The Teachers' File

THERMODYNAMICS: WHAT ONE NEEDS TO KNOW

by Carl S. Helrich

Abstract. Thermodynamics is the foundation of many of the topics of interest in the religion-science dialogue. Here a nonmathematical outline of the principles of thermodynamics is presented, providing a historical and conceptually understandable development that can serve teachers from disciplines other than physics. The contributions of Gibbs to both classical and rational thermodynamics, emphasizing the importance of the ensemble in statistical mechanics, are discussed. The seminal ideas of Boltzmann on statistical mechanics are contrasted to those of Gibbs in a discussion of the microscopic interpretation of the second law. The role of information theory is discussed, and the modern ideas of Prigogine and nonequilibrium are outlined in some detail with further reference to the second law. Implications for our interaction with God are considered.

Keywords: ensemble; entropy; first law; information theory; nonequilibrium; second law; statistical mechanics.

There is little in the interaction of physics with religion and theology that does not involve thermodynamics, and particularly the second law. If we are interested in eschatological questions, then we should consider the fate of the universe from the point of view of physics. Whether the universe will reverse the present expansion and collapse or continue to expand depends on the total mass of the universe, and at present these questions are open (Coles 1998). These are not themselves thermodynamic questions. But if a collapse were to bring the universe back to a state that is essentially identical to any through which our present universe has already passed, there seem to be thermodynamic implications. If there were no collapse,

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there might be eschatological questions, for then the universe would apparently die from the increase in entropy.

Thermodynamics is of primary importance in biophysics. When we ask questions about the function of any biological system, such as the brain, we encounter thermodynamic principles. Indeed, the concept of top-down causality and our attempts to understand what Arthur Peacocke has called the "causal joint," for the connection between us and God, must be rooted in thermodynamics (Peacocke 1993, 151–60). Of course questions regarding prebiotic evolution and the primordial emergence of life must be thermodynamically based and formulated (Eigen 1971).

My own experience in the religion and science dialogue is that there is widespread interest in thermodynamics among colleagues from outside of the sciences as well as among those of us who engage the questions as scientists. Thus, I believe a discussion paper on basic thermodynamics may serve the community well. With appropriate acknowledgment of my own limitations in both physics and theology, I nevertheless undertake an explanation of the basics of this most subtle of sciences.

What follows is a verbal description of thermodynamics that alludes to its mathematical structure and attempts to provide a comprehension of and appreciation for that structure. I begin with an outline of the origin of the two principal laws of thermodynamics in their classical form. I then consider the basis of statistical mechanics and the rational foundation of thermodynamics. Information theory has become part of many discussions of statistical mechanics. I try to provide some indication of where it fits in. I then outline some of the more recent developments that are of particular importance in considerations of nonequilibrium. I conclude with some consideration of the interaction of God with us, which is obviously of primary importance in our discipline.

I shall not consider the implications of quantum mechanics directly. This neglect should not, however, be construed to imply that thermodynamics is separate from quantum mechanics. The intimate relationship of quantum mechanics and thermodynamics is considered in marvelous detail by Martin Klein in his analysis of the ideas of Max Planck and Albert Einstein that contributed to the first discoveries of the quantum (Klein 1977). The methods of Josiah Willard Gibbs are also independent of the microscopic (atomic) model chosen and are not limited to Newtonian mechanics. The implications of the Fermi-Dirac and Bose-Einstein statistics, which must be considered in the thermodynamics of quantum systems, as they relate to our discipline, are discussed by Robert John Russell (1988).

THE FIRST AND SECOND LAWS

Thermodynamics considers individual systems. These are identifiable parts of the universe for which we can define properties such as temperature, pressure, and mass. If the system is open, we also must have some knowledge of the fluxes of mass, energy, and perhaps momentum crossing the boundaries of the system. These constitute the boundary conditions, which must be carefully considered. To define all these properties of the system we use the concept of *equilibrium*. To define such things as pressure and temperature we must be able to speak of a constancy of the property over certain dimensions and times. If the conditions vary between rather large limits over very short times, a measurement is impossible. The energies of individual molecules may perhaps have meaning, but not, for example, the temperature. Temperature is a property defined by the concept of thermal equilibrium between systems, with equilibrium defined as a condition that exists if the system properties do not change over a sufficiently long time. So we take the usual beginning point of the thermodynamicist and claim interest initially only in equilibrium systems. If we accept the definition of temperature as a system property that is equal in measured value for systems in thermodynamic equilibrium (which is actually a law of thermodynamics), then we have only two laws that need concern us: the first and the second laws. We can safely neglect the third law (Nernst's Hypothesis; cf. Wilson 1960) as of no consequence for our discussion.

The first law is simply conservation of energy. James Prescott Joule justified it experimentally in the 1840s (cf. Park 1988, 255–57). Hermann von Helmholtz provided a philosophical basis in his classic *Über die Erhaltung der Kraft* (1847). The law equates the thermodynamic work done on an isolated (adiabatically enclosed) system to the increase in internal energy of the system. Thermodynamic work is any action that can be reduced to the raising or lowering of a weight in a gravitational field, providing a definition of internal energy in measurable terms (the positions of weights). Heat transfer is defined in terms of differences in the work required to produce the same change in state in isolated and nonisolated (diathermally enclosed) systems. The result is the familiar form of the first law, in which heat transfer is equated to the sum of internal energy change and work done. The care in definition of terms should be noted; this is a characteristic of the science and the basis of its authority. We suspect that the first law is universally valid. The Compton Effect provides evidence of the validity of energy conservation at a submicroscopic level (Pais 1991, 234). But we have no data for the entire universe.

The second law places a limitation on the behavior of a system not contained in the first law. The system can develop (in time) only in a certain fashion. Here we first encounter the concepts of nonequilibrium and time irreversibility, even though we are speaking of systems in (initial and final) states of equilibrium. The second law actually has two rather clear, simple, and equivalent statements regarding possible processes, from which Rudolf Clausius' formulation of the entropy principle follows. Both of these contain the wording, "No cyclic process exists in nature whose sole effect is. . . ." Important here are the words "cyclic" and "sole," and the processes denied are the transfer of heat from a cold body to a hot body and the complete conversion of heat, extracted from a single temperature reservoir, into an equivalent amount of work. It is almost axiomatic that the more obvious the verbal expression, the more difficult the equivalent mathematical formulation. This is particularly evident in the development of the second law. A. H. Wilson's treatment is the best I know of and the one to which I constantly return in my own teaching (Wilson 1960). The treatment is not mathematically difficult but is impeccably rigorous in its logic, preserving the style that characterizes the science. Wilson carefully develops the mathematical formulation based clearly on the verbal statements, producing the celebrated Carnot Theorem as an intermediate and necessary step and obtaining a definition of thermodynamic (absolute) temperature as an incidental. The final mathematical formulation of the second law is the Clausius Inequality, which demands that the sum (integral) around a cyclic process of the quotient of heat transferred (reversibly) to the thermodynamic temperature be less than or equal to zero ($\oint \frac{\delta Qreversible}{T} \leq 0$). The quotient in the sum ($\frac{\delta Qreversible}{T}$) is the incremental change in the entropy (ϵντροπι, "evolution") for the system, *dS*. From the Clausius Inequality it can be shown that for any spontaneous process occurring in an isolated system the entropy must increase. This is the physically important content of the second law, which provides a direction for physical processes and, ultimately, for the directionality of time.

THE GIBBS FORMULATION

To make any more progress we must consider Josiah Willard Gibbs, the great American mathematical physicist of the nineteenth century. There is a peculiarity in the two laws outlined above. They each involve a heat transfer, which is not a proper mathematical function. Gibbs put the mathematical formulations of these two laws together, obtaining a single expression composed only of proper thermodynamic functions. What resulted was a beautiful and complete mathematical theory for the interaction of (bulk) matter with energy.

It is perhaps true that unless students have studied physics they will not appreciate the beauty and remarkable nature of a complete mathematical picture of something as extensive as the interaction of matter and energy. So it may be difficult to convey the idea that in the Gibbsian formulation of thermodynamics we have one of the grandest of structures. In it we have a complete picture based ultimately on simple and irrefutable principles constructed with the most rigorous of mathematics. Albert Einstein once claimed that thermodynamics, being thus based and constructed, was the science that would not change, regardless of subsequent discoveries. The Gibbs formulation is also completely independent of any microscopic picture of matter. The formulation holds whether atoms exist or not and, if they exist, whether the mechanics is classical or quantum. Thermodynamics is Newtonian in the philosophical sense that there are no hypotheses regarding things that cannot be measured. The laws are mathematical and based only on identifiable experiments.

STATISTICAL MECHANICS: THE RATIONAL FORMULATION OF THERMODYNAMICS

But this is in no sense the end. Gibbs was particularly interested in formulating thermodynamics as a branch of "rational mechanics." In the 1870s Ludwig Boltzmann in Vienna published a series of papers attempting to provide a microscopic (atomic) basis for the laws of thermodynamics (Boltzmann 1871, 1872, 1877). This was easy for the first law, because any microscopic picture of matter would certainly entail (conservative) interactions between the particles (atoms, molecules) and a final conservation of energy for the closed system. At issue was the fact that the second law provided a directionality to the natural processes occurring in matter. Boltzmann himself considered the nineteenth century to be the century of Charles Darwin, whose concept of evolution had shown that natural time had a directionality. Finding a microscopic basis for this directionality would be a crowning glory.

Boltzmann considered the simplest system: the ideal gas, consisting of particles so widely separated that the time spent by any particle (atom or molecule) in interaction with other particles was an insignificant part of the particle's lifetime. Most of the time the particles moved in straight lines. The problem was fraught with difficulties from the beginning. After Boltzmann's first paper was published, his friend Josef Loschmidt brought a fundamental problem to his attention: the basic equations of motion in the Newtonian (Lagrange/Hamiltonian) formulation were reversible in time, and it is a logical inconsistency to claim that an irreversibility will arise from these (Broda 1983). The impossibility of this step was later formulated rigorously by Henri Poincaré (outlined in Prigogine 1980, 157). Of course Boltzmann recognized that his microscopic formulation of the second law did not stand as a basic principle. Indeed the step that made the system irreversible was apparent. In his formulation Boltzmann had written an expression for the rate at which collisions occurred between the particles of the system. This was clearly a hypothesis and independent of the mechanics. This is the celebrated "collision number postulate" (*Stosszahlansatz*) of the Boltzmann picture (Ehrenfest and Ehrenfest [1912] 1990). Boltzmann saw that this could be interpreted as the probability of collision, and the entropy formulation then became a probabilistic rather than an exact formulation (Boltzmann 1877; Ehrenfest and Ehrenfest [1912] 1990; Chapman and Cowling 1970, 67–85, 407; Tolman 1962, 134–79).

But this did not reconcile the difficulties. Without any reference to a microscopic picture, thermodynamics stood as the most beautiful of formulations of the laws of physics. It was the epitome of exact science. This was the way things should be! Max Planck once claimed that the highest calling of the scientist was such an exact formulation (Klein 1977). So Boltzmann's claim that the second law, at least when formulated for a gas, is a statistical law was not greeted with enthusiasm. If this were true, then exact physics was at an end.

Of course there was then, and there still is, no compelling reason to anticipate that a simple probabilistic interpretation, which works for a gas, will work in a universal situation. But that is small comfort. Even if this result was an anomaly for a gaseous system, it must eventually be acknowledged as such.

Gibbs provided a different and more satisfying basis for the microscopic formulation of thermodynamics: the ensemble. The concept was very simple and should be understood if we hope to appreciate the second law in a rational formulation. Gibbs acknowledged from the outset the uncertainty regarding any molecular picture of matter. In the preface to his book *Elementary Principles in Statistical Mechanics*, Gibbs stated that

> In the present state of science, it seems hardly possible to frame a dynamic theory of molecular action which shall embrace the phenomena of thermodynamics, of radiation, and of the electrical manifestations which accompany the union of atoms. . . . Certainly one is building on an insecure foundation, who rests his work on hypotheses concerning the constitution of matter. Difficulties of this kind have deterred the author from attempting to explain the mysteries of nature, and have forced him to be contented with the more modest aim of deducing some of the more obvious propositions relating to the statistical branch of mechanics.

This is followed by the statement known to every student of statistical mechanics and kinetic theory:

> Here there can be no mistake in regard to the agreement of the hypotheses with the facts of nature, for nothing is assumed in that respect. The only error into which one can fall, is the want of agreement between the premises and conclusions, and this, with care, one may hope, in the main, to avoid. (Gibbs [1902] 1960, x)

With explicit modesty and broad strokes, Gibbs here announced his intent to the world. If considered soberly, however, it is clear that there is a bold sweeping nature to this program. If Gibbs was not going to make hypotheses regarding the microscopic state of matter, on what was he going to base his new approach? The answer is the ensemble, defined as that collection of systems having the same macroscopic (thermodynamic) properties. That is, the ensemble consists of those systems which cannot be distinguished from one another by any measurement. The reason for taking this approach is fundamental: we must base our theory only on what we can claim to know on the basis of well-defined and understood measurements. This is the same principle followed by Niels Bohr in the development of the quantum theory (Bohr [1927] 1987; [1949] 1987; Messiah 1966, 149–59; Dirac 1958, 1–14; Pais 1991, 295–320). By basing his statistical mechanics on such a firm foundation, Gibbs essentially guaranteed that his formulation would be followed in the future. For Gibbs the measured quantities were temperature, pressure, and numbers of particles (densities and volumes).

It is clear that for any conceivable picture of the structure of matter there are an infinite number of possible arrangements of the molecules which could result in these macroscopic properties. There is also no way to determine which of these systems is present in our laboratory at any instant, even if our microscopic picture has an increased validity. Gibbs's idea was to treat the systems themselves statistically, rather than the molecules. Any claim that we make about the configuration of the molecules constituting the system under consideration must be deduced from the distribution of the systems of the ensemble. For systems in equilibrium, which were the only ones considered by either Boltzmann or (explicitly) Gibbs, this distribution depends on the system energy rather than on the arrangement of the molecules. Of course the system energy may depend on whether the molecules are freely moving, as in a gaseous state, or bound in a crystal, as in a solid state. But neither of these provides the possibility of speaking about single molecules or even groups of molecules.

The Gibbsian formulation of the entropy (at equilibrium) bears a satisfying resemblance to that of Boltzmann, as it must. But for the reasons outlined above, the formulation provides no interpretation in terms of the probability of location of groups of molecules in a system. Mathematically the entropy is, in the Gibbsian formulation, given by the average over all systems in the ensemble of the logarithm of the probability density in phase space ($S = -k_B \int_\Omega d\Omega P(\Omega) \ln (P(\Omega)) = -k_B < \ln (P) >$). This is not as easily interpreted as the probability of molecular configurations provided in the Boltzmann picture. But, as Gibbs told us at the outset, the microscopic picture is not present.

INFORMATION THEORY AND THERMODYNAMICS

Information theory has been of considerable interest to both the physics and the theology communities because of the promise this theory seems to hold for a deeper understanding of some of the difficult issues we face. Claude Shannon, at Bell Telephone Laboratories, provided a mathematical formulation of information and uncertainty for systems (Shannon 1948). Not surprisingly, this bears a striking resemblance to Gibbsian statistical mechanics. Indeed, if one maximizes one's uncertainty about a physical (thermodynamic) system one obtains the same basic description of the system as that obtained from Gibbsian statistical mechanics. This fact produced considerable interest and activity (cf. Jaynes 1957a, b, 1963; Mead and Papanicoulau 1984), the results of which have contributed to the tools of such disciplines as condensed matter physics (Fedders and Carlsson 1985). Perhaps the most complete mathematical formulation of this was given by the Russian mathematician A. I. Khinchin in his book *Mathematical Foundations of Information Theory* (1957). The treatment, although not itself simple, is based on a simplified system called Markovian. Such a system is without memory. A model of such a system is the late-night path followed by an intoxicated person, the so-called random walk. In this situation the next step is determined only by the present location of the person and bears no relationship to the previous steps. This situation is realized physically by a system in which the molecules are uncorrelated, that is, the condition in which no molecule bears any relation to any other molecule in the system. Although such a condition is approximated by an equilibrium gas, it is obviously an unrealistic hypothesis for any nonequilibrium system. That is, regardless of any intuitive sense we may have, a rigorous link between information theory and what may be called entropy for nonequilibrium systems has not yet been established.

NONEQUILIBRIUM THERMODYNAMICS

As indicated above, in the classical thermodynamic description only equilibrium states are considered. The requirement that entropy increase in a spontaneous process ocurring in a closed system is a requirement about the relationship between an initial state and a final state obtained from that initial state by a nonequilibrium process. In the Clausius Inequality nothing is said about the details of the process itself. By the middle of the twentieth century, with the formulations of Clausius, Maxwell, Boltzmann, and Gibbs, we still had no method to deal with the actual nonequilibrium processes that may interest us. Ilya Prigogine provided us with the formulation we needed for the nonequilibrium process (Prigogine 1961; 1980; 1997).

In a sense Prigogine's formulation, like many well-framed theories, appears as a rather simple and logical step. Since the work of Joseph Lagrange on analytical mechanics (Lagrange 1788), physicists have almost expected formulations of the laws of the universe to be presented in the form of a variational principle. That is, something will be a minimum (or at least an extremum) for a possible physical process. This is the idea in the Prigogine formulation of the second law for a nonequilibrium process.

If we have a general (open) system undergoing a nonequilibrium process, we may expect that the entropy of the system will be affected by an interaction with the outside. We may then speak of an entropy flux into (or out of) the system through the boundaries. This entropy flux is rather well known and is often employed to describe nonequilibrium. Bernd-Olaf Küppers has recently pursued possible consequences of boundary conditions in an interesting fashion (1997). The flux term, however, is not the only term producing a change in the system entropy. Clearly there must be the possibility that something is occurring in the system itself that drives the process. This is the entropy production term, the central term in the formulation. The centrality of this term should also be clear. The Clausius formulation dealt with a closed system in which a spontaneous process was to occur. According to Prigogine, the rate of entropy production within the system is a minimum for any physically realizable process. A very clear and simple description of this is provided by Prigogine in his little book *Thermodynamics of Irreversible Processes* (1961).

Once we have a formulation of the second law for nonequilibrium processes, we must subject it to the tests of experiment. Perhaps the most obvious challenge for the theory is the description of a chemical reaction. The chemical reaction is the most logical and certainly the most interesting of spontaneous processes that can occur in a closed system. Indeed, the description of entropy production rates and the corresponding requirements placed on the properties of the chemically reacting system do provide us with an understanding of chemical affinities and chemical rates (Prigogine 1961). For the student of evolution, the questions of primordial self-replication are of interest. It is of particular interest to ask whether or not certain (relatively simple) self-replicating systems can be expected to form structures. Although to leap from there to the claim that we have a window on the fundamentals of biological origins is unwarranted, it would be satisfying at least to say that we can show that it is not an impossible occurrence (Eigen 1971). To make this step we must consider systems far from equilibrium. There the coupling of internal entropy production rates and entropy flux across the system boundary, with the principle of minimum entropy production, has been shown to produce stable states with spatial or time structures. This is very satisfying and is certainly not to be anticipated from a claim that the second law results in disorder. Of course structural formation is at the heart of all of biology and produces the complexification that is often discussed (Ashbrook and Albright 1997).

THE IRREVERSIBILITY OF TIME

Time is one of the greatest mysteries that has confronted us. There is, for example, the classic remark of Augustine of Hippo that he understood time until he was asked to explain it, at which point he had no idea what time is (*Confessions* 11.14). There is also Einstein's contention that "for us convinced physicists, the distinction between past, present and future is an illusion, although a persistent one" (quoted in Prigogine 1997, 165). Einstein was referring to the fact that in our laws, whether those of classical or quantum physics, time is reversible. There is mathematically no difference between past and future. One of the great difficulties encountered by

Boltzmann, as indicated above, was that the laws of mechanics are reversible. It is possible, however, to write a description of a single system that is completely reversible in time and to then perform an ensemble average of this description resulting in a description that is time irreversible. This is done by Yurii L. Klimontovich (1986, 409–25), although he has no reflective discussion. This most intriguing result shows that something in the ensemble averaging of the single system description has, rather mysteriously, made the time irreversible. While this certainly tells us something about time and the statistical description of nature, it is not at all clear what that something is. We cannot claim an understanding simply by pointing to a fact, regardless of how well known that fact may be.

Prigogine has suggested that we give up our description of a many-body system based on the trajectories of individual atoms or molecules and instead base our most fundamental description on distributions (Prigogine 1997). The reason for this approach is more fundamental than the intriguing observation I mentioned regarding ensemble averaging. It is true that Newtonian (Lagrange/Hamiltonian) mechanics is incorrect if we demand that a theory have a universal applicability. But although we know that the earth is spherical, we do not demand spherical maps for navigation over short distances. In this spirit we have no need to abandon classical mechanics when treating motions of molecules separated (for the greatest part of their lifetimes) by large distances. Nevertheless, even in classical mechanics we encounter certain difficulties that cannot be swept under the rug. The mathematical problem of interacting bodies cannot, in general, be solved. This was shown in 1889 by Henri Poincaré (1889). Because we usually use Newtonian mechanics to obtain practical (approximate) descriptions of situations, this fact is not normally considered to be of importance. However, we cannot then take the subsequent step of claiming that the system of a number of interacting bodies is still, in principle, describable by Newtonian mechanics. The reasoning becomes circular. Newtonian mechanics provides a complete description of one- and two-body systems. We also know, for example, that a system of multiple planets exists. We can write the equations for this multiple-body system and claim that with sufficient computing power we can, to some approximation, solve the system. However, even though we may solve it to our practical satisfaction, none of this says that we have an exact solution to the original set of equations. Poincaré has shown that the solution does not exist.

While this is of no practical interest, even in much of astrophysics, it is of great importance if we are going to make absolute claims based on what we believe to be reality. Realizing this difficulty, Prigogine began his description at the level of the distribution (function) rather than at the level of the particle trajectory. This is, of course, completely consistent with the ideas of Gibbs, who claimed that all we can logically discuss is the ensemble. One result of this work has been the demonstration that the thermodynamic and (particle) trajectory descriptions are complementary in a sense similar to the complementarity discovered by Heisenberg for the momentum and position of a single particle. It is in the thermodynamic description that we have an irreversible time.

The implications of this are important for an understanding of thermodynamics, which was our initial objective. This discovery raises our comprehension of the thermodynamic description of the universe to a different level. It is no longer sensible to try to understand thermodynamic behavior on the basis of the motion of individual molecules. The microscopic description of the second law is not to be found by postulating particlelike molecules and then imposing a statistics on them. The answer is to be found in the thermodynamic description itself. That is, we must look at the distribution function description rather than an imagined particle description. This becomes critical when we consider precisely those systems that interest us most: biological and cosmological systems. The difficulties pointed to by Poincaré arise for systems that interact continuously, such as we encounter in biology and cosmology, and particularly as we project our thoughts back toward the beginning of the universe.

The result is not the understanding we may have sought. That understanding does not exist. But we encounter greater implications than we had anticipated. We are familiar with the epistemological and perhaps ontological limits imposed by the Heisenberg indeterminacy principle (Bohr [1927] 1987; Heisenberg 1930). William Pollard and Robert Russell invoked this in their work on understanding what Arthur Peacocke called the "causal joint" linking us to God (Pollard 1958; Russell 1988; Peacocke 1993). The issue, of course, is not to point to an actual mechanism but to understand the freedom that exists within the physical system as we comprehend it (at this time). Considering the distribution rather than the particle as fundamental broadens the epistemological limit beyond that of Heisenberg.

DISCUSSION

I have presented here a basic outline of the principles of thermodynamics in a nonmathematical form. Of course, for the equilibrium (classical) case there is a beautiful mathematical formalism. But bringing that to the forefront in this discussion would not have served my purpose here. The mathematically inclined reader can find the formalism in the references cited. It is my sincere hope that even those readers who are familiar with the mathematics and the physics will benefit from the description of the ideas. The thread of the argument is rigorous.

Most of the discussion has concentrated on the second law, because I consider this to be the most important for topics of particular interest in discussions of the interaction of science and religion. In the presentation I have distinguished between the theory of Boltzmann, which provided our original understanding of the microscopic picture of the second law, and the modern theory of Prigogine. It is to this latter theory that we must turn if we are to discuss nonequilibrium. As I have pointed out, claims that the second law is a statement regarding disorder in a system are based on Boltzmann's interpretation and are limited to gas systems at or near equilibrium. To approach nonequilibrium we must take two new steps. We must use the ensemble theory of Gibbs, and we must take the distribution function, rather than the individual molecules, to be primary. This does not mean that we deny the atomic picture of matter. But it does mean that we must accept the conclusion that no understanding of entropy can be formulated in terms of individual molecular states (Prigogine 1997).

This should not come as a surprise if we acknowledge that the second law is the only law in physics that contains an irreversibility. Here we must seek a scientific understanding of time directionality. As theologians, philosophers, or scientists, we realize the problems involved whenever we attempt to understand time. Indeed, we should be very skeptical of any simple explanation of time irreversibility.

On reflection, it seems that the complementarity Prigogine discovered between the particulate and thermodynamic descriptions is of considerable importance in any attempt to understand God's interaction with the world and with us specifically. There is, of course, a correspondence to the indeterminacy principle of Heisenberg which still allows us to speak in terms of particles as long as we relinquish the ability to measure the initial conditions. But because of the intimate link between the Heisenberg principle and measurement, we must, if we rely on the Heisenberg principle to attempt an understanding of God's interaction with us, at some point encounter the problem of measurement under circumstances in which no human observer can possibly play a role. We presently have no completely satisfactory resolution of this problem. At the same time, it is possible that God does not interact with us only at a single particle level. One fruitful approach may be to consider the ensemble as the critical ingredient when attempting to understand our relation to God. Even though we now have a very good picture of the particulate nature of matter and can modify biological molecules with site-directed mutagenesis, we must still invoke the ensemble and the methods of Gibbs if we want to understand the dynamics of biological systems. Any understanding we attain of the dynamical behavior of the human brain in particular will be in the context of the ensemble.

A detailed comprehension of our interaction with God, however, is not our goal. To claim such an understanding would be to deny a transcendence. It is unreasonable to either seek or anticipate such a comprehension. A reasonable goal might be a realization of the epistemological limitations inherent in the science itself as that impacts our understanding of our interaction with God. We have indicated here that the epistemological limit imposed by the ideas of Prigogine exceeds that of Heisenberg. It is also important to realize that the most fundamental subject for study in biology, because of the limitations imposed by the data, is the ensemble, not the individual molecule. The scientific implication of this is that reductionism grounded only in a molecular picture will (probably) not be a fruitful basis for understanding life. Biology is inherently irreversible, and the directionality of time is presently comprehended only at the ensemble level. This seems important theologically as well as scientifically. The epistemological limit is met before the point at which we can comprehend life. This provides a scientific basis for transcendence in the very definition of life itself.

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