B, respectively, then p is replaced by an amount

$$p + (1 - p)\, a - pb.$$

In words, to p is added an amount proportional to its maximum possible increase, and from p is subtracted an amount proportional to its maximum possible decrease; $a(1 - p)$ had an interpretation as increment in "excitatory potential" in Hull's theory and the term $-bp$ as an increment in Hull's "inhibitory potential."

Although Bush and Mosteller (1955) develop this particular form of model almost exclusively, we were well aware of other possible forms of operators and were often dissatisfied with these. We felt though that there was much to be said for taking one form of operator and pressing hard to make it do as much as possible rather than inventing entirely new forms of operators on every occasion. That approach has the advantage of creating some unity in the work and of gradually generating some facility for facing a new problem with the same somewhat limited set of tools. We both found it a congenial way to work although we did want to explore other possible forms of operators.

In our first paper, "A Mathematical Model for Simple Learning" (Bush & Mosteller, 1951a), we explained the relation between our approach and that of Estes; we emphasized reinforcement concepts, he association theory. We were all trying to describe instrumental conditioning or operant conditioning and not Pavlovian conditioning.

Our strong emphasis is on the discrete trial-by-trial approach, but we then try to relate to probabilities, latent time, and rates. This leads to differential equations and continuous results.

The paper discusses free-responding situations as in bar pressing in Skinner boxes under extinction, fixed ratio, and random ratio reinforcement conditions, as well as aperiodic and periodic reinforcement. Most of these interpretations required us to go to differential equations. We never emphasized them so heavily again. We wanted to describe the learning in fine detail. The differential equations were describing situations where we had little data available for verifying the assumptions we were making. When a pigeon ballistically put on a burst of pecks at a key, we could not believe that the individual pecks were independent in the sense of the model. True, one might still be able to fit the actual learning curves because the model may have several parameters and can fit a complicated shape. But the model should describe the detailed process, not just fit means. This idea of studying fine structure was new to mathematical learning theory.

To describe experiments in stimulus generalization and discrimination (Bush & Mosteller, 1951b), we need to measure similarity of stimuli. We generalized and adapted Estes's set theoretic approach to treat these problems and applied the results to several experiments and to some related cases with no experimental data available.

In an especially ambitious paper, Bush and Whiting (1953) relate the stochastic learning models, using a set-theoretic interpretation, to displacement. They review Neal Miller's list (Miller, 1948) of assumptions with delicate care, taking exception to some, and explain how Miller's assumptions might be related to mathematical assumptions in the model. The model is applied to experimental results on pigeons and extended to apply to a cross-cultural study by Whiting and Child (1953). It is even extended to describe a recovery effect following punishment. The thrust is to set up the general model, describe possible situations, and then show that the model can produce results compatible with the experimental or observational findings.

In a paper Bush worked on as a result of some seminars led by R. R. Sears (Brush, Bush, Jenkins, John & Whiting, 1952), a set of experiments with groups of pigeons tests some aspects of Neal Miller's theory of displacement. Miller had formulated a theory of generalization under conflict conditions, and Miller and Kraeling (1951) had performed an experiment giving the theory some support. The experiment reported by Brush *et al.* (1952) examined generalization under three conditions. Overall, the outcomes did not support the Miller position, although results for one of the three experimental groups were consistent with Miller's theory.

Bush used the mathematical learning model formulation to translate the observed results for pigeons from the Brush *et al.* (1952) experiment and the rate data from Miller and Kaeling (1951) into somewhat comparable measures.

One of our most important analytical devices was the idea of a statistical subject. We called them stat-rats, so christened, I believe, by Doris Entwisle. In the kinds of learning experiments we studied, there were many properties that a sequence of trials could have, and we could rarely measure the actual probability in the animal, except perhaps when it was near 0 or near 1. Consequently it was desirable to simulate an experiment by running stat-rats through it and applying the same mathematical model to generate the behavior of the stat-rats as the model being fitted to the experimental data was supposed to imitate the behavior of real subjects. Such simulations, now said to be generated by Monte Carlo method, made it possible to get at many theoretical properties of the data without mathematically computing extremely complex distributions. We exploited this idea repeatedly in *Stochastic Models for Learning* (Bush & Mosteller, 1955) especially in the Solomon–Wynne experiment (Chapter 11) and in the eight models paper (Bush & Mosteller, 1959) and Bush and Wilson (1956) used it in the T-maze experiment with paradise fish.

In the paradise fish paper, the distribution of lengths of runs of successes and of failures was of special interest, and may have been part of the stimulus for work on runs by Bush (1959), Bush and Sternberg (1959), and by Bush and Lovejoy in an unpublished paper "Learning to Criterion."

We had distinguished between contingent and noncontingent procedures.

Humphreys (1939) had college students guess whether or not a light would flash, and then it did or did not according to a predetermined schedule. We call this a non-contingent two-choice experiment because the chance of a flash did not depend on the choice. Brunswick (1939) used a two-choice procedure where the probability of reinforcement on the two sides differed—partial reinforcement for both .We called this contingent. We thought the asymptotic behavior of the subjects might be different in the two situations. The Bush–Wilson paradise fish experiment was one attempt to look into this. In the control group, the fish could not see that the other side of the T-maze got the reward, but in the experimental group they could. Two models were considered for the experimental fish: (1) the information model, the side with the food would increase the probability of the fish choosing it on the next trial; and (2) secondary reinforcement model, the side the fish went to was reinforced by the sight of food in the other box.

The data supported the secondary reinforcement model. Bush and Wilson's analysis is thorough and elaborate, looking hard at the fine-grain structure of the response sequences in the experiment.

An aspect of the work on mathematical learning fits well with the axiomatic ideal. One sets down conditions and from these flow the legitimate operations. Our feeling was that what we were doing was very empirical and ad hoc—choosing the particular operators and following them where they led, ordinarily from one set of experimental data or learning theory phenomenon to another. Once we took a somewhat different tack and asked whether there were essential properties that learning operator for probability models should have, and if so, what classes of operators would be admissible under these properties.

We came up with what we called the "combining classes" criterion: the probability vector should have the property that if some classes of behavior were actually treated treated by the organism and by the system of reinforcements in the "same" way, then it should not matter to the model whether these categories had their probabilities combined before applying the operators or after combining them. The criterion asks that the operation of combining categories be commutative with the operation we call the learning operator. We were initially pleased with this idea and after using up a great many paper napkins at the Midget Restaurant developing it, we made some progress. It was easy to find that the learning operators we had been using had this property. It was harder to settle what other models had the property. First L. J. Savage, and then later Gerald Thompson, helped us with this work, and the latter finally proved in Bush, Mosteller, and Thompson (1954) that essentially there were no probability models other than the linear operators we were using that had the requested property.

Far from pleasing us, this chilled us with the axiomatic approach. We realized that this had been an exercise that showed the tremendous power of a seemingly small axiomatic request. We knew very well that we wanted to be able to use other models than those having the form of operators that ours had. We also knew that by insisting on being prepared to combine classes making irrelevant distinctions, we were also imposing the form of operator on classes where the distinctions were relevant to learning. Mathematics can be very sneaky that way.

The John and Mary R. Markle Foundation gave money to the Social Science Research Council (SSRC) for some interuniversity summer research seminars in social science research. S. S. Wilks suggested that we apply for funds to run such an institute. George A. Miller and I organized it, though he insisted that I be chairman. That summer of 1951 at Tufts University, Cletus J. Burke, William Estes, George A. Miller, David Zeaman, Bush and I together with William McGill, Katherine Safford Harris, and Jane E. Beggs worked on learning models. Someone talked about current research progress nearly every day for about an hour and a half, but the rest of the time was spent on research. We lunched together at the faculty club, sometimes with Leonard C. Mead who arranged for our quarters and sometimes with the psychologist, Leonard Carmichael, then president of Tufts. It was the period when Bush and I made our greatest and fastest progress. Toward the close of that summer Bush and I decided to write a book, but it is a long way from deciding to doing, in this case three years.

In addition to the mathematical material, the book treated experimental data on free-recall verbal learning, avoidance training, imitation, T-maze experiments, three-choice experiments, and run-way experiments. The run-way experiments dealt with running times and required special developments to adapt our discrete approach to continuous distributions. We had especially to face the individual difference problem in a rather virulent form. The fine Weinstock data made this analysis especially worthwhile for us. In writing the book we had help from a number of research assistants: Lotte Lazarsfeld Bailyn, Doris Entwisle, David G. Hays, Solomon Weinstock, Joseph Weizenbaum, Thurlow R. Wilson, and Cleo Youtz.

Bush put together a model for what might be called insight or one-trial learning. When he presented it orally to a group of psychologists, they complained that he already had a model, the linear operator model, and that one model was enough. This attitude, although perhaps given partly in fun, distressed us both, and so we thought it would be instructive to try to prepare a set of alternative models for one learning situation. We thought that it would be helpful to see what variety there would be in mathematical models that attempted to describe various theories of learning. This work led to our paper on eight models (Bush & Mosteller, 1959).

The data came from Solomon and Wynne's experiment on avoidance training of dogs. The dogs could avoid an intense electric shock by jumping a barrier within 10 sec after the occurrence of a conditioned stimulus. The data were the sequence of 25 shocks (S) or avoidances (A) made by the 30 dogs.

The models developed were: (1) the two-operator linear model, where p_n = probability of avoidance on trial n, q_n = probability of shock on trial n:

$$q_{n+1} = \begin{cases} \alpha_2 q_n & \text{if } S \text{ occurred on trial } n, \\ \alpha_1 q_n & \text{if } A \text{ occurred on trial } n; \end{cases}$$

(2) a Hullian model that gave $p_{n+1} = p_n + (1 - \alpha)(1 - p_n)$; (3) a Hullian model with dogs allowed to have individual differences in their learning parameters; (4) an early Thurstone model where $p_n = (n - 1)/(n + 1 + b)$, and b is a constant; (5) a late Thurstone model in which learning proceeded through an urn scheme leading to the probability of shock on trial n as

$$q_{ij} = 1/(1 + ic_2 + jc_1), \qquad i + j = n - 1$$

where i is the number of shocks, j the number of avoidances so far; (6) a Markov model with constant transition probabilities, the probability of S following S being taken as a, of A following A being taken as 1; (7) a Restle model based on the idea of conditioning of cues, not explained here, led to

$$p_n = 1 - \frac{(1 - \theta)^{n-1}}{\theta + (1 - \theta)^n};$$

(8) a Kreshevsky or one-trial learning model, where the organism originally has probability p of avoiding on each trial, but has probability β of moving to a probability of 1 of avoiding. This model is a slightly specialized version of the model Bush had presented to the psychologists. The more general version moved the subject from p to p', where p' is not necessarily 1. The paper encouraged others interested in particular psychological positions to consider improving by judicious complication the models we had offered. The Restle and the two-operator models fit well and others failed in identifiable ways. Thus someone wanting to improve on these models would know what difficulties he needed to correct.

We were especially pleased when at least minute we got in the mail the model from Frank Restle which we were able to add to the list. One trouble with some of our models was that although they represented psychological positions honestly, we were not always able to give the different models the same number of free parameters. This meant that a model like the one we had developed with more free parameters could be fitted more closely than another with fewer because of the artifact of having additional parameters. The matter is not quite this simple because some models may not have good use for an extra parameter even if it is offered, and how it is inserted makes a difference. The idea of the paper was partly to show that there were many models that could be brought to bear on a problem, and second that we could, by simulation, find out how models failed. We were not especially interested in showing which model fit more closely.

The general problem of fitting models and deciding what to regard as the important statistics in circumstances where the models are not exactly right encouraged a good deal of work on the statistical properties of the learning models. Bush and Saul H. Sternberg (1959) produced a very attractive paper on the single operator linear model.

If p_n represents the probability of error on trial n and $p_{n+1} = \alpha p_n$, $0 \leqslant \alpha \leqslant 1$, then they found such results as

$$\text{the expected errors in } N \text{ trials:} \quad p_1 \frac{1-\alpha^N}{1-\alpha}$$

$$\text{variance of total errors in } N \text{ trials:} \quad p_1 \frac{1-\alpha^N}{1-\alpha} - p_1^2 \frac{1-\alpha^{2N}}{1-\alpha^2}$$

the distribution, mean, and variance of the number of trials before the first success. They found the expected total number of runs of errors in N trials, and of the number of error runs of length j in an infinite sequence. This leads to theorems about autocorrelation of errors, and about trials to a criterion, for example, the mean number of trials until c or more successes.

Since previous theories of runs and trials to criteria had been largely limited to conditions with fixed probabilities, even though this paper dealt with the simplest linear model, it was a nice opening gun in a series of such papers Bush and his coworkers developed.

In "Sequential properties of linear models," Bush (1959) presents information about the statistics of runs for four different simple learning models. He not only derived the formulas but also provided tables and approximations for sums required for practical work.

"Tests of the 'Beta Model',"[1] by Bush, Galanter, and Luce (1959) compares the fit of a beta model to that of an alpha model. The "alpha model" is the set of linear models we have largely been describing and the beta model is one that uses a more complicated function of the previous probabilities. For example,

$$p_{n+1} = \frac{p_n}{p_n + \beta_i(1-p_n)} \quad \text{if alternative } i \text{ occurs.}$$

This paper could be regarded as a supplement to the "eight models" paper in two directions; its adds a model and analyzes two additional experiments.

Since R. Freed Bales had developed a large project on the study of small groups of people, studying interactions between people was very popular in the Department of Social Relations. David G. Hays and Bush did an experiment on group action closely related to Humphreys's original experiment. In the single subject experiment, the subject predicts one of two alternatives E_1 or E_2, and the corresponding events E_1 or E_2 (such as red or green light flashes) occur with fixed probability (0.75 for E_1,

[1] Although the paper was written after Bush went to the University of Pennsylvania, I've included it in this series.

0.25 for E_2, in this experiment). Instead of a single subject making the prediction, Hays and Bush had groups of three predict.

Using the standard linear model approach, they developed two models, one the group-actor model, the other the voter model. In the group-actor model, the occurrence of E_2 on trial n reduces the probability p_{n+1} of predicting E_1 on the next trial to

$$p_{n+1} = \alpha p_n \qquad \text{(when } E_2 \text{ occurs on trial } n\text{)}$$

with a similar reduction in the probability of predicting E_1 when E_2 occurs

$$q_{n+1} = \alpha q_n \qquad \text{(when } E_1 \text{ occurs on trial } n\text{).}$$

Essentially the group is acting as a single individual.

In the voting model, each individual is regarded as obeying the same model as the group-actor model, but with a common value of α, not identical with the one above. If two or more individuals vote for the event E_i, $i = 1, 2$, it is chosen.

The results of the experiment showed that in several respects both models fitted rather closely. All the same, the authors thought they should develop a more general model allowing for the possibility of group support—once one person chooses E_i, this makes it more likely for the next person. To test the third model required something that had not been kept, data on individuals. Since this was planned as the first of a sequence of investigations, the article (Hays & Bush, 1954) looks hopefully into a future which, except for Hays's doctoral diseration, never developed.

When Gardner Lindzey decided to organize the *Handbook of Social Psychology*, with Gordon Allport's help, he pressed Bush and me into writing a piece on quantitative techniques (Mosteller & Bush, 1954). I pulled the topics together and wrote a draft. Bush saw the value of making the effort more systematic in two ways: (a) by providing an example of the application of each method; and (b) by providing tables or approximations in order to use the methods. These additions make the difference between an assembly of ideas and a practical article for the research worker, and they doubled the length of the article. Nevertheless Bush was not comfortable with our usual Bush and Mosteller authorship and insisted that we reverse the order for this occasion.

I have asked John M. Roberts to write of his work with Bush on an article in anthropology (Roberts, Arth, & Bush, 1959). He reports:

> My collaboration with Dr. Robert R. Bush began when the Department of Social Relations, Harvard University assigned us to a joint office in Emerson Hall. He had a number of books on game theory on his shelves, and we fell to talking about them and about game theory. We developed the idea of conducting a cross-cultural study of games, naively thinking that the ethnographic descriptions of games would support a fine-grained analysis of game play. We obtained a modest grant and hired Dr. Malcolm Arth, who was then an undergraduate at Boston University, to travel to Yale to code the game materials in the old Cross-Cultural Survey files. This he did until the money ran out. At first we

> were disappointed by the poor quality of the materials, but we three eventually published a joint paper based on the data.[2]
>
> Bush contributed to the enterprise in a number of ways, but his most important contribution was the introduction of a classification of games which was derived from one advanced by von Neuman whom he had known at Princeton. This classification into games of physical skill, strategy, and chance has proved to be extraordinarily robust and has been used in a number of subsequent cross-cultural studies. He also insisted on rigor in the coding, and this made possible the later study.
>
> Bush had an active and stimulating imagination. I found him to be an exciting colleague and a fine collaborator. Those conversations that we had in Emerson Hall gave me an interest which has influenced my entire professional career. I am very glad that I knew him then and I am sorry that our contact terminated after I left Harvard.

It seems a fair inference that this positive experience with Bush and corresponding good experiences with some other applied mathematicians and statisticians partly explains why Roberts, an anthropologist with no special quantitative or mathematical training, has since then been a leader in encouraging mathematical and quantitative developments in anthropology. Thus through his research efforts, Bush has influenced the administrative development of applications of mathematics in the social sciences.

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[2] Regarding this paper Malcolm Arth writes: "Typically of Bush, he insisted that this first-year graduate student (Arth) take second authorship with himself listed last, because Bush felt he had done the least amount of work on the paper."

Robert R. Bush

Later Career

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AND

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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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