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STANLEY SMITH STEVENS

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A Biographical Memoir by

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STANLEY SMITH STEVENS was born in Ogden, Utah, to Stanley and Adeline (Smith) Stevens. He attended Mormon schools in Salt Lake City and after being graduated from high school in 1924 was sent on a three-year mission to Belgium and Switzerland for the Mormon Church. He returned in 1927 to enroll in the University of Utah and in 1929 transferred to Stanford University, where he received the A.B. degree in 1931. After two years of graduate study, he received his Ph.D. degree in psychology from the Department of Philosophy, Harvard University, where he served under E. G. Boring as assistant in psychology from 1932 to 1934. The following year he spent studying physiology under Hallowell Davis at the Harvard Medical School, on a National Research Council Fellowship; in 1935–1936 a fellowship from the Rockefeller Foundation enabled him to become a Research Fellow in physics at Harvard.

Psychology had achieved departmental status at Harvard in 1934, and in 1936 Stevens accepted a position as instructor in experimental psychology. He was promoted to assistant professor of psychology in 1938, gained academic tenure as associate professor of psychology in 1944, and became professor of psychology in 1946. In 1962, at his own request, his title was changed—he became "the world's first Professor of Psychophysics."

Stevens spent much of his boyhood in the polygamous household of his grandfather, Orson Smith, in Logan, Utah, surrounded by cousins of all ages. It was a hard, frontier style of life, but he later wrote that "the hardships of the adults were mostly lost on us children." It ended in 1924 with the deaths of both parents and his subsequent departure on the mission to Belgium. In 1930 Stevens married Maxine Leonard, and in 1936 they had a son, Peter Smith Stevens. Shortly afterward Maxine was overwhelmed by a postpartum depression that devastated their lives; she returned to Utah to live with her parents and died two decades later. In 1963 Stevens married Geraldine Stone.

In 1940, at the request of the U.S. Air Force, Stevens and L. L. Beranek created joint laboratories at Harvard to study the effects of intense noise in military aircraft and the possibilities of reducing it. Stevens was director of the Psycho-Acoustic Laboratory; Beranek, of the Electro-Acoustic Laboratory. The Psycho-Acoustic Laboratory was housed in the basement of Memorial Hall, a monstrous Victorian–Gothic building erected in 1875 as a dining hall. The laboratory began in the old furnace room; its rapid expansion into the abandoned kitchens was a project that occupied much of the director's attention—much of the work was done with his own hands. During the first year, young adults were exposed to 115 decibels of noise for periods of seven hours, during which a battery of psychomotor tests was conducted. Their performance was not impaired by noise, although they suffered temporary hearing losses. The major effect of noise was to make voice communication impossible, so the program of the laboratory shifted to testing and redesigning components of intercom and radio systems. To carry on this work, Stevens assembled a large and distinguished staff and welded them into a highly effective team; by the end of the war, the Psycho-Acoustic Laboratory had expanded to some fifty people.

The laboratory continued after the war with a reduced staff, and in 1947 Stevens brought Georg von Békésy to the United States to become a member of it. The remaining space in the basement of Memorial Hall was remodeled under Stevens's close supervision in order to accommodate the Department of Psychology in 1946, and from 1949 to 1962 Stevens served as director of the Psychological Laboratories as well as of the Psycho-Acoustic Laboratory. Stevens rechristened his own laboratory in 1962 as the Laboratory of Psychophysics. In 1965, over Stevens's strong objections, the laboratories and Department of Psychology were moved again, this time to William James Hall, which had been built for the Department of Psychology and Social Relations.

The accomplishments of the Psycho-Acoustic Laboratory during the war brought well-deserved credit to its director, and during the years immediately following the war Stevens was active in the bureaucratic affairs of science at the national level. He was consultant to the Research and Development Board from 1946 to 1952, Chairman of the National Research Council Division of Anthropology and Psychology for three years, and recipient of a Presidential Certificate of Merit. His interest in these activities declined after 1952, however, as he increasingly preferred to devote his major efforts to his own research.

Stevens was a member of the American Philosophical Society, the Society of Experimental Psychologists, the Acoustical Society of America, the Optical Society of America, the American Psychological Association, the Eastern Psychological Association, the American Physiological Society, the Psychonomic Society, the American Association for the Advancement of Science, the Philosophy of Science Association, the Society for Neuroscience, the American Academy of Arts and Sciences, Phi Beta Kappa, and Sigma Xi. His awards included the Warren Medal of the Society of Experimental Psychologists in 1943, the Distinguished Scientific Contribution Award of the Amer-

ican Psychological Association in 1960, the Beltone Institute Award for distinguished accomplishments as an educator in 1966, the Rayleigh Gold Medal Award of the British Acoustical Society in 1972. He was elected a member of the National Academy of Sciences in 1946.

He died quietly but unexpectedly in his sleep on January 18, 1973, while attending a meeting of the Winter Conference on Brain Research in Vail, Colorado. He is survived by his wife, his son, and three grandchildren.

Such are the facts. It is probably worthwhile to summarize them for reference purposes. But such facts are little more than the skeleton of a man's life. Like most skeletons, they give barely a hint of the man himself or what he suffered and accomplished.

In some "Notes for a Life Story" written in 1970, Stevens commented that his career "exhibits no plan or purpose, no over-reaching strategy, only tactical maneuvers brought on when circumstance has confronted desire. A series of accidents, in fact. Any man's life builds on a succession of accidents. That explains only part of it, however, for among the chance encounters there are some that take effect, whereas against other exposures a person stands as though inoculated with some natural antibody." As chance would have it, those encounters that took effect on Stevens thrust him into at least four separate careers. There was Stevens the administrator of laboratories. There was Stevens the professor and educator. There was Stevens the philosopher of science. And there was Stevens the scientist. The overreaching design that his friends can see in his life grew out of his art in blending these careers, using each in the service of the others.

ADMINISTRATOR

My introduction to Dr. Stevens occurred in August 1942. I was a new graduate student, interested in speech and hearing; the teacher who had sent me to Harvard recommended me for

employment in the Psycho-Acoustic Laboratory. I was directed to Dr. Stevens's office and found him in what I came later to recognize as a characteristic posture—legs extended, ankles crossed, feet on corner of desk. As he sat up and turned to greet me I saw a handsome man in his mid-thirties—tall and muscular, round-shouldered with long arms and large hands, a 4-4-4 on the somatotype scales; a long face with a high forehead and excellent features; wavy black hair and a natty moustache; an open, level gaze and an expression that in repose seemed sad, even disapproving, but could break into an irresistibly winning smile. When he wished, he could be one of the most affable people I have ever met. I remember leaving that brief meeting completely charmed and excited by the prospect of being paid for what I wanted to do anyhow.

In appearance he could have been a matinee idol, but the idea of S. S. Stevens as an actor would strike anyone who knew him as absurd. He could never have spoken lines from another's script. He was his own man, if ever anyone was. I did not actually join the laboratory until eighteen months later; by then I had learned that my first impression was only one side of a very complex personality.

Stevens was a primitive—he had in him the force of Nature. When the clouds gathered and thunder rolled forth, he was as little concerned as Nature for who might be caught in the storm. When the skies cleared and you found to your surprise that the landscape was still where it had been before, the day could be filled with sunshine. Those who could not weather the storms disliked him, and even those who admired him often found him difficult to work with. When he was seriously interested in a problem, he could move forward only at full speed—sometimes he ran over you. But those willing to stick it out were greatly rewarded.

He was not really as difficult to get along with as many seemed to think. It was a matter of understanding his ways. Sometimes he would appear at the door of your room and bark,

"Know what you're doing?" Once you recognized this as his way of saying "good morning," the what-have-I-done panic subsided. Stevens's gruffness protected a basically shy person.

Other insights into his mannerisms took longer to come by, however. For years I thought him inordinately secretive, often carrying his reluctance to give out information so far as to withhold his decisions from those directly affected by them. I eventually learned that his natural retentiveness was only part of the reason. An equally important part was that often he had not yet made the decision one thought he was withholding. As he said of himself, "Decision never comes easy to me, and trying to decide to do something often tears me apart more than doing it." He had a great interest in the stock market, and all his friends with any capital sought his advice on investments; but he himself did little trading. "In order to be successful," he said, "you have to average two correct decisions on each trade. I am congenitally unsuited to the making of even one decision—correct or not."

Administrators are decision makers. A man who is "congenitally unsuited" to making decisions obviously cannot be a good administrator. Stevens knew that. Indeed, he often used it as proof of his incompetence when he wanted to avoid administrative responsibilities. He had an intense dislike for administration—for making decisions, for accommodating superiors, for compromising his own opinions, for interrupting his work to cope with the crisis of the day.

In truth, however, he was a superb administrator. His methods might not work elsewhere, but for the head of a wartime laboratory they were remarkably effective. If success is to be measured in terms of assembling a good team of scientists, using them wisely and keeping them happy in their work until they do better than they know how, then Stevens was a very successful administrator. He was an astute judge of intellectual horseflesh. He was a wise man with broad experience, so when he did make a decision, however painful the process

may have been, it was usually the right decision. And because decisions did not come easy, he was never tempted to overcontrol. He lavished his concern on good equipment and an optimal arrangement of the laboratory environment. He painstakingly edited or rewrote reports. He set an example of dedication, working fourteen hours a day. These may not be practices recommended in manuals on how to become a successful administrator, but they worked for him. His was not the strategy of an executive, but of a patriarch.

The laboratory was his famliy, and members were given the duties and privileges of siblings, nephews, or cousins. The head of this extended family was concerned for the welfare of his kindred, and he rewarded them or disciplined them for their own good and the good of the group. This family provided not only for work, but also for the social life of its members—dinner at the Faculty Club; in the early days, a group foray to Boston's Chinatown or three carloads of incompetent but enthusiastic beginners invading the Fresh Pond Municipal Golf Course; later, weekends at "the farm" in New Hampshire, with maintenance work in the summers and skiing in the winters. At the time it seemed perfectly natural and fulfilling.

As in any family, everyone was on first-name terms. It never occurred to us to call him "S. S. Stevens"—he was "Smitty" to everyone. Anyone who tried calling him "Stanley" was lucky to be merely ignored.

Smitty was a close man with a dollar, and he spent his laboratory budget as if it were his personal checking account. Younger staff members, frustrated in their hopes of receiving what they regarded as deserved raises in pay, could be heard to call him miserly or worse. When confronted on the subject, he would explain that if a staff member's salary were too high, he would be priced out of the market when the time came to leave Harvard. Certainly a frugal childhood and the lean depression years had left their mark on Smitty, as on most of his peers. But in his case, it went deeper than a mere respect for

money. Retentiveness was a personality trait. He disliked discarding or replacing personal possessions. He disliked lending books. He liked documentation and record-keeping. His memory was excellent and detailed. He held to his opinions regardless of their popularity. He was intensely loyal to his students and collaborators. He retained his identity as a Mormon of frontier stock. He saw variability as noise, masking the central invariances of both life and science. Even his contempt for "the seductive myth that experience writes on an empty slate" was consistent; genetic endowment is something you can hold onto. He was instinctively conservative, in the true sense of that much-abused term.

Smitty expected a full day's work, and to insure that he got it, he often would wait near the door of the laboratory in the morning to intercept late arrivals. One rainy morning during the early days of the Psycho-Acoustic Laboratory, a staff member who had arrived late hurriedly hung his hat where water dripped from it onto the Webster's dictionary below. In a rage, Smitty threw the offending hat to the floor and stamped on it, loudly berating its tardy owner. For many months, the scene was recounted in whispers by the awed onlookers.

Such episodes were exceptional, but no one ever doubted that the director was intensely concerned about every detail of the work that went on in his laboratory. Usually the battles were intellectual. Smitty would not tolerate fuzzy thinking, and his blunt, honest criticism wounded many tender egos. The fact that he was usually right didn't make it easier to take. Anyone willing to play the paternal role is bound to inspire ambivalence, but at least you knew he really cared about you and your work. His combination of wisdom, shrewdness, and intelligence, coupled with his training as a debater in school and college, made him almost invincible in arguments; but if you ever convinced him that you had a better idea, Smitty respected you for it. He could be as severe and critical of himself as of others.

His recipe for administrative success cannot be generally recommended. Even he would not have been so successful with his methods had it not been for the organizational gifts of his secretary and his administrative assistant, Didi Stone.

The Psycho-Acoustic Laboratory continued on a reduced scale after the war. In the early fifties, when Smitty lost interest in administrative matters, he probably would have been willing to reduce it to a one-man show in support of his own research. But Georg von Békésy's beautiful research depended on support from Smitty's grants, so he continued. Although Harvard could never find a way to give Békésy a faculty appointment, Smitty believed he was a great scientist and made every effort to provide space, facilities, assistants, and money for his work. When his judgment was vindicated by the awarding of a Nobel Prize in medicine to Békésy in 1961, Smitty seemed more elated than the recipient.

But the laboratory continued to shrink. Smitty's career as an administrator had ended even before 1962, when a stubborn president of Harvard forced him to step down from the post of director of the Psychological Laboratories and then in 1965 compelled him to leave his beloved basement, shaped for over a quarter of a century to meet Smitty's every need. It was disgracefully ungenerous treatment of a senior professor who had contributed so much for so long to make Harvard's Department of Psychology one of the world's best. Thus, this facet of his life ended on an unfortunately bitter note.

TEACHER

It may seem anomalous that a man could base a distinguished career as an administrator on his dislike of making decisions, but that pales into insignificance beside the anomaly of Stevens's accomplishments as an educator.

As a young instructor at Harvard, he taught the laboratory course in experimental psychology and sections of Boring's introductory course. Later he added a course in mathematics

for psychologists. But he disliked lecturing and was a mediocre classroom teacher. Gordon Allport, chairman of the psychology department, opposed Stevens's promotion in a long letter that detailed his shortcomings, citing particularly his open disparagement of teaching. Another of Allport's objections was the aversion shown him by some of the students. President Conant discussed the letter with Stevens, who later wrote, "Allport was right, of course, for in neither temperament nor appearance am I the outgoing teacher . . . I told Conant, as I had already told Allport, that I would teach my courses faithfully, but to enjoy standing before classes was beyond my power."

Smitty summarized his educational philosophy in one sentence: "Anyone worth teaching doesn't need to be taught." As with all his strong opinions, there was a well-developed network of arguments linking this sentence to his more general views of life and people. He applauded Boring's proposal that the department should abandon undergraduate teaching entirely. Only the most outstanding students should be admitted to the department, for graduate study; any who did not fulfill their promise should be asked to leave at the end of the first year. Graduate education should be based on seminars and research apprenticeships. This is very close to the system followed at the Rockefeller University, and I know how well it works; but it was totally unacceptable at Harvard, both to the administration and to Stevens's colleagues in psychology.

He predicted that if his colleagues persisted in giving undergraduate lectures on popular subjects, psychology would attract students who would change it from a science into socially relevant but intellectually empty do-goodism. When in his opinion that prediction had been fulfilled, his reaction was to refuse to call himself a psychologist. Stevens tried to coin a new name for the old-time science and helped found the Psychonomic Society; but he decided that he preferred the title "psychophysicist" for himself. The chairman of the department used

an increase in undergraduate enrollment in negotiating for increased support from the dean, but such arguments held no appeal for Smitty. His unsuccessful efforts to reform Harvard were a continuing source of frustration to him.

Stevens believed firmly in the primacy of nature over nurture, in the inheritance of intelligence, in the dependence of personality on body type, in the genetic basis of schizophrenia. These opinions contrasted sharply with those of B. F. Skinner, who was invited to return to Harvard, very much on Smitty's initiative, in 1948. Neither Skinner the environmentalist nor Stevens the nativist could carry the day, so graduate education in the department continued much as it had in the days of Boring and Allport.

Smitty's responsibilities as a lecturer were interrupted by the war; they never resumed. He offered seminars in mathematical psychology and sensory psychology and until 1965 focused his efforts on one semester of the proseminar that was required of all first-year students. That semester concentrated on the history of psychology and on sensation and perception. It was a punishing course, with 150 pages of technical reading to be covered each week, and highly competitive examinations. Students gave weekly reports, but the professor was never reluctant to come down into the heat of debate and straighten them out. It was like an initiation ritual—no one enjoyed it at the time, but afterwards most seemed glad they had gone through it. And some students discovered that their formidable professor was not as dangerous as he sounded, that the brand of science he practiced was really fascinating once you got the hang of it.

But the classroom was merely a recruiting area. The real teaching went on elsewhere. "Actually," Smitty wrote, "two forms of teaching give me great joy: the joint endeavor of laboratory apprenticeship, and editorial give and take. In those two ways I seem always to be teaching. But deans count you

at work only when you stand before a group with your mouth moving." Working with Smitty on an experiment—setting up the equipment, running each other as judges, pulling people out of the halls to serve as subjects, plotting the data, and arguing what they meant—was a rich experience for a young student. When it came to writing up the results, the interaction intensified. Stevens was a master of clear, expository prose; his own was marred only a by tendency, usually curbed, to become slightly more flowery than necessary. He liked the sound of a well-rounded sentence.

The amount of time he was willing to devote to a word-by-word review of his students' writing was extraordinary. He had a gift for acting dense when it served his pedagogical purpose. How often I remember this exchange:

SSS: "What does this mean?"
GAM: "Oh, Smitty, you know what it means."
SSS: "Of course I know what it means. But look at what it *says!*"

And then would come the rephrasings. Behind Smitty as a critic and editor stood E. G. Boring, the consummate stylist. Both knew that the job of a scientist was not complete until the results of his experiment had been communicated. Both believed that an author should do all in his power to save the reader's time. And if they judged a student worthy of tutoring, both felt that the most valuable skill they could give him was an ability to phrase his thoughts clearly and briefly. Boring worked alone in his study and sent you five pages of single-spaced commentary. Stevens called you into his office and made you work through it with him. Both had their most direct effects through their editing. You cannot write clearly unless you think clearly, and their lessons in clear thinking were more valuable than all the psychological facts they taught you.

Although Smitty did not include it on his list of enjoyable forms of teaching, he was constantly instructing you about something. Whether he was guiding you down an advanced slope on his short skis, taking you to see a Harvard commencement, unpacking a newly arrived piece of equipment, or driving a new wellpoint at the farm—whatever you did with him was accompanied by a steady but unobtrusive sharing of knowledge and opinion. He loved to teach; it gave him a way to overcome his shyness and to reach out to people with gifts in hand.

His teaching was not limited to graduate students. Some of his best pupils were the postdoctorals who came year after year. He spent three years editing the *Handbook of Experimental Psychology* when he could easily have done it in one if he had not felt compelled to educate even the most distinguished contributors. In his own way, and for the limited audience he commanded, he was one of the most effective teachers of his generation.

A strong personality invites analysis, and those who had to find ways to coexist with Smitty found him a fascinating topic for speculation. His style of life, his independence of fashion, his tastes and outspoken opinions made him a thing apart, even in a community of individualists like Harvard. We sometimes debated whether he was an intellectual—he was more a man of action and argument than the intellectual stereotype seemed to allow. Consistent and well-established opinions guided him where others preferred to chat about current best-sellers or the latest intellectual fads. We knew that E. G. Boring, with his great historical knowledge, was an intellectual. G. W. Allport was a paradigm intellectual, so graceful and flexible in style and mind as to seem positively slippery at times. He was always prepared to shift the subject when irreconcilable differences loomed ahead; Stevens sought out the differences and tried to overpower them. Two such men could not long cooperate in

one small department, and in 1946 Allport joined with Clyde Kluckhohn, Talcott Parsons, and Samuel Stouffer to create the Laboratory and the Department of Social Relations.

There were many benefits to Harvard from the new department, but some of us felt it was as much an accommodation to personality differences as an innovation in teaching and research. Smitty felt that the fission of the department gave the real scientists a chance to concentrate on the serious business of psychology. He never really forgave those of us who worked to reunite the interesting problems of social psychology with the scientific methods of experimental psychology.

In spite of his uncompromising opinions of the ways his colleagues undertook to teach psychology, Smitty was not really opposed to teaching. He was merely opposed to being asked to do it in any style but his own.

PHILOSOPHER

Smitty once told me that someone had discouraged him from studying philosophy because he did not write well enough. The remark stuck in my mind because it conflicted so sharply with my view of him as an excellent expository writer. Whether or not the evaluation was correct, Smitty abandoned any aspirations to become a professor of philosophy. The surge of interest in the philosophy of science in the 1930s, however, did provide him an opportunity to become a philosopher, a role that suited him far better than the role of professor of philosophy.

Oddly enough, the initial impetus seems to have come from a physicist, P. W. Bridgman. It is odd because Smitty's exposure to philosophy and philosophers had been considerably more than incidental. As an undergraduate at Stanford he had shunned courses in science and mathematics in favor of "the windy subjects," as he later called them; at that time, "the philosopher image seemed most congenial." As a graduate student at Harvard, he had begun to discover his eventual

vocation; but psychology was still administratively a part of the Department of Philosophy, so there was more philosophy to be studied. And from 1936 to 1940 he had to pass each day through the offices of the philosophy department on the first floor of Emerson Hall in order to reach the psychological laboratories on the third floor—it was often convenient to take a seat at lectures there. But it was Bridgman's solipsistic operationism in *The Logic of Modern Physics* that stimulated Stevens to write three philosophical essays on operationism in psychology in 1935–1936.

The problem he attacked in those papers had been set for him by E. G. Boring, who in 1932 was struggling to escape the traditional cleavage between mind and body that he had inherited from Titchener. He asked Smitty, his laboratory assistant, to read the manuscript of *The Physical Dimensions of Consciousness*. Smitty commented later that "an operational restatement of psychology's basic concepts was Boring's real aim," but at the time neither of them was able to do it. Bridgman's operationism showed Smitty the way, and, with Boring's considerable help, the three papers were written in 1935.

The argument, briefly stated, was that scientific concepts are defined by the operations scientists perform; that discrimination is the basic operation of all scientists; that psychology is the science whose responsibility it is to test and measure discrimination; and that psychology can accomplish this by analizing mentalistic concepts such as experience, sensation, and sensory attributes in terms of the operations available to study them. Thus, discrimination was to replace immediate experience as the basis of all science, and discrimination is defined as a concrete, physical, differential response on the part of a living organism.

Toward the end of the 1930s, the logical positivism of Vienna was transplanted to America, where it produced enormous ferment. These ideas enriched and reinforced Smitty's

philosophical leanings toward physicalism, and in 1939 he published a tutorial paper for his colleagues in psychology that reviewed operationism, logical positivism, physicalism, the unity of science, semiotics, the hypothetico-deductive method, and their relevance to the theoretical foundations of scientific psychology. In one way or another, he wrote, "they all assert essentially that science seeks to generate confirmable propositions by fitting a formal system of symbols (language, mathematics, logic) to empirical observations, and that the propositions of science have empirical significance only when their truth can be demonstrated by a set of concrete operations."

Smitty's interest in these ideas was not confined to any armchair; they were directly pertinent to his work as a practicing scientist. What was really troubling him was measurement. In 1936 he published a scale for the measurement of the psychological magnitude, loudness, as a function of the acoustic amplitude of the stimulus; and in 1937 (with Volkmann and Newman), a similar scale for the measurement of the psychological magnitude, pitch, as a function of the frequency of vibration. Loudness and pitch are subjective experiences, but if one rejects on philosophical grounds the cleavage between the physical and mental, then what were these measurements measuring? When a critic charged that they did not measure anything, that they were meaningless, how was one to reply?

Between 1932 and 1940 a committee of the British Association for the Advancement of Science had debated the question: Is it possible to measure human sensation? In its final report, the committee chose Stevens's scale of loudness as a concrete example, one which was said by its author to have all the formal properties of other basic scales, such as those used to measure length and weight. The members of the committee could not agree among themselves. For those who rejected the possibility of measurement, the critical argument seemed to be that there was no possible operation for adding two sensations together

comparable to the operations of placing two lengths end-to-end or two weights in the same scale pan.

The only way to meet this objection was to demonstrate that psychologists have other operations, just as objectively describable as those for length and weight, that endow subjective scales with all the desirable properties of the basic scales of measurement in physics. In order to sustain such a claim, however, it was necessary to understand precisely what the relations were in physics between the measurement operations and the properties of the resulting scales. Bridgman gave him part of the answer. Another part was to be found in physicist N. R. Campbell's broad definition of measurement as the assignment of numerals to objects or events according to rules. But Stevens's problem was to make explicit the various rules for assigning numerals, the group structure of the resulting scales, and the statistical operations applicable to measurements made with each type of scale.

At a Congress for the Unity of Science in 1939, Stevens made a preliminary attempt to classify types of scales and illustrate them by examples from sensory psychophysics. "It was a botch," he said later, but he felt he was on the right track. "I began to tabulate the various kinds of scales and the kinds of operations needed to create them. Then it became clear that each kind of scale permitted a different mathematical transformation, and suddenly one evening in Emerson Hall the picture snapped into focus—there exists a hierarchy of scales defined by the mathematical transformations that leave the scale form invariant." Consultation with G. D. Birkhoff sent Stevens to the library to learn more about mathematical groups. His final classification was revealed to the Psychological Round Table in December 1940 and published in 1941 at the next Congress for the Unity of Science. The name he proposed—"nominal" for the permutation group, "ordinal" for the isotonic group, "interval" for the linear or affine group, and "ratio" for

the similarity group—have since become so standard that many authors who use them are unaware of their origin.

The answer to his critics was now complete. Scales of measurement are to be evaluated not in terms of the tangibility of the objects or events that are measured, but in terms of the operations of measurement that are used.

Having reached this operational resolution of his original problem, Smitty's interest in the broader issues of philosophy seemed to recede. In 1940 he and Rudolf Carnap organized a monthly discussion group at Harvard on the Unity of Science, but his growing responsibilities for the Psycho-Acoustic Laboratory reduced his participation in such discussions to a sometime thing. After the war he published several articles expanding on his classification of scales, but his philosophical ideas dwindled into odd paragraphs tucked away here and there in the more popular summarizations of his scientific work.

The closest he came again to explicit philosophical pronouncement seems to have been in an article in *Science,* in which he dubbed his views "schemapiric"—a hybrid of the formally schematic and the empirically substantive. In his schemapiric view of science, words and symbols serve only the neutral purpose of implementing a schematic structure, which may be related by operational rules to an empirical structure. But it was just a new name for views he had hammered out thirty years earlier. Whatever he called it, Stevens had found a philosophy he could live by.

SCIENTIST

It is a long way from a poor Mormon household in Logan, Utah, to a professorship at one of the world's leading universities. Others strong and talented enough to pursue parallel courses to Harvard generally preferred, when they got there, to take on protective coloration from their new environment. Stevens may have envied them at times, but he could never have imitated them. The lessons he had learned along the way

were too much a part of him, too important for what he wanted to do as a scientist.

In his "Notes for a Life Story," he describes how he worked to pay his way through college. "Summers I worked for the Idaho Power Company, starting as a grunt (hole digger and lineman's helper), living in a tent in a construction camp beside a mountain stream where you almost froze to death at night. I later worked up to summer utility man. That meant that I had a new job almost every two weeks as I replaced the man on vacation, whether meter reader, waterheater installer, or night troubleshooter. Forty years later, it becomes clear that my education for science took place more in the summers than in the winters." At Harvard, when he built a laboratory, he worked side by side with the carpenters and electricians. When he set up an experiment, he worked alongside the shop man—usually Ralph Gerbrands. Skilled hands and a knack for coaxing experimental equipment to perform were valuable tools in Stevens's scientist's kit. He never forgot the skills learned at the Idaho Power Company. The tough and skeptical view this experience had given him of his fellow man wasn't wasted either.

Although at various times and in various ways he contributed to a wide variety of psychological problems, Stevens's central concern throughout his life was psychophysical measurement. This has always been a particularly seminal area of research in psychology; techniques of measurement worked out under the well-controlled conditions of the laboratory have been repeatedly generalized to the measurement of attitudes, abilities, and other topics of greater personal and social importance than the sensory magnitudes they were designed to measure. As a result, many psychologists who have worked in this field have had more interest in the methodology they used than in the results they obtained. Such colleagues were a constant irritation to Stevens, who never viewed measurement as an end in itself. He was not averse to generalizing his measure-

ment techniques beyond their application to sensory magnitudes—to the measurement of physique and temperament, the prestige of occupations, socioeconomic status, the value of money, perceptions of national power, the seriousness of offenses and the severity of punishments—but he always evaluated such work in terms of the meaning of the results, not the technical versatility of the measurement operations used to obtain them. In a subject rampant with methodolatry, Stevens's contributions were always refreshingly sensible. It was the tendency to elevate the means over the ends that eventually gave operationism a bad name among scientists, but Stevens was never guilty of this.

One of the many schisms that divide psychologists into warring camps is that between the nomothetic and the idiopathic, between the search for universal laws and the concern for individual differences. With respect to problems of measurement, it becomes a question of whether one is more interested in the first or the second moment of the distribution. There is always a distribution, of course, and psychologists have performed valuable services by informing people where they stand in it with respect to their peers. Great statistical sophistication has supported such studies of individual variability. Stevens's interest, however, was not in the variance, but in the invariences of the measurements. Elaborate statistical analysis never impressed him. "What scientific discoveries," he once asked, "owe their existence to the techniques of statistical analysis or inference?" In his *Handbook* he urged his colleagues to "cultivate a love for invariance" and to "seek uniformities in heterogeneity." He believed that "the delineation of the conditions of invariance for any phenomenon would tell us all we want to know about the matter," and that a scientist's responsibility is to provide "measures that will stay put while his back is turned." It was good advice, but difficult for many psychologists to take.

In the measurement of sensory magnitudes, Stevens's own area of central concern, tradition was against him. In the nineteenth century, G. T. Fechner had based psychophysical

measurement on the counting of just-noticeable differences (jnd's). If, for example, you wished to measure the brightness of a light, you were supposed to count the number of jnd's a person could detect between complete darkness and the light to be measured. Since the magnitude of a jnd was proportional to the magnitude of the stimulus to which it was added, this argument led Fechner to determine a logarithmic relation between the stimulus intensity and the sensory magnitude—each time the intensity is doubled, there should be a constant increment in sensation. L. L. Thurstone later provided the statistical rationale: The size of the jnd depends on inherent variability in the sensory system. The variability of any measurement is generally a function of the magnitude being measured. Hence, by observing the variability, one could infer the magnitude.

Although the argument seemed somewhat backwards, it was plausible enough to persuade psychologists for at least a century that sensory magnitudes are a logarithmic function of stimulus intensity. What bothered Stevens was that it wasn't true. The facts about differential sensitivity were true enough, but the relation between jnd's and sensation did not hold. If you ask people to adjust the intensity of one tone until it sounds twice as loud as another, for example, you find that loudness grows much more rapidly than the number of jnd's. Stevens pointed this out in 1936 when he proposed his first sone scale for loudness, based on a review of such direct estimations by B. G. Churcher. The vast disparity between the subjective magnitudes of different jnd's "is astonishing in view of the original assumption by which they were considered equal. Their integration for the purpose of obtaining a reasonable numerical scale for the measurement of the magnitude of 'sensation' is obviously not valid."

When this work was interrupted by the war, that is where the matter had to be left—as a puzzling disparity. The puzzle was sharpened by the fact that there was no comparable dis-

parity for pitch; jnd's for pitch are subjectively equal. In 1940 Stevens and Volkmann suggested that this difference should be explained by the difference in the discriminatory mechanisms that mediate pitch and loudness. When the frequency of a tone is changed, new excitation is substituted for old; when the intensity of a tone is increased, new excitation is added to old. Stevens later generalized this distinction to other sensory modalities, calling the additive attributes "prothetic" and the substitutive attributes "metathetic."

After the war he did not return immediately to this puzzle. In his autobiography Smitty speaks of the years from 1945 to 1952 as "seven lean years." He was busy planning and supervising the renovation of the basement in Memorial Hall, directing the Psycho-Acoustic Laboratory, commuting to the high councils of science in Washington, enjoying the honors and recognition he received, rebuilding the farmhouse in New Hampshire into a ski lodge and experimenting with skis, and for three of those years his major preoccupation, with the skilled editorial assistance of Didi Stone, was the 1400-page *Handbook of Experimental Psychology*. But his scientific publications during those years were accounts of prewar work on the theory of measurement or experiments conducted during the war. He felt increasingly defeated by success, unable to get back to the detailed work of research while his desk bloomed with important papers pressing for attention. He spoke of the "gnawing fear that the fire was spent, that science, the jealous mistress I had abandoned for war research, had now abandoned me."

He credited an argument with W. R. Garner about the scaling of loudness with providing the impetus that finally sent him back into the laboratory for the final twenty years of his life. As any operationist would have expected, the loudness scale that is measured depends on the experimental operations performed. You could count jnd's, ask people to adjust one magni-

tude until it was halfway between two others or to make one magnitude twice another, compare two ears with one, ask people to rate magnitudes or ratios between magnitudes, or use other clever schemes that psychologists had invented. And no two methods seemed to give exactly the same scale. On operational grounds, there seemed no better way to choose among methods than to flip a coin.

A more consistent operationist might have let it go at that, but Smitty simply could not accept the idea that there was not an underlying invariance in all that heterogeneity. And so he set out to find it.

He began by adopting the method of magnitude estimation—which he invented as the simplest and most direct procedure he could think of for getting at a person's impressions—as his basic experimental operation. A series of stimuli were presented in an irregular order, and the person was asked to assign numbers to them; he could assign any number he liked to the first stimulus, but thereafter the ratios of the numbers he assigned should correspond to the ratios of his subjective impressions. When Smitty plotted the data obtained in this manner against the stimulus intensity, he repeatedly found not a logarithmic function, but a power function. When loudness was the attribute to be judged, for example, every time the intensity was increased by 10 dB, the number assigned to it doubled. Whereas Fechner would have predicted that logarithmic increments in intensity would be judged as constant increments in loudness, Stevens found that they yielded constant ratios in loudness. The loudness, L, was proportional to the energy to the 0.3 power: $L = kI^{0.3}$. From this it follows that the underlying invariance is the simple principle that equal stimulus ratios produce equal subjective ratios.

There followed an intense period of work extending this insight to other sensory modalities. Many collaborators, one of the most important of whom was J. C. Stevens (no relation),

assisted in this work. In every case they found power functions, with exponents ranging from 0.3 for brightness up to 3.5 for electric shock. Discrepant results obtained by other, more complex judgmental operations were rationalized away—at least to Stevens's satisfaction—and the whole psychophysical structure built on variability by Fechner and Thurstone was replaced by the power law, or, as many now call it, "Stevens' Law."

Science has been likened to a vessel that the crew must continually rebuild during the voyage. This particular bit of reconstruction concerned a vital part of the craft, and it was not accomplished without considerable complaint from the other passengers. One repeated objection was that the yardstick Stevens was using was the number scale that people carry around in their heads—perhaps the invariance was attributable to their arithmetic habits rather than to their sensory transducers. In order to meet that objection, Stevens generalized the method to what he called "cross-modality matching." Magnitudes on sensory continuum, A, are matched to magnitudes of two other continua, B and C. The ratio of the exponents of the function matching A and B to the function matching A and C predicts the exponent of the function matching B and C. The numbers used in magnitude estimation can be regarded as simply another perceptual modality like the others; anyone who regards them with suspicion can dispense with them. Thus, Stevens came to regard all measurement as a matching procedure; numerical matching is merely a special case.

In 1965 Gösta Ekman, a distinguished psychophysicist in Stockholm, reviewed the subject and rendered this verdict on Stevens's accomplishment: "After a hundred years of almost general acceptance and practically no experimentation, Fechner's logarithmic law was replaced by the power law. The amount of experimental work performed in the 1950s on this problem by Stevens and other research workers was enormous,

and the outcome was an outstanding success. The power law was verified again and again, in literally hundreds of experiments. As an experimental fact, the power law is established beyond any reasonable doubt, possibly more firmly established than anything else in psychology.

Stevens continued active work on these problems until he died. The premature deaths of both Ekman and Stevens were terrible blows to psychophysics. Fortunately, however, in the weeks before his death, Stevens completed the manuscript of a book that, when published, will summarize psychophysics and preserve his contributions to this old but still vital branch of scientific psychology.

BIBLIOGRAPHY

KEY TO ABBREVIATIONS

Am. J. Psychol. = American Journal of Psychology
Am. Psychol. = American Psychologist
Am. Sci. = American Scientist
J. Acoust. Soc. Am. = Journal of the Acoustical Society of America
J. Exp. Psychol. = Journal of Experimental Psychology
J. Sound Vib. = Journal of Sound and Vibration
Percept. Psychophys. = Perception and Psychophysics
Proc. ——— Int. Congr. Acoust. = Proceedings of the ——— International Congress of Acoustics
Proc. Natl. Acad. Sci. = Proceedings of the National Academy of Sciences
Psychol. Bull. = Psychological Bulletin
Psychol. Rev. = Psychological Review
Q. J. Exp. Psychol. = Quarterly Journal of Experimental Psychology
Vision Res. = Vision Research

1934

The relation of saturation to the size of the retinal image. Am. J. Psychol., 46:70–79.
The attributes of tones. Proc. Natl. Acad. Sci., 20:457–59.
The volume and intensity of tones. Am. J. Psychol., 46:397–408.
Tonal density. J. Exp. Psychol., 17:585–92.
With E. B. Newman. The localization of pure tones. Proc. Natl. Acad. Sci., 20:593–96.

1935

The relation of pitch to intensity. J. Acoust. Soc. Am., 6:150–54.
The operational basis of psychology. Am. J. Psychol., 47:323–30.
With H. Davis and M. H. Lurie. The localization of pitch perception on the basilar membrane. Journal of General Psychology, 13:297–315.
The operational definition of psychological concepts. Psychol. Rev., 42:517–27.

1936

Psychology: the propaedeutic science. Philosophy of Science, 3:90–103.
With E. B. Newman. The localization of actual sources of sound. Am. J. Psychol., 48:297–306.

A scale for the measurement of a psychological magnitude: loudness. Psychol. Rev., 43:405–16.

With H. Davis. Psychophysiological acoustics: pitch and loudness. J. Acoust. Soc. Am., 8:1–13.

With E. G. Boring. The nature of tonal brightness. Proc. Natl. Acad. Sci., 22:514–21.

With E. B. Newman. On the nature of aural harmonics. Proc. Natl. Acad. Sci., 22:668–72.

The psychophysiology of hearing. Proceedings of the American Society of the Hard of Hearing, 17:30–35.

1937

With J. Volkmann and E. B. Newman. A scale for the measurement of psychological magnitude: pitch. J. Acoust. Soc. Am., 8:185–90.

On hearing by electrical stimulation. J. Acoust. Soc. Am., 8:191–95.

With J. Volkmann and E. B. Newman. On the method of bisection and its relation to a loudness scale. Am. J. Psychol., 49:134–37.

With E. B. Newman and H. Davis. Factors in the production of aural harmonics and combination tones. J. Acoust. Soc. Am., 9:107–18.

With J. G. Beebe-Center. Cardiac acceleration in emotional situations. J. Exp. Psychol., 21:72–87.

1938

With H. Davis. *Hearing, Its Psychology and Physiology.* New York: John Wiley & Sons, Inc.

With R. E. P. Youtz. On the pitch of frequency-modulated tones. Am. J. Psychol., 51:521–26.

With J. G. Beebe-Center. The emotional responses: changes of heart rate in a gun-shy dog. J. Exp. Psychol., 23:239–57.

1939

Psychology and the science of science. Psychol. Bull., 36:221–63.

With R. C. Jones. The mechanism of hearing by electrical stimulation. J. Acoust. Soc. Am., 10:261–69.

1940

With W. H. Sheldon and W. B. Tucker. *The Varieties of Human Physique.* New York: Harper & Row Pubs., Inc.

With J. Volkmann. The relation of pitch to frequency: a revised scale. Am. J. Psychol., 53:329–53.

With R. C. Jones and M. H. Lurie. Three mechanisms of hearing by electrical stimulation. J. Acoust. Soc. Am., 12:281–90.

With J. Volkmann. The quantum of sensory discrimination. Science, 92:583–85.

1941

With C. T. Morgan and J. Volkmann. Theory of the neural quantum in the discrimination of loudness and pitch. Am. J. Psychol., 54:315–35.

1942

With W. H. Sheldon. *The Varieties of Temperament.* New York: Harper & Row Pubs., Inc.

Rectilinear rectification applied to voltage integration. Electronics, 15:40–41.

1943

With L. D. Carson and W. R. Miles. Vision, hearing and aeronautical design. Journal of Aeronautical Science, 10:127–30.

1946

With G. A. Miller and F. M. Wiener. *Transmission and Reception of Sounds under Combat Conditions.* (Summary Technical Report of NDRC Div. 17, vol. 17-3) Washington.

On the theory of scales of measurement. Science, 103:677–80.

The science of noise. Atlantic Monthly, 178(1):96.

Machines cannot fight alone. Am. Sci., 34:389–400.

With H. Davis et al. The selection of hearing aids. Laryngoscope, 56:85–115, 135–63.

With J. Miller and I. Truscott. The masking of speech by sine waves, square waves, and regular and modulated pulses. J. Acoust. Soc. Am., 18:418–24.

1947

With H. Davis et al. *Hearing Aids. An Experimental Study of Design Objectives.* Cambridge: Harvard Univ. Press.

With K. D. Kryter and J. C. R. Licklider. Premodulation clipping in AM voice communication. J. Acoust. Soc. Am., 19:125–31.

With G. Stone. Psychological writing, easy and hard. Am. Psychol., 2:230–35.

With E. G. Boring. The new Harvard Psychological Laboratories. Am. Psychol., 2:239–43.

With J. P. Egan and G. A. Miller. Methods of measuring speech spectra. J. Acoust. Soc. Am., 19:771–80.

1948

With R. S. Harper. A psychological scale of weight and a formula for its derivation. Am. J. Psychol., 61:343–51.

1950

With J. E. Hawkins, Jr. The masking of pure tones and of speech by white noise. J. Acoust. Soc. Am., 22:6–13.

1951

Editor. *Handbook of Experimental Psychology.* New York: John Wiley & Sons, Inc.

Mathematics, measurement, and psychophysics. In: *Handbook of Experimental Psychology,* ed. by S. S. Stevens, pp. 1–49. New York: John Wiley & Sons, Inc.

1952

The NAS–NRC and psychology. Am. Psychol., 7:119–24.

1954

Biological transducers. Convention Records of the Institute of Radio Engineers, pt. 9:27–33.

Pitch discrimination, mels, and Kock's contention. J. Acoust. Soc. Am., 26:1075–77.

The ear and the eye (El oído y la vista). Acta Prima Congreso Extraordinario Sociedad Internacional Audiolójico, Buenos Aires, pp. 408–16.

1955

With J. C. G. Loring and D. Cohn. *Bibliography on Hearing.* Cambridge: Harvard Univ. Press.

On the averaging of data. Science, 121:113–16.

With M. S. Rogers and R. J. Herrnstein. Apparent reduction of loudness: a repeat experiment. J. Acoust. Soc. Am., 27:326–28.

With E. C. Poulton. On the halving and doubling of the loudness of white noise. J. Acoust. Soc. Am., 27:329–31.
Decibels of light and sound. Physics Today, 8(10):12–17.
The measurement of loudness. J. Acoust. Soc. Am., 27:815–29.

1956

With E. C. Poulton. The estimation of loudness by unpracticed observers. J. Exp. Psychol., 51:71–78.
The direct estimation of sensory magnitudes—loudness. Am. J. Psychol., 69:1–25.
Calculation of the loudness of complex noise. J. Acoust. Soc. Am., 28:807–32.

1957

With E. H. Galanter. Ratio scales and category scales for a dozen perceptual continua. J. Exp. Psychol., 54:377–411.
On the psychophysical law. Psychol. Rev., 64:153–81.
With E. Zwicker and G. Flottorp. Critical bandwidth in loudness summation. J. Acoust. Soc. Am., 29:548–57.
Concerning the form of the loudness function. J. Acoust. Soc. Am., 29:603–6.
Calculating loudness. Noise Control, 3(5):11–22.

1958

Adaption level vs. the relativity of judgment. Am. J. Psychol., 71: 633–46.
Problems and methods of psychophysics. Psychol. Bull., 55:177–96.
Some similarities between hearing and seeing. Laryngoscope, 68(3): 508–27.
With A. S. Carton and G. M. Shickman. A scale of apparent intensity of electric shock. J. Exp. Psychol., 56:328–35.
Measurement and man. Science, 127:383–89.

1959

Measurement, psychophysics and utility. In: *Measurement: Definitions and Theories,* ed. by C. W. Churchman and P. Ratoosh, pp. 18–64. New York: John Wiley & Sons, Inc.
With G. Stone. Finger span: ratio scale, category scale and jnd scale. J. Exp. Psychol., 57:91–95.

Cross modality validation of subjective scales for loudness, vibration, and electric shock. J. Exp. Psychol., 57:201–9.

Tactile vibration: dynamics of sensory intensity. J. Exp. Psychol., 57:210–18.

On the validity of the loudness scale. J. Acoust. Soc. Am., 31:995–1004.

The quantification of sensation. Daedalus, 88:606–21.

1960

With J. C. Stevens and J. D. Mack. Growth of sensation on seven continua as measured by force of handgrip. J. Exp. Psychol., 59:60–67.

With J. C. Stevens. Warmth and cold: dynamics of sensory intensity. J. Exp. Psychol., 60:183–92.

Psychophysics of sensory function. Am. Sci., 48:226–53.

On the new psychophysics. Scandinavian Journal of Psychology, 1:27–35.

Ratio scales, partition scales and confusion scales. In: *Psychological Scaling: Theory and Applications,* ed. by H. Gulliksen and S. Messick, pp. 49–66. New York: John Wiley & Sons, Inc.

With T. S. Reese. Subjective intensity of coffee odor. Am. J. Psychol., 73:424–28.

With G. S. Reynolds. Binaural summation of loudness. J. Acoust. Soc. Am., 32:1337–44.

With J. C. Stevens. The dynamics of visual brightness. Harvard University, Psycho-Acoustic Laboratory (August).

1961

Psychophysics of sensory function. In: *Sensory Communication,* ed. by W. A. Rosenblith, pp. 1–33. Cambridge: MIT Press.

The auditory input–output function. Proc. 3d Int. Congr. Acoust., pp. 78–80. Amsterdam: Elsevier Pub. Co.

With H. L. Lane and A. C. Catania. Voice level: autophonic scale, perceived loudness and the effects of sidetone. J. Acoust. Soc. Am., 33:160–67.

To honor Fechner and repeal his law. Science, 133:80–86.

Toward a resolution of the Fechner–Thurston legacy. Psychometrika, 26:35–47.

Procedure for calculating loudness, Mark VI. J. Acoust. Soc. Am., 33:1577–85.

1962

The surprising simplicity of sensory metrics. Am. Psychol., 17:29–39.

With H. Terrace. The quantification of tonal volume. Am. J. Psychol., 75:596–604.

With J. R. Harris. The scaling of subjective roughness and smoothness. J. Exp. Psychol., 64:489–94.

With M. Guirao. Loudness, reciprocality, and partition scales. J. Acoust. Soc. Am., 34:1466–71.

In pursuit of the sensory law. (2d Klopsteg Lecture, November, at Northwestern University)

1963

With J. C. Stevens. The dynamics of subjective warmth and cold. In: *Temperature—Its Measurement and Control in Science and Industry,* vol. 3, pt. 3, ed by C. M. Herzfeld, pp. 239–43. New York: Reinhold Publishing Corp.

With M. Guirao. Subjective scaling of length and area and the matching of length to loudness and brightness. J. Exp. Psychol., 66:177–86.

With J. C. Stevens. Brightness function: effects of adaptation. Journal of the Optical Society of America, 53:375–85.

The basis of psychophysical judgments. J. Acoust. Soc. Am., 35: 611–12.

1964

Sensory transform functions. In: *Information Processing in the Nervous System,* ed. by R. W. Gerard and J. W. Duyff, pp. 53–60. Princeton: Excerpta Medica Foundation.

With R. Harper. Subjective hardness of compliant materials. Q. J. Exp. Psychol., 16:204–15.

With T. S. Aiba. Relation of brightness to duration and luminance under light- and dark-adaptation. Vision Res., 4:391–401.

With M. Guirao. Measurement of auditory density. J. Acoust. Soc. Am., 36:1176–82.

With M. Guirao. Scaling of apparent viscosity. Science, 144:1157–58.

Concerning the psychophysical power law. Q. J. Exp. Psychol., 16: 383–85.

1965

With F. Warshofsky and the editors of *Life* magazine. *Sound and Hearing.* New York: Time–Life Books.

On the uses of poikilitic functions. In: *Stimulus Generalization,* ed. by D. I. Mostofsky, pp. 24–29. Stanford: Stanford Univ. Press.

With M. Guirao and A. W. Slawson. Loudness, a product of volume times density. J. Exp. Psychol., 69:503–10.

With A. L. Diamond. Effect of glare angle on the brightness function for a small target. Vision Res., 5:649–59.

1966

Transfer functions of the skin and muscle senses. In: *Touch, Heat and Pain,* Ciba Foundation Symposium, pp. 3–17. London: J. & A. Churchill Ltd.

Matching functions between loudness and ten other continua. Percept. Psychophys., 1:5–8.

Quantifying the sensory experience. In: *Mind, Matter, and Method,* ed. by P. Feyerabend and G. Maxwell, pp. 215–33. Minneapolis: Univ. of Minnesota Press.

A metric for the social consensus. Science, 151:530–41.

With D. Panek. Saturation of red: a prothetic continuum. Percept. Psychophys., 1:59–66.

Power-group transformations under glare, masking and recruitment. J. Acoust. Soc. Am., 39:725–35.

Duration, luminance, and the brightness exponent. Percept. Psychophys., 1:96–100.

With T. Indow. Scaling of saturation and hue. Percept. Psychophys., 1:253–72.

On the operation known as judgment. Am. Sci., 54:385–401.

With H. B. Greenbaum. Regression effect in psychophysical judgment. Percept. Psychophys., 1:439–46.

1967

Masking and sensory dynamics. In: *Acoustic Noise and Its Control,* pp. 1–3. Institute of Electrical Engineers Conference Publication 26, January.

Intensity functions in sensory systems. International Journal of Neurology, 6:202–9.

With M. Guirao. Loudness functions under inhibition. Percept. Psychophys., 2:459–65.

1968

Ratio scales of opinion. In: *Handbook of Measurement and Assessment in Behavioral Sciences,* ed. by D. K. Whitla, pp. 171–99. Reading, Mass.: Addison-Wesley Publishing Co., Inc.

Measurement, statistics, and the schemapiric view. Science, 161: 849–56.

Tactile vibration: change of exponent with frequency. Percept. Psychophys., 3:223–28.

Le quantitatif et la perception. Bulletin de Psychologie, 22:696–715.

Psychophysics and the measurement of loudness. In: Proc. 6th Int. Congr. Acoust., Tokyo, Japan. Amsterdam: Elsevier Pub. Co.

Edwin Garrigues Boring: 1886–1968. Am. J. Psychol., 81:589–606.

1969

Measurement and social science. et al. [*sic.*], 2(1):5–6.

On predicting exponents for cross-modality matches. Percept. Psychophys., 6:251–56.

Sensory scales of taste intensity. Percept. Psychophys., 6:302–8.

With B. Bond. Cross-modality matching of brightness to loudness by 5-year-olds. Percept. Psychophys., 6:337–39.

1970

Neural events and the psychophysical law. Science, 170:1043–50.

On the quantitative evaluation of noise. In: *Transportation Noises: A Symposium on Acceptability Criteria,* ed. by J. D. Chalupnik, pp. 114–28. Seattle: Univ. of Washington Press.

1971

Sensory power functions and neural events. In: *Handbook of Sensory Physiology,* vol. I, ed. by W. R. Loewenstein, pp. 226–42. Berlin: Springer Verlag.

Issues in psychophysical measurement. Psychol. Rev., 78:426–50.

1972

Perceived level of noise by Mark VII and decibels (E). J Acoust. Soc. Am., 51:575–601.

Psychophysics and Social Scaling. Morristown, N.J.: General Learning Press. 27 pp.

A neutral quantum in sensory discrimination. Science, 177:749–62.
Stability of human performance under intense noise. J. Sound Vib., 21:35–56.
Calculating the perceived level of light and sound. J. Sound Vib., 23:297–306.

1974

Perceptual magnitude and its measurement. In: *Handbook of Perception,* vol. 2: *Psychophysical Judgment and Measurement,* ed. by E. C. Carterette and M. P. Friedman. New York: Academic Press, Inc.
Notes for a life story. In: *Sensation and Measurement: Papers in Honor of S. S. Stevens,* ed. by H. R. Moskowitz, B. Scharf, and J. C. Stevens, pp. 423–46. Boston: D. Reidel Publishing Co.

1975

Psychophysics: Introduction to its Perceptual, Neural, and Social Prospects, ed. by Geraldine Stevens. New York: Wiley-Interscience.