equation that expressed mathematically certain very general features of the data such as continuity, indestructibility, incompressibility, homogeneity, and so forth. Where before we appealed to the fact that at three o'clock the hour hand had a fifteen minute start on the minute hand, now we turn our attention to boundary conditions that restrict the range of functions satisfying the differential equation.

2.5 Restricted Invariance. ~~6.36~~ Place and time, no less than individuality and continuity, pertain to the empirical residue. It follows that the function to be determined will hold independently of particular places and times for, as has been seen, particular places and times are, in their basic aspect, continua of individual differences.

Thus, Newton's first law of motion is to the effect that a body continues in its state of rest or of uniform motion as long as no external force intervenes. This law might be regarded as a positive correlation between zero acceleration and zero force. But directly it regards constant velocities and its contention is that such velocities pertain to the empirical residue. If there is an acceleration, mechanical analysis has to assign a corresponding force. If there is no acceleration, then mechanical analysis does not have to bother about assigning any force. Like rest, constant velocity lies outside the range of problems envisaged by Newtonian mechanics. It is a residual feature that needs no positive explanation.

Indeed, there could be no ~~possi~~ positive explanation of a constant velocity. For it is mere change of place and mere change of time. One can account for change in velocity, and one does so by the law of force. One might account for the conservation of acquired velocity, but that would be, perhaps, a philosophic question rather than a mechanical one. But one cannot assign ~~any~~ positive explanation for every element in change of place for, since places are continuous, since a continuum ~~xx~~ is a non-countable infinite set of differences, there would be needed a non-countable infinite set of positive explanations for every instance of constant velocity. But a non-countable infinite set of positive explanations is impossible. Therefore, a single explanation has to serve for the whole duration of a constant velocity, and that is provided ~~by~~ when one explains the acceleration that terminates in the constant velocity.

However, ~~the point we are making is more general than Newton's first~~ law. Its premise is the impossibility of a non-countable infinite set of explanations to match a non-countable infinite set of differences in a continuum. What follows from that premise, receives its general formulation in the postulate of the Special Theory of Relativity, namely, ~~that the principles and laws of physic~~
that the mathematical expression of the principles and laws of physics is ~~are~~ invariant under transformations from one set of coordinate ~~axes to another set moving with a relative,~~ constant ~~velocity~~.

However, as is clear from its premise, the point we are making is more general than Newton's first law of motion. The argument rests on the impossibility of a non-countable infinite set of positive explanations. If it may under-pin Newtonian mechanics, it may also under-pin Maxwell's ~~electromagnetic~~ theory of the electro-magnetic field. Hence, if we may use the technical formulation of the postulate of the Special Theory of ~~Relat~~ Relativity, we may conclude that the mathematical ex ression of the principles and laws of physics is invariant in form under transformations from one set of coordinate axes to another set moving with a relative constant velocity. (See Lindsay and Margenau, 101 f, 326 ff.)

2.6 Equivalence ~~3.30~~

An even more general heuristic anticipation can be set forth.

The empirical inquirer measures and correlates the results of measurements to reach the functions that relate things directly to one another. There follows a principle of equivalence for all observers.

For, since the function sought relates things directly to one another, the relations of things to observers are omitted. Because the relations of things to observers are omitted, the functions cannot be modified by variations in the relations between the observers and the things. Because there cannot be any such modification, the functions must be the same for all observers.

It is to be noted that the principle of equivalence goes far beyond mere independence of particular places and particular times. Colors ~~are~~ as observed vary with the position, velocity, acceleration, of the observer; they vary with the intensity of the light by which he views them; they vary with the condition of his eyes, such as his need of spectacles and his possible color-blindness. But colors as explained by a series of wave-lengths of radiation are necessarily the same for all observers; all conceive them in the same fashion; ~~all~~ no one is handicapped by color-blindness.

Now this principle of equivalence represents a property of the direct relations of things to one another. Such a property can be employed as a premise to determine what the relations are. How can such a premise be formulated? A partial formulation is to take the origin and orientation of coordinate axes as representing the observer, and to ~~make require~~ say that functions, representing principles or laws, satisfy the principle of equivalence if they remain invariant in form under ~~the group of continuous transformations. The invariance represents the independence of the functions~~ the group of continuous transformations. For if the observer moves about, he does so in some continuous fashion. But the functions representing laws are independent of any such motion of the observer. And this independence is guaranteed to them by their invariance under continuous transformations.

Such is the postulate of the General Theory of Relativity, which has had some confirmation, and of the Generalized Theory of Gravitation, which as yet has not been put in a form that admits an empirical test.

Certain observations are in order.

First, scalars, vectors, and generally tensors are quantities that may be defined by their transformation properties. Thus, a set of n quantities formsa contravariant vector if they transform according to the same rule as the differentials of the coordinates. A set of n quantities forms a covariant vector if they transform in an opposite manner to the differentials of the coordinates. Contravariant and covariant tensors are sets of n^2 and higher orders of quantities that transform in a more complicated but analogous fashion. Hence, by expressing physical principles and laws in covariant form, automatically there is attained invariance under the group of continuous transformations. Footnote. [On the tensor calculus, the reader may consult for a brief outline the second chapter of G. C. McVittie's Cosmological Theory, London 1937, Methuen's Monographs on Physical Subjects.]

Secondly, invariance will be obtained only in so far as there are expressed the relations of things to one another. As soon as equations are made more specific by appealing to observational data of any kind, there is introduced a determination from relations to observers; and then invariance is no longer to be expected. Perhaps this accounts for the fact that in the General Theory of Relativity the equations remain invariant only as long as the coefficients, g_{ij}, remain in place. See Lindsay and Margenau, p. 368.

Thirdly, the same consideration seems relevant when one attempts to understand the apparent/incompatibility of General Relativity and Quantum Mechanics. As will appear presently, Quantum Mechanics is concerned with observables. It seeks formulations of things in their relations to us while General Relativity rests on the relations of things to one another and only in its applications turns to relations to us.

Fourthly, the heuristic significance of the principle of equivalence, interpreted as a principle of covariance, is not that it restricts the field of possible laws but rather that it gives a determinate meaning to the empirical investigator's preference for the simplest laws. As A. Einstein has advanced in his autobiography (Albert Einstein, Philosopher-Scientist, ed. P.A. Schlipp, Library of Living Philosophers, 1949 and 1951, p. 69 New York, Tudor Publishing Company), any law could, perhaps, be expressed in covariant form but within the restriction of such a form one can begin by working out the simplest laws and, if they fail, advance to the more complex.

Fifthly, of interest in this connection is Einstein's conviction that data alone are insufficient to guide the constructive efforts of intelligence. There also is needed a formal principle that functions as does the negation of the possibility of a perpetuum mobile in thermodynamics. Such a formal principle Einstein believed he had found in his postulate of invariance, first, in Special Relativity and then in General Relativity. See ibid., pp. 53, 57, 69.

2.7 ~~[illegible]~~ Summary

Before we turn to the consideration of statistical laws, a summary would seem to be in order.

After noting the similarities between mathematical and empirical insights ([redacted]1) and the differences between them ([redacted]), we raised the question of the origin and nature of the clues, hints, suggestions that lead up to insight.

As a clue for insight into clues we took the solution of a simple algebraic problem ([redacted]1) and proceeded to generalize.

What is to be known, when the insight occurs, is anticipated by the mere fact of inquiry and is named the "nature of...," the "such as to...," the "sort of thing that...."

But similars are similarly understood. Hence, the "nature of..." may be specified by means of a classification based on sensible similarity; and when insight occurs, this preliminary classification will yield place to a systematic account that speaks of things, not in terms of their relations to our senses, but in terms of their relations to one another. Thus, the "nature of..." is replaced by the more precise anticipation of an unspecified correlation to be specified, of an indeterminate function to be determined ([redacted] (2.3).

Now functions can be determined, not only by the empirical process of reaching formulae that all known measurements satisfy, but also by appealing to quite general considerations and arguing from them to differential equations which restrict the group of possibly relevant functions. Quite obviously, both procedures can be combined and commonly are combined to obtain a scissors-like action that approaches a solution both from above and below [redacted] (2.4).

Further, when differences form a non-countable infinite set, as is the case with place and time, there cannot be a distinct explanation for each element of difference. Hence constant velocity has to be regarded as residual ~~as~~ and, in fact, it is so regarded in Newton's first law of motion. More generally, the mathematical expression of principles and laws has to be invariant under transformations between inertial systems in accordance with the postulate of Special Relativity ([redacted] (2.5).

Indeed, ~~since~~ inasmuch as principles and laws express the relations of things to one another and omit all reference to the relations of things to observers, it follows that the mathematical expression of principles and laws must be invariant in symbolic form under continuous transformations ([redacted] (2.6).

Finally, one may add that these considerations supply only an abstract scheme. In concrete inquiry they are employed not singly but together. As a science develops, all that already is known serves to render more determinate and precise the general heuristic anticipations that have been outlined.

3.0 Statistical Heuristic Structures.

The fact of inquiry is an anticipation of something to be known by understanding. Hitherto only one type of such anticipation has been considered, namely, the anticipation of a correlation, a function, a law, a system. The investigator measures, plots his results upon a graph, and expects to find a smooth curve or formula that will be satisfied, not only by the measurements he has made, but also by all the relevant measurements that he or anyone else ever will make.

Now it is well to encourage investigators in that expectation, to tell them that, if they do not discover any law, then perhaps they are measuring the wrong things, that they are not excluding some extraneous influence, that if only they are dogged enough then some day someone will discover the relevant correlation, function, law.

Still, encouragement must not be carried to the point of deception. As we have seen, there is an empirical residue, and the insight relevant to it consists in grasping, not the system to which it conforms, but its ultimately non-systematic character, its escape from the dominance of system. Hence, with respect to an aggregate of data or measurements, the anticipation, constituted by the fact of inquiry, is not a single assertion but rather a disjunction. The anticipation is, not that there is some correlation to be grasped, but that either there is such a correlation or else there is not. The positive ~~one~~ member of the~~ir~~ disjunction has been considered in ~~our~~ the foregoing account of anticipations of the systematic. We now ~~turn to the other member, to anticipations of the non-systematic.~~ must endeavour to clarify the meaning of anticipations of the non-systematic

3.1 The Non-Systematic.

What is the non-systematic?

One might say that mechanics, prior to Newton, was non-systematic. Then there did not exist the set of basic principles which Newton formulated and by which he ~~xorking~~ worked into unity both Galileo's law of falling bodies and Kepler's laws of planetary motion. But there is a more radical meaning to the term, non-systematic, and then it involves the negation of any ~~any~~ regular sequence, formula, correlation, function. Thus, when a body falls in a vacuum, it is possible to predict what distance has been traversed at each instant throughout the fall. But when an unbiassed, cubical die is cast with appropriate recklessness, then one of six faces will be uppermost and, on any throw, any of the six may be uppermost.

The notion may further be clarified by contrasting the non-systematic with the indeterminate. If a pair of dice were repeatedly shaken in a glass box and cast, then under suitable conditions a high-speed movie camera could record the data necessary for the complete determination of every ~~phase~~ stage of the movements of the dice. Not only would the trajectory and momentum of each movement be determinate, but also the relations between successive trajectories and momenta. However, if one attempted to find some rule or law governing the relations between successive trajectories or successive momenta, one's efforts would be doomed to failure. Finally, because one could not determine any such rule or law, it would not be possible to deduce from the initial states of a throw what the final results would be.

The non-systematic then is quite compatible with the determinacy of data. It is quite compatible with the determinacy of relations between the successive determinate

3.1 The Non-Systematic.

To reach a classical correlation, function, rule, law, theory, system, there is needed an initial insight into some particular case. By that insight one may master an indefinite multitude of exactly similar cases. Still such universality is not enough. The significance of the initial insight is that it can lead to further insights that master ever more dissimilar particular cases until eventually one reaches a general case and brings under one's control a definable range of particular cases. So Galileo's understanding of the free fall regarded, not bodies of some determinate size, shape, and weight falling at some fixed inclination from the vertical, but bodies of any size, any shape, any weight, falling at any inclination from the vertical.

Now a heuristic anticipation of the non-systematic implies, not a denial of concrete insight into particular cases, but a denial of the possibility of the abstract generalization that subsumes a range of particular cases under a general case. In other words, the non-systematic is not to be identified with the non-intelligible. While the non-systematic excludes the generality of classical correlations, functions, rules, laws, theories, systems, it need not exclude the intelligibility to be reached by inspection and insight into particular cases.

For example, in a particular case dice may be cast from a determinate receptacle in a determinate manner upon a determinate surface; sufficient information on the case could be attained with the help of a slow-motion film; insight could analyze the total movement into a sequence of mechanically homogeneous stages; each stage could be subsumed separately under known laws of motion, gravity, air resistance, impact, friction, and elasticity; and the total movement would be no more than the sequence of the stages. Still, dice can be cast from any sort of receptacle, in any manner whatever, upon any type of regular or irregular, fixed or moving surface. There would be no point in attempting to repeat the above laborious procedure for the infinity of particular cases; and if casting dice is non-systematic, there exists no general case of the classical type to provide an alternative to a pointless repetition of ~~no~~ merely particular ~~repetitions~~ investigations.

3.2 Actual Frequency.

Where classical generality fails, statistical generality may be sought.

Let us say, then, that there exists an actual frequency if, from some determinate antecedent, O, there always follows one and only one of the alternatives, P, Q, R, ... For in any $\underline{n}$ occurrences of the antecedent, O, the alternative, P, will occur on a determinable $\underline{p}$ occasions, Q on q occasions, R on r occasions, etc. Accordingly, the actual frequency of P in a given n occurrences of O will be p/n, the actual frequency of Q will be q/n, the actual frequency of R will be r/n, etc., so that necessarily $n = p+q+r+\ldots$

stages of a process. ~~But it is not compatible~~ with the subsumption of these determinate relations under a rule or law. And so it is not compatible with the deduction of determinate results from determinate antecedents.

3.2 The general characterization of the non-systematic may be put in a more manageable form by defining an actual frequency.

There is an actual frequency if, from some determinate antecedent, O, there always follows one and only one of the alternatives, P or Q or R or... For in any n occurrences of the antecedent, O, then P will occur a certain number of times, say p, Q will occur a certain number of times, say q, R will occur a certain number of times, say r, etc. Hence, the actual frequency of P in a given n occurrences of O will be p/n, the actual frequency of Q will be q/n, the actual frequency of R will be r/n, etc., so that necessarily

$$n = p + q + r + \ldots.$$

Finally, these actual frequencies will be non-systematic if it is not possible to define an Op, Oq, Or, P', Q', R', such that P' always follows Op, Q' always follows Oq, R' always follows Or, etc., so that the indeterminateness of the alternatives is eliminated.

It is to be noted that when a set of alternative consequents has been defined, then it is possible by combinations to construct further sets of alternatives. Thus, one can consider the actual frequency of the combination "either P or Q," or of the combination "P on a first occasion and Q on the second occasion," etc., etc.

One may add at once that the actual frequency of a number of alternatives taken together is the sum of their actual frequencies taken separately. Thus, the actual frequency of "either P or Q" will necessarily be $(p + q)/n$. Similarly, the actual frequency of the total set of alternatives will necessarily be n/n or unity.

3.3 A Generic Notion of Probability.

Let us now generically define a probability as the proper fraction from which ~~an~~ actual frequency does not diverge ~~systematic~~ systematically.

The definition posits an ideal proper fraction, which it names a probability. It admits that this ideal proper fraction will not be coincident with actual frequencies. It denies that the divergence between the ideal and the actual will be systematic.

Suppose, for instance, that the probability of casting a "six" with a single die is 1/6. Then, on the first six throws, a "six" may occur twice, on a second once, on a third not at all, etc. The actual frequency hops about in random fashion while the probability always remains the same 1/6. There is then a divergence between the actual and the ideal. But this divergence is non-systematic, so that the differences between the actual and the ideal cannot be reduced to any rule or law.

Certain clarifications are in order.

First, the reason for the definition is, perhaps, obvious enough. Actual frequencies are non-systematic; they vary from case to case; and their variation is not subject to any rule or law. But a probability is an ideal fraction; it is the same for every case of a given kind; it is the representative of the universal, abstract, necessitating, systematizing tendencies of understanding. Hence, if probability and actual frequency coincided, then either both would be systematic or both would be non-systematic. If they diverged and the divergence were systematic, then the actual frequency would have to be the systematic resultant of the systematic probability and the systematic divergence from probability. One meets the requirements of the problem only if 1) the actual frequency is non-systematic, 2) the probability is somehow systematic, ~~and then~~ 3) the actual ~~frequency diverges non-systematically from the probability.~~

~~Secondly, certain systematic features have been found in actual frequencies.~~

frequency may diverge non-systematically from the probability, and 4) the actual frequency cannot diverge systematically from the probability.

Secondly, it follows that the probability of a set of alternatives is the sum of the probabilities of the alternatives taken singly. For, as we have seen, the actual the frequency of such a set is the sum of the actual frequencies of the members of the set (3.2) and, moreover, there cannot be a systematic divergence between actual frequency and probability. But there would be such a systematic divergence if the probability of the set were not the sum of the probabilities of the members of the set. Accordingly, one must deny the consequent and its antecedent to affirm that the probability of a set of alternatives is the sum of the probabilities of the alternatives taken singly.

Thirdly, a probability is not the mathematical limit of a series of actual frequencies. For a series of terms tends to a mathematical limit inasmuch as divergence from that limit can be made as small as one pleases. But actual frequencies do not converge upon probability. They hop about at random. They approach the probability only to recede. Instead of converging, they diverge. But they cannot make their divergence effective, for they cannot get any system into it.

Fourthly, though a probability is not a mathematical limit, there are unobjectionable assumptions that may be introduced so that the non-systematic divergence of probability becomes virtually equivalent to the convergence characteristic of the mathematical limit. See Lindsay and Margenau, pp. 165 ff.

Fifthly ~~Fourthly~~, our procedure will be to distinguish two radically different meanings of the term, probability. As defined, probability is an ideal proper fraction from which actual frequencies can diverge but not systematically. ~~But~~ However, one also speaks of the probability of opinions and then one does not mean that there is some fraction relevant to the opinion. What is probability in this second sense and what is its relation to probability in the first sense, are questions that must for the moment be postponed.

3.4 ~~Case~~ Specific Differences.

It is one thing to calculate the probability of throwing a "four" with a single, unbiassed die, another to make the same caluclation when a pair of dice are used, and a third to do so when the dice are loaded. In all three cases there is the same generic element: actual frequency diverges non-systematically from the proper fraction named probability. But this genus divides into three distinct species, and the basis of the division resides in the manner in which probability is determined.

The first species is equiprobability. Its conditions are that 1) when an antecedent, O, occurs, then there occurs one and only one of a set of _n_ alternatives and 2) there is no systematic favoring of any of the _n_ alternatives. From the conditions it follows that the probability of the occurrence of any given alternative will be 1/_n_. For were the probability some ~~ks~~ other fraction, say _a/n_, where _a_ is less or greater than unity, then that alternative could not diverge systematically from _a/n_ and so must suffer systematic discrimination, if _a_ is less than unity, or ~~sxs~~ receive systematic favoring, if _a_ is greater than unity.

The second species is a derivative of the first. Its conditions are that 1) when an antecedent, O, occurs, then there occurs one and only one of a set of _n_ alternatives, 2) there is a systematic favoring of some alternatives, but 3) this systematic favoring can be reduced to a case in which there is no systematic favoring.

Thus, when a pair of dice are cast, there are eleven possible results, of which some regularly occur more frequently than others. However, this favoring can be eliminated by considering the thirty-six alternatives constituted by combining ~~any~~ each of the six faces of one die with each of the six of the other. No one of ~~the~~ thirty-six alternatives is favored in any systematic manner, and so the second species is reduced to the first.

The second species of probability is investigated at length by applying the mathematical theory of combinations. The basic formula assigns the probability, _P_, of _r_ successes in _n_ tries, when _p_ is the probability of one success in one try. This formula is worked out in any suitable text and along with it the reader will find the approximations developed by Laplace, Poisson, and Gauss.

The third species does not admit reduction to the first or to the second. There is an antecedent followed by one and only one of a non-systematic set of alternatives. But one cannot settle by inspection what the alternatives are; and their respective probabilities neither are equal nor are reducible to ~~a~~ the case of equiprobability. Thus, when dice are loaded, some combinations might never occur; moreover, the occurrence of any given face of a loaded die is not equal in probability with the occurrence of any other face, for there is some systematic favoring.

The third species may be described as involving a systematic element which, however, does not succeed in completely dominating the results. There is a systematic element, otherwise the alternatives would be equiprobable. But the systematic element does not succeed in dominating the results, for they are found to be non-systematic.

To meet the problem set by the third species, the relevant technique would seem to be 1) to loosen the heuristic anticipations for dealing with data that can be reduced to system and 2) to compensate~~ing~~ for this loosening by introducing probabilities in place of precise predictions.

What would such loosening be? First, anticipations of the systematic are 1) that the data will satisfy some one law or function, 2) that this function will be a solution of the differential equations that represent general features of the problem. Secondly, these anticipations can be loosened. Instead of expecting one function to cover all the data, one may expect a series of eigenfunctions, say ψ_λ, and a corresponding series of eigenvalues, say p_λ. Again, instead of expecting the single function to be a solution of a differential equation, one may expect the eigenfunctions and eigenvalues to be the solutions of an operator equation, say,

$$P\psi_\lambda = p_\lambda \psi_\lambda$$

where P is the operator, that is, a mathematical entity that changes one function into another.

What is the compensating? The foregoing yields a set of observables, the eigenvalues, p_λ. Those that occur will possess some probability, else they would not occur; and they will not possess more than probability, else a systematic ~~solution would work. There will be, then, some state-function, say , standing in some relation to the eigenfunctions, and from it so that it can be determined~~ ~~solution would work. There exists, then, some function from which the probabilities can be calculated, and it will be determinable from the eigenfunctions which, through the operator equation, select the observables that possess some probability.~~ solution would work. There exists then some state function from which the probabilities can be calculated; and one may expect the eigenfunctions to lead to the determination of the state function, for if they succeed in selecting the observables with some probability, they should be able to contribute to the determination of the respective probabilities.

Is this guess-work? Certainly, it is not a rigid deduction. On the other hand, it is not purely arbitrary. It is the fruit of an insight based upon clues where, as is always the case, the insight takes one beyond the clues. There must be some loosening of systematic anticipations, for the data dealt with are only partially under the influence of what one might name a systematic component. There must be some compensation for this loosening, else there would be no conclusions at all. But the exact course of the loosening and the compensating is guided by insights into mathematical possibilities and, however strangely, the resulting postulates of Quantum Mechanics have proved highly successful.

3.5 Summary ~~6.15~~

Let us now attempt a summary.

As classical heuristic procedure is based on an anticipation of the systematic, so statistical heuristic procedure is based on an anticipation of the non-systematic. The data of experience are either 1) totally systematic, or 2) totally non-systematic, or 3) partly systematic and partly ~~non~~ non-systematic. If one assumed that the sole procedure was classical, one would assume that the data must be totally systematic. If one assumed that the sole procedure was statistical, one would assume that the data must be totally non-systematic. But if one is ready to use either procedure, then ~~the data~~ one makes no assumptions about the data. The content of experience may be totally systematic, totally non-systematic, or partly systematic and partly non-systematic. No matter which alternative is, in fact, correct, either classical or statistical procedure will work.

The non-systematic that is envisaged by statistical procedure is the actual frequency. It is some proper fraction, say p/n, where n is the number of occurrences of some antecedent, O, and p is the number of occurrences of some consequent, P.

replaced

The unknown to be reached by statistical procedure is named a probability. It is an ideal proper fraction from which actual frequencies may diverge but cannot diverge systematically. It is determined in one of three manners. For either none of the possible alternatives or else some of them are favored in a systematic fashion. If there is no systematic favoring, there is the first species of equiprobability. If there is some systematic favoring, then either it can be reduced to a case of equiprobability or else it cannot. If it can, Newton's formula or one of its approximations will be relevant. If it cannot, then an axiomatic structure, such as is employed in Quantum Mechanics, can be developed.

Such is the general heuristic scheme.

Its application is similar to the application of the classical scheme. A field of inquiry is selected. Observations and experiments are performed. Measurements are made and tabulated. In so far as the results of measurement are systematic, classical procedure is relevant. In so far as the results are non-systematic, then one seeks a probability function which they satisfy.

replaced by 38a, 38b, 38c, 38d.

3.5 Summary.

Classical method is not content with mastery of particular cases but goes beyond them to the abstract generality expressed in correlations, functions, laws, theories, systems. However there is an empirical residue; particular cases can consist in coincidental manifolds of distinct instances of ~~special cases~~ general cases; and corresponding to each coincidental manifold there is no general case of the classical type. Still this negation of systematic generality is not the negation of all generality. For if one supposes data to be involved in the non-systematic, one cannot suppose that they diverge systematically from ideal norms.

Among such ideal norms the most familiar is the probability of the occurrence of one of alternative possibilities; and the mode of its determination also supplies its subdivision. If there is no systematic favoring of any of the alternatives, there is equiprobability. If there is systematic favoring that can be reduced to equiprobability, Newton's formula becomes the relevant anticipation. Finally, when there is systematic favoring that cannot be reduced to equiprobability, some special axiomatic structure has to be invoked.

There is then, a statistical heuristic structure and it complements classical structure. In any selected field of inquiry, experiments are performed, measurements are made and the results are tabulated. In so far as the general intelligibility of the measurements is systematic, classical procedure is relevant. In so far as the general intelligibility of the measurements is not systematic, a probability function is to be sought. Finally, since antecedently the general intelligibility of measurements may be either systematic or non-systematic, a general theory of measurements must envisage both alternatives. May I ask whether this requirement, rather than particular hypotheses on the accuracy or the distorting effect of measuring, can be regarded as the ultimate basis of the insight into operators that is offered by G. Temple in The General Principles of Quantum Theory [Methuen's Monographs on Physical Subjects, London 1951]?

May further suggestions be made? As long as physicists were engaged in introducing ever more complex modifications of Bohr's image of the atom, they were endeavouring to mount through particular cases to the general case. When they decided to limit their equations to observables (i.e. variables admitting experimental control), they surrendered not generality but systematic generality. Again, in so far as Quantum Theory may be said not to offer insight into particular cases, it suffers on that level a perhaps irremediable incompleteness; on the other hand, interpreted as a statistical theory, it possesses fully the completeness of the non-systematic general case.

If such suggestions are to be traced out, it is not to be forgotten that our account of probability suppose an explicit advertence to insight; that other accounts do not; and that the other accounts not only possess the field but also penetrate the interpretation of scientific results. Only a critical and creative effort, meticulously separating methodological assumptions from scientific hypotheses, can determine adequately the relevance of the present analysis to the problems in which scientists are involved; or in the simpler words of Einstein's rather celebrated remark, the cognitional theorist has to attend, not to what scientists say, but to what they do.

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appendix(1)

Appendix to Chapter II.
On the Use of the Terms "Classical" and "Statistical".

In ordinary usage, "classical" and "statistical" are not opposed. The opposite to "classical" is "quantum," and the opposite to "statistical" is "mechanical". This usage may be illustrated by the fourfold classification of 1) classical mechanics (Newton), 2) classical statistics (Boltzmann), 3) quantum mechanics (Schrödinger, Heisenberg), and 4) quantum statistics (Bose-Einstein, Fermi-Dirac).

The trouble is that this fourfold classification seems incomplete. For relativity mechanics is opposed to classical mechanics and, while special relativity enters into combination with quantum mechanics (Dirac), general relativity seems as opposed to it as Einstein himself. Further, if these complications are not to be neglected, it is necessary to go behind the terminology to a systematic conception of the conceptions entertained by interpreters of physical theory. As is obvious, however, the purpose of this appendix is not to expound and justify a systematic view but simply to clarify the linguistic usage that we have found convenient by contrasting its assumptions with the assumptions that seem to underlie more common modes of speech.

From our viewpoint, then, the fundamental disjunction regards the interpretation of laws of the Newtonian and Einsteinian type. Such laws will be said to be interpreted concretely if they are taken to relate imaginable terms. The same laws will be said to be interpreted abstractly if they are taken to relate terms that are defined implicitly by the laws themselves.

On the first ~~step~~ alternative of concrete interpretation, the law is completely deterministic in principle. It is true enough that the law is expressed by a mathematical formula of wide generality and that further determinations will have to be ~~made~~ added before any application to concrete instances can occur. It also is true that the further determinations cannot be deduced from the law as a mathematical or as a physical formula. But on concrete interpretation the law is not simply a physical formula; it relates imaginable times; and because times are imaginable inasmuch as their various dimensions are assignable, it follows that for concrete interpretation the law is fully determinate in principle.

However those that accept the first alternative split into two groups. The first group not only affirms concrete interpretation but also affirms that concretely interpreted laws of the Newtonian type exist. The second group agrees with the first in admitting concrete interpretation but differs from it by affirming that, if any such laws seem to be verified, the verification is mere macroscopic appearance. The agreement and difference of the first and the second group seem to me to correspond to the agreement that unites and the difference that separates ordinary conceptions of classical statistics and quantum mechanics.

On the second alternative of abstract interpretation, the foregoing debate is replaced by a distinction. Concretely interpreted laws of the Newtonian and Einsteinian type are resolved into abstract and concrete components. The abstract component is the verified correlation of implicitly defined correlatives. The concrete component is the schematic or non-schematic situation.

The abstract component is determinate but not fully determinate. It is determinate in its own abstract order as an element in abstract system. But it becomes fully determinate only when it is applied successfully to concrete situations. Such application calls for two further types of information: first, one must know which laws in what combination are relevant to the given situation; secondly, one must know what numerical values are to be substituted for the variables and general constants of the abstract laws.

Now while there are well-known difficulties in obtaining accurate numerical values by measurement, a far more radical difficulty arises when one does not know exactly which combination of laws is relevant to a given situation, for then one is unable to go about the task of measuring in any orderly and economical manner. Fortunately, however, there do exist schematic situations in which ~~the~~ a happy constellation of circumstances and an appropriate combination of laws have the encouraging implication that the same laws will be applied over and over again in an indefinite sequence. Such, for example, is our planetary system, which has provided the most striking instances of accurate deduction and long-term prediction.

Unfortunately, there also are non-schematic situations. Then the task of applying abstract laws to concrete situations is at the mercy of circumstance, and the relevant circumstances form a diverging and scattering series of ever more numerous and remote conditions. For example, a planetary system has a beginning and comes to an end; either event can occur only once for any given system, and then it can occur in any of a notable range of different manners.

Still the existence of non-schematic situations, so far from blocking human intelligence, gives it a new impetus. Statistical investigation becomes the key to an account of the emergence and ~~s~~ survival, the numbers and distribution, the differentiation and development of schematic situations. Classical anticipation of the systematic and statistical anticipation of the non-systematic cease to be disparate alternatives. They become complementary techniques in gaining insight into a universe in which the thrust of probability generates from the ~~non-systematic~~ non-schematic ever more numerous and developed instances of the schematic.

Accordingly, our contrast ~~of~~ between classical and statistical rests not on current issues but on their transposition. On the basis of cognitional analysis the opposition between determinism and indeterminism is sublated in favour of a more comprehensive structure. Classical laws are reinterpreted so that Einstein's differential equations are regarded, not as statements about events at point-instants, but as mathematical expressions of the abstractness of classical laws. ~~Stat~~ Statistical laws are reinterpreted so that indeterminacy has its root in the abstractness of classical laws, its factual ground in the existence of non-schematic situations, and its significance in the type of explanation associated not with the name of Laplace but with the name of Darwin.

(ends p 107 in BL's manuscript)