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THE BIRTH OF THE SOCIETY OF MULTIVARIATE EXPERIMENTAL PSYCHOLOGY

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This paper describes the inception of a scientific movement (the Society of Multivariate Experimental Psychology); the individuals who begot it; the two journals and numerous scientific articles which resulted; its effects in stimulating new areas of teaching and of theoretical development; and its international reception as well as its successes and failures in stimulating the creation of satellite societies on the same pattern.

In my first visit to the United States in 1937 to work as research associate with E. L. Thorndike, I was surprised to note courses labeled "experimental psychology." Was not all psychology experimental? Of course, it was a case of mislabeling brass instrument experiments. An experiment is an analysis of carefully observed data, controlled or uncontrolled – and most real life data are uncontrolled.

As personality, clinical, and social psychologists started to acquire a sense of guilt about their largely subjective procedures, they began, in the 1940s, to turn to the methods and standards of experimental psychology. But to my horror they viewed this as a shift to "experimental psychology," held up to them as a bivariate, controlled design rather than to the multivariate designs that could alone encompass and analyze the complex, uncontrollable gestalts and patterns with which they are compelled to deal. I watched this imitative conservatism for most of tweny years with little opportunity to deflect the trend by other than one or two articles and some allusions in my textbooks. By the later 1950s, however, I found enough colleagues active in factor analysis and other multivariate designs to justify founding a professional group.

Since the formation of the Royal Society in 1660, much has been done to cause signal advances in specialized areas through the formation of self-conscious sister societies. Yet some scientists, as individuals, have debated the necessity of scientific groups.

Anticipating some opposition to organizing a group, I decided to move somewhat tentatively and obliquely. With a grant from the Office of Naval Research to review developments in personality research by multivariate methods, I called a three-day meeting in 1959 at Allerton Park House at the University of Illinois in Champaign-Urbana. Attending were leaders from the United States, England, and Germany who enthusiastically agreed on the methods' success. With some difficulty, a core of five colleagues approved the formation of a permanent society – The Society of Multivariate Experimental Psychology (SMEP) – that would sponsor an annual meeting. I drew up and circulated among a dozen people a constitution with several odd features: (1) annual meetings should not be held in large, distracting cities, but in country motels or small universities in peaceful and beautiful surroundings (this was criticized as cutting out the evening "night club" trip); (2) to avoid distinguished deadwood, all members should retire at age sixty-five and be re-elected, if they wished, on the same basis as

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newcomers; (3) the size of the group was to be limited to sixty well-chosen members. My theory was that about half would attend meetings, and that thirty could be gathered informally around a single, large "executive's table" in face-to-face discussion. The proposed constitution also required the retirement to inactive status of anyone who did not present a paper for three years.

At a later and larger meeting all these rules were passed except my "anti old duck" motion. The central, most positive injunction – that new members should be chosen on the basis of experimental, substantive work, not of virtuosity in statistical ideas – was, and has remained, the most difficult to implement. The emphasis on actual skill in discovery – as with the Nobel prizes – calls for originality and steadfastness not always found in psychometric armchair theorizing. This emphasis was to distinguish us from the Psychometric Society – a potential source of confusion of functions. Although this central objective was formally accepted, it proved so difficult to implement that, for some years, I have felt it might as well never have been passed. The first problem was that the most eminent people in the field at that time were more easily distinguished by their grasp of multivariate statistics than by psychological discoveries. Lee Cronbach, Ledyard Tucker, Louis Guttman, Harold Gulliksen, D. R. Green, H. H. Harman, R. Sokal, and M. Tatsuska had to be invited. Nevertheless, we wanted to include some types of multivariate experimenters like J. P. Gilford, Hans Eysenck, and others with excellent but smaller contributions. The necessary inclusion of the eminent statisticians threatened to create and perpetuate an emphasis not intended for the Society. To this impasse I responded with the assertion that the Society had to help create the type of men it wanted. That it has done.

In a practical and communal sense, the Society functioned well from the beginning. Its annual meetings were well attended and carried out with marked enthusiasm. The level of discussion was frank, free, and excellent. Members knew each others' work well, and therefore arguments could proceed without preliminary definitions and without fear of personal resentments. Several entirely new and valuable contributions to psychology were made in the first five years, and all members recognized that they were learning much that was not attainable elsewhere.

New members were chosen by circulating among the memberships the full bibliographies of those who were nominated. Very gradually the Society got the substantive, empirical, "discovering" types. However, competition was intense, because few left the Society or asked to be put on the inactive list. Incidentally, at every meeting, especially if held in a university, there was a "gallery," at least as numerous as the participants, of local graduate students and faculty vistitors, who listened but rarely entered discussion of papers. All members agreed that there were decidedly more qualified psychologists in the country wanting to join the Society than the limit of sixty members allowed. Rather than lose the companionable and efficient spirit of the roundtable group by enlarging the membership, regional groups were set up with the same rules as the main Society. Thus, two years after the formation of the main SMEP, there was set up a Midwest (MW) SMEP in Chicago, a Southeast (SE) SMEP in Atlanta, a Southwest (SW) SMEP in Kansas, and a Canadian group in Alberta. As far as we know, the first and second of these met only periodically, but the others met regularly. Under L. Bolton and Charles Burdsal the SW group is especially vigorous and sponsors its own journal. Later, a SMEP group was formed in Hawaii; and another, in Europe, run by R. Warburton until his death. Meetings were held in England and in the Continent. There is no East Coast group, and Guilford's attempt to form a West Coast group failed.

Thus, the regional approach has worked only to raise problems which led Madison Bentley and Benjamin Fruchter to advocate a different plan – a modified national society. However, highly successful regional groups demonstrate that the plan can work and that they should be studied as models. A major cause of failure in the opinion of the present writer, has been that the national society has paid absolutely no attention to the branches. There is no communcation between them – not even annual reports. This is unfortunate, for there is no doubt that the "provincial" societies have contributed significantly to the objectives of the movement – increasing attention to teaching multivariate techniques in the universities. Enquiry shows that at least half the newly graduated PH.D.s in psychology, across the country, have completedly failed to grasp the relevance of multivariate experimental research to concept formation in their fields or to learn the new opening work on structured equations.

This lack of realization is particularly evident in group dynamics and cross-cultural research, where factor analysis of the significant dimensions of groups has led to new concepts, and to objective grouping of modern national cultures.

Two developments have contributed to an enormous increment in multivariate psychology over the past twenty-four years – the founding of SMEP and the development of the computer. As far as psychology is concerned the development of the computer was a timely accident. The founding of SMEP, however, was a direct and deliberate movement to get experimenters to think in multivariate terms. The movement has been further aided by the appearance of the two society-bound journals – *Multivariate Behavior Research (MBR)* and *Multivariate Experimental Clinical Research (MECR)*. Now in its 18th volume, *MBR* was started in 1966 despite some opposition by a segment of SMEP. *MECR* was also published in 1966. Desmond Cartwright, with the help of Samuel Messick, Philip Merrifield, Saul Sells, and the present writer saw *MBR* through the financial and editorial troubles of its infant years.

Prior to the founding of *MBR*, SMEP members had difficulty getting their articles published. Editors of substantive area journals frequently rejected their articles; the reason given, that readers would not understand them. Thus many substantive articles on clinical, personality, and social psychology, often providing the only answer possible to a complex problem, were denied readers by editors who themselves lacked sophistication in multivariate techniques. Multivariate Behavioral Research has solved the problem for the readers but not for area journals, which are still tied to traditional, relatively cramped technical treatment.

Multivariate Experimental Clinical Research (*MECR*), judging by its still limited circulation among clinicians, has yet a long way to go. It started under George Pierson, a creative younger psychologist, who ran into difficulties with a conservative psychology department and left the field. After *MECR*'s unfortunate beginning, Burdsal assumed the editorship, changed its format, and nurtured it for years. The journal, with its new approach to clinical problems, is better known abroad than in the United States.

At the same time one must notice that from 1970 to the present in the United States, there is a great birthrate of new journals than in any previous epoch. Scrutinized critically, however, they turn out to be almost all narrow and applied. Basic research has received little support, and a decline in grants has resulted in an appreciable mortality rate even among the optimisitically begun special applied journals. Nevertheless, it is easy to see that there has taken place a certain Russianization, in the form of appeal to immediate service to the proletariat – which is perhaps a product of competition with Russia. The period of flourishing of "parapsychologists" was understandably difficult for the launching of *MECR*, which adddresses itself to complex methods and deeper concepts. A typical instance is that the development of *P*-technique, which, with lead-and-lag analysis is actually the clinicians' only method of objectively establishing dynamic connections in the dynamic lattice of the individual patient, has, outside the pages of *MECR*, lacked any attempts at its refinement.

But here it is important to see the period as a general historical movement and to evaluate its scientific influence. It is quite evident, from a survey of papers and meetings, that since 1970 it has begun to change the general direction and scope of research. Factor analytic studies, for example, have increased perhaps tenfold, and have been accepted in journals in personality, clinical, and social psychology which previously regarded them as either to technical or "non-experimental." It is a sad "aside" on this movement that many papers of poor "computer exercise" quality have been accepted by editors without the depth to evaluate them. But the majority have at last brought out the "significant latent variables" in general areas, hitherto lost in a forest of speculation. On the practical side, clinicians and occupational psychologists have been supplied with weights for behavior specification equations, for discriminant functions, and for canonical correlations which give exactitude to their diagnostic and selection practices.

In terms of effects at a deeper theoretical level we may instance the change by John Nesselroade, Paul Baltes, and others of the theoretical formulations in developmental psychology, hitherto superficial in analysis and concepts. One may note also the location of the number and nature of psychological states, by *dr*- and *P*-techniques. In abilities there is the differentiation of fluid and crystallized intelligence, by John L. Horn and Jack McArdle, in place of Charles Spearman's "*g*." In perception we have precise models for attribution theory. There are studies of roles and attitudes; cluster analyses; information structure analyses; and analyses of mouse and rat learning. The higher order structures in personality are well explored; decision theory is formulated dynamically; genetic and enviromental effects are factor analytically separated.

Yet Joseph Royce, in a guest editorial following by ten years my own of 1966, still ventured to ask, "Have we lost sight of the original vision?" He cited a still overwhelming proportion of methodological and statistical contributions, for example, on multivariate scaling, instead of the two-handed use of bivariate and multivariate methods in pursuit of substantive psychological theory which I had advocated. He feared, "It is difficult for a single researcher to combine the necessary skills and knowhow of both approaches." But it appears his criticism went deeper, to the absence of theoretical developments in all the contributions, for he finally asked, "Where is the 'integrated man' . . . whose interest will encompass [breadth of method] in the service of evolving substantive therory)? . . . My reading of the original intent of SMEP was that a high proportion of the membership would be 'integrators'." This is a sad comment from the head of the Center for Advanced Theoretical Psychology, for we have undoubtedly to admit its truth.

Perhaps in the development of analysis of variances into person, situation, and such components, and the ensuing profiles of situations we have introduced some broader theory. Certainly we have done so in the development from "behaviorism" into structured learning theory, with its account of the rise of dynamic structures and the rewarding role of structures in the learning equation. SMEP has had to spend a generation in the development in psychology of men with the requisite technical skills. One hopes that the second generation will bring the broader theorists—the wise men growing out of the clever men.

In this connection the "coming of age" of SMEP in 1981 saw a portent—the rewriting of the 1966 *Handbood of Multivariate Experimental Psychology* under the editorship of Nesselroade. A comparison of the scope of the manuscripts that I have seen, with those in the 1966 edition gives a better answer to Royce's doubts. In this "bible" of SMEP it is like a New Testament added to the old, and its publication has carried us to the domain to which SMEP has aspired.

The Internal History of the Society of Multivariate Experimental Psychology

I have attempted to describe the situation in psychology that led to the formation of SMEP, and the impact it has had on research developments in the past twenty-five years. However, these results were obtained only through the interaction of persons, and to most people history is incomplete without the drama of these interactions. Since I write of the birth of a society now only a generation old I can do so only at the cost of some embarrassment to people now living; but hopefully they will forgive this in the interests of an attempt at a true history.

The process of birth of the Society in 1959-1960 was looked upon by me with trepidation, since I have always been a "back-room laboratory researcher" generally uninterested and unskilled in politics. But the scientific need was so great that I threw myself wholeheartedly if awkwardly into setting up the necessary machinery. I had perhaps sufficient scientific standing to go to the leaders in the field—Guilford, Tucker, Cronbach, Horn, Paul Horst, Green, Tatsuoka and others in America, and Eysenck, Warburton, K. Pawlik, and K. Schneewind in England and the Continent. I wrote them outlining the scheme as persuasively as I could. But, as prominent individuals frequently will, they expressed rather diverse views on the desirability and the direction of such a society. What held the Society together through its accouchement was a set of possibly statistically second rank men who were stubbornly possessed on vision. I think particularly of Sells, Merrill Roff, Charles Wrigley, John W. French, Horn, W. H. Holtzman, Fruchter, Robert C. Tryon, Cartwright, and Kenneth Howard, who would meet over a drink and thrash out essentials. At the end it turned out with them that the main features of the constitution seemed common sense. Some doubted the practicality of my aesthetic appeal for meetings in rural beauty spots, and my idea of resignation at sixty-five (with possibility of re-election) to keep the society young, was dismissed as slightly brutal. However, this group held up against the criticisms of a new society, lightly thrown out by the bigwigs, and so a constitution was drafted to put before a second meeting of the Society called in the early fall of 1961.

That constitution included: the aim of aiding research and teaching; the restriction to a total of sixty; the aim of recruiting new members from younger people; negligibly small dues (then $5 per annum); an executive committee of six persons—secretary, treasurer, annual meeting coordinator, and so forth; the annual meeting of three days each November, at a quiet place; assumption that any paper offered for reading by a SMEP member would be acceptable; recognition of need for a committee to study the teaching of multivariate concepts in psychology departments; and recognition of the need for a journal of multivariate experimental psychology (analogous to *JEP*). I added the last because area journals rejected as "too technical" the best multivariate contributions to their area, and substantive articles were forced inaptly into "math-stat" journals. For the rest the constitution had usual features, and it was passed as acceptable at the 1961 meeting. It was re-examined and revised in trifles at the 1966 meeting.

The list of Presidents of the Society over the years has covered the following distinguished contributers:

1960	Raymond B. Cattell
1961	" " "
1962	Saul B. Sells
1963	Charles Wrigley
1964	John W. French
1965	Benjamin Fruchter
1966	Chester W. Harris
1967	Robert C. Tryon (succeeded at his death by Merrill Roff)
1968	Donald W. Fiske
1969	Jacob Cohen
1970	Maurice Lorr
1971	Warren T. Norman
1972	Desmond S. Cartwright
1973	Harry H. Harman
1974	Lewis R. Goldberg
1975	Douglas N. Jackson
1976	John L. Horn
1977	Goldine C. Gleser
1978	John M. Digman
1979	Peter M. Beather
1980	Jerry S. Wiggins
1981	Jack Block
1982	J. Douglas Carroll
1983	Norman Cliff
1984	William Revelle
1985	Henry F. Kaiser
1986	A. Ralph Hakstian
1987	Andrew L. Comrey
1988	Willim Meredith
1989	John C. Loehlin

It will be recognized that substantively these cover well the areas of personality, social, clinical, industrial, educational, and psychometric psychology. The obvious gap is in learning theory and perception—the "process" area. But at this time there was very little impact of multivariate methods on the closed bivariate ranks of learning experimenters, with the slight exception of Cattell (1966), Allen Fleishman (1967), and Tucker (1966). It is a gap that is only now filling up with structured learning theory, attribution theory, and so on.

It was only after four years, in 1964, that the Society passed after a year's discussion a motion to produce its first journal, which finally appeared with its first volume in 1966. There was obscure opposition to this from Warren Norman, Joe Ward, and Lew Goldberg, but eventually the motion passed. The Editorial Committee was Cartwright, Merrifield, Messick, the present writer, and Sells. The first named did a conscientious, resourceful, and decisive job as editor for four years, and the last assumed the role of publisher through the Press of Texas Christian University, a job which he and his wife Helen Sells have supervised in exemplary fashion to this day. Indeed the sane growth both of the journal and the Society itself owes a lasting debt to Saul Sells. Even in its first years it was well received abroad, having, for example in 1967, some sixty subscribers in Germany, in the United Kingdom, and in Japan.

The editorship passed to Donald Fiske in 1972-1973, and then for ten years to Fruchter, until Ralph Hakstian took over in 1983. All of them proved excellent and attentive editors, attempting in the midst of an excess of statistical and methodological papers to preserve the Society's aim of dealing mainly with substantive findings. In an attempt to strengthen the latter the present writer proposed having whole issues devoted to one substantive area, and this was done for a clinical psychology number. But the flow of papers thereafter forced Cartwright back to the open system.

The range of substantive fields included in the first volumes was very wide; yet one may perhaps mention at random from these volumes articles by Guilford and Hoeffner such as "Sixteen Divergent-Productive Abilities"; John Hundleby's "The Trait of Anxiety"; and S. G. Vandenberg's genetic "Twin Study of Spatial Ability," as indicators of the range of substantive fields covered.

It was not until 1973 that publications in the clinical field, where multivariate methods are especially needed, reached a level calling for a special, second journal. This was not an official SMEP journal, but was launched on a shoestring by Pierson of Southern Oregon University and, after its first volume in 1974, very nearly came to grief. This gallant attempt was rescued in 1976 by Burdsal, The Institute for Personality and Ability Testing, and the University of Wichita, with Cattell, Eysenck, Guilford, S. E. Krug, and Paul Meehl as the editorial policy board. Nevertheless, the fact that it came out in a general retrenchment period has meant that Multivariate Experimental Clinical Research has had to charge for its pages, and has grown more slowly that *MBR*. *MBR* also began as a page-charging journal, but with gifts of $200 from many of the members and the skillful management by Saul and Helen Sells it was in 1976 able to forego those charges. If clinical psychology makes up its mind to become a science, however, the future of the MECR journal looks favorable; for its clientele, already in several countries, is bound to increase enormously.

In 1966 the first regional SMEP group, in the Midwest, directed by Howard and Michael Black, at Chicago, made its debut. It quickly reached a membership of over forty, some members being, as planned, also in the national SMEP. This was quickly followed by a Southeastern branch at Atlanta and by a European branch which met in Brussels in April 1963, and at Oxford a year later under the direction of Warburton and two years later under Pawlik of Hamburg University. Among the speakers were Warburton and J. N. Butcher. Cells were discussed for the East and West coasts and for Canada (under Douglas N. Jackson) but talks, especially by Merrifield 1970 in the East and Pierson, 1971 in the West, did not lead to action. Meanwhile a very active branch was started (1966) in the Southwest, under L. Bolton and Burdsal, and another in Hawaii with D. Blaine as first secretary-president. Some inherent instability seems to beset most of these, mainly perhaps from lack of support by the central SMEP, so that meetings sometimes remain uncalled, except in the Midwest under Howard and the ever-energetic Southwestern unit, with its own journal. The regional groups have fallen naturally into a practice avoided by the national group of arranging their meetings to coincide with the regional American Psychological Association (APA) meetings, which has seemed successful. By 1971 the Midwestern group had a lively membership of fifty-four active members about 10 percent of whom were also in the national SMEP. These cells were run on the same constitution as the national SMEP.

The attitude of the national SMEP seems to have been one of a hen with an excess of chicks. A response from Cartwright during his presidency to a letter of mine urging more formal connections said frankly, "I enjoy SMEP just the way it is and do not want to be bothered with regional groups. Informal groups can enjoy a monthly meeting as Ray says. . . . Recognition and organizing by SMEP will only make more work" (30 January 1964). Since none of these very busy researchers and teachers came forward for that organizing, the regional societies have sometimes subsided for lack of stimulus. However, at the 1970 SMEP meeting Eber reported that "both the Mid-western and the South-eastern regional societies were alive and well" (and the South-western certainly was). In analyzing causes of weakness in the regional societies, notably the East and West coasts, one must bear in mind that with the high admission standards it is not possible regionally to get the top people wholely from the ranks of psychologists. It must be recognized that, as happened in Hawaii, it is not easily possible to get a group with a common substantive interest to supplement the methodology. The Hawaiian experience was that psychologists, educators, sociologists, political scientists, and so on, had sufficient differences even in methodological problems to make if difficult to find papers of really common appeal.

Within the main SMEP, in 1975, it was felt that a stimulus in the way of formal recognition of outstanding contributions would be in order. The Cattell Award, materially a small reward in the form of a supply of key books and a certificate, was supported for the first few years by Cattell, and later, by generous contributions and common consent of the membership, by the Society. It is awarded each year to the nominated individual, within or outside the Society, offering the most signal contribution. Debate has arisen regarding the "under forty" rule, but it is generally agreed that it should reward performance by the young. The recipients have been Peter Bentler and John Nesselroade (1972), John Horn (1973), Nancy Hirschberg (1974), Paul Baltes (1975), A. Ralph Hakstian (1976), Myron Wish (1977), Patricia Cohen (1978), Gene Glass (1980), Robert Sternberg (1982), and Jack McArdle (1987). No awards were made in 1979, 1981, or 1983.

It can be reliably said that the Society itself has run remarkably smoothly and efficiently. Nevertheless, items in the constitution have been questioned from time to time. Fruchter raised the question whether instead of the plan for regional "cells" there should not be a split of the main Society (sixty members in each division) according to interest areas, but this was rejected. The "over 65" retirement rule finished up as an "inactive" and "active" listing of members, the latter consisting of those who had not attended meetings or given a paper for three years. In 1981 Bentler proposed increasing the membership from sixty to seventy-five—an item still under debate. In 1978 it was considered time to incorporate the Society as a "non-profit" organization, and after a final tidying of the constitution this was carried out in 1979. in retrospect, the fears expressed at the very birth of the Society seem now distant and irrelevant. It is true that societies easily become rivalrous and jealous of their territory. It was feared, in particular, that opposition would come on the one flank from the classical Society of Experimentalists, originally founded by Titchener, and said by some to be moribund in a bivariate tradition, and, on the other flank, from the Psychometric Society. It is true that some members of the latter were opposed, and wrote to suggest that SMEP was superfluous. But the point was made that *Psychometrika* published largely method papers, and also that SMEP followed themes on processes as well as individual differences.

The democratic process was always faithfully followed in the development of SMEP and it turned out that the liveliest disagreements occurred over the Society's journal, rather than the Society itself. A few differences have been mentioned. One occurred again when it was proposed that *MBR* should issue monographs, but it settled down to producing a valuable series.

Another debate occurred over its rejection rate, only some twenty articles being accepted out of about eighty submitted each year. Another occurred over my proposal for special issues, devoted to the bearing of multivariate research each on a special field. As indicated above only a clinical issue came forth, the remainder—social, industrial, learning, and so on— being vetoed by the editor on grounds of complexity of procedure. In Cartwright's editorship there was also editorial opposition to any notion of writing a special review of his article on the spirit and aims and achievements of the Society. After a thorough democratic overhaul by almost everyone in the Society such a review finally appeared in 1966, actualy evoking some applause properly shared by all.

An unsettled issue in *MBR*—but one shared with most scientific journals—concerns the reliability and the anonymity of peer consultants. The unreliability of reviewers' judgments is a frequent problem. The editor might often do better to make a decision on his own. One SMEP author writes that two reviewers were run into one sentence in "This is a rare and valuable study, clearly expounded; the way it is written indicates that the author is an illiterate idiot." I once submitted a multivariate experimental article to the *Journal of Experimental Psychology,* when Arthur Melton was editor and had it returned with "This is not an experiment." I dealt with the definition of an experiment in the *Handbook of Multivariate Experimental Psychology,* and felt it unnecessary to resubmit it. A major problem with consulting editors is that some—especially with a dislike for the given author—take the opportunity of discharging years of animosity in hysterical phrases, which actually drive sensitive younger people out of the field. My first suggestion to *MBR* was that all reviews should be signed. Otherwise it is an ambush; for the consultant always divines who the "anonymous" author is. I still hold to this, as I think it would cure half the trouble; but the collective wisdom of SMEP has so far been against it.

The reception of SMEP, judged when it "came of age" in 1981, needs deeper investigative probing than I have been able to give it. It is unquestionably respected, but a great many psychologists are ready to leave it and its works outside their ken. The restriction of membership does not seem to have evoke "envy, malice and all uncharitableness," and should not, for the regional cells are still not full. Within the Society the restriction is clearly appreciated as the origin of the free, competent, and frank discussion—and humor—which, all agree, is so different from an APA meeting.

Nevertheless, if its influence is to be what was intended, it needs to do more than fashion an elite, from which the principles can "filter down" from masters to pupils. In my opinion the growth of "regionals," properly integrated with the main Society, could do much of this. It is surely evident to everyone now that the APA is not primarily a scientific society, but a necessary political body, akin, say, to the American Medical Association. Some scientists in psychology have withdrawn into their own societies—notably the Psychonomic Society and SMEP. The forty-seven specialist sections of the APA are partly applied and partly substantive, carrying out psychology at a level comprehensive to the 80,000 members and their social associates. It represents the impasse of the most difficult science known to man. The separation of "shock troops" to make a deal assault on discovery, in societies like to Psychonomic and SMEP, is an inevitable constructive development. And today they partly well represent, respectively, the classical bivariate and the statistical multivariate branches that are possible.

The membership of SMEP has naturally changed a good deal in a generation. So have its topics, moving from individual differences to learning and perceptual processes and from factor analysis to structural equations, genetics, and much else. But through its ranks have passesd the most brilliant hundred or more in the field. It would take much research to find an outstanding multivariate experimenter who has not at some time been a member of the Society. Though possessed of its own two journals its influence has altered the content of most APA journals, few of which now reject an article highly relevant to their field on the ground that their readers will not understand it. A good illustration is in the area of developmental psychology which in a generation has passed from the discussions of nuresmaids and social workers to the beautiful technical conceptions of such writers as Nesselroade (editor of the new *Handbook of Multivariate Experimental Psychology*), Baltes, Bentler, Warner Schaie, McArdle, and many younger students. Learning and psychotherapy may next move to more comprehensive concepts emerging from the recent work of SMEP members and their students. SMEP has a short history, but so dynamic as to justify the hope and the expectation that it will have a long future.