

## MATHEMATICAL PSYCHOLOGY AND THE COMPUTER REVOLUTION

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As computers increasingly pervade science the question arises; Do psychologists have reason to study any more mathematics than is needed to program control of experiments, analyses of data, and theoretical ideas into a computer? And because of menu-driven programs to write programs, that soon will be mighty little mathematics. I argue that such a view is dangerously short sighted because, if adopted, it will seriously erode our somewhat fragile theoretical base. The problem is that many important and deep issues of theory construction simply cannot be touched by the kinds of ideas now embodied in or currently foreseen for computer simulations, including; our ability to reason about infinite systems; the fact infinite systems can relate effectively to empirical domains that are clearly finite in character; the frequent failure of finite mathematical approximations to reveal the simplicity and clarity found in infinite models; and the difficulty of understanding computational possibilities and problems without careful theoretical analyses. These points are illustrated in terms of specific models that exhibit some major cross-cutting themes of mathematical psychology; independence, tradeoff, feedback, invariance, and representation.

### INTRODUCTION

As computers increasingly pervade our scientific lives - ranging from the mundane delights of wordprocessing to complex theories whose only current embodiment is as a simulation - one cannot but ask;

Are there really any substantial reasons for students to learn any more mathematics than is needed to program their experiments, data analyses, and theoretical ideas into a computer?

And soon that will be mighty little mathematics because menu-driven programs are available that either themselves do the desired task- be it to control experimental runs, record mountains of data, conduct sophisticated statistical analyses, or prepare graphical summaries - or that help one to prepare a program tailored to do what one wants. As you know, programs already exist that produce dazzling graphical displays, plot any mathematical function, solve many classes of equations, and implement quite complex experimental controls. And there is every reason to believe that moderately sophisticated simulations will shortly be within reach of anyone with a little computer experience and hardly any mathematical knowledge.

For these reasons and because of the easy availability of powerful computers, some hold the view that working scientists need less and less mathematics. Despite the obvious appeal of this opinion to both students and faculty, it should be approached with considerable caution. Before acting on it we need to consider carefully whether such computer programs will actually suffice for psychology - or any other science, for that matter? I shall attempt to convince you not, at least not for the coming few decades. Indeed, I contend that such a view is dangerously short-sighted for the development of rigorous theory in psychology. If the view prevails that computer literacy is sufficient for theorists, then I believe that within a generation we will see serious erosion of psychology's somewhat fragile theoretical base. Such erosion will occur because many important and deep issues of theory construction simply cannot be touched by the kinds of ideas now embodied in or currently foreseen for computer programs.

This strong claim follows from four facts that are well known to mathematically inclined scientists, but tend to be less familiar to or fully appreciated by other scientists. They are;

- (1) Some mathematically talented people have, and current computer programs definitely do not have, an ability to think and reason to good purpose about infinite, often continuous, systems and about entire classes of such systems.
- (2) Some of these - more often than not, infinite --systems exhibit an uncanny knack of relating effectively to at least certain empirical domains that either are inherently finite or, for purposes of data analysis, are treated as such. A familiar example is the continuous probability distributions that are a staple of statistics.
- (3) In contrast to many infinite systems that, themselves, exhibit great simplicity and clarity, finite mathematical approximations to them more often than not fail to retain that simplicity and clarity.

- (4) Finally, without carrying out a careful theoretical analysis, few of us are able to tell whether the model driving a simulation is well behaved or whether it embodies some pernicious combinatorial or asymptotic explosiveness not apparent to casual inspection.

To illustrate aspects of these observations in a psychological context, I discuss three areas with which I am intimately familiar; the representation of preferences among uncertain alternatives; systems that are designed to integrate several sources of information, but observations of which are partially obscured by noise; and the concept of scale type in measurement theory.

Before going into these special cases, I wish to make a few general observations about computation and simulation.

#### COMPUTATION AND MATHEMATICAL PSYCHOLOGY

Until about 10 or 15 years ago, a considerable fraction of the mathematical effort in any science - psychology was no exception - was devoted to making feasible the computations required to use or to test explicitly formulated theories. Such work dominated the early volumes of, for example, *Psychometrika*. Included were techniques of approximation, clever algorithms to reduce the magnitude of the computation to what was then feasible using mechanical calculators, and of showing how a problem could be transformed into one for which numerical tables already existed. Such efforts continue to be of great importance in the field of numerical analysis whenever a computation - e.g., weather prediction, airflow patterns, or economic forecasting - taxes the power of even the largest supercomputer. But for most of us, most of the time, the computational power of a PC or minicomputer makes such research of very limited immediate concern. We can focus our talents on theoretical issues far more than computational ones.

It is not always the case that a computer algorithm employed in a calculation is the computational realization of some explicit theory. Beyond some vaguely formulated intuitions and the specific algorithm itself, no explicit theory may exist. A well known, very useful example of such a theory-free algorithm is multidimensional scaling (MDS). There simply does not exist a mathematical theory of MDS for which the algorithm is a computational realization. One suspects that there should be such a theory, but no really satisfactory one has been proposed. Were it to exist, then certain questions could be addressed mathematically that are now studied only by trial-and-error computations.

#### SIMULATIONS AND MATHEMATICAL PSYCHOLOGY

The role of the computer in simulating psychological processes is a somewhat more complex topic, about which sharp differences of opinion exist. As with computations, two cases need to be distinguished.

One case arises when the simulation is a detailed, specific realization of a well-specified process that has been explicitly formulated, but that has proved recalcitrant to our efforts to deduce explicit properties suitable for confronting data. For example, many Markovian processes proposed to model aspects of learning are of such analytic complexity that we are unable to arrive at explicit statistical properties of the model. In such cases, simulations of the random processes involved gain us empirical estimates of the statistics that are needed to evaluate the model. This was the sort of simulation that first arose with the advent of computers. Of course, to the degree that the model has free parameters that have to be estimated from the data, this can entail a lot of computation for different combinations of parameters, and we may very well run afoul of nasty misestimates arising from the fact that the statistics involved often have complex, non-linear relations to the parameters. Nonetheless, when used with care, such simulations can be very helpful.

For example, one major virtue of such a programming approach to theory is the relative ease with which recursiveness, feedback, and imbeddedness can be handled. Often, these ideas lead to quite intractable mathematics, which is readily dealt with in computer programs.

The second type of simulation differs from the first primarily in that the theory underlying the simulation is not explicit and detailed. Rather a theory-schema--often in the form of a flow diagram--is offered, and any specific simulation is one possible realization of the ideas embodied in the schema. Since the theory is nowhere explicitly stated with the degree of precision and detail typical of a mathematical model, the program is its author's sense of what is meant by a somewhat loosely formulated conceptualization of the processes. Let me cite two examples.

In his book *Image and Mind*, Stephen Kosslyn (1980) reported a simulation of how he thought the mind might generate a mental image of something from stored information about some class of entities--his examples were automobiles and German Shepherds. The simulation embodied several different processes that were suggested to him by some mix of his clever experiments and an awareness of familiar computer-program commands. The implementation, of course, cannot be in any way vague, although his written description of it of course is. The implementation simply was one programmer's version of what he believed

Kosslyn had in mind as was evolved from discussions and informed by repeated computer printouts.

A far more ambitious simulation effort is that of Allen Newell (in preparation) and his colleagues to develop a comprehensive "cognitive" apparatus they are calling SOAR. It is able to deal with virtually any cognitive problem on which we have a substantial amount of solid data--learning, psychophysics, memory, response times, etc. A number of general principles are enunciated in relatively broad terms; flow diagrams are used to illustrate the grand design; and ultimately highly specific computer code is developed in each of the domains that the theory spans to result in the simulation. It is viewed as a general cognitive schema--far more comprehensive in scope than anything before it--and each special part of the program is governed by a common set of ideas about how the system learns.

What do I see as problematical about such an approach? Why isn't it a fully acceptable substitute for the traditionally formulated theories? There are several, somewhat distinct problems.

1. Beyond flow diagrams and rather globally stated principles, the theory exists only in specific programs. The programs are in no way uniquely determined by the principles, and so far as I can see they are communicable from one person to another only in the form of long listings of computer code. Given the speed with which computer hardware and software comes and goes, one wonders if such a theory will be available in tangible form 20 or 30 years from now. Recall that vast data bases held on IBM cards are now largely unavailable because no one makes card sorters and most old ones are beyond practical maintenance. Only if the theory is continually rewritten and modified with the advent of new technology will it exist in communicable form.

2. As the complexity of the theory grows, the data used to test it seem to grow softer, less precise. Much of the adequacy of such theories depends on reproducing qualitative aspects, not detailed numerical aspects, of the data. Certainly qualitative agreement is essential, but experience strongly suggests that this is not enough. I think, for example, of the devilish time cognitive psychologists have had in trying to select among apparently simple distinctions. Recall the efforts begun by Sternberg (1969) and pursued by many (for a partial summary, see chapters 11 and 12 of Luce, 1986) to decide whether the search of short term memory is serial or parallel, and whether it is exhaustive or self-terminating.

3. Despite the apparent scope and generality of such a simulation, the actual realization is really something very, very specific. This is in sharp contrast to one trend in mathematical modeling of defining broad classes of models that are delineated by a series of very specific mathematical properties that can be, more-or-less individually, confronted by data. The goal is to evolve elementary propositions that seem well confirmed and that can be combined to create specific models.

#### MATHEMATICALLY EXPLICIT PRINCIPLES IN DECISION MAKING UNDER UNCERTAINTY

As a specific case in which mathematical modeling of the type just mentioned has been dominant, let me take individual decision making under uncertainty. The area began, basically, with a mathematical model of rational decision making that was proposed by von Neumann and Morgenstern (1947) in the second edition of their classic-*The theory of Games and Economic Behavior*. Subsequently, a number of attempts have been made to increase the scope of that theory - such as extending it to a rational model covering chance events for which probabilities are not available. This is the well know theory of subjective expected utility (SEU) of Savage (1954). Attempts have been made to test its adequacy by empirically probing various properties that underlie SEU. I cite three of these properties, of which the third is a whole collection of specific ones.

- (i) Transitivity of Preference--if X is preferred to Y and Y to Z, then X will be preferred to Z.
- (ii) Monotonicity of Preference--replacing an outcome of a gamble by something that is more preferred and keeping all the rest of the outcomes the same results in a gamble that is preferred to the unmodified one.
- (iii) Accounting Equivalence (or Lack of Certain Framing Effects)--certain pairs of uncertain alternatives that are logically identical in the sense that the decision maker receives the same outcomes under the same conditions are perceived as indifferent in preference.

The work, which has spanned 30 years and dozens of researchers, has explored experimentally the adequacy of these and other principles, and mathematically their consequences, when simultaneously invoked. Without going into the details, I believe that current evidence, after some earlier misunderstandings are discounted, favors transitivity (Bostic, Herrnstein, & Luce, submitted; Tversky, Sattath, & Slovic, 1988) and

monotonicity as being descriptively correct, but framing effects abound. The latter effects are now widely recognized by psychologists largely as a result of some striking examples due to Tversky and Kahneman (1986). One such example is the substantial differences in choices made, not only by student subjects but by professionals as well, depending on whether a situation is cast in terms of lives lost or lives saved. But equally, other less striking framing effects have been widely ignored by theorists, especially economists. This occurs whenever theorists accept, almost without comment, as a model for the domain of choice, a family of random variables. That assumption implicitly postulates that no psychological distinction exists between compound gambles--gambles whose outcomes are themselves gambles--and the corresponding logically equivalent simple gambles. Such a failure of framing and not a failure of monotonicity is, I contend, the basis of the classical Allais paradox that first suggested the von Neumann-Morgenstern theory is not descriptively accurate. One must be very cautious, indeed, about just which accounting equivalences to assume--only the simplest ones stand any chance of being correct.

Various of us (Luce, in press; Luce & Marens, 1985) have been and are exploring mathematically exactly what transitivity, monotonicity, and highly selected assertions of equivalences imply about the behavior to be expected. The predictions are rather more specific than one might anticipate. Gradually we are evolving new classes of models whose properties are well specified and tested in some detail.

In contrast to theory as simulation, a mathematical theory can be formulated in terms of a few quite explicit postulates, and so it can be transmitted easily from person to person, generation to generation. It relies on detailed explicit predictions and requires very carefully collected, very extensive data to probe it because, even in its simplest form, it is relatively close to describing much of the observed behavior. Because the theory is explicit, one can both test the underlying postulates directly as well as seek out sensitive predictions that can be confronted by hard data. Finally, again in contrast to simulations, it is readily possible to formulate entire classes of models that satisfy one or another of the behavioral principles we think might be involved. And using mathematical methods, it is possible to derive properties and/or representations of the entire class of models exhibiting these postulates.

In the long run, of course, one seeks to find and verify a set of axioms - by then they will be thought of as laws - sufficient for the model defined by them to be uniquely specified by data. At present we have a long way to go since the current theory leaves wholly unspecified the form of either the utility function or the weighting functions to say nothing of being static in the sense of not incorporating any time dependencies.

This slow, somewhat painful evolution of such an explicit theory in a highly constrained domain is in marked contrast to the chutzpa of attempting to simulate not just this limited class of behaviors, but just about everything else a person might do cognitively. I have no certainty as to which approach will, in the long run, give us a solid understanding of human behavior; but I am confident that the answer is not obvious.

## INFORMATION INTEGRATION MODELS OF SIMPLE CHOICE

Probably the single largest area of mathematical modeling in psychology is the vast array of both stochastic and deterministic models that attempt to account for subjects' behavior when making some simple response - detecting, recognizing, recalling, discriminating, and the like - to stimuli chosen from a limited set of possibilities. Most of these models are concerned with how the subject combines various sources of information to arrive at a response: information arising from the current stimulus, from previous stimuli and responses, and from various forms of feedback and instructions by the experimenter. As I say, there is a vast array of such models, and this is not the forum to describe them in any detail. Rather, I wish to make some quite general observations about the area and to try to tie it into the computer revolution.

Let me immediately acknowledge that such models often strongly invite simulation. Many involve some form of temporal probability process - so-called stochastic process - and it is not terribly difficult to develop programs to simulate them. Moreover, all of us resort to doing so when the mathematics gets intractable, for it provides us with an "empirical" understanding of processes that we seem not otherwise able to understand. But this is a path of last - not first - resort for theorists who have substantial mathematical training. Of course, I am concerned that it may become the primary avenue for the next generation of psychologists if we are not careful. So it is important to understand why an analytical understanding is valuable. Let me try to illustrate it by a couple of examples.

Every information-integration model I know of can be classed as one of two broad types. The one supposes the existence of an underlying, deterministic system that is describable by some system of equations - algebraic or differential, linear or non-linear - but what we observe is corrupted by some kind of noise. This sort of modeling is familiar from a great deal of statistical, physical, psychometric, and econometric practice. The basic problem is to try to see and estimate the underlying structure through the veil of noise. As the system theorist Rudolph Kalman has pointed out in a series of papers not widely familiar to psychologists (Kalman, 1982; Kalman, 1983; Kalman, Falb, & Arbib, 1969), for any

particular underlying system and postulated noise environment it is a purely mathematical problem, first, to work out how completely the underlying system can be identified from data that are subject to that noise, and second, to determine just what data are needed in order to achieve the optimal identification--he calls it realization--of the system.

He is at pains to point out that many well-known practitioners have engaged in decidedly unjustified practices of imposing criteria, like least squares and principal components methods, aimed at reducing the degrees of freedom beyond those that are mathematically inherent in the postulated system together with the nature of the corrupting noise. Note that what is involved here is not a computational limitation; it is one logically imposed by the noise environment in which the data are collected. One only understands what is possible by a careful mathematical analysis. Let me quote Kalman:

The principal modeling problems for the future are not statistical, but system-theoretical. The role of mathematics is much more than just using precise language. Mathematics is the main (and perhaps the only) creative tool in any deep system-theoretical investigation. Had the mathematical aspects received their due emphasis after the ...researches of the 1930's, econometrics would be much further ahead today as a viable scientific discipline. (Kalman, 1982, p.191).

I would be amiss not to point out our general failure, as theorists, to combine the two approaches I have so far mentioned. The axiomatic modeling of structure, illustrated by the study of preferences, and the systems modeling of noisy data remain isolated. The axiomatic approach takes the stance that our observations are inherently qualitative in character, and it asks how they can be represented mathematically, often as a system of numerical variables. We understand moderately well some aspects of the mathematics of such qualitative structures, but only in the context of noise-free data. The systems approach presupposes a representational form--often systems of linear equations--and asks how to identify the parameters of the system when the data are corrupted in some partially specified way. Both approaches are too idealized. Probably there aren't any interesting noise free psychological data. But equally, we usually do not have very good reasons for accepting particular systems of equations as being of the correct form.

Once stated, it is clear that the real problem is to begin with qualitative observations that are known to be corrupted and then to solve both types of problems as well as is logically possible": to infer simultaneously the

nature of the representation and to understand the uncertainty lying in the inference.

To do this is apparently very difficult. Indeed, I cannot cite a single example where it has been solved to anyone's full satisfaction. The nearest thing is some of the work of Jean-Claude Falmagne (1976, 1979) on noisy additive conjoint systems.

The second approach to the issue of variable data is to deny that they are noisy, attributing their haphazardness to an inherent, underlying stochastic process. This path has been widely followed in psychology - e.g. stochastic models of learning and memory (Estes, 1988; Norman, 1972) and of response times (Luce, 1986) are of this sort.

A good example of the inherent probability approach can be found in a recent paper of Riefer and Batchelder (1988) titled "Multinomial modeling and the measurement of cognitive processes". They argue as follows. The basic postulate underlying computational approaches to cognition is that a finite set of discrete processing states is involved in what amounts to a Turing machine. Riefer and Batchelder then show that, in a sense, multinomial models are a natural statistical generalization: there is some probability that the system is in a given state and conditional on that state there is a probability distribution over behavior patterns. By elementary probability, one arrives at an expression for the probability of the behavior. Assuming that the behavior on successive observations is independent - not always a viable assumption (see Section 6.6 of Luce, 1986) - they work out the appropriate statistical analysis and illustrate how this can be used to reach conclusions of interest to cognitive psychologists.

For example, consider interference in the study of memory. If the paired associates (A-B) and (A-C) are learned, there is interference but, surprisingly, the recall of B and C are statistically independent. Martin (1971, 1981) claimed this to be direct evidence against associative interference theories. Greeno, James, DaPolito, and Polson (1978), recognizing that such a conclusion is not immediately obvious, developed an explicit process model and performed an experiment to test it, but did not really provide a suitable statistical model and analysis. Riefer and Batchelder do, and they demonstrate that Martin's conclusions were justified, namely, associative theories of proactive interference are suspect.

Work of this character seems to be the primary way we have for gaining an understanding of basic cognitive processes. I emphasize that it is not sufficient to know how to use or even program a computer in order to carry out such research. To the extent we, as teachers, abandon requirements for mathematical training of our students, then to that extent

we also deprive the field of its ability to formulate and answer such questions.

#### SCALE TYPES: AN EXAMPLE OF ABSTRACT REASONING ABOUT STRUCTURES

Most experimental psychologists are acquainted with S.S. Stevens (1946, 1951) classification of measurement into several scale types: ordinal, interval, ratio, and absolute. Actually, as can be seen in Table 1, which gives the defining properties of the several types, the middle pair - interval and ratio - should either be the pair log-interval and ratio when the measurement scales are onto the positive real numbers or the pair interval and difference when they are onto the real numbers.

Two closely related questions must have occurred to many on hearing of his classification : Why these? And what others can occur?

The answer to "Why these?" was that, at the time some 40 years ago, all examples known from physics fell into one or another of these four general types. It was, if you will, a naturalistic typology.

The process of answering "What others can occur?" has proved far more subtle, and only in the 1980s has any real clarification come about. During the 1950s and 60s it became apparent that there were structures - e.g. semiorders (for a general survey see Fishburn, 1985) and the unfolding scaling model of the late Clyde Coombs's (1964) - whose scale types were not included among Stevens's four and which proved, moreover, not to be easily characterized. And that remains true today.

What has happened, however, is that we now understand fully the scale types of one very important general class of highly regular measurement structures. Let me try to describe some of the results.

The first thing to realize is that the concept of scale type is something intrinsic to the qualitative structure itself and does not depend upon studying directly its numerical representations. A permissible change of the representation induces a map of the structure onto itself. Under suitable circumstances, that map is in fact structure preserving - an isomorphism of the structure onto itself. It can be thought of as describing a symmetry of the structure, which is the term physicists use; mathematicians call it an automorphism (meaning, self-isomorphism).

Observe that ordinal, interval, and ratio scales all have the property - called homogeneity - that for each pair of points  $x$  and  $y$  in the structure, some automorphism maps  $x$  into  $y$ . Put another way, the structures are

very rich in automorphisms and each point is structurally like each other point. Thus, we are excluding, for the moment, all non-homogenous structures such as those that have singular points that exhibit peculiar properties not possessed by other elements in the structure. Examples are zero points and infinite ones. (For some results about non-homogenous structures, see Alper, 1987, and Luce, submitted).

Second, we note a major distinction between the ordinal and interval cases. In the interval scale case, if two apparently distinct automorphisms agree at two distinct points, then in fact the automorphisms are identical. By contrast, in the ordinal case agreement at any finite number of points does not force the automorphisms to be the same. We say that a structure is finitely unique when it is the case that, for some fixed integer  $N$ , whenever two automorphisms agree at  $N$  distinct points, then they are identical, i.e. agree at every point.

Narens (1981a, b) initiated the study of these two concepts in the context of measurement theory. Alper (1985, 1987) completed their investigation in the context of measurement structures that have representations onto the real numbers. The ultimate result is this: for those structures that (i) have numerical representations onto the real numbers and (ii) are both homogenous and finitely unique, there are just three general types of scales: difference (ratio), interval (log-interval), and some cases intermediate between these two. The following is an example of such an intermediate case: the group of transformations that map  $x$  into  $kx + s$ , where  $k$  is a fixed positive number,  $n$  ranges over the integers, and  $s$  is a real number. Although mathematical examples have been given of measurement structures of this type (Luce & Narens, 1985), I know of no scientific applications of them.

Of course, one next asks: What do we find to be possible when we look at non-homogenous structures and when we look at structures that are not finitely unique? Little is known about the latter, and some work has been done on the former, but this is not the place to describe it in detail.

One may also ask: What good does it do to know the possible scale types, what does it matter to the working scientist? Part of the answer is simply that a veil of ignorance has been partially lifted. But another part is that with so few scale types logically possible, it is feasible to consider complete classifications of structures of various types. For example, Luce and Narens (1985) explored those measurement structures that involve an ordering and a binary operation (of putting objects together) that is monotonic in the order. We were able to give a simple description of all such structures that are homogenous and finitely unique. In particular, it was our understanding of the interval scale case that led to the rank dependent theory of utility mentioned earlier and that has been

generalized to gambles with any finite number of outcomes (Luce, in press).

Let me emphasize that thinking about symmetries of infinite structures is not something that is easy to envisage reducing to computer code. Yet it is just what is needed in order to answer the question "What scale types can arise?" and to classify all structures of those scale types that involve, for example, a binary operation. Moreover, the sort of mathematics involved in these investigations is not, at present, seriously aided by knowledge of computer programming, and a psychologist whose entire training is focussed on programming is not likely to be exposed to such mathematics.

## CONCLUSIONS

The day may well come when computers are able to do abstract mathematics, to introduce novel definitions and prove new results by new methods. The French mathematician Ruelle has written"... [L]et me make the bold suggestion that perhaps in a few decades we shall see what nonhuman mathematics looks like. I am not predicting the imminent arrival of little green men from outer space, but simply the invasion of mathematics by computers. Since the human brain is a sort of natural computer, I see no reason why the artificial variety could not perform better in the specialized task of doing mathematical research. My guess is that, within fifty or a hundred years (or it might be one hundred and fifty) computers will successfully compete with the human brain in doing mathematics, and that their mathematical style will be rather different from ours. Fairly long computational verifications (numerical or combinatorial) will not bother them at all, and this should lead not just to different sorts of proofs, but more importantly to different sorts of theorems being proved. (Ruelle, 1988, p. 260).

However, with that time scale - 50 to 150 years - we should assume that such developments are largely irrelevant for the training of psychology graduate students in the coming decade or two. The question we must face is whether those who aspire to being theoretical psychologists need to understand mathematics beyond that necessary to master computer use. Depending on our answer, our curriculum will differ.

I sense some drift toward easing off on mathematical requirements, attempting to substitute computer simulations for theory as we have known it. That, of course, is the reason I have elected to address you on the matter. I do not think any such tradeoff exists and acting as if it does will have deleterious effects on the field. Do not misunderstand: I am not arguing for less training in the use of computers. On the contrary, that

must be a major factor in training. I do argue, however, that computational and simulation facility should not be achieved at the expense of reducing mathematical experience, at least not for theorists.

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**TABLE 1**  
Stevens' Classification of Scale Types (Augmented)

Numerical Domain			
Re	Re <sup>+</sup>		Parameters
Ordinal (homeomorphism group) $x \rightarrow f(x)$			Countable
$f: \text{Re} \rightarrow (\text{onto})\text{Re}$	$f: \text{Re}^+ \rightarrow (\text{onto})\text{Re}^+$	$f$ Strictly increasing	
Interval (positive affine group) $x \rightarrow rx + s, r > 0$	Log-interval (power group) $x \rightarrow tx^r, r > 0, t > 0$	Translation if $r = 1$ , Dilation if $r \neq 1$ or if the identity	2
Difference (translation group) $x \rightarrow x + s$	Ratio (similarity group) $x \rightarrow tx, t > 0$		1
Ratio (similarity group) $x \rightarrow rx, r > 0$			1
Absolute (identity group) $x \rightarrow x$			0