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SCIENCE

Measurement and Man

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In some parts of science we have reduced the business of measuring to simple routines, chores to be done by technicians—animate or inanimate. In these areas the basic and challenging problems of measurement have been solved, and the only task left is to implement, read, and record. In other parts of the discipline the problem of how and what to measure remains acute and real. The task is not simply to read a meter or gauge an effect; it is to devise a procedure by which to quantify some stubborn phenomenon—to reduce it to numerical order.

Much that pertains to man himself poses knotty problems of just this sort. How shall we measure his capacities, his attitudes, his sensations, or any of the many aspects of man that cannot be weighed in a balance or marked off on a stick? Is measurement possible here, and if so, to what degree? But first of all, what precisely do we mean by measurement and what are the forms it may take?

Mathematics versus Measurement

"Probably more nonsense," said N. R. Campbell, "is talked about measurement than about any other part of physics" (*1*). Crotchety as this remark may sound, Campbell did not intend thereby to belittle the power and beauty of physical measurement or the superlative ingenuity of laboratory practice. But the art of measurement is one thing; the understanding of its fundamentals is another. And Campbell—the author of *Physics: the Elements*—was trying to teach us the same truth that Whitehead had in mind when he observed that "it is harder to discover the elements than to develop the science" (*2*). This is the way it has been with measurement. Here as elsewhere it has often taken our greatest minds to discover the simplest things.

One of these things is the relation between measurement and mathematics. It seems clear to us now that the process of measurement is the process of mapping empirical facts and relations into a formal model—a model borrowed from mathematics. But this conception took form only in very recent times. It is the product of long centuries of intellectual struggle, to which many of the foremost mathematicians contributed. It is a conception that was impossible, even unthinkable, until the nature of mathematics as a postulational system became clarified.

Once a basic and elementary notion dawns upon us in its full clarity, we often wonder how our fathers could have missed perceiving it. It is a curious fact that, although the postulational method was applied to geometry some two millenniums ago, only in modern times were the fundamental assumptions of algebra exhumed from the hodgepodge of rules that govern algebraic practice. In this sense, the modern postulates of algebra represent the distilled wisdom of more than 3000 years of symbol juggling. They represent the outcome of our efforts to pare away the nonessentials in order to get a clear view of what constitutes the essence of a mathematical system.

And the essence is this: mathematics is a game of signs and rules, man-made and arbitrary, like the game of chess. It begins with a set of undefined terms and a set of unproved assumptions regarding their interrelations. The mathematician invents symbols, and at the same time he lays down rules to tell us how these symbols shall be allowed to combine and interact. Nowhere in this process, as we now conceive it, is there any reference to empirical objects—or any explicit concern for the world of sense and matter. Therein lies the revolutionary novelty, for not long ago, as human history goes, it was argued that negative numbers were "absurd" and "fictitious." For how could anything be less than nothing? You see, our ancestors thought it proper to test their mathematics by operations performed upon nature—upon actual objects—for they conceived arithmetic as a system of concrete numerical magnitudes whose relations should be verifiable in the empirical domain, and where in the real world were the negative objects?

The story of the slow and painful growth of the number system, the story of how the mathematicians, often against their own better judgment, began to write outlandish symbols, such as -3 and $\sqrt{-1}$, is a fascinating tale. It could occupy us at length, but we must forego it. Its bearing on our present concern relates mainly to its outcome. With each new kind of number admitted to the number domain—the negatives, the irrationals, the imaginaries, and so on—it became more clearly impossible to prove arithmetic by appeal to experiment. So in the end the formal, syntactical system of mathematics achieved its full emancipation, its complete decoupling from empirical matters of fact. Thence it took off into the realm of pure abstraction, where it properly belonged in the first place.

Why did this decoupling take so long? Why so much travail to achieve something so simple and obvious? The difficulty, it seems, was measurement. In particular, it was the fact that the early mathematicians did not readily discern the difference between measurement and mathematics. Man was usually more interested in empirical measurement than in mathematics—as the scientist, no

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doubt, still is—and it was the problem of measurement that first gave rise to arithmetic. In the beginning, mathematics and measurement were so closely bound together that no one seemed to suspect that two quite different disciplines were involved. The earliest scales of measurement were scales of numerosity—scales for the counting of pebbles or cattle or warriors. In some dim era in the past, somebody invented the system of natural numbers precisely for the purpose of representing what he did with collections of objects. No doubt this forgotten genius was oblivious to the formal-empirical dichotomy, which we now consider so crucial. But that is beside the point. However he may have regarded it, the fact is that he built himself a formal model to stand for an aspect of the empirical world, much as an architect draws a plan for a house. Kronecker once said, "God created the whole numbers; all the others are the work of man." Passable theology, perhaps, but surely bad history.

Since arithmetic was invented for measurement, it is not surprising that the isomorphic correspondence between whole-number arithmetic and the empirical numerosity of piles of pebbles is tight and complete. It was, in fact, the very tightness of this isomorphism that blinded the ancients to the essential difference between mathematics and measurement. But modern mathematics is no longer constrained to serve only as a syntax for quantitative discourse. Far from limiting itself to serving as a model for numerosity, or even as a model for such continuous dimensions as length, it has become largely nonquantitative in some of its more abstract reaches. This outcome has suggested to Gödel a startling thought—namely, that is was purely an historical accident that mathematics developed along quantitative lines (*3*). In one sense Gödel is undoubtedly right, and his conjecture is a profound commentary on the nature of mathematics. But the story of measurement suggests that this "accident" had about it a certain inevitability. Striving somehow to count his possessions, ancient man seems destined in the nature of things to have hit upon the concept of number and to have made therein his first triumphant abstraction. Given the deeply human need to quantify, could mathematics really have begun elsewhere than in measurement?

It is not, however, only in history that we see the slow development of the formal-empirical dichotomy. An analogous development takes place in the lives of all of us. Just as ontogeny to some extent repeats phylogeny, so in the life of each maturing child the struggle of the ages is reenacted in the child's attempt to grasp the abstraction of mathematics. He learns his first arithmetic with the aid of fingers or buttons or beads, and only with great labor does he finally, if ever, achieve the reoriented view that mathematics is an abstract game having no necessary relation to solid objects. Each of us has suffered through this process of revision. Even though you may have shifted gears more smoothly than I, still you may well sympathize with my own dismay at my first encounter with imaginary numbers.

The Nature of a Scale

In its broadest sense, measurement is the business of pinning numbers on things. More specifically, it is the assignment of numbers to objects or events in accordance with a rule of some sort. This process turns out to be a fruitful enterprise only because some degree of isomorphism obtains between the empirical relations among the properties of objects or events, on the one hand, and some of the properties of the number system, on the other. Some of these properties, and their uses in measurement, are these: (i) Identity: numbers may serve as labels to identify items or classes. (ii) Order: numbers may serve to reflect the rank order of items. (iii) Intervals: numbers may serve to reflect differences among items. (iv) Ratios: numbers may serve to reflect ratios among items.

These are ways in which we may deputize numbers to represent one or another aspect of a state of affairs in nature. Depending upon what kinds of empirical operations we are able to perform, one or more of these aspects of the number system may be used as a model to represent the outcome. The empirical operations are sometimes a matter of choice; more often they are limited by our experimental ingenuity. In any case, the nature of the operations determines that there may eventuate one or another of four kinds of scales (*4, 5*). These I have called "nominal," "ordinal," "interval," and "ratio." They are listed and described in Table 1.

The key to the nature of these different scales rests with the concept of invariance. How can we transform the numbers on the scale with no loss of empirical information? If all we can do about a set of objects is identify or classify them, we have only a nominal scale, and the numbers we assign can be permuted at will, for all that the numbers provide are labels. If operations exist for determining order, and if we have assigned numbers to reflect this fact, then the permissible scale transformation must be order-preserving. When intervals have empirical meaning—as on the ordinary temperature scale—we are limited to linear transformations. We can multiply by a constant and add a constant. And finally, if in addition to all this we can give empirical meaning to ratios, the only permissible transformation is multiplication by a constant, as when we convert from feet to inches. Any more liberal transformation entails a loss of information. In general, the richer the experimental operations, the greater is the isomorphism between them and the formal model of arithmetic, and the more restricted is the range of invariant transformations. [For a possible fifth type of scale having a still different transformation group, see (*5*) and (*6*).]

Each of these scales has its uses, but it is the more powerful ratio scale that serves us best in the search for nature's regularities. On these ratio scales we measure basic things, like numerosity, length, and weight, and, depending on our artistry, we contrive more elusive measures, like the charge on the electron or the strength of a magnetic field.

Why, it may be asked, do we bother with the other types of scales? Mostly we use the weaker forms of measurement only *faute de mieux*. When stronger forms are discovered we are quick to seize them. But science is an art. There are no *ab initio* principles to tell us how to be clever in devising procedures of measurement. The way to empirical discovery lies not through mathematics, even, but through the exercise of uncommon experimental sense and ingenuity. We invent mathematical models, but we discover measures in the laboratory. As Norbert Wiener (*7*) said, "Things do not, in general, run around with their measures stamped on them like the capacity of a freight car; it requires a certain amount of investigation to discover what their measures are."

Perhaps those who stand apart from the practice of the scientific art, and who philosophize about the "scientific method," think there really is such a thing, and that it can be captured in a book of rules. But the man on the laboratory stool is likely to agree with Hildebrand that "there is no such thing as *the* scientific method" (*8*). If you think science is a simple and unitary thing, try asking several scientists to define it. One of the entertaining things about science is that no one has succeeded in explaining precisely what it is.

However you define the scientific activity, measurement pervades most of the enterprise. Measurement is essential to the determination of functional relations, to the discovery of order and regularity. I need not extol it further, for we all know the reality of its power. In fact, we take it so much for granted that it becomes almost unthinkable that the pursuit of measurement did not always stand in high regard.

I vividly recall Professor Whitehead, peering over his lectern in Harvard's Emerson Hall and rasping out wisdom in his high-pitched voice: "If only the schoolmen of the Middle Ages had measured instead of classifying, how much they might have learned." Under the influence of Aristotelian logic, with its emphasis on classification, the schoolmen forsook the Pythagorean tradition, which taught the primacy of number and measurement. Classification, to be sure, is a first and essential step on the road up the hierarchy of scales. It gets us to the nominal level. But this is no more than a quarter-way house on the road to measurement in its more powerful forms. The revival of modern science in the 17th century—the century of genius—was a revival of the Pythagorean outlook, a revival of measurement. With Galileo, Newton, and the rest, science became primarily quantitative, and so it has remained.

In his diagnostic satire entitled *Science is a Sacred Cow,* Standen perceived correctly the modern order of things when he put measurement at the top of the scientist's totem pole [see (*9*)].

Measurement in Psychophysics

Measurement, as we have seen, is more than the pedantic pursuit of a decimal place. Its vital and absorbing aspect emerges most clearly perhaps when it becomes a question of measuring something that has never been measured. Or better still, something that has been held to be unmeasurable. Quantification is a respectable enterprise in physics and chemistry, and even in much of biology. But what about man, and the measurement of his higher processes? Are we always objective and emotionally neutral about this prospect?

The economist Edgeworth (*10*) once wrote, "There is an old prejudice still reviving, however often slain, against the reign of law in psychology, as incompatible with the higher feelings." Some there are, I suppose, who still feel that quantification, by some brutal rigor, will shatter the human spirit if we probe with the aid of numbers. But man can hardly fall in stature by understanding man, or even by quantifying that understanding. The greater beauty of discovered order will surely more than compensate for the nostalgic pain of a romantic yearning to remain securely inscrutable.

However we regard this issue, the fact remains that man is undergoing measurement. We are all familiar with the highly developed business of testing human performance and ability, and with the pioneering work of Binet, who launched us on the road to the measurement of the IQ. This measure, with its approximate invariance over the child's growing years, stands as one of the first-rate contributions to human understanding. Interesting issues for the theory of measurement arise almost daily in these burgeoning fields of ability assessment. But since this is not my own area of interest, let me turn to another quest: the measurement of sensation.

Modern experimental psychology had its beginnings in this inquiry, which started just about a hundred years ago—in the 1850's.

Let me pose the problem in this way. Suppose you look at a photograph in the bright sunlight and then again in a dimly lighted room. The remarkable fact is that the picture looks much the same

Table 1. A classification of scales of measurement. Measurement is the assignment of numbers to objects or events according to rule. The rules and the resulting kinds of scales are tabulated below. The basic operations needed to create a given scale are all those listed in the second column, down to and including the operation listed opposite the scale. The third column gives the mathematical transformations that leave the scale form invariant. Any number x on a scale can be replaced by another number x' where x' is the function of x listed in column 2. The fourth column lists, cumulatively downward, examples of statistics that show invariance under the transformations of column 3 (the mode, however, is invariant only for discrete variables).

Scale	Basic empirical operations	Mathematical group-structure	Permissible statistics (invariantive)	Typical examples
Nominal	Determination of equality	Permutation group $x' = f(x)$ where $f(x)$ means any one-to-one substitution	Number of cases Mode "Information" measures Contingency correlation	"Numbering" of football players Assignment of type or model numbers to classes
Ordinal	Determination of greater or less	Isotonic group $x' = f(x)$ where $f(x)$ means any increasing monotonic function	Median Percentiles Order correlation (type 0: interpreted as a test of order)	Hardness of minerals Grades of leather, lumber, wool, and so forth Intelligence-test raw scores
Interval	Determination of the equality of intervals or of differences	Linear or affine group $x' = ax + b$, $a > 0$	Mean Standard deviation Order correlation (type I: interpreted as r) Product moment (r)	Temperature (Fahrenheit and Celsius) Position on a line Calendar time Potential energy Intelligence-test "standard scores" (?)
Ratio	Determination of the equality of ratios	Similarity group $x' = cx$, $c > 0$	Geometric mean Harmonic mean Percent variation	Length, numerosity, density, work, time intervals, and so forth Temperature (Kelvin) Loudness (sones) Brightness (brils)

under the two conditions. Despite a change of illumination of perhaps several thousand-fold, the light parts of the picture look light and the dark parts dark. The perceived relation between light and shade within the picture remains highly stable, is subjectively constant. But just what is it that is subjectively constant, we may ask. There are at least two possibilities. One is that the subjective *difference* between the light and shade remains constant as we go from outdoors to indoors. The other is that the subjective *ratio* between the light and shade remains constant. If we could find out which of these relations holds, then we would know, for these conditions, the law that relates subjective brightness to the physical intensity of the stimulus.

Back in the 1850's two major figures in science, Fechner and Plateau, both considered the problem and reached quite opposite conclusions (a fact that suggests that you cannot settle the matter merely by looking at pictures!). Fechner argued that the subjective *difference* between light and shade remains constant, and that therefore the subjective brightness is a logarithmic function of stimulus intensity. That is the well-known Fechner's law. Plateau argued that the *ratio* remains constant, and that therefore the subjective brightness is a power function of stimulus intensity.

Formula-wise we may state these two laws as the relation between psychological value ψ and physical value ϕ in this way:

$$\text{Logarithmic law: } \psi = k_1 \log \phi$$

$$\text{Power law: } \psi = k_2 \phi^n$$

The exponent n is a constant whose value may vary with sense modality and with conditions of stimulation.

Of course, the champions of these laws cited other facts and evidence, and for a hundred years this issue has stood as a kind of antinomy in psychophysics. If you have heard only of Fechner in this connection, it is because it was he who defended his view more fiercely, who more tirelessly outargued his critics. Plateau's interest was only casual, and, as a matter of fact, he later changed his mind—and for a reason that was not really relevant (see *6*). So the field was left mainly to Fechner. But others revived the power law from time to time, and the contradiction persisted.

How can this conflict of opposing laws —the logarithmic and the power law— be resolved? By measurement, of course. All that is needed is a scale for the measurement of sensation. But that is easier said than done.

The Operational Principle

At this point, let me try to clarify a sticky issue. This question of sensation and its measurement has often gotten itself bogged down in metaphysical debate. Ever since Descartes set mind apart from matter, we have been trying in one way or another to put them back together again, for if we accept the dualistic view that mind is something apart, something inaccessible to science and measurement, the game is lost before the first move is made. To rescue science from this hopeless gambit, three modern developments have converged on a common solution. The three are behaviorism in psychology, operationism in physics, and logical positivism in philosophy (*11*). Despite certain differences in language and emphasis, all three of these movements have sought to clarify our scientific discourse by ridding its concepts of metaphysical overtones and untestable meanings. Under the operational view, length is what we measure with rods; time is what we measure with clocks. However well grounded in common sense may seem the notions of Absolute Space and Absolute Time, the physicist, as physicist, can *know* nothing about them—for he can *do* nothing about them.

Equally inaccessible are the nonoperational aspects of sensation. What we can get at in the study of living things are the responses of organisms, not some hyperphysical mental stuff, which, by definition, eludes objective test. Consequently, verifiable statements about sensation become statements about responses—about differential reactions of organisms. In psychology, perhaps even more than in physics, this operational stance is indispensable to scientific sense and meaning. In line with this necessity, let us agree that the term *sensation* denotes a construct that derives its meaning from the reactions, verbal or otherwise, made by an organism in response to stimuli. I know nothing about your sensations except what your behavior tells me. But what is equally true, we know nothing about the charge on the electron except for what its behavior discloses. We must be thoroughly operational in both instances.

Now, some will object that there is a difference here: that electrons do not study themselves, whereas men do. This is true enough. But if the science of man is to contain public, repeatable, verifiable generalizations, we must always in effect study the other fellow—we must pursue "the psychology of the other one." The psychologist as experimenter may look in upon himself if he cares to, and he may often thereby gain insight into fruitful hypotheses. But these hypotheses can lead to valid general laws only after they have been verified under experimental control on other people. If the experimenter serves as an observer in his own experiment, as I often do, he must proceed to treat his own responses as objective data, on a par with those of other observers. This manner of working, it seems to me, is the only sound, objective, operational approach. In what follows, therefore, I hope it will be taken for granted that I mean no more by sensation than what experiment tells us. Our goal is to make quantitative order of the reactions of sensory systems to the energetic configurations of the environment.

Conflicting Laws

Let us return now to our problem. Fechner, as I have said, won the first round, and for almost a century it looked as though the logarithmic law would prevail over the power law. Two rather convincing kinds of evidence seemed to favor it. First, there was the argument based on differential sensitivity, which we measure by noting how large an increment must be added to a stimulus in order for a person to detect the difference a certain percentage of the time. These just noticeable differences turn out to be roughly proportional to the magnitude of the original stimulus (Weber's law). There is a kind of relativity here. You can detect a candle added to a candle, but not a candle added to the light of the noonday sun. Fechner noted this principle and then proceeded to *postulate* that each just noticeable difference corresponds to a constant increment in sensation.

At this point we are reminded of what Bertrand Russell said in another connection about postulation: "The method of 'postulating' what we want has many advantages; they are the same as the advantages of theft over honest toil" (*12*).

Be that as it may, if we grant Fechner's postulate, and if Weber's law is true, it follows that sensation grows as the logarithm of the stimulus.

The other line of evidence is exemplified in the astronomer's scale of stellar magnitude, which appears to date from Hipparchus (about 150 B.C.). Before the days of photometry, men looked at the stars and judged their apparent brightness on a scale from 1 to 6, where 1 stands for the brightest stars and 6 for the faintest. Successive numbers on the scale were assigned to successive equal-appearing intervals of stellar magnitude. Then an interesting thing happened. Men finally learned to measure the brightness of the stars by photometric methods, and, much to Fechner's delight, it turned out that the magnitudes assigned by the simple process of looking and judging were spaced by approximately equal steps on a logarithmic scale of photometric value. In keeping with this fact, the step on the modern scale of stellar magnitude has now been standardized at 4 decibels (0.4 log unit) (*13*). [Actually, the early astronomers' scales differed among themselves, and most of them were slightly, but systematically, different from the logarithmic scale (*14*).]

So here we have two classes of sensory measures lending some degree of credence to the logarithmic law: the results of measuring differential sensitivity and the results of *partitioning* a sensory continuum into equal-appearing intervals.

Then what about Plateau's view? Is there any experimental evidence that supports the power law? Actually, Plateau appears to have been the first experimenter to bring the partitioning method out of the heavens and into the laboratory; or, more precisely, into the studio, for he asked eight artists to paint a gray that would appear halfway between extreme black and white. The eight grays, independently produced, turned out to be *"presque identiques."* Furthermore, the goodness of the partition into equal intervals—black to gray to white—appeared to remain stable under different degrees of illumination. Starting from this latter fact, Plateau conjectured his power law.

Unfortunately, for reasons we will consider shortly, the method of partitioning is not capable of verifying the power law. It was because Plateau did not know this fact that he later felt obliged to change his mind about the law. Actually, however, he never should have changed it, for he was right in his basic conjecture. The correct law is the power law.

Ratio Scale of Sensory Magnitude

In our struggle to discover the measures of things, we do not always hit upon the simplest and easiest procedure first off. Fechner's method of constructing a scale by the tedious process of measuring just noticeable differences and counting them off was involved and indirect—and even included one of Russell's larcenous postulates in the bargain. Plateau's method was more direct, certainly, but it aimed, at best, only at the construction of an interval scale—one on which the zero point would be arbitrary and on which ratios could have no meaning.

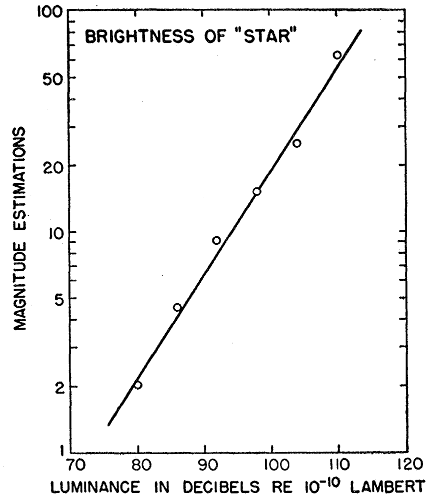

Fig. 1. Direct magnitude estimations of the apparent brightness of a small target subtending an angle of about 1.5 minutes of arc. The observer was first shown a luminance of 92 decibels and told to call it "10." Relative to this modulus he then estimated the other brightnesses, which were presented twice each in irregular order. Points are medians for 15 observers. The straight line in this log-log plot determines a power function with an exponent of 0.47.

Clearly, if a ratio scale was to be achieved, judgments of subjective ratios would have to be made. In the early 1930's the first serious efforts to get people to respond to ratios of sensory magnitude finally got under way, and over the past few years a swelling tide of ratio scaling procedures has given this whole subject an exciting new look. It turns out that the ordinary thoughtful observer *can* make quantitative estimates of sensory events. He can adjust a light so that it appears half as bright as another, or a fifth as bright, or a tenth as bright. He can also set it to a given multiple of the apparent brightness of a standard light. Furthermore, given some standard brightness, to which is assigned an arbitrary value such as 10, the typical observer can assign numbers to other brightnesses proportional to their apparent level, as he sees them. These and several others are the procedures used.

On 17 different perceptual continua the application of these methods has resulted in power functions. To a fair approximation, estimated subjective magnitude is proportional to the stimulus magnitude raised to a power. The exponents, experimentally determined, have ranged from about 0.3 for loudness to 3.5 for the subjective intensity of electric shock applied to the fingers. The fundamental psychophysical law that emerges from these findings is simply this: equal stimulus ratios produce equal subjective ratios. That is all there is to it. The proportionality between stimulus ratios and subjective ratios is a pervasive first-order relation, observed in empirical studies on numerous perceptual continua. Second-order departures from this law are sure to exist (we already know about some of them), but the wide invariance of the first-order relation is a matter of prime importance.

I was particularly interested to see what form the ratio scale of subjective magnitude would take for small luminous targets resembling a star, for the astronomers' estimates of stellar magnitudes gave us the first psychological scale, though it was not a ratio scale. Fifteen subjects were asked to assign numbers proportional to the apparent brightness of a small spot of light resembling a star, whose intensity was varied over a range of 30 decibels (*15*). The median estimates gave a close approximation to a power function with an exponent of 0.47. Thus, the apparent subjective magnitude of the "star" grows approximately as the square root of the photometric level (see Fig. 1). (The exponent here is greater than that for larger luminous targets, where the exponent is close to one-third.)

Now the question arises, why did the early astronomers' scale approximate a logarithmic function, whereas direct estimations of apparent brightness give a power function? This stubborn question, which has long been a puzzle, actually turns out to have a very simple answer. It hinges on the fact that a person's sensitivity to differences (measured in subjective units) is not uniform over the scale—a fact related to Weber's law. A given difference that is large and obvious in the lower part of the range is much less impressive in the upper part of the scale. This asymmetry in the observer's sensitivity to differences produces a systematic bias whenever he tries to partition a continuum into equal-appearing intervals. On all continua of the class I have called "prothetic" (*6*), of which brightness is one, we observe that the scale constructed by partitioning into categories is a convex function of the ratio scale obtained by direct estimation —that is, the category scale plotted against the ratio scale gives a curve that is concave downward (see the upper curve in Fig. 2).

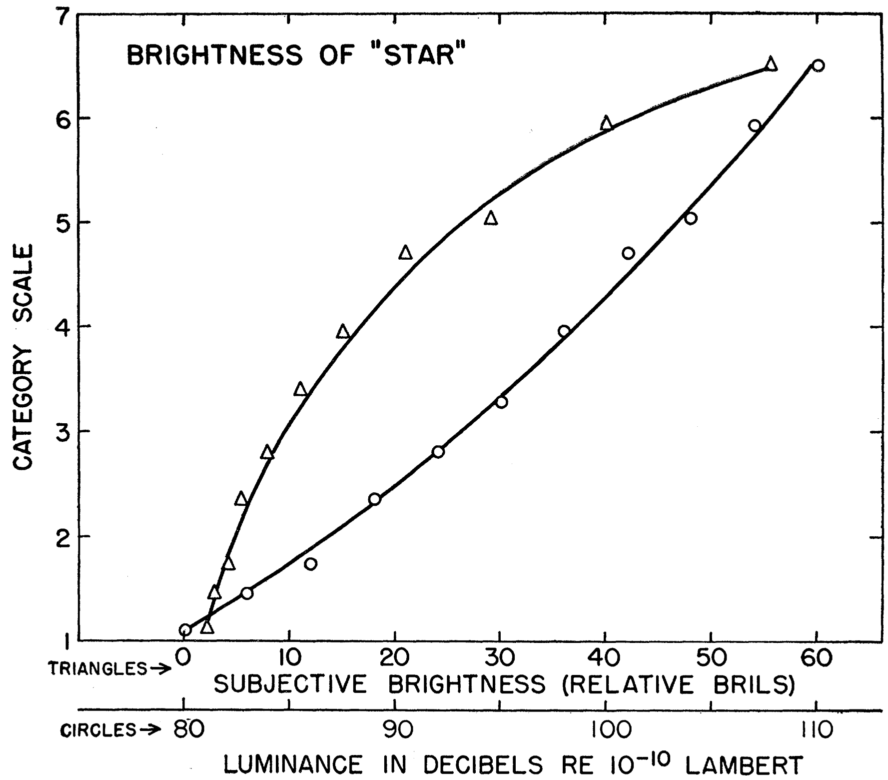

Fig. 2. Judgments of brightness on a category scale from 1 to 7. A luminance of 80 decibels was presented and called "1," and one of 110 decibels was presented and called "7." The observer then judged the various levels twice each in irregular order. Points are averages for 15 observers. The results are plotted against two different abscissa scales. The triangles are plotted against the magnitude scale obtained from the line in Fig. 1. The circles are plotted against the luminance scale in decibels. Note that the triangles determine a curve that is concave downward. The lower curve (circles) suggests that partitioning into a finite number of categories produces a function that is roughly logarithmic, but not precisely so.

The systematic bias that warps our judgments whenever we try to divide a segment of a prothetic continuum into equal-appearing intervals was presumably operating, of course, when the early astronomers arranged their scale of stellar magnitudes. The bias was apparently strong enough to make this scale approximate a logarithmic function of photometric intensity. But this roughly logarithmic outcome really helps Fechner's argument not at all, for when we look more carefully at the processes involved, we find that the form of the scale of stellar magnitudes is merely another example of the fact that man exhibits a built-in bias whenever he tries to partition a segment of a prothetic continuum. It is too bad that Plateau, when confronted with the results of another experiment on partitioning (conducted by Delboeuf), let himself be persuaded to renounce the power law.

Our confidence in the view that some kinds of partitioning are subject to bias gathers strength from the finding that not all partitioning is distorted in this manner. On another class of continua, called "metathetic," where sensitivity is not asymmetrical, the process of partitioning may produce an unbiased, linear scale (*15*). Pitch is an example of a metathetic continuum, whereas loudness is prothetic. With loudness, the physiological process underlying our discriminations seems to involve the *addition* of excitation to excitation. With pitch, the process is believed to be the *substitution* of excitation for excitation, a change in the locus of the excitation. It is indeed interesting that the difference between these two basic classes of physiological mechanisms reflects itself in the behavior of the psychological scales which we construct from the sensory responses involved.

The Ear as a Compressor

Since scales of measurement bear little fruit if they do not serve to predict or explain anything, it is fair to ask what other insights into natural phenomena may stem from this boom in sensory measurement. I do not pretend to know where it all will lead, but I would like to cite one final example of its bearing on an interesting question.

One of the amazing properties of a sensory system like hearing is the almost incredible dynamic range of its operation. Energy ranges of billions to one are taken easily in stride (*16*). In order to encompass such dynamic ranges, in order to detect sound vibrations whose amplitudes are less than the diameter of a hydrogen molecule and, at the same time, respond adequately to a thunderous roar, the sensory system must behave in some sense as a "compressor." The interesting question is, where does the compression take place—in the end organ or in the central nervous system?

First, it is to be noted that the degree of the compression we are concerned with is given by the exponent of the power function relating loudness to sound intensity (*16*). This exponent of about 0.3 tells us that in order to double the apparent loudness we must multiply the energy by a factor of about ten (or the sound pressure by the square root of ten). Contrast this relation with the growth of the subjective intensity of electric shock, which shoots up as the 3.5 power of the current applied to the fingers (*17*). Here, when we double the current, the typical observer judges the shock to be some nine or ten times as great as it was previously. There is no compression under this direct electrical stimulation. On the contrary, the system behaves as though it contained an "expander" of some sort. Through the direct measurement of sensory magnitudes, a striking difference is revealed between the behaviors of two sensory mechanisms.

Now the question is, what would happen if we were to stimulate the auditory nerve directly with an electric current? Some of us once explored this problem in a group of clinical patients whose middle ears had been opened, for one reason or another, so that an electrode could be placed inside the open cavity (*18*). Since other nerves, such as the facial and the vestibular, were readily stimulated under these circumstances, we had reason to believe that electrical stimulation also reached the auditory nerve, as indeed it must have done in those ears that heard only a noise whose character bore no systematic relation to the frequency of the stimulating current. A random, unpatterned excitation of the auditory nerve fibers would be expected to result from a current applied to the middle ear, and an unpatterned excitation of fibers should lead to the perception of noise rather than tone.

The interesting thing, from our present point of view, was the rapid growth of the loudness of the noise as the current was increased. The patient was asked to compare the noise with a sound produced by an acoustic stimulus led to his normal, unoperated ear. He adjusted the loudness in his normal ear to match the loudness of the noise in the operated ear. This simple procedure disclosed a startling fact. The growth of loudness was many times steeper under electrical than under acoustical stimulation. The exponent of the power function under electrical stimulation was, in fact, of about the same order of magnitude as that observed when a 60-cycle current was applied to the fingers.

Many interesting questions are raised by these measurements, but one implication is clear. The "compression" observed in the normal response of the auditory system to a sound stimulus is apparently not an affair of the central nervous system, for if we bypass the ear and stimulate the auditory nerve directly, we detect no compression. Rather, there results an "expansion" in the subjective response. Apparently, therefore, it is to the end organ itself that we must look for the mechanism of compression that governs the slow growth of loudness with acoustic intensity.

So it appears that, with the aid of scales constructed for the measurement of sensation, we may have disclosed a fundamental difference between two transducer mechanisms. The transduction of sound energy into nervous energy is by way of an "operating characteristic" that somehow compresses the over-all sensory response. The transduction of electrical energy into nervous energy seems to follow quite a different rule. To be sure, this outcome is but a trifle in the vast and relentless contest to unwind the tangle of nature, but it testifies, in simple example, to the profit that may accrue from measuring the "unmeasurable" (*19*).

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News of Science

Science Education Legislation for 1958

Congressional hearings are now being held on proposed legislation for additional Federal support for education, especially science and language education, in the United States. There are two major bills. On 28 January, Senator H. Alexander Smith of New Jersey, for himself and 10 other senators, introduced a bill entitled the "Educational Development Act of 1958" (S.3163). This bill contains the recommendations that were presented in President Eisenhower's Education Message to Congress on 27 January. An identical bill (H.R.10278) was introduced in the House of Representatives by Carroll D. Kearns of Pennsylvania. On 30 January, Senator Lister Hill of Alabama, for himself and 26 other senators, introduced S.3187, a bill entitled "The National Defense Education Act of 1958." A companion bill (H.R.10381) was introduced in the House of Representatives on the same day by Carl Elliott of Alabama. Several other bills dealing with educational matters have been introduced, but this analysis will be confined to the two major bills. All of the Senate bills have been referred to the Committee on Labor and Public Welfare, and all of the House bills to the Committee on Education and Labor.

For purposes of identification in the following discussion the bill introduced by Senator Smith and Congressman Kearns will be referred to as the Administration bill; the one introduced by Senator Hill and Congressman Elliott, as the Hill-Elliott bill.

Purposes. Both are omnibus bills with broad objectives. The purposes are similar, but there are some interesting differences in wording.

The purposes of the Administration bill are "to encourage and assist in the expansion and improvement of educational programs to meet critical national needs through the early identification of student aptitudes, strengthening of counseling and guidance services in public high schools, provision of scholarships for able students needing assistance to continue their education beyond high school; strengthening of science and mathematics instruction in the public schools; expansion of graduate programs in colleges and universities, including fellowships; improvement and expansion of modern foreign language teaching; improving state educational records and statistics; and for other purposes."

The purposes of the Hill-Elliott bill are "to strengthen the national defense, advance the cause of peace, and assure