# Philosophical Aspects of the Mind-Body Problem

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# The Mind-Body Equation Revisited

James T. Townsend

GUSTAV FECHNER believed in the fundamental mind-body unity of man. In fact, his monism formed the motivating philosophy which led to the two tomes of *Elemente der Psychophysik* first published in 1860. It was his hope that this work would influence philosophical thought concerning the mind-body problem. Nevertheless, he was quite adamant in stressing the firm empirical basis for his experiments and theoretical conclusions. Fechner felt that whatever the outcome of the mind-body controversy (assuming eventual resolution), his empirically derived laws would stand in their own right.

The late Edwin G. Boring remarks that Fechner "was never accepted as a philosopher" (Boring, 1950, p. 281), and in another place "Fechner's fame is as a psychophysicist and not as a philosopher with a mission" (Boring, 1950, p. 279). Thus, although there was some reference to Fechner at the Honolulu conference, in general we must accept the fact that Fechner's goal of proselytization within the realm of philosophy, via his psychophysics, was never attained. Nevertheless, Fechner undoubtedly had an enormous impact on the emerging experimental psychology. Indeed, as we shall see, the ramifications of his "solution" to the mind-body problem are still giving rise to empirical and theoretical research and controversy in psychology, which in turn create some eventual feedback to the parent tree of philosophy.

In this context, it seems quite manifest that fundamental contributions to science have often depended on or interacted with the contributor's philosophy. Newton's conception of space apparently was related to his acceptance of Euclid's axioms as empirical statements, as Kant's belief in an absolute space was to Newton's great Principia. Developments by nineteenth-century mathematicians in non-Euclidean geometry and empirical models for these constructions provided a philosophical base for Einstein's contributions to a new model of the universe (see, for example, Jammer, 1954). Recent interaction goes in reverse also, as witness the influence of scientific advance on the development of logical positivism or the (sometimes spurious) theological and philosophical interpretations of Heisenberg's principle. Even rejection of a point or a problem on one or the other side is, of course, additional comment on the mutual influences. Behavioristic psychology's eschewing of the mind-body problem helped determine content of experiment and theory especially in the area of "learning" for many years. We are apparently still seeing a backswing of experimental psychology to more "mentalistic" approaches which gained impetus with the innovation of the intervening variable and has gathered further acceptance with the advent of artifical intelligence, and communications and systems theory. With this apologia it will perhaps be clear that this exposition rests on the assumption tersely stated by Herbert Feigl, " ... that there is no sharp demarcation between (good) science and (clearheaded) philosophy" (Feigl, 1967).

From this point of view, it may be of interest to the philosophically minded to obtain a glimpse of where Fechner's contributions stand today, as well as to engage in some side excursions to a few related areas and issues. Our tour will be in no way exhaustive. I will attempt to introduce some novel thoughts on some of the topics rather than simply provide reviews, which would be severely limited by time and space. Interested readers are referred to the appended references. Hopefully, the intellectual diversions will help compensate for the somewhat desultory presentation.

We first briefly consider modern interpretation of Fechner's derivation of his mind-body equation, especially with respect to the mathematical principles involved. Next, the role of degree of precision in falsifiability will be encountered in remarks on (1) number of free variables in mathematical statements, (2) Meehl's recent article on a "methodological paradox," (3) a possibility for taking the importance of any given magnitude of "significance," in tests of the null hypothesis, into consideration when selecting the number of subjects to run, and (4) Fechner's logarithmic law versus an alternative proposed by S. S. Stevens. Following this, an apparent difficulty in applying Duncan Luce's (1959) findings that relate scale types and psychophysical functions to Stevens' power law will be noted. Finally, we will discuss the influence of experimental contexts on the form of psychophysical functions and methods of incorporating these effects into a mathematical theory of these functions.

Essentially, Fechner trichotomized the state of affairs into the external world, the body (the physical), and the mind. Excitation in the body was, he thought, related in a proportional manner to the magnitude of a stimulus and he endeavored to support this notion with several arguments, most of which appear irrelevant now. Given this relation, he assumed that an obtained curve relating the stimulus to sensation, and therefore to mind, must reveal the functional relationship that holds between mind and body. His contribution to this problem, resulting in the establishment of a function that maps stimulus values on numbers representing sensation magnitudes, is accepted as a rather monumental scientific effort, although current perspective now emphasizes his influence on other investigators and on psychophysical methodology rather than his final conclusions regarding the psychophysical law (which we shall use as equivalent to mind-body equation).

Fechner employed as a hub assumption Weber's law, which states that resolving power of a sensory system is inversely proportional to the magnitude of the stimulus, that is, $\frac{\Delta X}{X} = k$, where X is the stimulus magnitude, $\Delta X$ the stimulus increment necessary for a 50 percent correct discrimination rate, (a just noticeable difference or JND), and k is (Weber's) constant. Fechner next assumed that just as one may measure material by matching equal intervals of one substance (e.g., the wood of a yardstick) to equal intervals of another, one can use the (equal) ascending Weber ratios as building blocks which correspond at each increment to equal sensation steps. Thus, he believed in the equality of sensation JNDs and further that a sensation value corresponding to a particular stimulus magnitude could be determined by summing the component JNDs. If Fechner had paused here and employed the finite calculus (for an elementary treatment, see Goldberg, 1958) to establish a functional relation between stimulus and response, he would have obtained the equation,

$$X_n = (kh + 1)^n X_o,$$

where k is Weber's constant, h is the (constant) sensation increment for each stimulus JND, $X_o$ is the absolute threshold stimulus and $X_n$ is the stimulus magnitude in JNDs above threshold. Hence, this equation shows that for (n) equal steps in sensation one obtains a geometric increase in stimulus magnitude. We can also solve the difference equation in terms of equal stimulus increments (h') to obtain an expression structurally similar to Fechner's continuous solution:

$$Y_n = \sum_{i=1}^{n} [\frac{1}{X_o + ih'}] \frac{h'}{k},$$

in which Weber's law is implicit, or using direct summation, obtain

$$Y_n = \frac{1}{k} \sum_{i=1}^{n} \frac{X_i - X_{i-1}}{X_{i-1}}, \quad (Y_n = \text{sensation valve})$$

in which Weber's law is explicit. The latter can be verified by observing

$$Y_n - Y_{n-1} = \frac{1}{k} \frac{X_n - X_{n-1}}{X_{n-1}} = 1 \text{ (letting } h = 1).$$

Fechner, however, using his mathematical auxiliary principle, solved a continuous approximation to the difference equation, namely,

$$\frac{dX}{X} = k dY,$$

from which he obtained

$$Y = k' \ln\left(\frac{X}{X_0}\right), k' = \frac{1}{k}.$$

This expression also implies Weber's law, since $\Delta Y = k' \ln\left(\frac{X + \Delta X}{X_0}\right) - k' \ln\left(\frac{X}{X_0}\right) = k' \ln\left(1 + \frac{\Delta X}{X}\right) = k' \ln(1 + k)$, a constant, but we now know that the continuous approximation will not in general imply a result found with finite differences (Luce and Edwards, 1958). To illustrate this point, suppose the empirical relation

$$\frac{\Delta X}{X(X - 1)} = k, (X_0 > 1),$$

were discovered. Using Fechner's first assumptions we find the difference equation

$$X_n = k\Delta Y X_{n-1}(X_{n-1} - 1) + X_{n-1}.$$

Direct summation, which corresponds to simply graphing equal sensation increments against cumulative stimulus JNDs, gives

$$Y_n = \frac{1}{k} \sum_{i=1}^{n} \frac{X_i - X_{i-1}}{X_{i-1}(X_{i-1} - 1)}.$$

Similarly, it is easy to see how the stimulus values change in the special case where we set the (constant) sensation increments to $\Delta Y = \frac{1}{k}$; for this yields the difference equation solution,

$$X_n = X_0^{2^n}.$$

This expression shows, as we might expect, that for an equal sensation step the stimulus magnitude changes much faster than geometrically, in fact as a power of $2^n$ of absolute threshold. (Compare this with the consequence of letting $h = \frac{1}{k}$ in $X_n = (kh + 1)^n X_0$ which yields $X_n = 2^n X_0$). On the other hand, the associated differential equation would, according to Fechner's auxiliary principle, presumably be

$$\frac{dX}{X(X - 1)} = k dY,$$

and the solution to this expression is

$$Y = \frac{1}{k} \ln(1 - \frac{1}{X}).$$

Testing to see if this expression yields Weber's law, we derive

$$\Delta Y = \frac{1}{k} [\ln(1 - \frac{1}{X + \Delta X}) - \ln(1 - \frac{1}{X})]$$
$$= \frac{1}{k} [\ln (1 - \frac{1}{X + X(X - 1)k}) - \ln(1 - \frac{1}{X})]$$

which we easily see is not constant by letting $k = 1$, for then

$$\Delta Y = \ln(1 + \frac{1}{X}).$$

In fact, the only value for which constancy is attained is the degenerate, experimentally uninteresting $k = 0$.

The above question of correct derivation procedures does not appear to be of trivial importance, since expressions that are more complex than Weber's law seem to be required at stimulus extremes and under certain background conditions. Luce and Edwards (1958), taking a functional equations approach, have shown how one can obtain continuous solutions that are compatible with a general JND relationship. Recently, Levine (1970), Falmagne (1971), and Krantz (1971), have made further contributions to the understanding of scales derived from discrimination measures in general and JND functions in particular.

Fechner's logarithmic law seems to hold rather well for a number of what the late S.S. Stevens (1957) referred to as methathetic continua, but to be largely inappropriate for prothetic continua, these adjectives corresponding roughly to "qualitative" and "quantitative" respectively. An example of a prothetic stimulus continuum is energy in a sound wave and an example of a methathetic stimulus continuum is frequency in a sound wave. Fechner's first nonempirical assumption seems to be false for prothetic continua; namely sensation JNDs are not subjectively equal. It is not clear precisely why Fechner chose to define his sensation units in terms of resolving power of the sensory channel, but part of the explanation may be that he was overly impressed with the logarithmic relationship suggested by Laplace and others to hold for such "psychological" quantities as the subjective value of money (see for example, Fechner, 1966, pp. 197–198). The subjective value of an increment of money was taken to be inversely proportional to the wealth already accrued.

The greatest and most successful empirical assault launched against Fechner's law has been that of S. S. Stevens (see for example, Stevens, 1959; 1957). In contrast to Fechner's theoretically based approach, Stevens proceeded in a more experimentally direct manner. His experimental tasks involve the observer with making one of various estimates or productions related to his perception of the magnitude of the stimulus. Most of these methods rest on the rather strong and apparently implicit assumption that sensations lie on a ratio or log-interval scale (corresponding to a power function) and, further, that an observer can meaningfully report these sensations in terms of ratios. However, Stevens attempted to show, not without success, that the form of the psychophysical function is constant across these methods. A method fairly typical of Stevens' approach is that of magnitude estimation, and I will base most of my later remarks on this method. For later reference we note here that Stevens' basic obtained relationship reads: $Y = AX^B$ where A corresponds to the unit and B governs the curvature of the relation.

Apart from questions of validity of assumptions and correctness of mathematical derivations, one may ask about the ease with which one can test a theory. The concepts concerned with this latter problem are often subsumed under the general heading of "falsifiability."

## Falsifiability of Psychological Laws

Before taking up the question of falsifiability for the psychophysical function, we will discuss problems of falsifiability within psychology and statistical decision making. This discussion will include some comments on a recent article by Paul Meehl (1967) and some suggestions for dealing with scientific meaningfulness of hypotheses alternative to the null hypothesis and selecting the proper sample size. It will conclude with a remark on the falsifiability of a logarithmic function versus a power function.

The aspects of falsifiability with which we begin refer to what Popper (1959) has called degree of universality and degree of precision. Popper uses an example of the orbits of heavenly bodies (more universal) or planets (less universal) being posited to be either circles (more precision) or ellipses (less precision) to illustrate the concepts. It is difficult to say how much such considerations play a subconscious role in the theoretical undertakings of psychologists; unfortunately it is quite rare to see them overtly used either in theory formation or, for that matter, in theory evaluation.

It is not always easy to tie down exactly what we should mean by degree of precision. In psychology, when testing two models, it is common to attempt to equate the number of parameters in each to give each an equal opportunity to fit the data. But the complexity of the resulting theoretical curves is often related to the way in which the parameters enter the functions of each model. (Straight lines are said to be less complex than curves, and curves with an inflection point more complex than those always positively or negatively accelerated.) So this rather simplistic criterion seems to go away in certain areas, such as signal detection. A typical simple yes-no detection experiment involves the presentation on each trial of a signal plus noise, or of noise alone, the observer's task being to report as best he can whether the signal was present. The observer's hit frequency, P(HIT), is defined as the probability of his making a correct response, P(yes|signal + noise). The observer's false alarm frequency, P(FALSE ALARM), is defined as the probability of his reporting the presence of a signal when only noise is presented, P(yes|noise alone). These two probabilities completely describe his performance frequencies. As the experimenter manipulates the pay-off structure and the intensity of the signal relative to the noise, the observer's hit and false alarm frequencies change. A theoretical or empirical function relating P(HIT) to P(FALSE ALARM) as the experimenter manipulates the situation is called an ROC (receiver-operating-characteristic) curve. This type of experiment can be viewed as the study of discrimination behavior where physical and motivational variables are manipulated.

There exist theories that are sufficiently complex to make predictions for several dependent variables but which predict straight-line ROC curves (e.g., Townsend, 1966) with slope 1; similarly, there are theories of rather astonishing simplicity that predict curvilinear ROC curves (see for example, the exponential model of Green and Swets, 1966, pp. 78–81). Furthermore, it is usually easy to equate number of parameters in testing ROC predictions against the data in these diverse models. However, note that a model that posits that P(HIT) = A $P^2$(FALSE ALARM) + B P(FALSE ALARM) + C, that is, that the probability of a hit is a quadratic function of the probability of a false alarm, implies the coefficients:

$$-1 \leqslant A \leqslant 0, B = 1 - A, C = 0.$$

These coefficients guarantee that the ROC curve will look familiar (roughly similar to the curvilinear ROC function predicted by signal detectability theory), but there are no values of the coefficients that make the curve symmetric about the antidiagonal except for the limiting trivial case of $A = 0$. Thus, most of the parametric freedom was used in simply obeying rather general constraints of a reasonable ROC function and the generality given by two-parameter signal defectability theory (Green and Swets, 1966, pp. 62–64) does not accompany the quadratic function. It might be remarked peripherally that a strict interpretation of Fechner's threshold views implied a straight-line ROC curve and that, although strict threshold theories appear to be negated for yes-no signal detection experiments, they can on occasion account quite well for data obtained in forced-choice detection situations (see for example, Atkinson and Kinchla, 1965; Kinchla, Townsend, Yellot, and Atkinson, 1966).

Meehl (1967) has discussed what he refers to as a methodological paradox. The idea seems to be that an increase in experimental precision or in number of subjects run (tested, etc.) results in increased falsifiability for physical theories but in decreased falsifiability for psychological and other social science theories. The main contributing factors are (1) the tendency to formulate hypotheses which predict only that a difference exists or that a difference in one direction should exist, and (2) because of the high degree of intercorrelation of psychological variables, groups differing on almost any independent variable will be almost certain to differ in a positive or negative direction on the dependent variable. In contrast, physical theories are more likely to predict (point) numerical values; so, although a purist might object that everything pretty much affects everything else in physics, too, in the sense of various measurable characteristics or events being correlated, this is largely ameliorated by the point predictions. In fact, it is probably the case that the impossibility of eliminating all "irrelevant" variables in physical experiments would lead ultimately to the falsification of any (point prediction) theory, due to the concomitant constant errors. This is because the empirical mean would converge in probability to the incorrect value.

Meehl's critique must relate to the modal state of affairs rather than to theorizing dilemmas that arise by necessity; for example, there is nothing that prevents even a low-level theory (i.e., not involving a high degree of theoretical structure) from predicting the null hypothesis, in which case falsifiability increases with increased precision or sample size. Occasionally a neophyte in psychology is taken to task for attempting to "prove the null hypothesis. However, it is the centention here that the obverse aspect of Meehl's criticism is that the null hypothesis is a point prediction and hence in some ways more justifiable than predicting that a difference exists.

Meehl also scores psychologists for failure to appreciate the difference between

$$\{[T \Rightarrow O] \,\&\, \sim O\} \Rightarrow \sim T \text{ and } \{[T \Rightarrow O] \,\&\, O\} \rightarrow T,$$

where "$\Rightarrow$" is logical implication and "$\rightarrow$" is inductive inference.

There are two asymmetries between these two modes of inference. One is that the first rules out all theories that imply O, including T, but the second (excluding the second problem, to be discussed shortly) offers unique support for T if and only if T is the only theory that predicts O; therefore the larger the class of theories that predicts O, the less the support offered T by $[(T \Rightarrow O) \,\&\, O] \rightarrow T$. The second asymmetry, somewhat weaker than the first, is associated with the typical universal quantification of theoretical predictions. Thus, within the classical hypotheticdeductive framework, O must occur in empirical test every time, if the theory is true. One failure of O to evidence itself and T is falsified forever, but T is never quite verified completely since it is always possible that the "next" test will discover $\sim$O and hence $\sim$T.

Although the above reasoning is compelling as far as it goes, it would be more pertinent for modern science if both asymmetries were discussed within the context of statistical decision theory. For example, suppose a theory T predicts a significant one-tail difference, and the prediction is confirmed at some given $\alpha$ level (i.e., probability of a Type I error, the probability that significance is obtained when, in fact, no difference exists). Then if one is willing to posit an a priori probability distribution over the set of possible theories, including theory T and the null hypothesis, he can compute the a posteriori probability that T is true. This approach allows us to handle the first asymmetry in a more rigorous manner. The Baysians argue (fairly convincingly, it seems to me) that when we (even non-Baysians) interpret scientific results we informally or intuitively assign a priori values or ranges of values to the set of possible theories and make some rough estimate in our heads concerning the likelihood it is, in fact, our own theory that is true (it is not necessary to know all other possible theories, only some probability for our theory versus all other theories); and that therefore we may as well perform some actual computations. The Baysian point of view, of course, prescribes the setting of the decision cut point, and therefore the $\alpha$ level, by maximizing expected value of the experiment.

For the second asymmetry also, we can employ statistical thinking. Errors of measurement and inextricable "irrelevant" random variables greatly reduce our expectation of obtaining a predicted result on every test, even though the theory is correct and predicts logically that a certain result should occur all the time. The statistical aspects seem to largely outweigh the strictly logical aspect. For instance, suppose that (now) T predicts that a difference between two groups of 4 exists and that the standard error of the mean is 1. Suppose also that it is about equally likely, a priori, that T is true as that the null hypothesis $T_n$ is true, and that either one or the other must be true. If we let $\alpha = .0001$ and a significant result is obtained, it follows that the probability that T is true, *given* significance, is very, very close to 1 (on the order of 0.99984+). It would therefore seem that, here, the verification induction problem (closely connected with the second asymmetry) would not ordinarily cause a scientist much concern. Although this example is extreme and contrived, it does suggest that the logical asymmetries can be ameliorated or possibly exacerbated (for the latter, suppose one's theory predicted the $T_n$ in this example) by the laws of chance.

A similar problem, intimately related to theory testing, concerns the practical or scientific importance of a (true) difference. Although the larger the sample size (N) we employ, the more probable that statistical significance will be attained if *any* difference exists, in practice differences below some point may be useless or uninteresting. Furthermore, it is easy to construct simple but reasonable examples where not only does the a posteriori expectated (i.e., mean) difference conditionalized on statistical significance *increase* with *decreasing* N, but also the a posteriori probabilities that the various possible alternative hypotheses gave rise to the significant difference shift toward the larger alternatives as N decreases.

Table 1 gives such an example (slide rule accuracy) in the case where the possible (true) means are $m = 0$, 1, or 4, where $m = 0$ with probability 1/4, $m = 1$ with probability 1/2, and $m = 4$ with probability 1/4. It is assumed that the population standard deviation, $\sigma$, is 1, that $\alpha = 0.10$ (constant) and that the distributions for each m are normal. The table then shows for $N = 1, 9, 100, +\infty$: (1) the values of the overall probability that statistical significance is obtained, (2) the conditional or a posteriori probabilities that the null hypothesis ($m = 0$) or either of the alternatives ($m = 1$, $m = 4$) is true given significance, and (3) the overall a posteriori expectation. The case where the a priori probabilities are uniform (1/3 for each, rather than 1/4, 1/2, 1/4) turns out to give the same types of changes with manipulations of N. Thus, if one is not so much interested in obtaining significance per se as being assured that, if it is obtained, it is a meaningful result, it may behoove him to refrain from acquiring a very large sample size. This antinomy does not, of course, actually contradict such other laws as that, for a fixed level of confidence, the interval corresponding to that confidence increases in width as N decreases or that the likelihood of a correct decision increases as N increases (as some quick calculations with the above example will show). Hence, these remarks should not be interpreted as a claim that small N is better than large N, but only that the frequently heard admonition to acquire as large a sample size as time and cost permits may not always be justified.

*TABLE 1 Probability of Significance, A-Posteriori Probabilities, and A-Posteriori Expectation (Conditional on Statistical Significance) as Functions of Sample Size N*

| | N | | | |
|---|---|---|---|---|
| | 1 | 9 | 100 | ∞ |
| P (Significance) | .47 | .63 | .68 | .78 |
| P (M = 0\|Significance) | .05 | .04 | .04 | .03 |
| P (M = 1\|Significance) | .42 | .56 | .60 | .65 |
| P (M = 4\|Significance) | .53 | .40 | .37 | .32 |
| E (M\|Significance) | 2.54 | 2.15 | 2.08 | 1.94 |

[M] = set of alternative means = {0, 1, 4}
E = expectation (average)
$\alpha$ = .10

More generally, statistical decision theory can be employed to utilize any number of variables and/or aspects to help determine not only a cut point on a decision axis, but also the best value of N. We shall develop the general mathematics of the situation here; specific distributions and functions could be inserted to fit particular cases. In order to prevent losing sight of the forest because of the trees, we assume at the outset the necessary continuity and differentiability of the various functions.

The definitions of our required symbols are given in the following list:

$\bar{x}$ ≡ observed (experimentally determined) mean
m ≡ difference of true mean from zero, $m \geq 0$
$m^*$ ≡ upper limit on true mean, $0 \leq m^* \leq +\infty$
o ≡ no difference, $m = 0$, corresponds to null hypothesis

$d(\cdot|\bar{x}) \equiv$ probability of coming to decision $\cdot$ ($\cdot$ = o or a)
a $\equiv$ decision corresponding to rejecting the null hypothesis, i.e., conclude that $m > 0$
$f(m|\bar{x}) \equiv$ conditioned probability of m given $\bar{x}$
$f(m) \equiv$ a priori probability of m
N $\equiv$ sample size
$c(N) \equiv$ cost function for obtaining N samples
$L(x,y) \equiv$ loss function for deciding y when x is true
$E(L|\bar{x}) \equiv$ expected average loss given $\bar{x}$
$E(L) \equiv$ overall expected loss

The function L(x,y) tells us the importance of concluding that a difference exists (a: $m > 0$), L(m,a) where one does and the importance of deciding that no difference exists (o: $m = 0$), L(0,o) when in fact, none does. It also shows the loss involved where we incorrectly conclude that no difference exists, L(m,o), $m > 0$, and that occurring when the incorrect decision is made that a difference does exist, L(0,a). Typically, $L(m,a) \leq 0$, $(m > 0)$, $L(0,o) \leq 0$ and $L(m,o) \geq 0$, $(m > 0)$ and $L(0,a) \geq 0$. We then wish to minimize the overall expected loss E(L) for given loss (L) and cost functions (c) and for given probability distributions (f). We write integrals of f as if f is always a density function but generalizations in notation are obvious for non-Riemannian integration and without loss of generality we could make f(0) a probability mass rather than a density. One further assumption we make is that N and c(N) are independent of L,f. N is implicit in affecting, of course, $f(\bar{x}|m)$ by way of its standard error $\sigma_{\bar{x}}(m) = \frac{\sigma(m)}{\sqrt{N}}$, where $\sigma(m)$ is the standard deviation in the probability density function with mean m.

We shall first write down the expected loss for any given empirical mean $\bar{x}$ and any fixed N. It will be convenient in the following to let $m > 0$ distinguish the possible (true) alternative means from the null mean.

$$E(L|\bar{x}) = f(0|\bar{x})\,[d(o|\bar{x})\,L(0,o) + d(a|\bar{x})\,L(0,a)] + \int_{\{m>0\}}^{m^*} f(m|\bar{x})\,[d(o|\bar{x})\,(L(m,o) + d(a|\bar{x})\,L(m,a)\,]dm,$$

where $\{m > 0\}$ indicates that the integrand is "summed" over all values of m that are greater than 0.

If we determine a method of minimizing this expression for any N, $\bar{x}$, we can then show how to select (in principle) a value of N that will be optimized in terms of minimizing E(L) (and hence maximizing expected value).

Since $d(o|\bar{x}) = 1 - d(a|\bar{x})$, we can rewrite the foregoing expression as

$$E(L|\bar{x}) = d(a|\bar{x})\,[f(0|\bar{x})\,(L(0,a) - L(0,o)\,) - \int_{\{m>0\}}^{m^*} f(m|\bar{x})\,(L(m,o)$$

$$- L(m,a))dm\} + f(0|\bar{x})(L(0,o) - \int_{\{m>0\}}^{m^*} f(m|\bar{x}) L(m,o)\, dm.$$

Next, since

$$f(0|\bar{x})(L(0,a) - L(0,o)) \text{ and } \int_{\{m>0\}}^{m^*} f(m|\bar{x})(L(m,o) - L(m,a))dm$$

give the average total weighted losses for saying o or a respectively for the given $\bar{x}$, it follows that $E(L|\bar{x})$ will be minimized by taking $d(a|\bar{x}) = 0$ (always accepting the null hypothesis) when

$$f(0|x)(L(0,a) - L(0,o)) > \int_{\{m>0\}}^{m^*} f(m|\bar{x})(L(m,o) - L(m,a))dm,$$

letting $d(a|\bar{x}) = 1$ (always rejecting the null hypothesis where the inequality is reversed) and arbitrarily picking a response when the two sides are equal. (Note that the term

$$f(0|\bar{x}) L(0,o) + \int_{\{m>0\}}^{m^*} f(m|\bar{x}) L(m,o)\, dm$$

does not affect the decision and is a constant loss [or gain], always present.) This decision rule may be summarized as:

$$\text{If } f(0|\bar{x})(L(0,a) - L(0,o)) \begin{Bmatrix} > \\ < \\ = \end{Bmatrix} \int_{\{m>0\}}^{m^*} f(m|\bar{x})(L(m,o) - L(m,a))dm$$

$$\text{then, let } d(a|\bar{x}) = \begin{Bmatrix} 0 \\ 1 \\ 1/2 \end{Bmatrix}.$$

It will be helpful in the remainder of the derivation to note that

$$f(0|\bar{x}) = \frac{f(\bar{x}|0)f(0)}{f(\bar{x})}, \quad f(m|\bar{x}) = \frac{f(\bar{x}|m)f(m)}{f(\bar{x})}$$

and that the $f(\bar{x})$'s cancel out in the above inequality.

With this decision rule in hand, we now peruse the overall expected loss, employing again $f(\cdot|\bar{x}) = \dfrac{f(\bar{x}|\cdot)f(\cdot)}{f(x)}$.

$$E(L) = \int_{\bar{x}=-\infty}^{\infty} \{d(a|\bar{x})\{\frac{f(\bar{x}|0)f(0)}{f(\bar{x})}(L(0,a) - L(0,o)) - \int_{\{m>0\}}^{m^*} \frac{f(\bar{x}|m)f(m)}{f(\bar{x})}.$$

$$(L(m,o) - L(m,a))dm] + \frac{f(\bar{x}|0)f(0)}{f(\bar{x})} L(0,o) + \int_{\{m>0\}}^{m^*} \frac{f(\bar{x}|m)f(m)}{f(\bar{x})} L(m,o)\,dm] \cdot f(\bar{x})d\bar{x} + c(N).$$

By our decision rule, this complicated expression devolves itself to

$$E(L) = \int_{\bar{x}=-\infty}^{+\infty} f(\bar{x}|0)f(0)\,L(0,o)d\bar{x} + \int_{\bar{x}=-\infty}^{+\infty} [\int_{\{m>0\}}^{m^*} f(\bar{x}|m)f(m)L(m,o)dm]d\bar{x} + \int_{\{\bar{x}|d(a|\bar{x})=1\}} [f(\bar{x}|0)f(0)(L(0,a) - L(0,o)) - \int_{\{m>0\}}^{m^*} f(\bar{x}|m)f(m)(L(m,o) - L(m,a))dm]d\bar{x} + c(N),$$

and assuming sufficient regularity conditions, this becomes

$$E(L) = f(0)L(0,o) + \int_{\{m>0\}}^{m^*} f(m)L(m,o)dm + f(0)(L(0,a) - L(0,o))\,P(d(a|\bar{x}) = 1|0) - \int_{\{m>0\}}^{m^*} P(d(a|\bar{x}) = 1|m)f(m)(L(m,o) - L(m,a))dm + c(N).$$

This expression could now be differentiated with respect to N and thence investigated for values of N that will yield relative or absolute minima for E(L). We should pause here to note that without including c(N), we can expect E(L) to decrease monotonically as a function of N, and that minimizing E(L) is therefore different decision strategy than simply basing our decision, for example, on E(m|significance). Nonetheless, the importance of the difference m will affect where the decision threshold is placed.

This is about as far as we can go with no knowledge of the pertinent functions to be employed. It may be quite difficult in particular cases to carry out the above program analytically and probably represents too much labor and expense for many experiments to justify. Nevertheless, in cases where great cost of sampling is involved or when it is very important to be sure an "important" difference is "really" there if statistical significance is obtained, the present procedure can be carried out on digital computers.

From a philosophy of science point of view it might be interesting to conditionalize E(L) on subsets of the set of possible theories and investigate the a posteriori support for these subsets as functions of N, different loss functions, and the like.

From these statistical matters, we return to questions more immediately related to the mind-body problem.

With regard to degree of precision, Fechner's predicted psychophysical function makes a stronger statement about the world than does that relationship described by Stevens. Fechner's prediction is written $Y = k \log(\frac{X}{X_0})$ where $X_0$ is the threshold stimulus value, k is the reciprocal of Weber's constant, and k and the base of the logarithm determine the unit. Stevens' curve is usually given by $Y = a(X - X_0)^b$, where again $X_0$ is interpreted as a threshold, a corresponds to the unit, and b is a positive real number. To convince oneself of the latitude of Stevens' function relative to that of Fechner's, one need merely note that by choosing b greater than or less than 1, one can make the function positively or negatively accelerated without affecting the sign of the first derivative, whereas we are constrained to a negatively accelerated function with the logarithmic expression as long as we demand (as we must) that the function be monotonic increasing. Poulton (1968) in remarking on the scope of Stevens' power function has suggested that its usefulness be checked by trying out other functions with the same number of parameters, such as a polynomial of degree 2. The polynomials, however, suffer from a constitutional deficiency (for $X \geqslant 0$) not unlike that of the log function: namely, a polynomial cannot be both always increasing *and* negatively accelerated. This is because the leading coefficient determines, for large X, the sign of all the derivatives. For example, a quadratic function must be positively accelerated to be always increasing and $\geqslant 0$. Of course, one may fit part of a concave-down parabola to the points (as when they appear to level off) but even then the second derivative is fixed, rather than a function of X as for Stevens' function. Thus, it is seen that allowing noninteger exponents purchases an investigator a great deal of fitting power regardless of the formula's ultimate validity. One constraining factor that the power function does have is that it can possess no inflection points in a finite stimulus range.

These comments on degree of precision in psychophysical functions are especially germane in the case of Stevens' work since the form of the function seems to have been induced from data (albeit a wide range of data) rather than predicted on the basis of an underlying theory. A straight line is a straight line regardless of whether it is obtained by curve fitting or by prediction, but when more complex curves appear, one should be chary of prematurely generalizing the expression for an empirically obtained curve.

## Measurement Theory and the Psychophysical Function

Measurement theory has come a substantial way since Campbell (1920) implicitly made disreputable any measurement other than that corresponding to additive numerical operations. We may ask what part, if any, measurement theory has played in the establishment of a psychophysical equation. Actually, strange though it appears, there have been few attempts to establish a scale of sensation by the accepted method (see Suppes and Zinnes, 1963) of proving a representation theorem establishing a system of measurement of sensation within a modality, and a consequent uniqueness theorem giving the admissable transformations on the numbers representing measurements of sensation.

There are conclusions that might be drawn if we know independently the scales of measurement appropriate for sensation and stimulus. Duncan Luce (1959) has shown the scale types (corresponding to the class of admissable transformations) of the dependent variable and the independent variable can, within certain limits, very strongly restrict the possible laws holding between the independent and dependent variables. If the independent variable is on a ratio scale and we assume on the basis of Stevens' indirect evidence that sensation lies on a ratio or log-interval scale, then Luce shows that the intervening function must be a power function or possibly (in the latter case) of the form $Y = \delta\exp(\alpha x^{\beta})$.

Luce's results hold for the general formula $Y = a(X - X_o)^b$ if the independent variable is designated as $X - X_o$ rather than X. However, to be quite precise, it is not the experimenter who subtracts the threshold value from the input; on the contrary, it is the sensory or perceptual system of the observer. Hence, it seems somewhat specious to refer to $X - X_o$ as the independent variable, although the experimenter does set the background level (except for the observer's internal noise) of the stimulus. What if the internal function is formed not by taking a power of $X - X_o$, but in some other fashion? For example, let $b = 2$. Then it might be that the first stage of processing involves subtracting $X_o$ from X and then squaring this difference (for $X \geq X_o$); this would be in consonance with the assumption that $X - X_o$ is on a ratio scale. However, it might just as well be the case that the observer's system takes X, squares it, subtracts $2XX_o$ from it and adds $X_o^2$ to it. The basis for $X - X_o$ being a ratio scale-independent variable then would seem rather tenuous. In fact, it is not immediately clear how one would ever test this proposition in terms of extensive scale properties (Suppes and Zinnes, 1963).

If X is indeed mapped by the observer's system onto $X - X_o$ and the latter is on a ratio scale, how does this fit in with Luce's development which says that two ratio scales must be related, if at all, by a power transformation? The quantity $X - X_o$ is, of course, a difference transformation. The rub is that $X \rightarrow X - X_o$ for *all* X is not a positive nonconstant function between 0 and $X_o$, which Luce assumes. In fact, a reasonable transformation which shows the ratio scale properties of X and $X - X_o$ is

$$T(kX) = \begin{cases} 0 \text{ if } X < X_o \\ k(X - X_o) \text{ if } X \geq X_o, \end{cases}$$

and hence $T(kX) = k\,T(X)$.

## Influence of Experimental Contexts and the Use of Mathematical Models

Aside from the problems associated with the threshold, the results of a magnitude estimation study depend heavily upon certain aspects of the experiment that affect the observer's use of the number system. This in turn seems rather dramatically to affect the exponent of the power function. Poulton (1968) made some very trenchant comments on these types of problems. Even apart from possible confounding factors due to the penchant for averaging across observers in these studies (Stevens does not pretend to possess a curve that will fit everyone), since the exact form of the function for magnitude estimation seems to depend on so many extraneous factors (extraneous to the transduction and transmission through sensory channels), one may well be concerned about the fruits of this great body of research.

Two comments are apposite here. First, even if we knew no more than whether an exponent for a given sensory system was greater than or less than one, we would know considerably more about the system than we did when we began. It is perhaps something of a pleasant surprise even that a sensory channel can be described behaviorally in terms of a single-signed second derivative. Second, even the knowledge of likely ranges of exponents and thresholds may prove of value for at least some aspects of communication engineering. Where it is not possible or feasible to precisely control for the observer's use of the number system, which may be discussed as a function of his response bias, one may use mathematical models which include structure both for sensory processes as well as for response bias (which depend upon experimental response-constraints, and learning and motivational variables). Indeed, in almost any circumstance, they can prove of value as "filters" to expose more cleanly the sensory characteristics. This function is partially distinct from their use as theoretical descriptions and predictors.

Luce (1959) has developed an elegant theory resting primarily upon a single axiom, and he has applied this theory to the problem of magnitude estimation. He has shown, among other things, what conditions on the response-bias function and the stimulus-generalization function must be satisfied for the arithmetic and geometric means to be proportional to the "true" psychophysical function (within his theory).

Atkinson (1963); Atkinson and Kinchla (1965); Kinchla, Townsend, Yellott, and Atkinson (1966); Luce (1963); and Krantz (1969) have suggested intuitively pleasing theories for simple signal detection situations which I have recently generalized to models of complete identification experiments (i.e., recognition of one of a finite set of signals on each trial) (Townsend, *1968*, 1971a, 1971b). The ideas inherent in the model are capable of motivating a very general theory for the magnitude estimation setting. Basically, the model posits that the result of a single sensory stimulation is the activation of a hypothetical sensory state; a decision is then made by the observer on the basis of this sensory state, and it is here that his response bias may enter. Hence, we may impose a conditional probability distribution relating the presented stimulus S to the hypothetical sensory states, which we will denote by a random variable s. Similarly, we may postulate the existence of another distribution relating an activated sensory state to the set of possible responses, which we call R. Lee (1963) discussed this type of model when the set of sensory states is on a continuum, the latter probably more appropriate than a finite set of discrete states for direct scaling experiments.

Although frequently the conditional geometric mean has been employed to describe (usually to define) the psychophysical function because of skewed data, it is far from certain that this strategem "reveals" the underlying sensory functions. At any rate, we will here confine our remarks to a few simple observations about the arithmetic mean. In this case, it seems reasonable, as a first approximation, to define the psychophysical function as the expected value (mathematical arithmetic means) of the sensory state conditionalized on a particular stimulus presentation, i.e., $Y = E(s|S)$. Since what is usually obtained in a magnitude estimation experiment corresponds to

$$E(R|S) = \int_s \int_R R\, f(s|S) g(R|s) dR ds = \int_s \{\int_R g(R|s) R dR\} f(s|S) ds,$$

it is immediately apparent that a sufficient condition to permit $E(R|S) = k\, E(s|S)$. is that $E(R|s) = \int g(R|s) R dR = ks$, where k is a constant. That is, if the average response given a particular sensory state, s, is proportioned to s, then the average response would be proportional to the average sensory state resulting from stimulus S. One can, of course, impose as much theoretical structure on the distributions as is consonant with one's aims.

My own bias would favor increasing utilization of mathematical models, both as possible substantive explanations, and as devices to reveal those underlying characteristics of the observer in which the investigator is interested. For instance, the effects upon responses noted by Poulton might well provide the basis for some first approximations to the response function for the above model. A purely empirical and/or purely scaling approach seems to leave many areas of ambiguity.

In summary, we have discussed the mind-body equation in reference to several points of methodology and epistemology. We have seen that Fechner, significant though he was in the establishment of an important segment of today's psychology, employed a mathematical technique that is legitimate only in a few special cases, the most noteworthy fortunately being Weber's law. We have also seen that current investigations have clarified the problems involved with this project of Fechner's considerably and brought alternative conceptions and competing theories to the mind-body scaling problem. Finally, we have tried to make the point, via somewhat discursive comments on issues related to the mind-body problem, that considerations and application of such concepts and areas of falsifiability, measurement theory and substantive mathematical models can play an important role in the delineation and explanation of psychophysical processes as well as theory testing in general.

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