Chapter 6

Foundations of Probability Theory and Statistical Mechanics

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1. What Makes Theories Grow?

Scientific theories are invented and cared for by people; and so have the properties of any other human institution — vigorous growth when all the factors are right; stagnation, decadence, and even retrograde progress when they are not. And the factors that determine which it will be are seldom the ones (such as the state of experimental or mathematical techniques) that one might at first expect. Among factors that have seemed, historically, to be more important are practical considerations, accidents of birth or personality of individual people; and above all, the general philosophical climate in which the scientist lives, which determines whether efforts in a certain direction will be approved or deprecated by the scientific community as a whole.

However much the "pure" scientist may deplore it, the fact remains that military or engineering applications of science have, over and over again, provided the impetus without which a field would have remained stagnant. We know, for example, that ARCHIMEDES' work in mechanics was at the forefront of efforts to defend Syracuse against the Romans; and that RUMFORD's experiments which led eventually to the first law of thermodynamics were performed in the course of boring cannon. The development of microwave theory and techniques during World War II, and the present high level of activity in plasma physics are more recent examples of this kind of interaction; and it is clear that the past decade of unprecedented advances in solid-state physics is not entirely unrelated to commercial applications, particularly in electronics.

Another factor, more important historically but probably not today, is simply a matter of chance. Often, the development of a field of knowledge has been dependent on neither matters of logic nor practical applications. The peculiar vision, or blindness, of individual persons can be decisive for the direction a field takes; and the views of one man can persist for centuries whether right or wrong. It seems incredible to us today that the views of Aristotle and Ptolemy could have dominated thought in mechanics and astronomy for a millenium, until GALILEO and others pointed out that we are all surrounded daily by factual evidence to the contrary; and equally incredible that, although thermometers (or rather, thermoscopes) were made by GALILEO before 1600, it required another 160 years before the distinction between temperature and heat was clearly recognized, by JOSEPH BLACK. (Even here, however, the practical applications were never out of sight; for GALILEO's thermoscopes were immediately used by his colleagues in the medical school at Padua for diagnosing fever; and JOSEPH BLACK's prize pupil was named JAMES WATT). In an age averse to any speculation, FRESNEL was nevertheless able, through pure speculation about elastic vibrations, to find the correct mathematical relations governing the propagation, reflection, and refraction of polarized light a half-century before MAXWELL's electromagnetic theory; while at the same time the blindness of a few others delayed recognition of the first law of thermodynamics for forty years.

Of far greater importance than these, however, is the general philosophical climate that determines the "official" views and standards of value of the scientific community, and the degree of pressure toward conformity with those views that the community exerts on those with a tendency to originality. The reality and effectiveness of this factor are no less great because, by its very nature, individual cases are more difficult to document; its effects "in the large" are easily seen as follows.

If you make a list of what you regard as the major advances in physical theory throughout the history of science, look up the date of each, and plot a histogram showing their distribution by decades, you will be struck immediately by the fact that advances in theory do not take place independently and randomly; they have a strong tendency to appear in small close clusters, spaced about sixty to seventy years apart. What we are observing here is the result of an interesting social phenomenon; this pressure toward conformity with certain officially proclaimed views, and away from free speculation, is subject to large periodic fluctuation. The last three cycles can be followed very easily, and the pressure maxima and minima can be dated rather precisely.

At the point of the cycle where the pressure is least, conditions are ideal for the creation of new theories. At these times, no one feels very sure just where the truth lies, and so free speculation is encouraged. New ideas of any kind are welcomed, and judged as all theories ought to be judged; on grounds of their logical consistency and agreement with experiment. Of course, we are only human; and so we also have a strong preference for theories which have a beautiful simplicity of concept. However, as stressed by many thinkers from OCCAM to EINSTEIN, this instinct seldom leads us away from the truth, and usually leads us toward it.

Eventually, one of these theories proves to be so much more successful than its competitors that, in a remarkably short time the pressure starts rising, all effective opposition ceases, and only one voice is heard. A well-known human frailty — overeagerness of the fresh convert — rides rough-shod over all lingering doubts, and the successful theory hardens into an unassailable official dogma, whose absolute, universal, and final validity is proclaimed independently of the factual evidence that led to it. We have then reached the peak of the pressure cycle; a High Priesthood arises whose members believe very sincerely that they are, at last, in possession of Absolute Truth, and this gives them the right and duty to combat errors of opinion with all the forces at their command. Exactly the same attitude was responsible, in still earlier times, for the Spanish Inquisition and the burning of witches.

At times of a pressure maximum, all free exercise of the imagination is frowned upon, and if one persists, severely punished. New ideas are judged, not on grounds of logic or fact, but on grounds of ideological conformity with the official dogma. To openly advocate ideas which do not conform is to be branded a crackpot and to place one's professional career in jeopardy; and very few have the courage to do this. Those who are students at such a time are taught only one view; and they miss out on the give and take, the argument and rational counter-argument, which is an essential ingredient in scientific progress. A tragic result is that many fine talents are wasted, through the misfortune of being born at the wrong time.

This high-pressure phase starts to break up when new facts are discovered, which clearly contradict the official dogma. As soon as one such fact is known, then we are no longer sure just what the range of validity of the official theory is; and we usually have enough clues by then so that additional disconcerting facts can be found without difficulty. The voice of the High Priests fades, and soon we have again reached a pressure minimum, in which nobody feels very sure where the truth lies and new suggestions are again given a fair hearing, so that creation of new theories is again socially possible.

Let us trace a few cycles of this pressure fluctuation (see Fig. 1). The pressure minimum that occurred at the end of the eighteenth century is now known as the "Age of Reason".

During a fairly short period many important advances in physical theory were made by such persons as LAPLACE, LAGRANGE, LAVOISIER, and FOURIER. Then a pressure maximum occurred in the first half of the nineteenth century, which is well described in some thermodynamics textbooks, particularly that of EPSTEIN [1]. This period of hostility toward free speculation seems to have been brought about, in part, by the collapse of SCHELLING'S *Naturphilosophie*, and its chief effect was to delay recognition of the first law of thermodynamics for several decades. As already noted, FRESNEL was one of the very few physicists who escaped this influence sufficiently to make important advances in theory.

Another pressure minimum was reached during the third quarter of the nineteenth century, when a new spurt of advances took place in a period of only fifteen years (1855—1870), in the hands of MAXWELL, KELVIN, HERTZ, HELMHOLTZ, CLAUSIUS, BOLTZMANN, and several

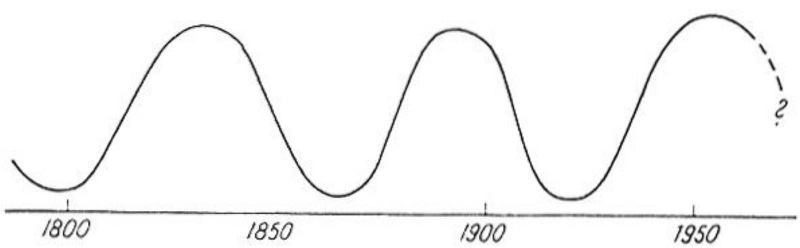

Fig. 1. Some recent fluctuations in social pressure in science

others. During this short period thermodynamics, electromagnetic theory, and kinetic theory were developed nearly to their present form; but the very success of these efforts led to another of the inevitable pressure maxima, which we recognize as being in full flower in the period 1885—1900. One of the tragedies (at least from the standpoint of physics) caused by this was the virtual loss of the talents of POINCARÉ. While his contributions to physical theory are considerable, still they are hardly commensurate with what we know of his enormous abilities. This was recognized and explained by E. T. BELL [2] in these words: "He had the misfortune to be in his prime just when physics had reached one of its recurrent periods of senility." The official dogma at that time was that all the facts of physics are to be explained in terms of Newtonian mechanics; particularly that of particles interacting through central forces. Herculean efforts were made to explain away MAXWELL'S electromagnetic theory by more and more complicated mechanical models of the ether — efforts which remind us very much of the earlier single-minded insistence that all the facts of astronomy must be explained by adding more and more Ptolemaic epicycles.

An interesting manifestation toward the end of this period was the rise of the school of "Energetics", championed by MACH and OSTWALD, which represents an early attempt of the positivist philosophy to limit the scope of science. This school held that, to use modern terminology, the atom was not an "observable", and that physical theories should not, therefore, make use of the concept. The demise of this school was brought about rapidly by PERRIN's quantitative measurements on the Brownian motion, which verified EINSTEIN's predictions and provided an experimental value for AVOGADRO's number.

The last "Golden Age of Theory" brought about by the ensuing pressure minimum, lasted from about 1910 to 1930, and produced our present general realitivity and quantum theories. Again, the spectacular success of the latter — literally thousands of quantitatively correct predictions which could not be matched by any competing theory — brought about the inevitable pressure rise, and for twenty-five years (1935—1960) theoretical physics was paralyzed by one of the most intense and prolonged high-pressure periods yet recorded. During this period the official dogma has been that all of physics is now to be explained by prescribing initial and final state vectors in a Hilbert space, and computing transition matrix elements between them. Any attempt to find a more detailed description than this stood in conflict with the official ideology, and was quickly suppressed without any attempt to exhibit a logical inconsistency or a conflict with experiment; this time, a few individual cases can be documented [*3*].

There are now many signs that the pressure has started down again; several of the supposedly universal principles of quantum theory have been confronted with new facts, or new investigations, which make us unsure of their exact range of validity. In particular, one of the fundamental tasks of any theory is to prescribe the class of physical states allowed by Nature. In MAXWELL's electromagnetic theory, for example, any mathematical solution of MAXWELL's equations is held to represent a possible physical state, which could in principle be produced in the laboratory. In quantum theory, we were taught for many years that the class of possible physical states is in 1:1 correspondence with solutions of the Schrödinger equation that are either symmetric or antisymmetric under permutations of identical particles. Our confidence in the universal validity of this rule has, recently, been shaken in two respects. In the first place, study of "parastatistics" has shown that much more general types of symmetry in configuration space can also be described by the machinery of quantized wavefunctions, and these new possibilities are not ruled out by experimental evidence. Secondly, the superposition principle (which may be regarded as a consequence of the above-mentioned rule, although it is usually considered in a still more general sense) holds that, if ψ_1 and ψ_2 are any two possible physical states, then any linear combination $\psi = a_1 \psi_1 + a_2 \psi_2$ is also a possible physical state. But with the appearance of superselection rules, we are no longer sure what the range of validity of the superposition principle is.

The discovery of parity nonconservation was a great psychological shock; a principle which had been taught to a generation of physicists as a universally valid physical law, so firmly established that it could be used to rule out *a priori* certain theoretical possibilities, such as WEYL's twocomponent relativistic wave equation, was found not to be universally valid after all; and again we are unsure as to its exact range of validity, and WEYL's equation has been resurrected.

Several quantum mechanics textbooks assure us that the phenomenon of spontaneous emission places a fundamental irreducible minimum value on the width of spectral lines. Such statements are now confronted with the laser, which — in instruments now commercially available, and as simple to operate as a sixty-watt light bulb — produce spectral lines over a million times narrower than the supposedly fundamental limit! Thus, all around the edges of quantum theory we see the familiar kind of crumbling which, historically, has always signalled the incipient breakdown of the theory itself.

I hasten to add that, of course, none of these developments affects the basic "hard core" of quantum theory in any way; they show only that certain gratuitous additions to quantum theory (which had, however, become very closely associated with the basic theory) were unsound in the sense that they were not of *universal* validity. But it is inevitable that, faced these developments, more and more physicists will ask themselves how many other principles are destined to crumble a little at the edges, so that they can again be considered valid objects for inquiry; and not articles of faith to be asserted dogmatically for the purpose of discouraging inquiry.

In particular, the uncertainty principle has stood for a generation, barring the way to more detailed descriptions of nature; and yet, with the lesson of parity still fresh in our minds, how can anyone be quite so sure of its universal validity when we note that, to this day, it has never been subjected to even one direct experimental test?

Today, elementary particle theorists are busily questioning and re-examining all the foundations of quantum field theory, in a way that would have been regarded as utter heresy ten years ago; and some have suggested that perhaps the whole apparatus of fields and Hamiltonians ought to be simply abandoned in favor of more abstract approaches. It would be quite inconsistent with the present mood of theoretical physics if we failed to question and re-examine *all* of the supposedly sacred principles of quantum theory.

For all these reasons, I think we are going to see a rapid decrease in pressure in the immediate future, and another period of great theoretical advances will again be socially possible in perhaps ten years. And I think we can predict with confidence that some of the clues which will lead to the next round of advances are to be found in the many suggestions already made by dissenters from the Copenhagen theory — suggestions which have, thus far, been met only by sneers and attacks, which no attempt to study their real potentialities.

2. Statistical Mechanics

At this point, I see that you are looking about anxiously and wondering if you are in the right room; for the announced title of this talk was, "Foundations of Probability Theory and Statistical Mechanics". What has all this to do with statistical mechanics? Well, I wanted to say a few things first about general properties of physical theories because statistical mechanics is, in several respects, an exceptional case. Statistical methods exist independently of physical theories, and so statistical mechanics is subject to additional outside interactions from other fields. The field of probability and statistics is also subject to periodic fluctuations, but they are not in phase with the fluctuations taking place in physics (they are right now at a deep pressure minimum); and so the history of statistical mechanics is more complicated.

In particular, statistical mechanics missed out on the latest pressure minimum in physics, because it coincided with a pressure maximum in statistics; the transition to quantum statistics took place quietly and uneventfully without any real change in the basic formalism of GIBBS, and without any extension of the range of applicability of the theory. There was no advance in understanding, as witnessed by the fact that debates about irreversibility continue to this day, repeating exactly the same arguments and counter-arguments that were used in the time of BOLTZMANN; and the newest and oldest textbooks you can find hardly differ at all in their presentation of fundamentals. In short, statistical mechanics has suffered a period of stagnation and decadence that makes it unique in the recent history of science.

A new era of active work in statistical mechanics started, however, about 1955, in phase with a revolution in statistical thought but not at first directly influenced by it. This was caused, in part, by practical needs; an understanding of irreversible processes became increasingly necessary in chemical and mechanical engineering as one demanded more efficient industrial processing plants, stronger and more reliable materials, and bigger and better bombs. There is always a movement of scientific talent into areas where generous financial support is there for the taking. Another cause was the appearance of a few people who were genuinely interested in the field for its own sake; and perhaps it helped to reflect that, since it had been virtually abandoned for decades, one might be able to work in this field free of the kind of pressure noted above, which was paralyzing creative thought in other areas of physics.

Regardless of the reasons for this renewed activity, we have now made considerable progress in theoretical treatment of irreversible processes; at least in the sense of successful calculation of a number of particular cases. It is an opportune time to ask whether this has been accompanied by any better understanding, and whether the foundations of the subject can now be put into some kind of order, in contrast to the chaos that has persisted for almost a century. I hope to show now that the answer to both of these questions is yes; and that recent developments teach us an important lesson about scientific methodology in general.

Let me state the lesson first, and then illustrate it by examples from statistical mechanics. It is simply this: *You cannot base a general mathematical theory on imprecisely defined concepts. You can make some progress that way; but sooner or later the theory is bound to dissolve in ambiguities which prevent you from extending it further.* Failure to recognize this fact has another unfortunate consequence which is, in a practical sense, even more disastrous: *Unless the conceptual problems of a field have been clearly resolved, you cannot say which mathematical problems are the relevant ones worth working on; and your efforts are more than likely to be wasted.* I believe that, in this century, thousands of man-years of our finest mathematical talent have been lost through failure to understand this simple principle of methodology; and this remark applies with equal force to physics and to statistics.

2.1. BOLTZMANN's Collision Equation

Let us consider some case histories. BOLTZMANN sought to describe the approach to equilibrium in a gas in terms of the distribution $f(x, p, t)$. In his first work, this function was defined as giving the actual number of particles in various cells of phase space; thus if R denotes the set of points comprising a region of six-dimensional phase space, the number of particles in R is to be computed from

$$n_R = \int_R f(x, p, t)\, d^3x\, d^3p\,. \tag{1}$$

After some physical arguments which need not concern us here, BOLTZMANN concluded that the time evolution of the gas should be described by his famous "collision equation",

$$\frac{\partial f}{\partial t} + \sum_\alpha \left[\frac{P_\alpha}{m}\frac{\partial f}{\partial x_\alpha} + F_\alpha \frac{\partial f}{\partial P_\alpha}\right] = \int \partial^3 P' \int \partial \Omega\, (\bar{f}\bar{f}' - f f')\,\sigma \tag{2}$$

where F_α is the α-component of external force acting on a particle; and the right-hand side represents the effects of collisions in redistributing particles in phase space, in a way familiar to physicists. As a consequence of this equation, it is easily shown that the quantity

$$H_B = \int f \log f \, d^3 x \, d^3 p \tag{3}$$

can only decrease (in this equation we integrate over all the accessible phase space); and so BOLTZMANN sought to identify the quantity

$$S_B = -k H_B \tag{4}$$

with the entropy, making the second law of thermodynamics a consequence of the dynamical laws, as expressed by (2). As we know, this was challenged by ZERMELO and LOSCHMIDT who produced two counter-examples, based on time-reversal and on the POINCARÉ recurrence theorem, showing that Eq. (2) could not possibly be an exact expression of the dynamical equations of motion, *and thereby placing the range of validity of Boltzmann's theory in doubt.*

At this point, confusion entered the subject; and it has never left it. For BOLTZMANN then retreated from his original position, and said that he did not intend that $f(x, p, t)$ should represent necessarily the *exact* number of particles in various regions [indeed, it is clear that the only function f which has exactly the property of Eq. (1) is a sum of delta-functions: $f(x, p, t) = \Sigma_i \, \delta(x - x_i) \, \delta(p - p_i)$, where $x_i(t)$, $p_i(t)$ are the position and momentum of the i-th particle]. It represents only the *probable* number of particles; or perhaps the *average* number of particles; or perhaps it gives the *probability* that a given particle is to be found in various regions. The decrease in H_B is then not something which must happen *every* time; but only what will *most probably* happen; or perhaps what will happen *on the average*, etc.

Unfortunately, neither BOLTZMANN nor anybody else has ever become more explicit than this about just what BOLTZMANN's f; and therefore BOLTZMANN's H-theorem, means. When our concepts are not precisely defined, they are bound to end up meaning different things to different people, thus creating rooom for endless and fruitless debate, of exactly the type that has been going on ever since. Furthermore, when we debate about imprecise concepts, we can never be sure whether we are arguing about a question of fact; or only a question about the meaning of words. From BOLTZMANN's day to this, the debate has never been able to rise above this level.

If you think my characterization of the situation has been too laconic, and unfair to many honest seekers after the truth, I invite you to examine a recent review article on transport theory [*4*]. On page 271, the author states that "The Boltzmann distribution function — is the (probable) number of particles in the positional range $d^3 x$ and the velocity range $d^3 v$ —". On page 274 this is altered to: "The quantity f, the Boltzmann distribution function — is, roughly speaking, the average number of particles in a cell in the $x-v$ space (the μ-space). f refers to a single system. A more precise definition of f can be obtained through the use of the master function P." Consulting this master function, we find that neither the definition of P, nor its connection with f, is ever given. This, furthermore, is not a particularly bad example; it is typical of what one finds in discussions of BOLTZMANN's theory.

Let us note some of the difficulties that face the practical physicist because of this state of utter confusion with regard to basic concepts. Suppose we try to assess the validity of BOLTZMANN's equation (2) for some particular problem; or we try to extend it to higher powers in the density, where higher order collisions will become important in addition to the binary ones that are taken into account, in some sense, in (2). If we agree that f represents an *average* number of particles, we must still specify what this average is to be taken over. Is it an average over the particles, an average over time for a single system, an average over many copies of the single system, or an average over some probability distribution? Different answers to this question are going to carry different implications about the range of validity of (2), and about the correct way of extending it to more general situations. Even without answering it at all, however, we can still see the kind of difficulties that are going to face us. For if $f(x, p, t)$ is an average over something, then the left-hand side of (2) is also an average over this same something. So also, therefore, is the right-hand side if the equation is correct. But on the right-hand side we see the product of two f's; the product of two averages.

If you meditate about this for a moment, I think you will find it hard to avoid concluding that, if f is an average, then the right-hand side ought to contain the average of a product, not the product of the averages. These quantities are surely different; but we cannot say how different until we say what we are averaging over. *Until this ambiguity in the definition of Boltzmann's f is cleared up, we cannot assess the range of validity of* Eq. (2), *and we cannot say how it should be extended to more general problems.* Because of imprecise concepts, the theory reaches an impasse at the stage where it has barely scratched the surface of any real treatment of irreversible processes!

2.2. Method of GIBBS

For our second case history, we turn to the work of GIBBS. This was done some thirty years after the aforementioned work of BOLTZMANN, and the difficulties noted above, plus many others for which we do not have time here, were surely clear to GIBBS, who was extremely careful in matters of logic, detail, and definitions.

All important advances have their precursors, the full significance of which is realized only later; and the innovations of GIBBS were not entirely new. For example, considerations of the full phase space (Γ-space) appear already in the works of MAXWELL and BOLTZMANN; and GIBBS' canonical ensemble is clearly only a small step removed from the distribution laws of MAXWELL and BOLTZMANN. However, GIBBS applied these ideas in a way which was unprecedented; so much so that his work was almost totally rejected ten years later in the famous Ehrenfest review article [*5*], which has had a dominating influence on thought in statistical mechanics for fifty years. In this article, the methods of GIBBS are attacked repeatedly, and the physical superiority of BOLTZMANN's approach is proclaimed over and over again. For example, GIBBS' canonical and grand canonical ensembles are dismissed as mere "analytical tricks", which do not solve the problem; but only enable GIBBS to *evade* what the authors consider to be real problems of the subject!

Since then, of course, the mathematical superiority of GIBBS' methods for calculating equilibrium thermodynamic properties has become firmly established; and so statistical mechanics has become a queer hybrid, in which the practical calculations are always based on the methods of GIBBS; while in the pedagogy virtually all one's attention is given to repeating the arguments of BOLTZMANN.

This hybrid nature — the attempt to graft together two quite incompatible philosophies — is nowhere more clearly shown than in the fact that the "official" commentary on GIBBS' work [*6*] devotes a major amount of space to discussion of ergodic theories. Now, it is a curious fact that if you study GIBBS' work, you will not find the word "ergodic" or the concept of ergodicity, at any point. Recalling that ergodic theorems, or hypotheses, had been actively discussed by other writers for over thirty years, and recalling GIBBS' extremely meticulous attention to detail, I think the only possible conclusion we can draw is that GIBBS simply *did not consider ergodicity as relevant to the foundations of the subject*. Of course, he was far too polite a man to say so openly; and so he made the point simply by developing his theory without making any use of it. Unfortunately, this tactic was too subtle to be appreciated by most readers; and the few who did notice it took it to be a defect in GIBBS' presentation, in need of correction by others.

This situation has had very unfortunate consequences, in that the work of GIBBS has been persistently misunderstood; and in particular, the full power and generality of the methods he introduced have not yet been recognized in any existing textbook. However, it is not a question of placing blame on anyone; for we can understand and sympathize with the position of everyone involved. I think that a historical study will convince you, as it has convinced me, that all of this is the more or less inevitable result of the fact that GIBBS did not live long enough to complete his work. The principle he had discovered was so completely new, and the method of thinking so completely different from what had gone before, that it was not possible to explain it fully, or to explore its consequences for irreversible phenomena, in the time that was granted to him.

GIBBS was in rapidly failing health at the time he wrote his work on statistical mechanics, and he lapsed into his final illness very soon after the manuscript was sent to the publisher. In studying his book, it is clear that it was never really finished; and we can locate very accurately the place where time and energy ran out on him. The first eleven chapters are written in his familiar style — extremely meticulous attention to detail, while unfolding a carefully thought out logical development. At Chapter 12, entitled, "On the Motion of Systems and Ensembles of Systems Through Long Periods of Time", we see an abrupt change of style; the treatment becomes sketchy, and amounts to little more than a random collection of observations, trying to state in words what he had not yet been able to reduce to equations. On pages 143—144 he tries to explain the methodology which led him to his canonical and grand canonical ensembles, as well as the ensemble canonical in the angular momenta which was presented in Chapter 4 but not applied to any problem [*7*]. However, he devotes only two sentences to this; and the principle he states is what we would recognize today as the principle of maximum entropy! To the best of my knowledge, this passage has never been noted or quoted by any other author (it is rather well hidden among discussions of other topics); and I discovered it myself only by accident, three years after I had written some papers [*8*] advocating this principle as a general foundation for statistical mechanics. This discovery convinced me that there was much more to the history of this subject than one finds in any textbook, and induced me to study it from the original sources; some of the resulting conclusions are being presented in this talk.

GIBBS' discussion of irreversibility in this chapter does not advance beyond pointing to a qualitative analogy with the stirring of colored ink in water; and this forms the basis for another of the EHRENFEST'S criticisms of his work. I think that, had GIBBS been granted a few more years of vigorous health, this would have been replaced by a simple and rigorous demonstration of the second law based on other ideas. For it turns out that all the clues necessary to point the way to this, and all the mathematical material needed for the proof, were already present in the first eleven chapters of his book; it requires only a little more physical reasoning to see that introduction of coarse-grained distributions does not advance our understanding of irreversibility and the second law, for the simple reason that the latter are experimentally observed *macroscopic* properties; and the fine-grained and coarse-grained distributions lead to just the same predictions for all macroscopic quantities. Thus, the difference between the fine-grained and coarse-grained H-functions has nothing to do with the experimentally observed entropy; it depends only on the particular way in which we choose to coarse-grain.

On the other hand, the variational (maximum entropy) property noted by GIBBS does lead us immediately to a proof, not only of the second law, but of an extension of the second law to nonequilibrium states. I have recently pointed this out [*9*] and supplied the very simple proof, which I think is just the argument GIBBS would have given if he had been able to complete his work. However, this is not the main point I wish to discuss tonight, so let us turn back to other topics.

In defense of the EHRENFEST'S position, it has to be admitted that, through no fault of his own, GIBBS did fail to present any clear description of the motivation behind his work. I believe that it was virtually impossible to understand what GIBB'S methods amounted to, *and therefore how great was their generality and range of validity*, until the appearance of SHANNON'S work on Information Theory, in our own time [*10*]. Finally, until recently the situation in probability theory itself, which was in a high-pressure phase completely dominated by the frequency theory, which only sneers and attacks on the theories of LAPLACE and JEFFREYS, has made it impossible even to discuss, much less publish, the viewpoint and approach which I believe has now solved these problems.

Now, in order to lend a little more substance to these remarks, let's examine some equations, the net result of GIBBS' work. Considering a closed system (i.e., no particles enter or leave), the thermodynamic properties are to be calculated from the Hamiltonian $H(q_i, p_i)$ as follows. First, we define the *partition function*

$$Z(\beta, V) = \int \exp(-\beta H)\, dq_1\, dp_1 \ldots dq_n\, dp_n, \tag{5}$$

where we integrate over all the accessible phase space, and the dependence on the volume V arises because the range of integration over the coordinates q_i depends on V. If we succeed in evaluating this function, then all thermodynamic properties are known; for the energy function (which determines the thermal properties) is given by

$$U = -\frac{\partial}{\partial\beta} \log Z \tag{6}$$

in which we interpret β as $(kT)^{-1}$, where k is BOLTZMANN'S constant and T the KELVIN temperature; and the equation of state is

$$P = \frac{1}{\beta} \frac{\partial}{\partial V} \log Z. \tag{7}$$

Now, isn't this a beautifully simple and neat prescription? For the first time in what has always been a rather messy subject, one had a glimpse of the kind of formal elegance that we have in mechanics, where a single equation (HAMILTON'S principle) summarizes everything that needs to be said. Of all the founders of statistical mechanics, only GIBBS gives us this formal simplicity, generality, and as it turned out, a technique for practical calculation which the labors of another sixty years have not been able to improve on. The transition to quantum statistics took place so quietly and uneventfully because it consisted simply in the replacement of the integral in (5) by the corresponding discrete sum; and nothing else in the formalism was altered.

In the history of science, whenever a field has reached such a stage, in which thousands of separate details can be summarized by, and deduced from, a single formal rule — then an extremely important synthesis has been accomplished. Furthermore, by understanding the basis of this rule it has always been possible to extend its application far beyond the original set of facts for which it was designed. And yet, this did not happen in the case of GIBBS' formal rule. With only a few exceptions, writers on statistical mechanics since GIBBS have tried to snatch away this formal elegance by grafting GIBBS' method onto the substrate of BOLTZMANN'S ideas, for which GIBBS himself had no need. However, a few, including TOLMAN and SCHRÖDINGER, have seen GIBBS' work in a different light — as something that can stand by itself without having to lean on unproved ergodic hypotheses, intricate but arbitrarily defined cells in phase space, Z-stars, and the like. Thus, while a detailed study will show that there are as many different opinions as to the reason for GIBBS' rules as there are writers on the subject, a more coarse-grained view shows that these writers are split into two basic camps; those who hold that the ultimate justification of GIBBS' rules must be found in ergodic theorems; and those who hold that a principle for assigning *a priori* probabilities will provide a sufficient justification. Basically, the confusion that still exists in this field arises from the fact that, while the *mathematical content* of GIBBS' formalism can be set forth in a few lines, as we have just seen, the *conceptual basis* underlying it has never been agreed upon.

Now, while GIBBS' formalism has a great generality — in particular, it holds equally well for gas and condensed phases, while BOLTZMANN'S results apply only to dilute gases — it nevertheless fails to give us many things that BOLTZMANN's "collision equation" does yield, however imperfectly. For BOLTZMANN's equation can be applied to irreversible processes; and it gives definite theoretical expressions for transport coefficients (viscosity, diffusion, heat conductivity), while GIBBS' rules refer only to thermal equilibrium, and one has not seen how to extend them beyond that domain. Furthermore, in spite of all my carping about the imprecision of BOLTZMANN's equation, the fact remains that it has been very successful in giving good numerical values for these transport coefficients; and it does so even for fairly dense gases, where we really have no right to expect such success. So, my adulation of Gibbs must be carried to the point of rejecting BOLTZMANN's work; it appears that we need both approaches!

All right. I have now posed the problem as it appeared to me a number of years ago. Can't we learn how to combine the best features of both approaches, into a new theory that retains the unity and formal simplicity of GIBBS' work with the ability to describe irreversible processes (hopefully, a *better* ability) of BOLTZMANN's work? This question must have occurred to almost every physicist who has made a serious study of statistical mechanics, for the past sixty years. And yet, it has seemed to many a hopelessly difficult task; or even an impossible one. For example, at the 1956 International Congress on Theoretical Physics, L. VAN HOVE [*11*] remarked, "In contrast to the case of thermodynamical equilibrium, no general set of equations is known to describe the behavior of many-particle systems whenever their state is different from the equilibrium state and, in view of the unlimited diversity of possible nonequilibrium situations, the existence of such a set of equations seems rather doubtful".

Now, while I hesitate to say so at a symposium devoted to Philosophy of Science, the injection of philosophical considerations into science has usually proved fruitless, in the sense that it does not, of itself, lead to any advances in the science. But there is one extremely important exception to this; and it is in exactly the situation now before us. At the stage in development of a theory where we already have a formalism successful in one domain, and we are trying to extend it to a wider one, some kind of philosophy about what the formalism "means" is absolutely essential to provide us with a sense of direction. And it need not even be a "true" philosophy — whatever that may mean — for its real justification will not lie in whether it is "true", but in whether it does point the way to a successful extension of the theory.

In the construction of theories, a philosophy plays somewhat the same role as scaffolding does in the construction of buildings; you need it desperately at a certain phase of the operation, but when the construction is completed you can remove if it you wish; and the structure will still stand of its own accord. This analogy is imperfect, however, because in the case of theories, the scaffolding is rarely ugly, and many will wish to retain it as an integral part of the final structure. At the opposite extreme to this conservative attitude stands the radical positivist, who in his zeal to remove every trace of scaffolding, also tears down part of the building. Almost always, the wisest course will lie somewhere between these extremes.

The point which I am trying to make, in this rather cryptic way, is just the one which we have already noted in the attempt to evaluate and extend BOLTZMANN's collision equation. Different philosophies of what that equation means carry different implications as to its range of validity, and the correct way of extending it. And we are now at just the same impasse with regard to GIBBS' equations; *because their conceptual basis has not been precisely defined, the theory dissolves in ambiguities* which have prevented us, for sixty years, from extending it~~o~~ to new domains.

2.3. Conceptual Problems of the Ensemble

The fact that two different camps exist, with diametrically opposed views as to the justification of GIBBS' methods, is simply the reflection of two diametrically opposed philosophies about the real meaning of the GIBBS ensemble; and this in turn arises from two different philosophies about the meaning of *any* probability distribution. Thus, the foundations of probability theory itself are involved in the problem of extending GIBBS' methods.

Statistical mechanics has always been troubled with questions concerning the relation between the ensemble and the individual system, even apart from possible extensions to nonequilibrium cases. In the theory, we calculate numbers to compare with experiment by taking ensemble *averages*; that is what we are doing in Eqs. (6) and (7). And yet, our experiments to check these predictions are not performed on ensembles; they are performed on the one *individual* system that exists in the laboratory. Nevertheless, we find that the predictions are verified accurately; a rather astonishing result, but one without which we would have little interest in ensembles. For if it were necessary to repeat a thermodynamic measurement 1,000 times and average the results before any regularities (laws of thermodynamics) began to appear, both thermodynamics and statistical mechanics would be virtually useless to us; and they would not appear in our physics curriculum. Thus, it appears that a major problem is to explain why GIBBS' rules work in practice; and not only why they work so well, but why they work at all!

We can make this dilemma appear still worse by noting that the relation between the ensemble and the individual system is usually described by supposing that the individual system can be regarded as having been drawn "at random" from the ensemble. I personally have never been able to comprehend what "at random" means; for I ask myself: What is the criterion, what is the test, by which we could decide whether it was or was not really "random"? Does it make sense to ask whether it was *exactly* random, or *approximately* random? — and neither the literature nor my introspection give me any answer. However, even without understanding this point, the real difficulty is obvious; for the *same* individual system may surely, and with equal justice, be regarded as having been drawn "at random" from any one of an infinite number of *different* ensembles! But the measured properties of an individual system depend on the *state of the system*; and not on which ensemble you or I regard it as having been "drawn from". How, then is it possible that ensemble averages coincide with experimental values?

The two different philosophical camps try to extricate themselves from this dilemma in two entirely different ways. The "ergodic" camp, of course, is composed of those who believe that a probability distribution describes an objectively real physical situation; that it stands for an assertion about experimentally measurable *frequencies*; that it is therefore either correct or incorrect; and that this can, in principle, be decided by performing "random experiments". They note that what we measure in any experiment is necessarily a time average over a time that is long on the atomic scale of things; and so the success of GIBBS' methods will be accounted for if we can prove, from the microscopic equations of motion, that the *time average* for an individual system is equal to the *ensemble average* over the particular ensembles given by GIBBS.

This viewpoint has much to recommend it. In the first place, physicists have a natural tendency to believe that, since the observed properties of matter "in the large" are simply the resultant of its properties "in the small" multiplied many times over, it ought to be possible to obtain the macroscopic behavior by strict logical deduction from the microscopic laws of physics; and the "ergodic" approach gives promise of being able to do this. Secondly, while the necessary theorems have not been established rigorously and universally, the work done on this problem thus far has made it highly plausible that, in a system interacting with a large heat bath, the *frequencies* with which various microscopic conditions are realized in the long run are indeed given correctly by the GIBBS canonical ensemble. This has been rendered so extremely plausible that I think no reasonable person can seriously doubt that it is true, although we cannot rule out the possibility of occasional "pathological" exceptions. Thus the "ergodic" school of thought has, in my opinion, very nearly succeeded in its aim of establishing equality of time averages and ensemble averages *for the particular case of Gibbs' canonical* *ensemble*; and in the following I am simply going to grant, for the sake of the argument, that this program has succeeded entirely.

Nevertheless, the "ergodic" school of thought still faces a fundamental difficulty; and one that was first pointed out by BOLTZMANN himself, and stressed in the EHRENFEST review article. Curiously, there exists to this day a group of workers in Europe who refuse to recognize the seriousness of this difficulty, and deny that it invalidates their approach. The difficulty is that, even if one had succeeded in proving these ergodic theorems rigorously and universally, the result would have been established only for time averages *over infinite times*; whereas the experiments which verify GIBBS' rules measure time averages only over finite times. Thus, a further mathematical demonstration would in any event be necessary, to show that these finite time averages have sufficiently approximated their limits for infinite times.

Now we can give simple and general counter-examples proving that such an additional demonstration *cannot* be given; and indeed that any macroscopic system, given a time millions of times the age of the universe, still could not "sample" more than an infinitesimal fraction of all the microscopic states which have high probability in the canonical emsemble; and thus any assertion about the *frequencies* with which different microscopic states are realized in an individual system, is completely devoid of operational meaning.

The easiest way of seeing this is just to note that, if a macroscopic system could sample all microscopic states in the time in which measurements are made, so that the measured time averages would be equal to ensemble averages, then the measured values would necessarily always be the *equilibrium* values; we would not even know about irreversible processes! *The fact that we can measure the rate of an irreversible process already proves that the time required for a representative sampling of microstates must be much longer than the time required to make our measurements.* Thus, any purported proof that time averages over the finite times involved in actual measurements are equal to canonical ensemble averages would, far from justifying statistical mechanics, stand in clear conflict with the very experimental facts about irreversibility that we are trying to account for by extending GIBBS' methods!

The thing which has to be explained is, not that ensemble averages are equal to time averages; but the much stronger statement that ensemble averages are equal to experimental values. The most that ergodic theorems could possibly establish is that ensemble averages are equal to time averages over infinite time, and so the "ergodic" approach cannot even justify equilibrium statistical mechanics without contradicting experimental facts. Obviously, such an approach cannot be extended to irreversible processes where, in order for ensemble theory to be of any use, the ensemble averages must still be equal to experimental values; but the very phenomena to be explained consist of the fact that these are *not* equal to time averages.

The above line of reasoning convinced me, ten years ago, that further advances in the basic formulation of statistical mechanics cannot be made within the framework of the "ergodic" viewpoint; and, rightly or wrongly, it seemed equally clear to me that the really fundamental trouble which was preventing further advances, both in statistical mechanics and in the field of statistics in general, was this dogmatic, single-minded insistence on the frequency theory of probability which had dominated the field for so many years. At that time, virtually every writer on probability theory felt impelled to insert an introductory paragraph or two, expressing his denunciation and total rejection of the so-called "subjective" interpretation of probability, as advocated by LAPLACE, DE MORGAN, POINCARÉ, KEYNES, and JEFFREYS; and this was done, invariably, without any attempt to understand the arguments and results which these people — particularly LAPLACE and JEFFREYS — had advanced. The situation was, psychologically, exactly like the one which has dominated American Politics since about 1930; the Republicans continually analyze the statements of Democrats and issue counter-arguments, which the Democrats contemptuously dismiss without any attempt to understand them or answer them.

On the other hand, I had taken the trouble to read all of JEFFREYS' work, and much of LAPLACE'S, on probability theory; and was unable to find any of the terrible things about which the "frequentist" writers had warned us. On the philosophical side I found their arguments to be, far from irresponsible and useless, so eminently sound and reasonable that I could not imagine any sane person disputing them. On the mathematical side, I found that in problems of statistical estimation and hypothesis testing, any problem for which the "frequentist" offered any solution at all was also solved with ease by the methods of LAPLACE and JEFFREYS; and their results were either the same or demonstrably superior to the ones found by the frequentists. Furthermore, the methods of LAPLACE and JEFFREYS (which were, of course, based on BAYES' theorem as the fundamental tool of statistics) were applied with equal ease to many problems which, according to the frequentist, did not belong to the field of probability theory at all; and they still yielded perfectly reasonable, and scientifically useful, results!

I don't want to dwell at length on the situation in probability theory, because time is running short and a rather large exposition of this, with full mathematical details, is being readied for publication elsewhere. But let me just mention one example of what one finds if he takes the trouble to go beyond polemics and study the mathematical facts of the matter.

In problems of interval estimation of unknown parameters, the frequentist has rejected the method of LAPLACE and JEFFREYS, on grounds that I can only describe as ideological, and has advocated vigorously the method of confidence intervals. Now it is a matter of straightforward mathematics to show that, whenever the frequentist's "estimator" is not a sufficient statistic (in the terminology of FISHER), there is always a class of possible samples for which the method of confidence intervals leads to absurd or dangerously misleading results, in the sense that it yields a wrong answer far more frequently (or, if one prefers, with far higher probability) than one would suppose from the stated confidence level. The confidence interval can, in some cases, contradict what can be proved on strict deductive reasoning from the observed sample. One can even invent problems, which are not at all unrealistic, in which the probability of this happening is greater than the stated confidence level!

This is something which, to the best of my knowledge, you cannot find mentioned in any of the "orthodox" statistical literature; and I shudder to think of some of the possible consequences, if important decisions are being made on the basis of confidence interval analyses. The method of LAPLACE and JEFFREYS is demonstrably free from this defect; it cannot contradict deductive reasoning and, in the case of the aforementioned "bad" class of samples, it automatically detects them and yields a wider interval, so that the probability of a correct decision remains equal to the stated value. Once one is aware of such facts, the arguments advanced against the method of LAPLACE and JEFFREYS and in favor of confidence intervals (i.e. that it is meaningless to speak of the probability that θ lies in a certain interval, because θ is not a "random variable," but only an unknown constant) appear very much like those of the 17th century scholar who claimed his theology had proved there could be *no* moons on Jupiter, and steadfastly refused to look through GALILEO's telescope.

Since the reasoning by which the "frequentist" has rejected LAPLACE's methods is so patently unsound, and since attempts to extend, or even justify, GIBBS' methods in terms of the frequency theory of probability have met with an impasse, it would appear that we ought to explore the possibilities of applying LAPLACE's "subjective" theory of probability to this problem. At any rate, to reject this procedure without bothering to explore its potentialities, is hardly what we mean by a "scientific" attitude! So, I undertook to think through statistical mechanics all over again, using the concept of "subjective" probability.

It became clear, very quickly, that to do this makes all the unsolved problems of the theory appear in a very different light; and possibilities for extension of GIBBS' methods are seen in entirely different directions. Once we clearly and explicitly free ourselves from the delusion that an ensemble describes an "objectively real" physical situation, and recognize that it describes only a certain *state of knowledge*, then it is clear that, in the case of irreversible processes, the knowledge which we have is of a different nature than in the case of equilibrium. We can then see the problem as one which cannot even be formulated in terms of the frequency theory of probability. It is simply this: *What probability assignment to microstates correctly describes the state of knowledge which we have, in practice, about a nonequilibrium state?* Such a question just doesn't make sense in terms of the frequency theory; but, thanks to the work of GIBBS and SHANNON, I believe that it makes extremely good sense, and in fact has a very general and mathematically unambiguous solution in terms of subjective probabilities.

3. The General Maximum-Entropy Formalism

If we accept SHANNON's interpretation (which can be justified by other mathematical arguments entirely independent of the ones given by SHANNON) that the quantity

$$H = -\sum_i p_i \log p_i \tag{8}$$

is an "information measure" for any probability distribution p_i; i.e. that it measures the "amount of uncertainty" as to the true value of i, then an ancient principle of wisdom — that one ought to acknowledge frankly the full extent of his ignorance — tells us that the distribution that maximizes H subject to constraints which represent whatever information we have, provides the most honest description of what we know. The probability is, by this process, "spread out" as widely as possible without contradicting the available information.

But recognition of this simple principle suddenly makes all the maximum-minimum properties given by GIBBS in his Chapter XI — what I believe to be the climax of GIBBS' work, and just the place where time and energy ran out on him — acquire a much deeper meaning. If we specify the expectation value of the energy, this principle uniquely determines GIBBS' canonical ensemble. If we specify the expectations of energy and mole numbers, it uniquely determines GIBBS' grand canonical ensemble [8]. If we specify the expectations of energy and angular momentum, it uniquely determines GIBBS' rotational ensemble [7]. Thus, all the results of GIBBS on statistical mechanics follow immediately from the principle of maximum entropy; and their derivation is astonishingly short and simple compared to the arguments usually found in textbooks.

But the generalization of GIBBS' formalism to nonequilibrium problems also follows immediately (although I have to confess that I spent six years trying to do this by introducing new and more complicated principles, before I finally saw how simple the problem was). For this principle in no way depends on the physical meaning of the quantities we specify; there is nothing unique about energy, mole numbers, or angular momentum. If we grant that it represents a valid method of reasoning at all, then we must also grant that it applies equally well to *any physical quantity whatsoever*. So, let us jump immediately, in view of the time, to the most sweeping generalization of GIBBS' formalism.

We have a number of physical quantities about which we have some experimental information. Let them be represented by the Heisenberg operators $F_1(x, t)$, $F_2(x, t)$, ... $F_m(x, t)$. In general they will depend on the position x and, through the equations of motion, on the time t. For example, F_1 might be the particle density, F_2 the density of kinetic energy, F_3 the "mass velocity" of the fluid, F_4 the (yz)-component of the stress tensor, F_5 the intensity of magnetization, ..., and so on; whatever information of this type is available, represents our definition of the nonequilibrium state.

Now we wish to construct a density matrix ϱ which incorporates all this information. When I say that a density matrix "contains" certain information, I mean by this simply that, if we apply the usual rule for prediction; i.e. calculate the expectation values

$$\langle F_k(x, t)\rangle = \mathrm{Tr}\left[\varrho\, F_k(x, t)\right] \tag{9}$$

we must be able to recover this information from the density matrix. Thus, the mathematical constraints on the problem are that the expectation values (9) must agree with the experimental information:

$$f_k(x, t) = \mathrm{Tr}\left[\varrho\, F_k(x, t)\right], \qquad x, t \text{ in } R_k \tag{10}$$

where $f_k(x, t)$ represent the experimental values, and R_k is the space-time region in which we have information about f_k; in general it may be different for different k. Subject to these constraints, we are to maximize the "information entropy"

$$S_I = -\,\mathrm{Tr}\,(\varrho \log \varrho) \tag{11}$$

which is the appropriate generalization of (8), as found many years ago by VON NEUMANN. The solution of this variational problem is:

$$\varrho = \frac{1}{Z} \exp\left\{\sum_{k=1}^{m} \int_{R_k} d^3x\, dt\, \lambda_k(x, t)\, F_k(x, t)\right\} \tag{12}$$

where the $\lambda_k(x, t)$ are a set of real functions to be determined presently (they arise mathematically as Lagrange multipliers in solving the variational problem with constraints), and for normalization the partition function of Gibbs has been generalized to the *partition functional:*

$$Z[\lambda_k(x, t)] \equiv \mathrm{Tr} \exp\left\{\sum_{k=1}^{m} \int_{R_k} d^3 x \, dt \, \lambda_k(x, t) \, F_k(x, t)\right\}. \tag{13}$$

The $\lambda_k(x, t)$ are now to be found from the conditions (10), which reduce to

$$f_k(x, t) = \frac{\delta}{\delta \lambda_k(x, t)} \log Z \tag{14}$$

which is a generalization of Gibbs' equation (6); where δ denotes the functional derivative. Mathematical analysis shows that (14) is just sufficient to determine uniquely the integrals in the exponent of (12); it does not necessarily determine the functions $\lambda_k(x, t)$, but it does determine the only property of those functions which is needed in the theory; a very interesting example of mathematical economy.

The density matrix having been thus found, prediction of any other quantity $J(x, t)$ in its space-time dependence is then found by applying the usual rule:

$$\langle J(x, t)\rangle = \mathrm{Tr}\left[\varrho J(x, t)\right]. \tag{15}$$

In Eqs. (12) to (15) we have the generalization of Gibbs' algorithm to arbitrary nonequilibrium problems. From this point on, it is simply a question of mathematics to apply the theory to any problem you wish.

Of course, it requires a great deal of nontrivial mathematics to carry out these steps explicitly for any nontrivial problem! If Gibbs' original formalism was somewhat deceptive, in that its formal simplicity conceals an enormous amount of intricate detail, the same is true with a vengeance for this generalization. Nevertheless, it is still only mathematics; and if it were important enough to get a certain result, one could always hire a building full of mathematicians and computers to grind it out; there are no further questions of principle to worry about.

For the past three years, my students and I have been exploring these mathematical problems, and we have a large mass of results that will be reported in due course. Without going into further details, let me just say that all the previously known results in theory of irreversible processes can be derived easily from this algorithm. Dissipative effects such as viscosity, diffusion, heat conductivity are obtained by direct quadratures using (15), with no need for the forward integration and coarse-graining operations characteristic of previous treatments. For static transport coefficients we obtain formulas essentially equivalent to those of Kubo; we can exhibit certain ensembles for which Kubo's results, originally obtained by perturbation theory, are in fact exact.

Because we are freed from the need for time-smoothing and other coarse-graining operations, the theory is no longer restricted to the quasi-stationary, long-wavelength limit. It gives, with equal ease, general formulas for such things as ultrasonic attenuation and for nonlinear effects, such as those due to extremely large temperature or concentration gradients, for which previously no unambiguous theory existed. Because of these results, I feel quite confident that we are on the right track, and that this generalization will prove to be the final form of nonequilibrium statistical mechanics.

Let me close with a couple of philosophical remarks, relating this development to things I mentioned earlier in this talk. In seeking to extend a theory to new domains, some kind of philosophy about what the theory "means" is absolutely essential. The philosophy which led me to this generalization was, as already indicated, my conviction that the "subjective" theory of probability has been subjected to grossly unfair attacks from people who have never made the slightest attempt to examine its potentialities; and that if one does take the trouble to rise above ideology and study the facts, he will find that "subjective" probability is not only perfectly sound philosophically; it is a far more powerful tool for solving practical problems than the frequency theory. I am, moreover, not alone in thinking this, as those familiar with the rise of the "neo-Bayesian" school of thought in statistics are well aware.

Nevertheless, that philosophy of mine was only scaffolding, which served the purpose of telling me in what *specific* way the formalism of GIBBS was to be generalized. Once a philosophy has led to a definite, unambiguous mathematical formalism by which practical calculations may be carried out, then the issue is no longer one of philosophy; but of fact. The formalism either will or will not prove adequate in practice; and it will be judged, quite properly, not by the philosophy which led to it, but by the results which its gives. If you do not like my philosophy, but you find that the formalism, nevertheless, does give useful results, then I am quite sure that you will be able to invent some *other* philosophy by which that formalism can be justified! And, perhaps, that other philosophy will lead to still further generalizations and extensions, to which my own philosophy makes me blind. That is, after all, just the process by which all progress in theoretical physics has been made.

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