FUSION ENERGY FOUNDATION

PLASMA AND ORDER IN THE UNIVERSE

$1.50

Newsletter

September 1976

Plasma and Order in the Universe

EDITORIAL

STATISTICAL MECHANICS AND PLASMAS

FUSION NEWS

ABOUT THE COVER: Ludwig Boltzmann (1844-1906) and his theory of entropic mechanics are confronted with growing evidence that many physical processes spontaneously proceed to states of greater order rather than decaying as the Second Law of Thermodynamics demands. Here a series of three computer-generated solutions to equations describing a two-dimensional fluid show such a progression to greater order. This is the first publication of these computer solutions which were done by F. Tappert and D. Hardin in 1971. More computer solutions appear on page 22.

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Fusion Energy Foundation

Newsletter

Vol. II, No. 2
September 1976

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EDITORIAL

Fusion and the Second Law of Thermodynamics

The Fusion Energy Foundation has made the point many times that the function of plasma-fusion research is twofold: (1) to produce as quickly as possible economical (high beta) fusion reactors to meet gross energy needs in a rapidly developing world and (2) to push up to and beyond the frontiers of physics, opening up whole new domains of plasma-based technology as well as achieving fundamental breakthroughs in our understanding of the lawful ordering of the physical universe.

In meeting both these objectives, we have also pointed out that it will become increasingly important first to take into account and then to theoretically and experimentally master the tendency of high energy plasmas to form well-ordered micro and macroscopic structures. These structures unquestionably are crucial in determining or mediating plasma behavior, whether in terms of diffusion of particles or of global geometry and magnetohydrodynamic stability.

As a practical matter, it has already been demonstrated by researchers in the United States, the Soviet Union, Australia and elsewhere that the nonlinear interactions associated with well-defined plasma configurations are dominant at the energy input levels required for fusion in the relativistic electron beam and laser beam approaches to inertial confinement. Incredibly, as will be reported in the next issue, Energy Research and Development Administration officials of the U.S. are threatening to downgrade these rapidly developing lines of research in favor of the ion beam approach — ostensibly because this approach is expected to avoid the nonthermal effects of the electron beam and laser beam.

It is difficult to determine whether this latest display of ERDA logic is more outrageous if motivated by ignorance or if by deceit. U.S. officials are well aware both that the Soviets intend to mount a brute force research effort on the e-beam and laser beam fronts and that they have left a clear opening for scientific and industrial collaboration with the United States. The same officials most certainly know that the first e-beam-produced fusion neutrons were driven by "messy" nonlinear beam-target interactions. Furthermore, U.S. scientists, including some affiliated with ERDA, have testified before Congress that the laser fusion timetable could be moved up by six years if it were adequately funded. These well-known facts certainly provide at least circumstantial evidence that ERDA's sudden infatuation with the ion beam — an approach which should be developed in its own right — derives not so much from the presumed relative merits of ion beam research as from ERDA's desire to hamper fusion research. After all, ERDA makes no bones about its commitment to coal gasification über alles.

Beyond Thermonuclear Thinking

The situation would be bad enough if ERDA's administrators were simply incompetent. It is well past the time for the entire scientific community to move in conception and in practice beyond the grip of **thermonuclear** thinking.

With this necessity in mind, the current issue of the Newsletter features two special essays that definitively remove the basis for reliance on plasma "equilibrium" criteria for fusion research. In doing so, one must go to the heart of the matter — the presumed universal validity of the Second Law of Thermodynamics and its statistical basis, a subject first thoroughly studied in the late 19th century by physicist Ludwig Boltzmann.

Boltzmann's contribution to physics lies in his assertion and partial demonstration that the Second Law of Thermodynamics (the tendency for increase of entropy) is not an independent axiom vis-a-vis the rest of the structure of physics, including the First Law. As he correctly grasped, the basis for this result lies in the fundamental property of discreteness, and the interactions which are codeterminate aspects of discreteness.

In fact Boltzmann demonstrated that the symmetry properties of interactions associated with the then-known laws of physics were responsible for distributing and redistributing energy among physical modalities resulting in the Second Law, or increase of entropy. The result of the Boltzmann H-Theorem, however, provides the following, not previously understood inference: The conclusive disproof of the universal validity of the Second Law through any one crucial counterexample simultaneously demonstrates — on necessary epistemological grounds — that linear interactions and simple energy transformation properties are not fundamental, but rather are subsumed aspects of higher-order, nonlinear processes.

Boltzmann's problem was that he never left the linear world, although he recognized that the general theorems which he sought to prove were crucially dependent on the quality of interaction among atoms. His doggedness on this point marked his work for attack by the two schools that attempted to wreck or coopt Boltzmann: the Energeticists and the so-called pure statistical mechanicians.

The Energeticists, led by Ernst Mach and Wilhelm Ostwald, hysterically denied the reality of discreteness. Instead they posited that knowledge ended at the phenomenological description of transformation of energy into qualitatively different types. While subsequent clear-cut demonstrations of atomism plucked the feathers from this bit of nonsense, its effects linger on in the bowdlerized systems and paradigm theories of the so-called holists.

Beginning most prominently with Gibbs, however, **formal** statistical mechanics became the hegemonic perversion of Boltzmann's work. Gibbs and all subsequent formalists made axiomatic the assumption that physical processes would proceed, inevitably, toward the regions of highest state density in phase-space. Therefore they posited, one need only develop a calculus of the phase-space(s). Further, this result need merely be computed for an "ensemble," instead of a real physical system. This outlook eliminated causality before quantum mechanics even came on the scene. In fact the Gibbs approach requires only secondary adjustments in order to incorporate the differences of quantum mechanical states from classical ones.

Quantum phenomena, however, cannot be fit into the statistical mechanical framework when one considers the problem with any epistemological rigor. How else can the very existence of particles and discontinuous energy distributions in general be understood if not from the standpoint of nonlinear interactions? Quantum phenomena force statistical mechanics to give up **equi**-partition, as in the Planck solution of the black-body radiation problem.

Physics and Negentropy

Boltzmann did not allow himself the tools to fight back effectively against his opponents or to carry the problem further. In denouncing Hegel and ignoring Cantor, Boltzmann foreclosed the possibility of grasping that all physical processes, in whatever apparent combinations of fields and particles, are actually of transfinite order. The simple neutral gas that obeys the Second Law is merely a limiting case of the characteristically nonlinear behavior that emerges when sufficient energy density is attained in the plasma state.

Here we come to a clinical example of the psychological role of ideology in hampering science. Dr. Bardwell's article in this issue is the first comprehensive historical review of postwar theoretical attempts to account for self-subsisting fluid and plasma structures — and their implications for plasma thermodynamics — as well as of experimental observations. As this groundbreaking article demonstrates there have been adequate — but largely ignored — theoretical and experimental bases at hand for two to three decades not only to grasp what constitutes fundamental plasma behavior, but to understand how and why that overturns the Second Law — on its own terrain of physics. The article also clears the way to locate where fundamental breakthroughs will next come in physics: in reformulating the properties of physical interaction and associated geometries free from the prejudice of reduction to linearity.

Even on "this side" of future breakthroughs, the development of fusion will be greatly strengthened by empirically following out the potentialities of devices based on nonlinear configurations. The failure to fund various, relatively inexpensive plasma focus and pinch experiments designed to explore such possibilities is thus criminally stupid.

Fortunately, the fusion community is astir with increasing recognition that it is important to take advantage of "natural" plasma behavior in all sorts of devices. This perspective will prove to be crucial not only for ultimately achieving high-efficiency fusion reactors. In addition a fusion-based economy will require a more advanced outlook than a Boltzmannian synthesis of Lagrangian mechanics and thermodynamics; it will require a physics consistent with the basic invariant of the physical universe — **negentropy**.

Linearity and Entropy

Ludwig Boltzmann and The Second Law of Thermodynamics

Dr. Morris Levitt

INTRODUCTION

A proper evaluation of the contribution to physics, as well as the limitations, of the theoretical work of the Austro-German physicist Ludwig Boltzmann (1844-1906) resolves the larger historical problem of thermodynamics. The chief paradox of thermodynamics is that although it both derived from and helped to stimulate the 19th century industrial revolution, its results are totally incompatible with the driving force of that revolution — the Idea of Progress. Further, although thermodynamic theory is appropriate for the design of engines and chemical processing plants, it is patently inapplicable to the most extensive part of the inanimate domain — plasma.

Poorly educated scientists have been able to ignore these contradictions throughout most of the 20th century by latching on to a rationale concocted by their more ideological colleagues — the statistical view of physics. This view asserts that at the most fundamental (microscopic) level of interaction, there is no longer causality, only pseudo-laws of probability describing what goes on. Laws at the everyday (macroscopic) level, this view holds, are simply an averaging out of the lower-level chaos. If there is orderly development going on somewhere, proponents of this view explain it away as an accident fortunately balanced by yet greater chaos somewhere else.

It has been ignorantly or slanderously asserted that this statistical view is directly derived from Boltzmann. The truth is that Boltzmann was no such indifferentist; in fact, he was a troubled agnostic trying to hold on to the core of Enlightenment thought. Boltzmann's entire scientific career was motivated by the task he set for himself: to demonstrate a **causal** basis — coherent with established or possibly new laws of physics — for thermodynamic phenomena.

Boltzmann's success in developing methods to partially achieve this objective and his identification of problems lying outside the scope of his work created the conditions for resolving the paradox of thermodynamics from a more advanced standpoint.

The major findings of thermodynamics and Boltzmann's efforts to base those findings on an atomic description of matter are briefly summarized here as a guide to understanding the article which follows.

Thermodynamics: Bookkeeping

Thermodynamics reflects its peculiarly hybrid origins. On the one hand it was made possible by the Industrial Revolution's impetus for the systematic application of science in industry, the "scientization" of technology by post-French Revolution institutions such as the Ecole Polytechnique, and by scientists like Sadi Carnot. On the other hand thermodynamics represents a great anti-geometric flattening of the laws of physics into ultrascalar or analytic form, under mid-century English and Austro-German auspices exemplified by the conceptually barren Lord Kelvin.

The two laws of classical thermodynamics are essentially bookkeeping statements about the conversion of energy into various forms. The laws concern balancing the books, but ignore the essential question of what processes underlie such energy transformations. The development of the concept of entropy from the Second Law, nevertheless, is ingenious and sets the stage for Boltzmann's work.

Consider the Second Law in the form of its prohibition of any process in which the only result is the transfer of heat from a cooler to a hotter body. One of the consequences of this form of the Second Law is that it is possible to define a new characteristic of a thermodynamic system, like a gas, called the entropy (and denoted by the symbol S). The relationship between the above statement of the Second Law and the derivation of the additional thermodynamic property, entropy, is seen in the following result: When the system is absorbing heat or performing work, a small change in the system produces a change in the entropy that must be greater than or equal to the small amount of heat absorbed, divided by the temperature. If the system is isolated, and no heat can be exchanged, then in undergoing any change, the entropy of the system must stay the same or increase. (A clear development of how the definition and properties of entropy may be derived from more general statements of the Second Law can be found in Enrico Fermi's monograph on thermodynamics.) These results indicate that once the entropy is maximized in a system, no more meaningful changes — energy exchanges or conversion to work — can take place within it.

Boltzmann undertook to explain these results using the preliminary program that follows:

(1) Show that for every macroscopic thermodynamic state — characterized by properties such as temperature, volume, and pressure — there corresponds a large number of microscopic states representing different, but equivalent, configurations of the atoms.

(2) Develop a means of computing the number of such microstates for each thermodynamic state, especially for the equilibrium thermodynamic state that has the highest number of equivalent internal configurations.

(3) Show that the entropy of a thermodynamic state is a simple function of the number of microstates with which it is consistent.

Having developed points one through three, Boltzmann (and in independent work on the first two points, Maxwell) had at his disposal two results for further elaboration. The first was that there was a definite distribution of atoms among velocity states, the Maxwell-Boltzmann distribution, obtained when the maximization of point two was performed. The second was the strong hint that the law of entropy increase was related simply to the relative probabilities (that is, number) of microstates per macrostate.

Boltzmann's Real Contribution

If Boltzmann had left the situation there, he should be grouped with those contemporary scientists who imagine that the world is doomed to ultimate decay because of a random or probabilistic jumping among a priori equivalent and equally probable microstates. Ernst Mach's grudging reference to Boltzmann as "the last pillar" was not, however, undeserved. Boltzmann undertook to show that the thermodynamic transformations of a system that take place in accordance with the Second Law were due to overall population shifts among microstates caused by atomic interaction. From that standpoint Boltzmann discovered that the increase and maximization of entropy corresponded to statistically computed results, not as some fundamental principle but because the statistical results happen to be mathematically equivalent to the way in which simple atomic interactions populate the microstates.

This point, Boltzmann's central point, has been largely obscured by Boltzmann's contemporaries as well as by the current scientific community. It is exactly this discovery, however, that merits placing Boltzmann in the ranks of scientists like Planck, Einstein, and Schrödinger. Although they were unable to find satisfactory solutions, these men insisted in the face of overwhelmingly contrary opinion that the central problem in physics was to discover the basis for lawfulness and coherence.

We join that issue here, taking Boltzmann's results and insights as our point of departure.

Having made his major scientific contribution in the 1870s, Boltzmann shuttled between numerous Austrian and German universities until the 1880s. The latter part of his life was spent at the University of Vienna, where he attempted to defend and popularize his views against increasingly strong reaction from pure thermodynamicists. Acceptance of Boltzmann's work did not occur until after 1905, when there was widespread acknowledgment of the reality of atoms and quanta, although even then this acceptance took a thoroughly formalized and dogmatic probabilistic form.

For Boltzmann, the recognition came too late. In 1906, after a euphoric visit to the United States the previous year, Boltzmann sank into despair and committed suicide.

I. The Question of Interaction

Soviet theoretical physicist V.N. Tsytovich makes the following point in comparing the nonequivalent cases of (un-ionized) liquid and plasma turbulence in his 1972 monograph on *plasma turbulence*.

> The existence of charged particles in a plasma is very essential for the nature of the nonlinear energy transfer. In liquids the eddies interact only with one another, or, in other words, the whole energy of the turbulent motion is conserved and is only transformed from the biggest eddy to the lowest, or from smaller to larger K (wave number — ed.). The entropy in this kind of process increases because the phase volume that is proportional to the volume in K-space increases.
>
> In the presence of charged particles and due to the electromagnetic nature of plasma oscillations there exists the possibility of interactions and energy exchange between the turbulent motion and the particles. One of the most important of such processes is the individual scattering of oscillations by particles ... In such a process the particles can gain energy or, in other words, are heated. The particle entropy increases because of this heating, and to compensate this increase the phase volume occupied by the waves can decrease. In other words, the energy of turbulent motions can be transformed from higher to lower K....

As Tsytovich points out elsewhere, the simplest way of locating the difference between plasma and liquid behavior is by noting that while a liquid is essentially incompressable (due to short-range forces), plasma is elastic and strongly self-interactive. The latter point is also made in the definition of plasma characteristics by Soviet Tokamak pioneer Lev Artsimovich, now deceased, in his elementary text on plasma physics. There is a characteristic dimension in a plasma, the Debye length (DL), over which electrical restoring forces act to maintain electrical neutrality — despite individual particle ionization — within any volume element characterized by the DL.

In plasma, therefore, there is a much richer range of possibilities of *collective motion and interaction* among collective modes as well as among collective modes and individual ions. Most important, these interactions are generally nonlinear. The term nonlinear is usually taken by physicists to indicate the presence of interactive terms that produce coupling (mixing) of otherwise independent primary modes of a system. But its more general and relevant definition implies a truly self-reflexive interaction which determines modes that mediate their own development. In short, when nonlinear effects are dominant rather than marginal, there cannot be, and it makes no sense to speak of, fundamental modes.

Plasma behavior, therefore, is more complex phenomenologically than gases and liquids, and is also more highly ordered because of a more explicitly coherent relationship between micro and macrostructure. The example cited by Tsytovich of a collective mode that becomes more well defined as it energizes ions illustrates this point. Aside from their crucial relevance to the solution of the controlled fusion problem, these plasma characteristics are also relevant to the specific physical conditions in which the supposed universality of the Laws of Thermodynamics and statistical mechanics breaks down.

"We state at the outset one of the basic findings in this essay — that the First and Second laws are not independent statements of physical reality....Our basic thesis is,..., that manifest violations of the Second Law...and the manifest incoherence of continuum vis-à-vis quantum physics are directly attributable to a higher-order process of interaction as the proper characteristic of what is normally passed off as energy."

Classical thermodynamics and its statistical mechanical interpretation depend crucially on a restricted conception of physical interaction and a related, limited conception of what types of physical states are coupled by such interactions. Whether in classical mechanics as developed in its most generalized Hamiltonian form in the 19th century or in the quantum mechanics developed in the mid-1920s, it is assumed that the physical system of interest — defined by what is in it and how it is bounded or delimited (or in the classical case, constrained) — is either in or passing through a physical state that is characterized by the energy (the Hamiltonian, more generally).(1)

By examining this framework, which is usually thought to be irreducible, it is possible to demonstrate that the Second Law does not merely follow as a discovered statistical average, but that it is the result of a vicious circular argument that overlooks both obvious empirical evidence of macroscopic

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(1) In the classical case, the range of motions allowed the elements of the system by external constraints determines in a natural way generalized coordinates and associated momenta in an abstract phase space, whose values at any time define the state of the system. In general, the phase space is not Euclidean, but rather assumes the form of a linear Riemannian continuum, whose volume is conserved.

In the quantum case, where coordinates and momenta are conjugate variables, but not physically independent of each other and so not free to take on any value regardless of the value of the other, the mathematical framework for representing the physical system is an infinite dimensional abstract Hilbert space defined by the functions that are solutions to the wave equation.

negentropic development as well as somewhat more subtle evidence about the actual quality of energy. The Second Law is descriptively appropriate only for those systems in which fundamental forms of interaction take on a peculiarly **degenerate** form and relevant forms of energy content are apparently — and effectively — decoupled.

Our Basic Thesis

Therefore, we state at the outset one of the basic findings in this essay — that the First and Second Laws are not independent *statements of physical reality*. As Ludwig Boltzmann intuited and partially demonstrated, for all cases where energy forms can be expressed as self-evident modes in simple interaction, the Second Law follows from the equivalence of probabilistic and kinematic-dynamical considerations for these situations. Our basic thesis is, however, that manifest violations of the Second Law — and not merely as accountants' negative entropy — and the manifest incoherence of continuum vis-à-vis quantum physics are directly attributable to a *higher-order process of interaction* as the proper characteristic of what is normally passed off as energy.

Most immediately this means that Boltzmann's proof of the fact that the Second Law follows from the conservation of energy actually proves quite the **opposite of what Boltzmann and subsequent statistical physicists imagined: since almost all systems are** nonlinear (in the sense outlined above) and display significant tendencies toward *nonentropic behavior*, the Second Law is largely incoherent with interesting phenomena. The immediate implication is that, since the First Law (conservation of energy) implies the Second, the First Law must also be inadequate in some fundamental way. At the very least we would have to say that to the extent that the Second Law is correct, it describes *insignificant* situations, and to the extent that the Second Law tries to explain interesting situations (like a plasma), it is incorrect.

Ludwig Boltzmann (1844-1906) The founder of statistical mechanics, *Austro-German Bolztmann* was the staunchest defender of causality and the atomic basis of thermodynamics in his generation. Boltzmann's highest achievement was to demonstrate *that the tendency to entropy increase* has a physical and not merely a probabilistic basis. He was mercilessly attacked as "the last pillar" for his tenacious views about causality. Nearly blind and tormented by headaches, he ended his life by suicide in 1906.

In other words, Boltzmann's finding must be turned on its head.

This demands an appropriate higher-order conception of energy and interaction. **From an advanced standpoint,** particles and fields, the clearest expression of what are otherwise *irresolvable antinomies* may be **grasped as fundamentally geometrically different, but nonetheless unified aspects or projections of a single transfinite process. The particle as source of the** field cannot be constructed from the *physical or manifold particles* of the field. Such field properties account for neither the $E = mc^2$ energy content, **nor the striking stability of basic electromagnetic particles such as the electron and proton. Particles alone, on the other hand, cannot sufficiently structure or inter-mediate the physical universe as a whole or provide the axiomatic requisite for coherence, overall continuity.**

The actually invariant quality of the physical universe in general is self-mediating negentropic development, not the mere dynamics of self-evident — albeit interacting — modalities. We must concede to the empiricist or positivist that the *self-reflexive process is not always right at the surface of events*. It is usually either implied (as in the strikingly stable cases of the free

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GLOSSARY

Thermodynamics: The branch of physics based on the assumed general and universal principles governing the transformation of energy from one form into another. In the 19th century thermodynamics was divided between a **general** theory which dealt strictly with bulk (macroscopic) properties of heat and energy flow and transformation, and a **special** theory of the relationship between macroscopic properties and microscopic kinetics and interactions.

The Second Law of Thermodynamics: The generalization from the empirical observation for thermodynamic systems that heat does not spontaneously flow from a cooler body to a hotter one, or equivalently, that heat intake cannot be *completely converted into work*. The German physicist Clausius showed that this was equivalent to the principle of maximation of entropy for isolated systems.

The First Law of Thermodynamics: The principle of conservation of energy, which for thermodynamic systems takes the following form: The work done by a system is equal to the heat absorbed minus the decrease in internal energy.

Entropy: A thermodynamic parameter of a system which is related to the amount of heat the system absorbs in passing from one state to another at constant temperature and to the amount of work extractable from a system. At first considered a somewhat mysterious quantity, it was later demonstrated by Boltzmann to be related to the possible microscopic configurations associated with a thermodynamic state.

Statistical Mechanics: The probabilistic theory first developed by Boltzmann to

Ernst Mach (1838-1916) The leader of the Energeticists who led the scientific vendetta against Boltzmann's interpretation of thermodynamics. Mach told Boltzmann in one debate, "I don't believe that atoms exist."

electron or ground state of an isolated hydrogen atom) or marginal (as in the case of atoms in a weak radiation field) at the level of reality whose laws or relations are being directly observed. In between these situations and the evidently negentropic phenomena of bio-ecological evolution and human anthropological progress lies the more conditional situation of the most widespread and variable state of matter — plasma.

Our basic point holds good across this spectrum of physical situations. In all cases, interactions are not simply a form of external communication between otherwise self-contained entities. The interaction is part and parcel of the conditions of existence of undeniably discrete particles. In its most fundamental nonlinear form it involves the interaction between the particle and its field as an active process. For all their technical crudities and conceptual shortcomings — as well as absolute errors — the notions of Kepler and Descartes about the active sources of force fields are therefore of higher-order coherence than any of the varieties of their Newtonian successors.

The so-called interaction between two elements of a physical system must, therefore, give rise to higher-order overall structure (beyond simple superposition) while still preserving (except for extraordinary circumstances such as ultra-high energy or matter-antimatter interactions) the characteristics of polar discreteness.

It is precisely the axiomatic assumption of externalization of interaction within linear state-function spaces that is the epistemological basis for the statistical derivation of the Second Law and the analytic representation of entropy. With these general comments and our earlier plasma example in mind, we now directly take up the theoretical rockbed of modern statistical physics, the equilibrium distribution law and H-Theorem of Boltzmann.

Henri Poincaré (1854-1912) A leading turn-of-the-century French scientist who attempted to prove that thermodynamics was inconsistent with classical mechanics.

II. Thermodynamics and Statistical Mechanics

In their modern forms, the fundamental assumptions of classical and quantum statistical mechanics are taken to be:

(1) (Classical) — The hypothesis of equal a priori probabilities for the different classical states defined by equal volumes in the phase space corresponding to the system of interest:

(2) (Quantum) — The hypothesis of equal a priori probabilities and of random a priori phases for the quantum mechanical states of a system. (Tolman 1938)

To the contemporary physicist it is usual to "regard these assumptions as reasonable postulates to introduce, but to be ultimately justified, however, by the agreement between deduced and empirical results." (Tolman 1938) These assumptions and the stronger ergodic hypothesis — that a system rapidly passes through all microscopic states hypothetically allowed to it — generated intense controversies in the late 19th century. In these skirmishes,

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account for macroscopic thermodynamic properties by averaging over the possible microscopic configurations associated with various macroscopic states.

Phase space: The abstract geometry defined by the variables needed to describe the microscopic motions of a thermodynamic system — for example, atomic velocity and position.

Lagrangian Mechanics: The formulation of Newtonian Mechanics which essentially states once the interactions in a physical system are known,the dynamics follow from the energy content as determined by the interactions. Lagrangian Mechanics is based on the related fallacies of linear interaction and ultimately linearizable geometry.

The H-Theorem: Boltzmann's highest achievement, in which he demonstrated that the tendency to entropy increase has a **physical** and not merely a probalistic basis in the population of micro-states determined by the microscopic interactions of a thermodynamic system.

Linear interactions: The class of interactions — for which thermodynamics is largely valid — in which the quality of interaction is unchanged by the dynamics and geometric configurations of the processes that arise from the interactions.

Nonlinear interactions: Interactions that have no fixed quality, but rather that lawfully evolve to a higher-order along with process geometry. When such interactions dominate, the Second Law of Thermodynamics is no longer relevant or valid.

Negentropy: The concept elaborated by Lyn Marcus (Lyndon LaRouche) as the fundamental invariant of the physical universe: the self-developing tendency to ever higher-order manifolds.

Boltzmann found himself under attack from both strict and unorthodox mechanists, as well as from anti-atomist positivists. The once hotly fought issues were largely resolved with the advent of Gibbs' "ensemble" formulation of statistical mechanics in 1902, the subsequent discrediting of ergodic assumptions, and the establishment of electromagnetic, particle, and atomic quantum phenomena and theory from Planck's black-body result of 1900 to the DeBroglie-Schrödinger wave mechanics of 1924-26.

All controversies about the relationships between the assumptions of statistical mechanics and single system mechanics, although of some historical interest, are of negligible epistemological significance. There is no need to make a great fuss about the fact that a physical system characterized by degenerate (that is, linear), forms of interaction proceeds to a more entropic state of equilibrium more or less in accordance with the formalisms of statistical mechanics. The question of real interest is under what conditions do marginal nonlinear processes begin to play a significant or even dominant role in the temporal development of such systems.

> *"There is no need to make a great fuss about the fact that a physical system characterized by degenerate (that is, linear) forms of interaction proceeds to a more entropic state of equilibrium more or less in accordance with the formalisms of statistical mechanics."*

To clarify this point, it is useful to summarize the simplest basic result of statistical mechanics, the H-Theorem, and then demonstrate that the conditions it assumes are far from reasonable. To simplify the discussion, and because no essential point is lost in the simplification, we will treat the standard textbook case of a dilute gas of some single species of atom or molecule. To begin, we define two related modes of characterizing the overall properties of the gas: the phase-space picture and the energy distribution function.

In the former, imagine that the position and momentum components for each of the N atoms determine a 6N dimensional Γ space. Then the gas can be characterized at any moment in time by a single point in phase space. Moreover, the time evolution of the system will map onto a trajectory in phase space that is contained within the phase volume permitted the system. Or, for the latter, we can construct the same six dimensional phase space for each of the atoms and characterize the average properties of the system by inquiring what fraction or number of atoms at a given moment has a phase space projection which falls within some volume δw about the point (q, p) in this u space.

This is closely related to saying that at any time t there is a characteristic distribution function f(E,T) δE that indicates the fraction of atoms with energies in some interval of energy, $E \pm \delta E$ The earliest questions posed in statistical mechanics were of the sort: is there a unique, most probable form of f(E), say $f_{eq}(E)$ which characterizes the equilibrium state of the gas; and, if the gas at some time t_0 is not characterized by f_{eq}, that is, $f(E, t_0) \neq f_{eq}(E)$, then can it be shown that the distribution function rapidly tends to f_{eq} for all t beyond some point. Closely associated with this abstract line of inquiry are the questions that actually motivated Boltzmann's work: Is there a relationship between the average statistical kinematic properties of a gas and the results of classical thermodynamics, particularly the Second Law in the form of the rule of increase of entropy?

III. The Second Law of Thermodynamics

We introduce the Second Law and its statistical interpretation with a simple illustration. (See Figure 1.) A physical system S_1 in contact with another system S_2 and some environment E exchanges an amount of heat energy Q with E and spends part of it, W, in doing work on S_2. According to the Second Law it is not possible for S_2 to reciprocate so that S_1 returns Q to E. If it were possible then S_1, S_2 and E would clearly constitute a perpetual motion machine. Now it is a fact that despite numerous heroic efforts involving electromagnetic, chemical, and mechanical arrangements, no perpetual motion machine of this type has ever been built. We can state with complete confidence that it never will be; but this is nonetheless entirely consistent with the primacy of the principle of negentropy.

Before resolving that paradox, we note that the earliest explanations of the Second Law were immediately recognized as being contradictory. In purely thermodynamic terms, the Second Law is quite independent of the principle of conservation of energy or any other laws of physics. In terms of our example, all one can say is that if heat Q flows into S_1, part of it goes into the internal energy U of S_1 and part into performing work W on S_2. There is absolutely nothing in the existing body of laws of physics to explain (at this level of description) why the whole operation, under at least some set of circumstances, cannot be reversed — or indefinitely recycled. These are the so-called reversibility and recurrence paradoxes (*Umkehreinwand* and *Wiederkehreinwand*) raised by Loschmidt and Poincaré-Zermelo, respectively, in the 1870s and 1890s against Boltzmann's kinetic theory. *(2)*

This poses a crisis, in fact, for the understanding of a phenomenon which commands the intuitive respect of all experienced adults. If S_1 and S_2 are

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(2) A compilation of original papers summarizing the exchanges between Boltzmann and his "pure" mechanist critics appears in *Kinetic Theory, Vol. 2* edited by S.Brush. Boltzmann's more heated battle with the Energeticists and others, as well as his general scientific outlook, are fully elaborated in *Theoretical Physics and Philosophical Problems* edited by B. McGuiness.

> *"The important issues, therefore, are not about the interpretation of Boltzmann's results, as he and his contemporary critics thought, but about when the prevailing conception of a physical system happens to be appropriate as a degenerate limit of the actually characteristic situation."*

simply two blocks of dissimilar material at respective temperatures, T_1 greater than T_2, and they are placed in contact such that an amount of heat Q flows from S_1 to S_2, why doesn't this excess heat ever flow back to S_1? It was this sort of problem that led Maxwell, Clausius, Boltzmann, and other physicists of the late 19th century to investigate the internal distribution of such bulk amounts of energy transfer to determine why they apparently are always irreversible.

"Unidirectional Processes"

The answer that Boltzmann came up **with and thought he had definitively demonstrated, was that it was** overwhelmingly more probable for any and all physical processes to be unidirectional. Tautologically enough, the preferred direction is that from the less to the more probable situation. The **core of Boltzmann's method and results, as laid down for example in his 1896 *Lectures on Gas Theory* is in fact entirely based on the theory of probability as applied to atomic kinetics.**

We can anticipate the two most basic and important results of Boltzmann's program, the derivation of the Maxwell-Boltzmann distribution function as the most probable equilibrium distribution and the H-Theorem (proof of passage of an arbitrary initial distribution function to the Maxwell-Boltzmann distribution with accompanying maximization of the entropy) by first considering a simpler, but related problem: the random walk or coin-flip distribution.

Suppose you take a coin and flip it N times. Record the number of heads and tails which come up. Call that operation the system under study. Now repeat the experiment a very large number of times, recording the relative number of heads and tails for each series of N flips. If we denote by n_i the difference between the number of heads and tails in a particular series N_i, what sort of distribution of the n_i is to be expected, that is, what is the most likely value of n — and why?

In order to have a basis for computing the expected result, only one fundamental assumption is required: that there is no determining law for the result of any particular flip and, therefore, the a priori probability of a head or a tail on a single flip is equal. Once that assumption is made, the *overall predicted result is simply a* process of counting. Counting what? The number of possible different series consistent with a given value of n, which can take on values from O to N. For example, the $n = 0$ result contains among others the following series:

HH . . . . HH . . . . HHTT . . . . TT . . . . TT
TT . . . . TT . . . . TTHH . . . . HH . . . . HH
THTHTH . . . . THTH . . . . THTH
HTHTHT . . . . HTHT . . . . HTHT

It follows that the probability of the value of n is simply the number, m_n, of possible independent arrangements corresponding to n, which is a function of n and N. *(3)*

Put in language which is close to the thermodynamic systems we are interested in, the probability of obtaining the macroparameter n depends on the *number of (equally likely) microstates* m_n giving the result n. From the result above, it can be seen that for large N, the probability of getting a value very

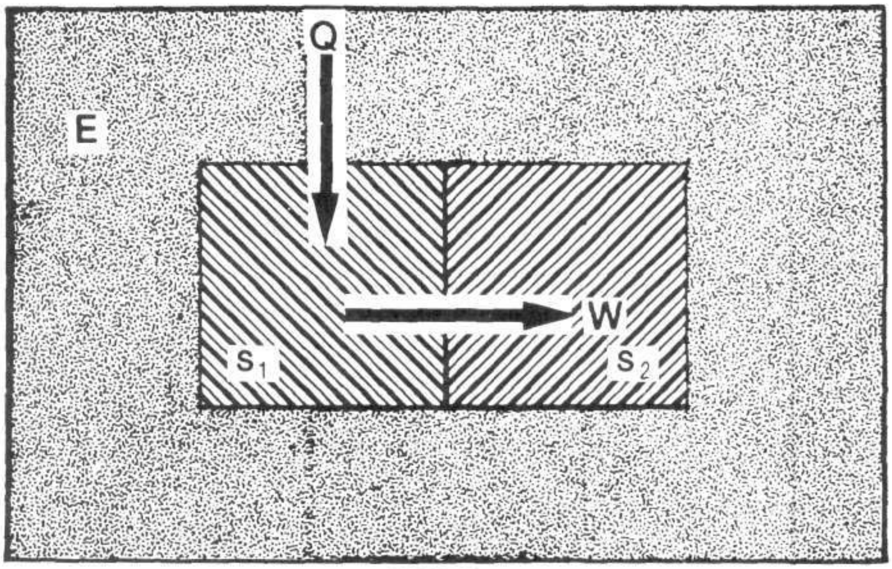

FIGURE 1

Conversion of heat to work in a Thermodynamic system.

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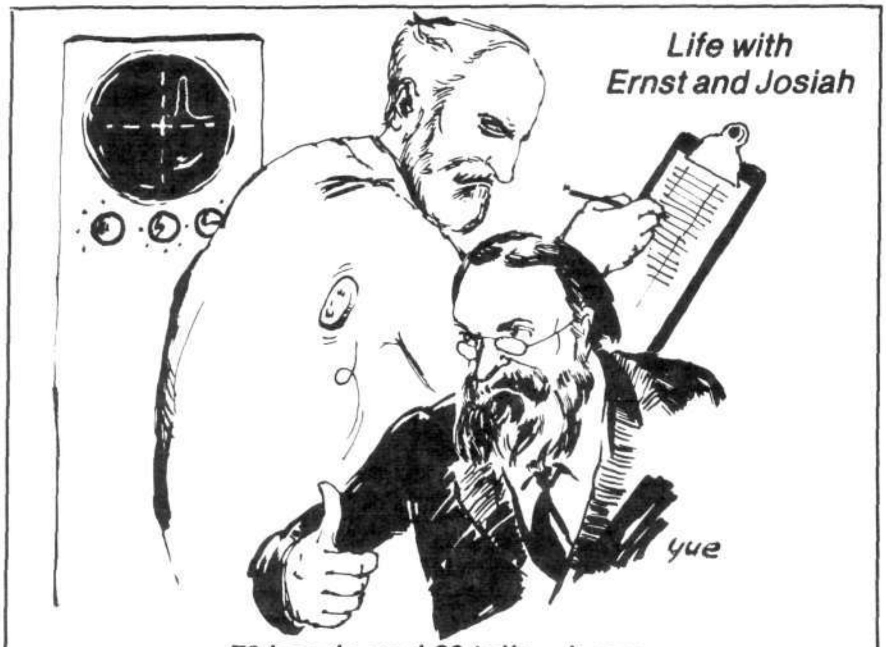
(3) For the case where N becomes large and typically n/N becomes small it can be shown using Stirling's approximation $(n! \simeq (2\pi n)^{1/2} n^n e^{-n})$ that the exact counting result $P(n) = \frac{N!}{[\frac{1}{2}(N+n)]![\frac{1}{2}(N-n)]!}\left(\frac{1}{2}\right)^N$ can be very closely approximated by the Gaussian distribution,

$$P(n) \simeq \left(\frac{2}{\pi N}\right)^{1/2} \exp(-n^2/2N) .$$



70 heads and 30 tails...hmm...
it could be the coin...Ernst, flip faster!

far from the most probable value $n = 0$ is extremely small.

Now, what does this have to do with the forms and transfer of energy and the Second Law? Once it is assumed that any physical system (or group of interacting systems) is described by a predetermined continuous or discrete aggregate of states (whether finite or infinite in number), each of which corresponds to some set of parameters characterizing the macrostate, there will be a tendency toward the states of highest microstate weighting — if there is no reason for the system to be in one microstate as opposed to another.

This brings us to a branching point in our discussion. On one hand, we recapitulate Boltzmann's statistical explanation of the law of entropy increase; on the other, we show that the theoretical reasoning is circular, since the assumptions about the nature of the physical system made by Boltzmann are just those for which a dynamical solution to the problem happens to correspond closely to the nonphysical statistical computation. The important issues, therefore, are not about the interpretation of Boltzmann's results, as he and his contemporary critics thought, but about when the prevailing conception of a physical system happens to be appropriate as a degenerate limit of the actually characteristic situation.

First, the Second Law as interpreted by Boltzmann. Consider the simplest type of system conceptualized as our S_1 earlier: a dilute gas of neutral atoms. From Boltzmann's standpoint, the basic characteristic of the gas is that it consists of N particles, which may have internal forms of energy, in motion. Boltzmann argued that the basic behavior of a gas could be described if one knew the evolution over time of the distribution function of the gas, describing the distribution of molecular energies about some most probable value.

Maxwell and Boltzmann demonstrated that in terms of the energy states assumed to be available to the gas as a whole, there was one distribution of energies (or velocities) that was overwhelmingly most probable in just the sense developed for the coin-flipping experiment. Boltzmann further showed that under a wide variety of assumptions about the nature of atomic collisions, a gas not initially in the Maxwell-Boltzmann distribution would alter its state through atomic collisions until it arrived at the Maxwell-Boltzmann distribution. This result provides a qualitative picture of the assumed basis for the Second Law: there is an extraordinarily high probability that at least part of the energy flowing into a working medium will be spread out over the internal states in a form not recoverable for performing work on the external world. Put in the usual descriptive terms, since randomized states have much higher probability, they are the endpoint of whatever intermediate processes take place. In the historical development of the subject, reversibility and recycling were taken account of by Boltzmann in citing the exceedingly low probability of macroscopic reversal compared with small fluctuations about the equilibrium state.

In thermodynamic terms, the fact that in undergoing a transition from state A to state B, the change in entropy (ΔS) — defined as a thermodynamic parameter of the state — is greater than (or, for reversible transitions, equal to) the integral

$\int \frac{dQ}{T} \leq [S(B) - S(A)]$, (this is an alternative formulation of the Second Law) indicates that aside from the overall energy balance, there are other internal features of a gas or other working medium which limit how much work it can do in undergoing a transition. While the First Law indicates **balance of work done, w, with heat** absorbed (Q) and change of internal energy, (ΔU), $W = Q - \Delta U$, the Second Law gives an upper limit for the amount of work that can be done by a thermodynamic substance in going from one state to another $W \leq -\Delta U + T[S(B) - S(A)]$. We now derive the explicit connection between thermodynamics and statistical mechanics suggested by these qualitative arguments.

IV. The Maxwell-Boltzmann Distribution and the H- Theorem

In the case of a dilute gas consisting of n molecules whose total energy is defined as in the range of E to $E + \delta E$, the simplest question of statistical mechanics is: what are the weightings of various distributions of the molecules in u space consistent with the total energy content. For similar molecules, it is assumed there is no difference in interchanging them — the statistical weighting is due only to the total number of possible physically equivalent configurations in u space:

$$P = \frac{n!}{n_1!\, n_2! \cdots n_i!}$$

where n_i is the number of molecules in some volume element w_i of u space.

It is a standard mathematical derivation (which is reproduced in Appendix A) to reduce this P to a Gaussian distribution just as was done for *our coin flipping experiment*. The result is that n_i is proportional to $\exp(-E_i/kT)$, where E_i is the energy of the i^{th} state and T is the absolute temperature.

This can be written as a differential, giving the fraction, δn_i, of particles with energies between E_i and $E_i + \delta E_i$

$$\delta n_i = nC \exp(-E_i/kT)\, \delta w(p,q).$$

Now, let us look at what this represents (which otherwise may be obscured by the minor technicalities of the derivation). If it is assumed that each equal range or volume of u space corresponding to some set of values of the generalized coordinates (q,p) of a single molecule, has equal a priori probability for any particular molecule, then the most probable distribution of n molecules among those *cells is dependent on two factors*: the energy of or associated with a region, weighted by $e^{-\beta\epsilon}$; and the extent to which a region characterized by ϵ gives a greater or smaller spread in range of u parameter (q,p), so that δw acts as a compounded weighting factor.

The result, the Maxwell-Boltzmann equilibrium distribution (which for the velocity distribution of a molecular gas looks like the curve shown in Figure 2), is simply the result of the fact that for higher velocity there are more equivalent u cells since $\delta w \approx v^2\, dv$, but their occupation is constrained by the $\exp(-mv^2/2kT)$ factor which derives from the condition of overall fixed energy and acts to keep high energy states from hogging it all. This is illustrated in Figure 3.

The combination (product) of these two factors limits the distribution of energies to a region near the most probable energy value $\bar{\epsilon} = \frac{1}{2}kT$ associated with the Maxwell-Boltzmann distribution, itself the most probable distribution.

What is inexplicable from the standpoint of this derivation is why the $e(-\beta\epsilon)$ term should be obtained in actual physical systems, where the actual life-

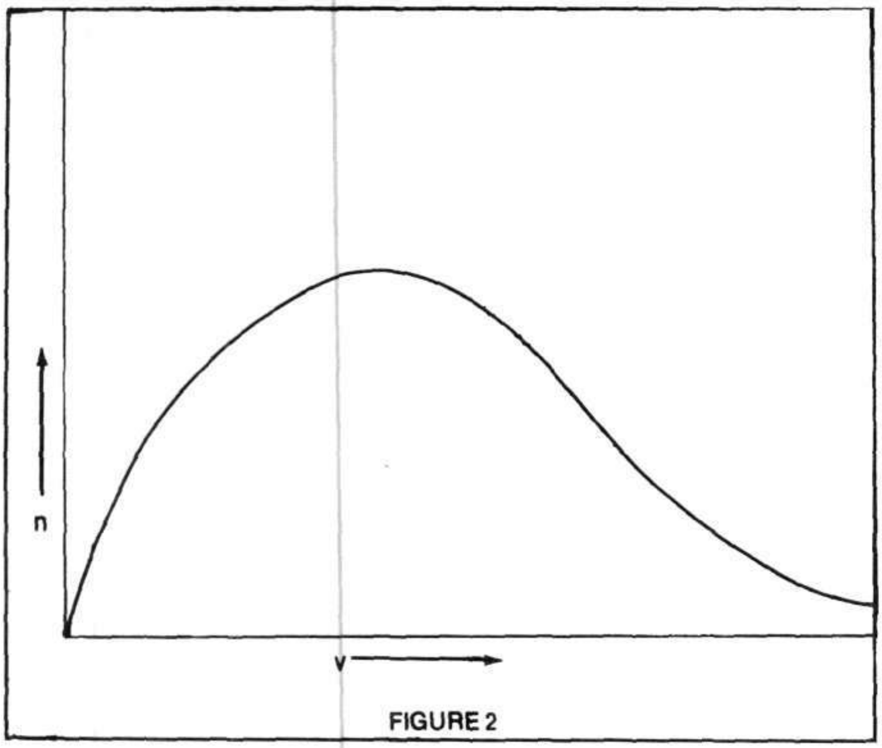


FIGURE 2

The Maxwell-Boltzman distribution of atoms among possible velocity states.

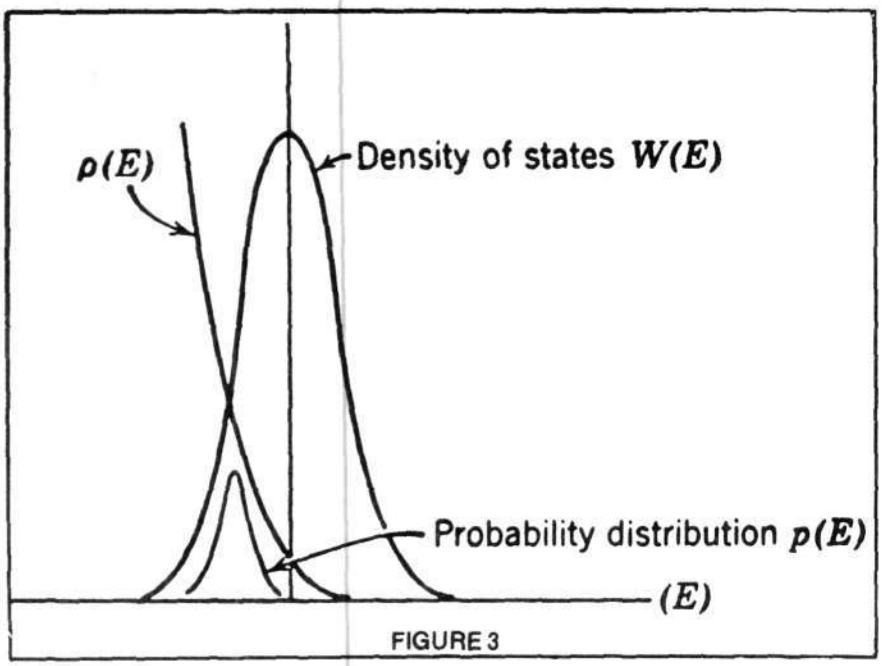


FIGURE 3

Graphical representation of how the distribution function $f^{(E)}$ results from the product of the Boltzman factor $e^{-\beta\epsilon}$ and the density of state function.

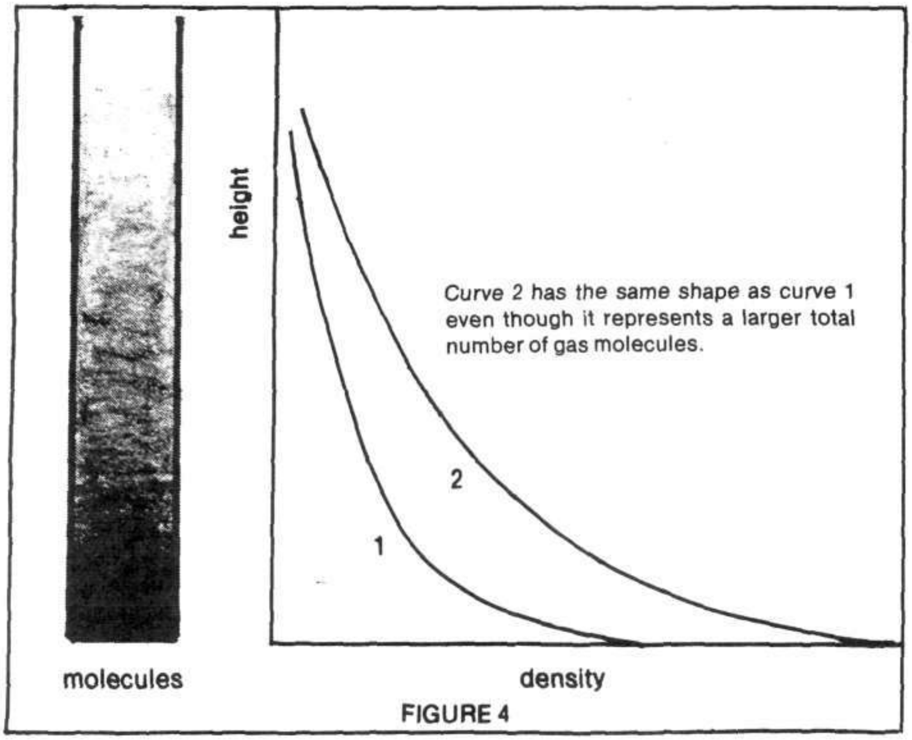


FIGURE 4

Fall-off in density with height of a thermalized column of gas in a gravitational field

This simple example illustrates the solution to the following profound problem: Why is it that for a system in thermodynamic equilibrium the **population** distribution among states (or energy levels) depends only on the two parameters, temperature (T) and energy (E), even though the **lifetime** (that is, the time that a **particular** particle on the average actually occupies a state) may be totally **independent** of either system temperature or state energy? If the lifetime is short, shouldn't the state have a relatively low population?

The answer lies in the fact that if only interactions of the type compatible with the H-Theorem (the Maxwell Boltzmann equilibrium distribution) are operative, then the rate at which states are populated will just match the depopulation rate, only if the $e(-\beta\epsilon)$ population condition is satisfied—independent of what value the depopulation rate takes on under specific conditions. Thus, for example, the $e(-mgz/kT)$ distribution is obtained no matter what the total number of molecules in the column or the temperature — that is, no matter what the average molecular collision rate (which is a function of density and veolcity) in the column or any particular height.

In contrast, once the external and internal constraints are removed, and higher-order interactions come into play, the statistical results yield to qualitatively different dynamics, as in the case of plasma turbulence.

time of any single atom in the i^{th} microstate depends on the physical interactions which generally determine quite different lifetimes for different states. An example will help to clarify why statistics sometimes works. Consider a column of gas that is maintained at uniform temperature T in the earth's gravitational field. Then the Maxwell-Boltzmann velocity distribution will obtain at all heights, but there will also be a distribution of molecules (or pressure) in height that is of the form

$$N(z) = C\exp(-Mgz/kT)$$

where z is the elevation of a gas element, since Mgz is the energy (here potential energy) of a gas element at an elevation z.

Because the system is maintained, or better, forced into an equilibrium condition with near fixed energy content, it assumes a configuration in which the tendency to fall in the external gravitational field is just matched by the higher rate of collisions with the denser gas below than with the more dilute gas above — independent of the lifetimes of any particular molecules in particular states (see Figure 4). This example begins to provide us with an insight into those situations in which constraints lead to a realized correspondence between the purely (non physical) statistical computation of statistical mechanics and the empirical physical results. In the column of gas a molecule is interacting both with the gravitational field and with other molecules. Each particular molecule has a strictly limited lifetime in any particular region of fixed height, which moreover varies widely from molecule to molecule at the same or different heights. The fact that the $e^{-\beta\epsilon}$ distribution is obtained is not, however, because of any laws of statistics, but because the real physical interactions in the gas and between the gas and its controlled environment of gravitational field and temperature are both of such a limited type that the gross results map onto the statistical distribution of states — which are themselves the scalar projective mappings of such limited forms of interaction. (4)

There is nothing mysterious here either. Computer studies of manageable small numbers of particles that solve the equations of motion more or less exactly verify that the restricted dynamics of such situations keep

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(4) It is ironic that Loschmidt, who is in general unhelpful in his debates with Boltzmann, did criticize this gas column example with great insight. His criticism centered on the impossibility of a macroscopic equilibrium for such a column. He correctly pointed out that circulation, largescale vortices, and like collective phenomena would always prevent a real gas from approaching the hypothesized equilibrium and isolation.

them locked into quasi-Maxwellian distributions. (Kittel 1958) (See Figure 5.) We show this more generally in the proof of the H-Theorem for spherical atoms. The reader may be relieved to know that it is physics and not statistics that gives the earth's atmospheric envelope its familiar form, rather than leaking away into space or collapsing to the surface.

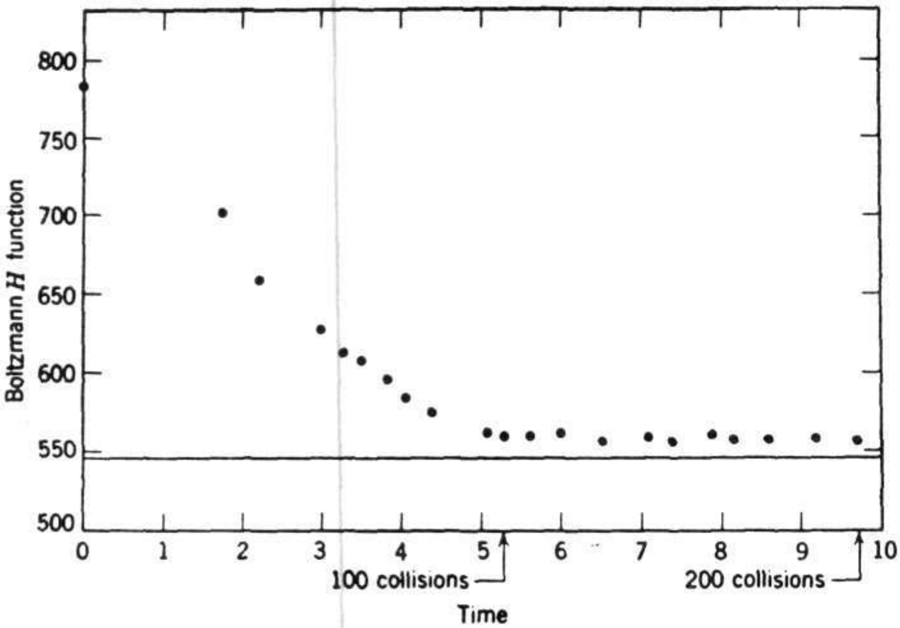


FIGURE 5

This figure shows the results of a computer solution for the equation of motion for a small ensemble of hard spheres, which began in a relatively "improbable" state in which the H-function had a value of 780. The minimum value of H for the set up is shown by the solid line at H 545. As the system evolves under the influence of the mutual collisons of the particles, H decreases toward its equilibrium and minimum value. (Kittel 1958)

Thus, the only principle which is established by such coincidence of statistical and empirical results is that for limited classes of interactions, macroscopic behavior under given boundary and constraining conditions is, in fact, at equilibrium independent of the details of microscopic behavior. The proper conclusion to be drawn, however, is not that such technologically useful results establish the universality and potency of statistical mechanics, but rather, that they reflect the limitation and poverty of certain subsumed modes of interaction. (For plasmas this is evidenced by dynamic metastability — even the sun is hardly in what by any stretch of ontology could be called thermodynamic equilibrium.)

This conclusion is no less valid for the theoretical pinnacle of statistical mechanics, which unifies it with the Second Law, Boltzmann's celebrated H-Theorem. (One of the most prominent early synthesizers of classical and quantum statistical mechanics, Richard Tolman, unabashedly stated "the derivation of this theorem and the appreciation of its significance may be regarded as among the greatest achievements of physical science.")

As indicated earlier, Boltzmann's dual objective was to demonstrate that a thermodynamic system in practically any initial state would proceed to the Maxwell-Boltzmann distribution, and in doing so, maximize entropy. Our first step in discussing the H-Theorem will be, therefore, to indicate the functional representation of statistical probability that Boltzmann found most appropriate to demonstrating both (1) the approach to equilibrium and (2) the relationship between the statistical probability of states in phase space (or more generalized microparameters) and the thermodynamic state parameter, entropy.

Proof of the H-Theorem

Avoiding unnecessary detail, the H-function is defined simply as the negative of the logarithm of the probability of a particular distribution in u space: $H = -\log P$. The (-log P) term can be expressed in the equivalent sum or pseudo-integral forms in terms of the cell occupation numbers, n_1, or distribution functions f defined earlier, so that

$$H = \sum n_i \log n_i (+\text{const.}) = \int f \log f \, d\!\!\!{}^{-}p \cdot d\!\!\!{}^{-}q \, (+\text{const.}).$$

The simplest, although least general, means of establishing the correspondence between H and S is to demonstrate as Boltzmann did that H calculated in the form above for an ideal gas is the negative of S computed for an ideal gas from the basic defining thermodynamic relationship $dS = dQ/T$. If one substitutes $f = f_{M-B}$ in the expression for H, and the ideal gas law, $\frac{1}{\rho} = \frac{RT}{MP}$ in the thermodynamic expression for S, it can be shown in a straightforward way that

$$S = \frac{Rk}{M} \log\left(\frac{T^{3/2}}{\rho}\right) = -H$$

and so, $S = -H$.

For this case, then, and as it can be

shown more generally, the thermodynamic entropy of a state is proportional to the logarithm of the statistical probability of that state. This is usually written as $S = k \ln w$,

where k is Boltzmann's constant. The principle of increase of entropy can then be generally related to the behavior (tendency toward a minimum) of integrals of the form

$$\int f \log f \, \overline{dq} \cdot \overline{dp} \quad .$$

The H-Theorem itself consists in demonstrating that the time derivative of H is negative, and so H is decreasing, until and unless f assumes the Maxwell-Boltzmann form, in which case

$$\frac{dH}{dt} = 0$$

and gross equilibrium is maintained.

Again, the detailed derivation of the H-Theorem is deferred to an Appendix (B). In this derivation, however, the essential assumption is of an interaction between individual elements of the system that obeys the following conditions (which are true for both quantum and classical mechanics):

(1) A specifiable set of initial states of the elements; and

(2) A known set of final states (after interaction) for the element.

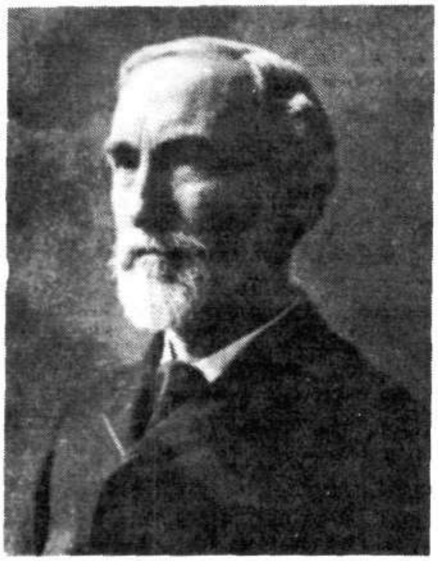
Boltzmann was then able to show that only in equilibrium (when the interactions going from a given set of initial states were exactly balanced by all interactions that had these states as final states) was

$$\frac{dH}{dt} = 0 \quad .$$

Otherwise, it must be negative.

The Gibbs Approach

The generalization of this result to any thermodynamic situation depends on one further assumption and a more abstract formulation of statistical mechanics due to Gibbs. The former is the notion that no matter how inhomogeneous or disequilibrated a system is, or how the definition of characteristic u space may itself change with time — as during chemical reactions — at any time one can define entropy of the system as the sum of entropies of macroscopically small but microscopically large, quasihomogeneous regions. Gibbs' formulation introduced two other elements: the idea of a representative ensemble of similarly prepared systems, rather than a particular physical system; and the idea of coarse grained average probability P (over the fine grained probability p in u space) of finding an ensemble member in a semimicroscopic region of u space. This most abstract formulation anticipates most of the modern interpretations of quantum mechanics, in avoiding description of a particular physical system.



Josiah Willard Gibbs (1839-1903) An early 20th century physicist who formalized statistical mechanics by removing its physical content.

It is sufficient here to note that the Gibbs approach to the H-Theorem (in either classical or subsequent quantum form) depends entirely on the assumption that no matter what type of interaction is involved, as time goes on, ensemble averages give increasing values of entropy (decrease of H) simply because of the available volume in phase space and spread of the ensemble into that volume. This is described in the Gibbs picture as the "inhomogeneous redistribution" of fine-grained probability, in which entropy increases as a result of growing local disparity between P and p. When quantum effects are added, additional terms contribute to the decrase of H because of the quantum-mechanical spreading of initial states in Hilbert space.

Without the Gibbs-type generalization, one can simply extrapolate on faith from the explicitly solvable case to more general situations.

Boltzmann was aware that while the H-Theorem might be valid under a wide range of conditions, its general proof required that "It would be necessary to prove that the following cases are not possible: ... (3) There are also stationary molecular-ordered state distributions. (This is) related to the case of the presence of external forces. **The impossibility of case 3 cannot be proved from the minimum theorem, and probably cannot be proved in general without special conditions (emphasis added)."** (Boltzmann 1896a)

However while noting that "the assumptions about the interaction of molecules during a collision have a provisory character, and will certainly be replaced by others," Boltzman nonetheless postulated generalization of the H-Theorem in two basic directions.

First to other states of matter: "We have looked mainly at processes in gases and have calculated the function H for this case. Yet the laws of probability that govern atomic motion in the solid and liquid states are clearly not qualitatively different in this respect from those for gases, so that the calculation of the function H corresponding to the entropy would not be more difficult in principle, although to be sure it would involve greater

"The reader may be relieved to know that it is physics and not statistics that gives the earth's atmospheric envelope its familiar form, rather than leaking away into space or collapsing to the surface."

"It is precisely the role of free will, of freedom, to intervene in the spread of linearity — to keep it in its place — as the necessary condition for continued human existence."

mathematical difficulties." (Boltzmann 1896a)

Second, he assumed validity of the H-Theorem even when there is admitted ignorance of the nature and effects of interactions: "Perturbations experienced by the molecules **as a result of the aether or the electrical properties of the molecules,** etc., must be left out of the theory because of our complete ignorance concerning such effects **(emphasis added)." (Boltzmann 1896b)** To his credit, however, Boltzmann makes explicit the basis for such generalization: "...gas theory does not assume that either the properties of the aether or the internal constitution of molecules can be explained by centres of force, but only that for the interaction of two molecules during a **collision the Lagrange equations of** motion are valid with sufficient accuracy for the explanation of thermal phenomena." (Boltzmann 1896b)

V. A Reinterpretation of Boltzmann's Results

We are now in position to specify the conditions under which developments will not conform to the thermodynamic-statistical schema. We reemphasize that all that has been derived is that if one has a priori knowledge of the full range of states of all types available to a system (such states characterized by energy dependence on various kinematic or field variables or interaction operators), the system will proceed to a final, equilibrium state, because the actual operative causal principles cohere with the result that the final state has so many ways it can internally rearrange itself and still be the same state — relative to possible rearrangements consistent with other, nonequilibrium states. Any real system must, of course, pass through only those states from moment to moment that it has direct access to in going to the final state — itself a condition of complex fluctuation about some mean final configuration.

It is well known that **this** is the basis for assuming, as Boltzmann did, that the universe as a whole is either on its **way to this heat death,** or has already gotten there, and we are just living out a final local fluctuation; that biological evolution or the functioning of any organism is simply chance order **bought at the expense of depletion** of the negative entropy reservoir. We can locate the source of these stupendous blunders in an aspect of Boltzmann's limited conceptual powers that is normally treated as simply a trivial feature of his computational formalism.

For Boltzmann, mechanical and thermic phenomena were taken to be strictly the sum of a plenum of a large number of discrete interactions. Once the number got large enough there was thought to be no essential difference between results obtained from summation **or from integration, and** macroscopic differential equations could be employed as the appropriate equations of motion for mechanical and thermal dynamics.

As Boltzmann put it, "The question is really whether bare differential **equations or atomistic ideas** will eventually be established as complete descriptions of phenomena." (Boltzmann 1896a)

On the other hand, in the separate domain of assumed pure field properties, electromagnetic radiation, as Planck — who originally eschewed any strictly atomistic-modal interpretation of the Second Law — discovered, the measured spectrum of black body radiation necessitates the assignment of a definite discrete volume in frequency phase space in computing the statistical weighting of **any radiation frequency mode.** *(5)*

The Problem with Boltzmann

The problem with Boltzmann and his successors is not simply their readiness to accept a **dichotomization** between discrete and continuum-based phenomena and to slough off differences between finite and infinite dimensional systems; that is only symptomatic. Rather, it is their shocking ignorance of long-established and readily available epistemological

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(5) The outstanding irony of Planck's historic introduction of the quantum of action (and associated Planck's Constant, h) is that he saw it as necessary to upholding the entropy law in the case of black-body radiation. (See M.J. Klein, "Planck, Entropy, and Quanta, 1901-06.") The well-known problem prior to Planck's solution was that if one assumed that the distribution of electromagnetic field energy over frequency modes satisfied the condition of equipartition of energy (each oscillator having energy kT), then the theory incorrectly predicted that most energy would be radiated at short wave lengths.

To theoretically reproduce the empirical black-body radiation function, Planck was forced to take atomism — which he had previously eschewed as a supplementary hypothesis to thermodynamics — seriously. On the assumption that energy was not distributed continuously over the field modes and in space, but rather was indivisibly concentrated into packets of characteristic size for each frequency (given by $E_\nu = h\nu$) the proper distribution function *is obtained, but the equipartition condition must be discarded.*

While the black-body energy distribution still displays maximum entropy for the equilibrium distribution among modes, the energy content of each mode indicates a complex coupling or interaction of *the electromagnetic field and its sources. (The average energy per* electromagnetic oscillator of frequency ν goes from kT to $h\nu/(e^{h\nu/kT} - 1)$. Thus while Boltzmann's constant, k, is related to atomicity and classical modes of interaction, displacement, and so forth, of atoms or partitioned modes ($k = R/A$ where R is the gas constant for ideal gases, and A is Avagadro's number), Planck's *constant, h, is representative of a more profound discreteness, that of* the interaction of field and particle.

Under conditions of higher electromagnetic field density in interaction with matter, as in laser fusion, there is no longer an *equilibrium distribution, as nonlinearity sets in at all levels of interaction.*

Life with Ernst and Josiah

It's almost Maxwellian ...

and ontological insights into the problem of discreteness and continuity that resulted in blindness to the implications of continuum characteristics *of thermal-mechanical phenomena and* quantized properties of the electromagnetic field. Besides publicly denigrating Hegel and the intrusion of all other metaphysics into physics, *Boltzmann ignored not only the work of* Riemann, but also of Cantor, who published in the same journal as Boltzmann.

As the empirical results taken as a **whole in conjunction with the most** advanced developments of 19th century philosophy and mathematical physics suggest, all *physical systems* are of **transfinite order, as is the physical** universe as a whole. We need not elaborate that here; it constitutes major elements of already published **theoretical work (Marcus 1975; Parpart 1976). Rather, we shall apply that work to the specific formalism of statistical mechanics simply to point out why the latter does not work in various circumstances. Even when it does work, we must emphasize that that does not mean the physical system is anything like what the naive physicist may imagine.**

Statistical mechanics and the Second Law provide empirically *verifiable* computed results when the transfinite order of the system of interest is composed of, that is reduced to, a simple linear superposition of **transfinite elements. Under such conditions** of limitation of internal and system-environment interaction, only a limited spectrum, or better, a fixed overall *manifold* of states is available. Under these conditions, there is nothing surprising about the empirical fact that the particular configuration which is most self-sustaining through the *limited interactions available* is indeed realized as an equilibrium state.

Once, however, the interaction — through self-development of originally marginal effects — attains a decisively nonlinear quality (in the sense of geometrization of process being an attribute of nothing less than process as a whole), then there is no longer a *single determined manifold of states* or **distribution functions. Rather, such** states and functions are continuously generated as local projections of higher-order processes, which give the *appearance of merely negative entropy* when measured with instruments of lower-order internal metric.

In this case the spectrum is not only not a priori knowable, when it appears there will be anything but a smearing out over all apparently available states. **There can be no talk of equal a priori** probability once there is no longer a *priori knowledge of the development of* the physical manifold. In these, more general, circumstances, the **perpetuum mobile** is the tendency to succession of nested higher-ordered manifolds. The H-Theorem integral

$$\int f \log f \, \delta w$$

on the other hand, presupposes a discrete-dimensioned, simple-continuum phase space (or series of spaces) rather than such a nonlinear continuum of higher-order manifolds.

Returning to our original example of weakly turbulent plasma, we see that in going from gas to partially ionized plasma to turbulent plasma, there is the emergence of not only additional or stronger forces or interactions but of qualitatively new relations between the *system and its elements, which are not* simply linear superpositions of characteristic modes. This has prompted the earlier cited Tsytovich to term high density electron beam *produced plasmas a "new state of* matter." How does one construct a phase space for a system whose elements are not linearly separable? *(6)*

Here then is the heart of the matter. If the universe were composed of linearly independent species of fields and particles, it might be entropic, but then it would also be incomprehensible. It is really this converse of the H-Theorem that Boltzmann showed. Since the universe is **not** entropic, the fact that the First Law implies the Second Law means that the First Law itself is inapplicable — either because nonlinearity is the guts of the interaction or because a static idea of energy is wrong. A properly nonlinear interaction — whether highly differentiated as in biological systems or more fluid as in strong turbulence in plasma — permits no such representation — or reality. Where locally degenerate situations are produced, they are **subsumed** by global higher-order processes. So what if an ore is in thermal equilibrium just before it goes into the fusion torch, or if its elements are in equilibrium again afterward in steel beams? There are but moments in a higher-order negentropic process.

It is precisely the role of free will, of freedom, to intervene in the spread of linearity — to keep it in its place — as the necessary condition for continued human existence. This explains why the final arbiter of scientific theory can be only advances in realized social-reproductive practice. For the physical universe to have evolved to this point, and for it to keep evolving positively, the tendency must be away from linear partitioning of energy — and from its ideological reflections: self-sufficiency, nationalism, natural selection and so forth — and toward **nonlinear integration**, where the efficacy of the individual is greatest.

Technologies until now have taken advantage of the useful property that there is relative macrostability of various substances, that chemical reactions proceed in a certain way, that the efficiences of steam engines can be computed, and so forth; that is, that the H-Theorem has some correspondence to reality because it is the nature of linear interactions to produce overwhelmingly preferred macrostates deriving from the type of spectrum or modes they generate. With the present relationship of resources to productive base and theoretical knowledge, however, the technological developments and scientific problems that confront humanity now as practical issues — from controlled fusion to extending the life span — demand theoretical mastery of the explicitly nonlinear domain.

APPENDIX A

There are three conditions that can be analytically combined — after again using Stirling's approximation — to give the maximum value of the probability (subject to the constraints) P associated with the $(n_1, n_2, \ldots, n_i, \ldots)$ distribution of molecules in u-space.

First, since log P is well approximated by

$$\log P = n \log n - \sum n_i \log n_i + (\text{const.})$$

the maximum value of P satisfies the variational relationship

$$\delta \log P = \sum_i (\log n_i + 1)\, \delta n_i = 0.$$

Second, if the number of molecules is fixed, one has the auxiliary relationship $\delta n = \sum_i \delta n_i = 0.$

Finally, if the total energy is indeed well defined, then any shift of molecules from one set of u cells to another must be so internally balanced that $\delta E = \sum_i \epsilon_i \delta n_i = 0$; where for the equilibrium, or maximum probability case, ϵ_i is the incremental increase of energy of the total system per molecule introduced into the i th cell of u space. That is, $\epsilon_i = \frac{\partial E}{\partial n_i}$. For a weakly interacting gas, ϵ_i will be largely or exactly equal to the energy that can be assigned directly to a molecule occupying the i th cell by virtue of its having generalized coordinates (q,p) when in the i th cell, and, therefore, $E = \sum_i \epsilon_i$ ·

In any event, combining the three conditions above by using the method of Lagrange's undetermined multipliers, one obtains the single equation $\sum_i (\log n_i + \alpha + \beta\epsilon_i)\, \delta n_i = 0,$ which must be satisfied no matter what variation n_i is made in occupation numbers of u cells. This can be satisfied only if for every region in u space, $\log n_i + \alpha + \beta\epsilon_i = 0$ which is solved by: $n_i = \exp(-\alpha - \beta\epsilon_i)$ $\overset{\text{or}}{\rightarrow}$

$$\delta n = nC \exp(-\beta\epsilon)\, \delta w(q,p) \ .$$

(The pressure exerted by such a gas of volume V on a physical surface bounding it is equal to $p = m/v\beta$. But for a perfect gas, $p = mkT/v$, and so $\beta = 1/kT$. The result $\alpha = nC\delta w$ comes directly from the normalization condition $\int \delta n = n$ ·)

APPENDIX B

The most general form of the time derivative of H is given by $\frac{dH}{dt} = \int \frac{df}{dt} \log f \, dq \cdot dp$. For the simplest types of systems and processes, such as collisions in a dilute gas of billiard ball type atoms, it can be explicitly demonstrated that $\frac{dH}{dt} \leq 0$.

For example, if such a gas is initially not characterized by a M-B distribution function, collisions will occur which populate and depopulate u space cells according to $\frac{dn_i}{dt} = V \frac{df_i}{dt} \delta w_i$,

where $f_i = f(w_i, t)$, w is the set of generalized internal coordinates and all momenta (if f is not dependent on normal space coordinates) and V is the gas volume. But dH/dt can then be decomposed into terms corresponding to transitions due to collisions,

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(6) Boltzmann recognized this problem in his own way, but thought it might be resolved by replacing approximate phenomenological theories with more exact atomic interactions. Interestingly enough, he identified the breakdown of modal descriptions in the face of turbulence: "...All concepts of phenomenology are derived from quasi-stationary processes and no longer hold good for turbulent motion....if every volume element of the body has a different motion...it is likely or at least possible that the different energy forms can no longer be sharply separated." (Boltzmann 1897, p. 45)

Plasma physics has brought the argument full circle, since statistical approaches work no better than conventional magnetohydrodynamic theory. See Bardwell's article in this issue and his commentary on Tsytovich's mooted soliton statistics in the FEF Newsletter, Vol. 1, No. 6, June 1976.

from u elements w_i, w_j to elements w_k, w_l,

where $$-\frac{dn_i}{dt} = -V\frac{df_i}{dt}\,\delta w_i = \frac{dn_k}{dt} = V\frac{df_k}{dt}\,\delta w_k \;.$$
$$= C f_i f_j$$

since the probability of (i,j) to (k,l) collisions depends on the probability of occupation of w_i and w_j and the fact that classically initial states i,j if completely specified, determine final states k,l.

So for all possible (i,j) to (k,l) events

$$\left(\frac{dH}{dt}\right)_{(i,j\to k,l)} = C f_i f_j(-\log f_i - \log f_j + \log f_k + \log f_l)$$

$$= C f_i f_j \log\frac{f_k f_l}{f_i f_j}\,.$$ For spherical atoms, there is an exact inverse event which must give

$$\left(\frac{dH}{dt}\right)_{(k,l\to i,j)} = C f_k f_l \log\frac{f_i f_j}{f_k f_l}\;.$$

For the mutually inverse pair of events, each of which has equal standing (though different probabilities) from the standpoint of statistical mechanics, the contribution to dH/dt is $C(f_i f_j - f_k f_l)\log\frac{f_k f_l}{f_i f_j}$

This result is of the form (x — y) log (y/x), which must be less than zero when $y \neq x$, and zero when y equals x. The last condition is, however, just that for equilibrium — the M-B distribution. This result can in fact be demonstrated for any closed cycle of collisions or interactions to give $\frac{dH}{dt} \leq 0$.

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About the Authors

Dr. Morris Levitt, the executive director of the Fusion Energy Foundation, has lectured widely on fusion and world development. Levitt received a BS in physics with honors at Case Institute of Technology in 1961 and a PhD at Columbia University in 1968. He was awarded a National Science Foundation postdoctoral research fellowship, and has taught courses on science and society at Queens College and other branches of the City University.

Currently Levitt is the managing editor of the International Journal of Fusion Energy, the director of the research and development staff of the U.S. Labor Party, and a national committee member of the National Caucus of Labor Committees. On behalf of the FEF, Levitt testified at the Republican convention in August on the necessity for a strong fusion plank — which the party subsequently adopted.

Levitt has authored a number of articles for scientific journals on atomic physics, especially in the field of electron-atom collisions and a recent article on "Certainty and Uncertainty: the Incoherence of Physicists" appeared in *The Campaigner.* Levitt is continuing his investigations of the origins of thermodynamics and quantum theory.

Dr. Steven Bardwell heads the plasma physics section of the Fusion Energy Foundation. Bardwell graduated from Swarthmore College in 1971 and received a PhD in plasma physics from the University of Colorado at Boulder in 1976. He is a member of Phi Beta Kappa, Sigma Xi, and the American Physical Society.

Bardwell's thesis research was on the theoretical treatment of the interaction between intense electric fields and plasmas, and he has produced several papers based on this research. Four of these papers were delivered at meetings of the American Physical Society, one at the last meeting of the International Geophysical Union, and one will be published in the *Astrophysics Journal.*

In addition to his work with the FEF, Bardwell is a member of the research and development staff of the U.S. Labor Party, and he has lectured extensively in the last three months to both lay and technical audiences. His current research is concentrated on the large variety of self-organizing phenomena that appear in almost all kinds of plasmas and the implications of this research for the philosophy of science and contemporary physics.

Frontiers of Plasma Physics II

The History of the Theory and Observation of Ordered Phenomena in Magnetized Plasmas

Dr. Steven Bardwell

I. Introduction

In Part I, I outlined a methodological overview of the importance of the observation of globally ordered phenomena in plasmas. The basic point I made in that article (which appeared in the FEF Newsletter, June 1976) is that an adequate understanding of plasmas demands our dealing with the self-defined structures that a plasma generates. In fact, there is good evidence, developed here, that the ordered phenomena in a plasma will provide the basis for a reconceptualization of our present physics.

The explanation of the highly nonlinear, globally coherent behavior of matter in a plasma state not only is key to a reformulation of physics on a nonentropic basis, but also is necessary for the development of fusion power. This fact, still denied by the controlled thermonuclear planning bureaucracy, is becoming clearer as almost every line of research runs into these phenomena when energy density increases. It is time to see these ordered phenomena — filaments, circulation cells, and so forth — as more than anomalies and, instead, to begin to use the self-organizing features of a plasma to achieve fusion conditions.

The occurrence of structures like solitons, vortex filaments, and plasmoids in the laboratory presents tremendous problems to a scientist whose outlook on physical phenomena is as much a function of the formalism used to understand those phenomena as the formalism is a product of the laboratory evidence. There is very much a reciprocal relationship between the facts and the theory used to understand the situation in which these facts occur. This principle, while almost a truism in many contexts, has had serious and immediate effects in the areas of plasma physics in which the predominately ordered behavior of high-energy plasmas occur. In fact, with the notable exception of a handful of physicists whose qualities otherwise are the proverbial proof of the rule, plasma physicists have looked at, but not seen, these structured plasmas for over 20 years now. Yet it is only recently and sporadically the case that the fundamental conceptual challenge of plasma behavior has been recognized by physicists outside of this handful.

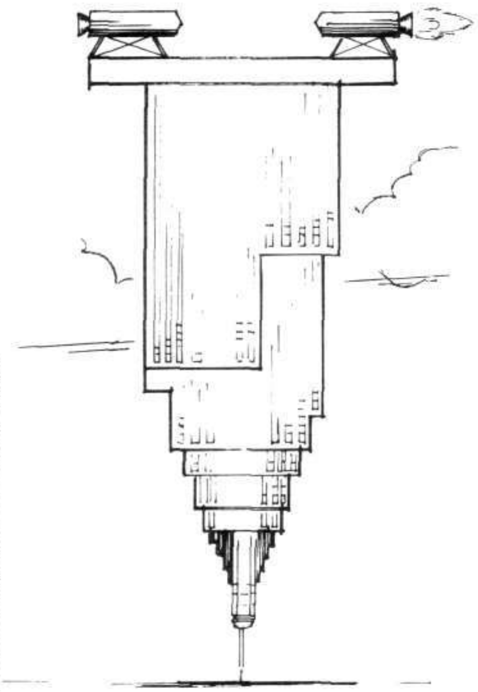
Certainly, the point of this article is not self-flagellation. A critical reevaluation of the recent history of ordered phenomena in plasmas is demanded by making the claim of primary importance for a class of experimental and theoretical results that have been around for a long time (by the time scale of the existence of plasma physics).

The Question of Seeing Order

The basic problem here is the question of seeing order. On the broadest level, contemporary physics as a whole has great difficulty with a description of ordered systems in anything other than an ad hoc way. A number of examples of this difficulty are shown in the historical survey below.

The problem arises from the general hegemony of a statistical approach to physics. While a study of Ludwig Boltzmann's work shows that the original statistical mechanics was formulated very carefully on exactly this point, it has come to be an article of faith in modern physics that conclusions, valid for a collection of particles with a small interaction energy (based on the tendency toward randomness) are applicable to any physical system, almost independent of its size or interaction. Perhaps the baldest form of this assumption is in the

A well-known plasma physicist summed up the problems of treating a plasma according to the strictures of entropic physics, by noting that there are two ways of building the Empire State Building. The first, pictured at right, is to build from the top down being careful at every point to arrange a series of rocket engines and suitable feed-back mechanisms so that the building is kept stationary. The second, and more usual, is to take advantage of gravity and build from the foundation up. There is a similar situation in present day fusion research. Without a doubt we will be able to build a fusion reactor on the assumption that there is an accessible high-energy equilibrium state for a plasma — we will muster enough ingenuity to make this thermonuclear approach work. However, the alternative of taking advantage of the highly nonlinear, self-structured nature of a plasma will have to be the basis of second-generation fusion technology.



axiom of statistical mechanics that equal areas of phase space have equal probability. This is the axiom that implies that the most random, most disordered state is the most likely. The key assumption here is of linearity (or the smallness of the interaction energy). It is an assumption simply not applicable in most systems and very emphatically not applicable in plasmas (especially plasmas with high density and temperature).

The results of this presumption of lawful disorder on the contemporary physicist are profound. As we shall see, plasma physicists have over and over again, seen plasmas in a magnetic field interact in such a way that the plasma and field form a self-contained, globally ordered structure (which, in addition, has turned out to have a detailed internal structure), but they have reported these findings, when reported at all, as imperfections in the experiment.

Even without a detailed look at these instances, it is clear that the present fusion research program (which is premised on the unimportance of these phenomena) is directed toward the achievement of a structureless, quiescent plasma. The Tokamak, for example, demands such a plasma, which, indeed, must be able to be in existence for between one and ten seconds, for success in this approach to fusion. (I believe that our ingenuity will be able to accomplish this formidable task, but that it will occur in spite of the plasma, and not by virtue of taking advantage of the natural and stable configurations that a plasma can support.)

The second, and connected, aspect of the difficulty in the recognition of these structures, is their highly dynamic character. In all the examples of these self-differentiated structures in plasmas, the plasma has, at best, what Wells, one of the foremost researchers in the field, has called "dynamic stability" (1969, 1970). At the very least, the structures move in relation to the plasma surrounding them, and they do not exist in a static equilibrium. Even the simplest observations of field reversal (Elmore 1958; Kolb 1959; Little 1961; Boyer 1960; Mjolbness 1961), rotation observed (Rostoker 1961; Linlor 1961), or circulation cells (Okhawa 1961; Bodin 1962; Barney 1966; Dory 1966; Harries 1968; Yoshikawa 1968, 1969a, 1969b) depend on a willingness to impart a fundamental significance to transitory and even disruptive phenomena, in a plasma which, it is hoped, can be confined for long times.

In part the structure of financial support in fusion research, where the money (and time pressure) is invested in projects that must show the possibilities for static confinement of the plasma, means that dynamic phenomena have been looked at as undesirable, or somehow, not of intrinsic and profound interest. Thus, as we shall see, none of the investigators listed above makes more than passing remarks about the really remarkable phenomena they saw. There are also a significant number of scientists who saw the phenomena but, because of this pressure, did not report them.

Equilibrium and the Process of Development

The connection between the assumption of both the importance and relevance of some static equilibrium and avoidance of ordered (dynamic or otherwise) structures follows from the comments I made above about statistical mechanics. In addition to the assumption of a tendency toward randomness, statistical mechanics results in the conclusion that an equilibrium state is possible. It has been shown (and it will be discussed below) that for a large and important class of systems, even the assumption that there exists an accessible and stable equilibrium is false. This remarkable result is due to Onsager (1949), and some of its consequences will be dealt with later. Without the possibility of a stable equilibrium state, the entire quality of explicable physical phenomena changes — and it changes exactly in the direction indicated by the widespread occurrence of dynamic and ordered behavior of plasmas.

Another way of approaching this problem of methodology is to contrast the various meanings of the word "why" in physics. All too frequently, the accepted answer to the question "why" some phenomenon or another exists in a plasma is based on an entirely ad hoc, qualitative description of the simple physical processes at work. This is clearly inadequate by itself but, unfortunately, this is often the standard of rigor, especially in interpretation of experimental work.

Several levels above this are the attempts to rigorously explain physical phenomena on the basis of (usually classical) contemporary physics. While it is questionable whether statistical mechanics, for example, is ultimately descriptive of the nonstatistical behavior of a plasma, there can be no objection to trying to drive the consequences of this theory to the limit of applicability, and in so doing to identify the exact points at which the present theory breaks down. This type of investigation is indeed essential in assessing the reformulation of science demanded by the phenomena I have identified. It is not sufficient, however.

There is a provocative analogy to be drawn with biology, that indicates some direction about the kind of answer adequate to a "why" about structure in plasma physics. In biology almost every question about a structure or pattern of events is answered by a statement about function. The specific part that a mitochondria plays in the metabolism of a cell, to take a specific example, is the answer to "why" this structure is present in almost every cell. The key to this methodology is identifying a process of development, a specific chain of dynamic processes in the context of which the cell is defined as a whole and its parts are subsumed by that process. The question yet to be answered for the structures in plasmas is what such a process looks like. The most advanced work done on the dynamics of plasma-structures implicitly assumes an answer to the question of the nature of this dynamic. Wells (1969, 1970), for example, is quite clear on his assumption that the self-development of structured plasmas is an inherent property of plasmas; and he proceeds with a thoroughness and rigor in his global and time-dependent treatment of these structures which is unrivaled in the field. Wells does not, however, articulate the essence of the processes that give rise to such remarkable phenomena. I will say more below about his work in relation to other work in the field, but the point I want to make here is that, even with Wells' work, the centrality of the function of these structures remains for the most part an unasked question.

These conceptual barriers to appreciation or observation of self-created, coherent structures have largely determined the history of scientific study of ordered behavior in plasmas. In the case of each now-identifiable type of such behavior, the completed research falls into two generally identifiable periods: the first, in which a large number of observations of the phenomena are reported, but where no conceptual insight into the significance of these anomalies is evident; and the second, an almost complete disappearance of observations of the phenomena, except by a very few investigators who go on to study the phenomena experimentally and theoretically quite consciously as a highly ordered and structured class of behavior.

Unbelievably, it is a fact that after one researcher begins to conceptualize the data in a way that makes clear the extraordinary nature of the phenomena, interest in the field usually declines rather than increases! This is an interesting empirical fact in itself, and given what I have said above about the interaction between a theory and the so-called facts that support it, it should be no surprise. In any case the pattern is consistently evident in the altogether too checkered history of plasma physics.

II. The Prehistory of Ordered Phenomena in Plasmas

There are four distinct lines of research on self-defined structures in plasmas:

(1) **Solitary waves, solitons, modulational instabilities, and a large class of like behavior.** These all occur without the imposition of an external magnetic field. Without any reflection on the importance of these effects, I am restricting my discussion here to phenomena in magnetized plasmas. Therefore, none of the most recent literature in this field is relevant here. The general conclusions indicated by the similar phenomena in magnetized plasmas, however, hold for the investigations done so far into soliton-like structures. (See Scott, et al. 1973 for a review.)

(2) **Statistical studies of plasma turbulence.** This line of research began in 1949 with the above-mentioned paper by Onsager. After a 20-year hiatus in which the results were confined to fluid mechanical studies (that is, fluid motion with no magnetic field), Onsager's is the only work that has inspired more than a few current investigations.

(3) **Fluid studies of self-generated vortex motion.** The stability and dynamics of vortices in a nonmagnetic fluid is old (Lamb 1936), and there has been a small but consistent study of similar "smoke ring"-like phenomena in plasmas.

(4) **Studies of the filamentary structures in plasmas.** These structures have sparked some astounding experimental work, especially in the study of the plasma focus machines, in the United States, Soviet Union, and Europe. However, as the viability of the pinch effect as a fusion device declined in the general opinion of the fusion community, research on these very short-lived phenomena almost dissappeared.

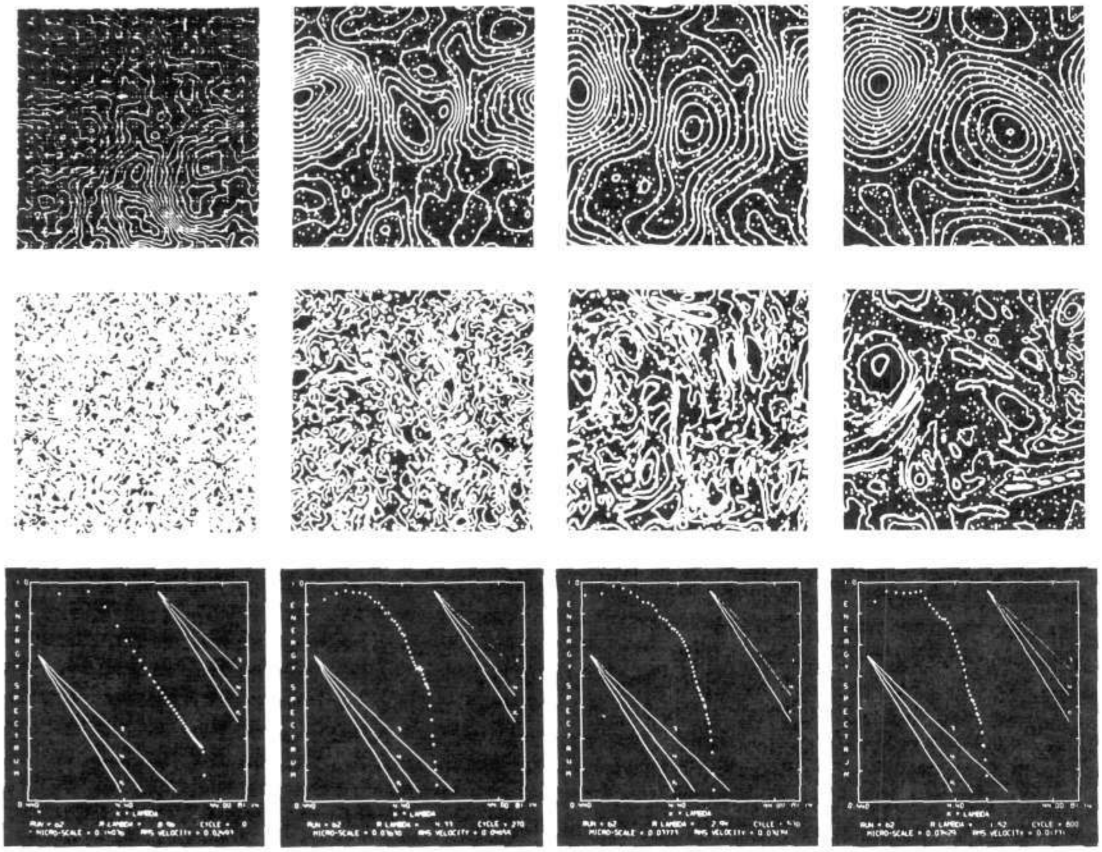
Statistical Studies of Plasma Turbulence

Turbulence in fluids has been studied for 100 years, starting with the experiments of Reynolds. The essential developments for the study of plasma turbulence began with a series of papers in the turbulence in fluids that characterized two-dimensional flows. By far the most remarkable paper was written by Onsager in 1949. Onsager remarked on a peculiar property of two-dimensional fluids made up of parallel vortices and shows two things about such a model system: First, no stable, quiescent, homogeneous equilibrium is possible if the energy in the system is large enough. This is because above a minimum energy (which turns out to be zero), the system is characterized by a negative temperature; and as Landau and Lifshitz showed (1968), no equilibrium in the conventional sense can exist for such a system. Second, and even more important, Onsager showed that this system tends to evolve in such a way that vortices of like rotation clump together. Specifically if one started out with a two-dimensional fluid in any sort of random motion and with sufficient internal energy (which, it turns out, can be mathematically analyzed in terms of a collection of vortices of different strengths), it will evolve until there are only two, large, counterrotating whirlpools left. As Onsager said "our system has some unusual properties."

The very significant importance of Onsager's result has been largely ignored in the 25 years since it was discovered. Onsager has proved a very strong converse to the H-theorem of Ludwig Boltzmann. Boltzmann demonstrated that for systems whose energy of interaction is small compared to the energy of the individual

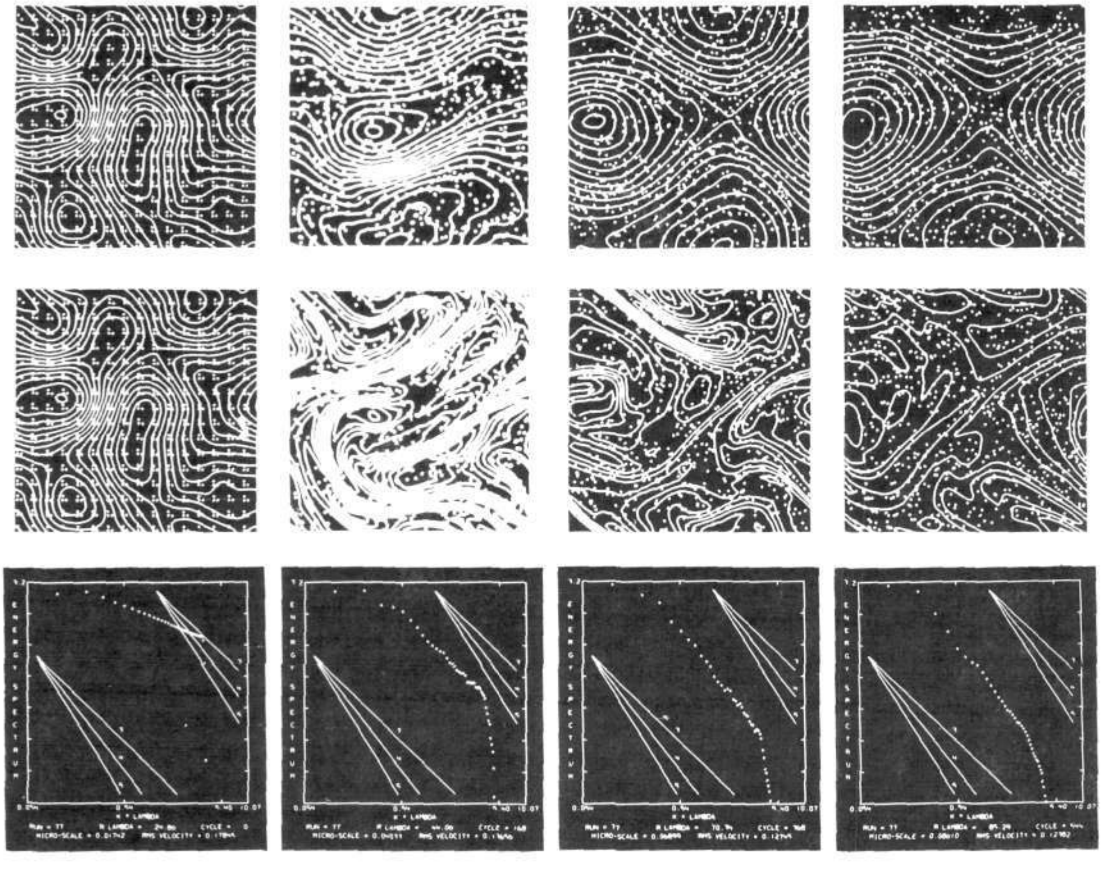
Computer-generated solutions to the fluid equations in two-dimensions from Tappert (1971).

Time moves from left to right. The upper frames are stream lines of the fluid. The second row is of the vorticity, a more sensitive measure than the stream function of the small-scale motions of the fluid. The third row shows the energy spectrum as a function of inverse length. Qualitatively the general tendency for small vorticies to clump together into large ones is evident. The spectrum shows this qualitatively, as the predominant motion is for energy to shift from right to left (from small length scales to large).



Computer-generated solutions to the magnetohydrodynamic equations in two dimensions from Tappart (1971).

As above, time moves from left to right, and the top row is stream lines. Here, however, in the plasma the second row shows magnetic field lines. The third, again, shows the energy spectrum. Notice, in addition to the formation of large vortical structures, the way that the stream lines and magnetic field lines orient parallel to each other to form a force-free structure.



elements (otherwise known as a linear system), the conventionally understood laws of dynamics and associated conservation laws imply the Second Law of Thermodynamics. That is, he was able to prove that any such linear system would approach an equilibrium state, characterized by maximal randomness. Boltzmann, however, recognized the centrality of the assumption of linearity, and he speculated on the kind of statistical mechanics that might be necessary were one to describe a nonlinear system.

The same is not true of subsequent authors who have ignored the importance of the assumption of the small interaction between the elements that make up the system. Although he took a special case, Onsager showed a very important class of behavior — one that characterizes nonlinear systems. Onsager's system of interacting line vortices, in fact, has no energy except energy of interaction (that is, there is no kinetic energy of the vortices), and so it is, by Boltzmann's definition, a highly nonlinear system. Hence, the behavior of this system shows that nonlinearity can imply not only a lack of an accessible equilibrium but even more, spontaneous evolution toward ordered states. This is remarkable! Nonlinearity in this example at the very least implies that the two critical assumptions of statistical mechanics break down: There is no static equilibrium possible, and there is no natural tendency toward more random configurations — a strong converse to the H-Theorem.

The immediate result of Onsager's 1949 paper was the establishment of a long line of studies of two-dimensional turbulence. Especially worthy of mention in this regard is the work of a number of investigators who tried to rigorously understand the possibility of an inverse cascade of energy, that is, the tendency for small vortices to clump together into large ones contrary to the usual tendency in fluids for turbulence to progress from large-scale lengths to small. (Fjortoft 1953; Charney 1962; Corrsin 1964; Kraichnan 1967, 1971, 1973, 1974a, 1974b, 1975; Leith 1968, 1972; Lilly 1969, 1971; Batchelor 1969; Saffman 1971) The chief result of this work, before 1972, was the analytic treatment of two dimensional turbulence as an idealization of three-dimensional problems, a treatment which resulted in the identification of constants of the motion for the fluid dynamics whose conservation resulted in a cascade of energy to longer length motion. None of these investigations, however, dealt with a basic examination of the remarkable implications of this result along the lines mentioned above.

The Application to Plasmas

In 1971 two observers independently noticed the implications of these fluid results for plasma physics. Tappert and Hardin did a number of as yet unpublished computer solutions of the two dimensional fluid equations and magnetohydrodynamic (MHD) equations that showed an astounding persistence of the tendency toward the formation of large-scale motion out of small. It should be mentioned that Tappert's computer movies also show very impressively the tendency toward self-generated magnetic fields in force-free configurations (that is, the tendency of the magnetic field lines to line up with the stream lines).

The informal circulation of these results, which very clearly supported the results of Kraichnan and others, and the soon-after published computer generation of purely fluid solutions (Deem 1971; Lilly 1971) started work at the University of Iowa on the application of these results to plasmas. Remarkably, the dynamics of a plasma in which the magnetic field is very strong (a low beta plasma) is identical to that of the two-dimensional vortex fluid. This fortuitous result, based on work by Taylor (1971) on the so-called guiding center approximation, allowed a mathematical analogy to be established between a fluid made up of vortices and a plasma in which the magnetic field restricts the plasma motion across the magnetic field to that of the long lines of charge that form on the magnetic field lines. With this mathematical similarity, Montgomery (1972a, 1972b,) and others (Vahala 1971) looked at the statistical mechanics of a guiding center plasma and in 1973 showed computer simulations (not just solutions to the model equations) of the effects of negative temperatures in a guiding center plasma. (Joyce 1973) This work was followed up by a number of other investigators. (Taylor 1972, 1973; Edwards 1974; Frisch 1975; Seyler 1975; Fyfe 1975)

The most suggestive of the results from these investigations are contained in the latest paper from the Iowa group (Fyfe 1975). In this paper, results previously restricted to low beta plasmas are generalized to the case of an arbitrary ratio between the energy in the magnetic field and the plasma particle flow. Thus, they treat the general problem of two-dimensional plasma turbulence analytically by applying the statistical mechanical methods that were previously restricted to the guiding center approximation. The results of this treatment, to date the most general of a large class of similar work (including three-dimensional plasma modeling by Frish, et al.) are of the same, striking quality that Onsager's work first suggested: The plasma has a tendency to order itself as more energy is put in. Montgomery and others have noted the fact that this should provide a way of describing self-generated magnetic fields, filamentary structure, and general vortex motion observed in a plasma, although there is almost no indication in these papers of the persistence and centrality of the structures that they are predicting theoretically. In any case, to summarize the results from all these studies, there is almost without question a transfer of turbulent energy to large scale vortices, a tendency which becomes more pronounced

> *"...All concepts of phenomenology are derived from quasi-stationary processes and no longer hold good for turbulent motion...if every volume element of the body has a different motion...it is likely or at least possible that the different energy forms can no longer be sharply separated." (Boltzmann 1897)*

as the energy in the plasma increases.

Unfortunately the work that has come out of these studies remains only suggestive. There are a number of important aspects of the phenomenon of a self-organizing plasma that have not been touched on in these studies and that, as I will show in analyzing other aspects of the observation of such behavior in plasmas, are essential in plumbing some of the implications for physics. The difficulties stem from the consistent avoidance of the original implications of Onsager's work. This is especially interesting since the implications of turbulence for statistical mechanics **were** remarked on by von Neumann in 1949, and his ideas must have had some currency at the time. *(1)* Von Neumann wrote the following about the results that Onsager and others had produced in the statistical theory of fluid turbulence:

> From the point of view of theoretical physics, turbulence is the first clear-cut instance calling for a new form of statistical mechanics. The existing theories (especially that one of Kolmogorov-Onsager-Weizsacker) suffice to show that these laws (for energy transport — ed.) will differ essentially from those of classical (Maxwell-Boltzmann-Gibbsian) statistical mechanics. Thus it is certain that the law of equipartition of energy between all degrees of freedom, which is valid in the latter, is replaced by something altogether different in the former.

The Implications for Statistical Mechanics

Perhaps because von Neumann's review of theories of turbulence — from which this quotation comes — was not published until his collected works appeared in 1967, his ideas were forgotten. In any case, it is essential to draw as clearly as possible the implications of the statistical treatment of plasma turbulence for statistical mechanics. There are three specific repercussions of epistemological significance that are immediately identifiable:

(1) **The idea of entropy.** In a system with a negative temperature and no accessible, stable equilibrium, entropy and energy (see below) must be reconceptualized to take the inherently dynamic nature of the system into account. Landau (1968) showed that the increase of entropy in a system, the nonnegativity of temperature, and a quiescent equilibrium are inextricably intertwined. The fact that the system as a whole (and not just a weakly coupled internal degree of freedom) is supporting a negative temperature means that the conventional idea of entropy, as a measure of the direction of spontaneous change in the system, breaks down. This is not actually surprising, since entropy is an essentialy local measure of the state of a system, measuring local order and disorder. Therefore, it is easily possible (and disconcerting) to have increasing entropy at the same time that there is the formation of large-scale ordered motions of the plasma. This is inadequate. *(2)*

(2) **The definition of energy.** The question of energy is more difficult. Drawing on the analogy with biological systems hinted at above, energy, to be adequate to describe an evolving system, must be derived explicitly from an understanding of that evolution and must be related to the energy **flows** that produce such an evolution. The implications of a social analogy are discussed in various works on economics (Marcus 1975). Without a detailed understanding of the evolution a plasma is carrying out, energy flows in this directed sense are difficult to conceptualize competently. Time-independent, equilibrium-statistical treatments are obviously inadequate. Indeed, almost all workers in this field have noted the problems that arise for the idea of energy when there is no proper thermodynamic limit for the phenomena in question. In the Onsager model, for example, the energy of the plasma has no thermodynamic average (in the sense that the time average and the most probable state of the system coincide), and large variations in the energy of any given mode occur for arbitrarily large times.

In addition to being an indication of the lack of equilibrium, this points toward the formation and existence of these large scale ordered vortices in relation to energy flows among the different modes. These flows go on, in some complicated way, indefinitely. If what I have said above is correct, then a study of the details of these flows of energy is essential, and the work that has been done so far relying on time averages of the energy in a mode must be seen as a first approximation to a dynamic picture of the plasma.

(3) **The question of internal structure.** Given a system of the sort I have described, the detailed structure of the state of the plasma assumes great importance. (This is not the case for a hypothetical equilibrium state.) As will become more evident from the subsequent discussion of other ordered phenomena, microscopic, seemingly marginal, phenomena frequently play a critical role in the evolution of the global properties of the system. A static equilibrium analysis based on statistical considerations has prevented any study of this sort of internal structure. In fact, it is generally a feature of metastable or dynamic, nonlinear systems that their evolution depends on phenomena that are at most marginal at some time before critical causal intervention of these phenomena

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(1) A somewhat technical note about the difference between fluid and plasma turbulence is in order here, since von Neumann himself tried to downplay the critical significance of Onsager's model. Von Neumann ascribed the anomalous character of the Onsager results in some degree to the fact that his model is two-dimensional, and hence, the fluid has additional conservation laws, specifically, of vorticity, that account for the generation of the large-scale vortices. For fluids, von Neumann is almost undoubtedly correct — three-dimensional fluid turbulence does not, in general, exhibit the self-organized behavior that the two-dimensional model predicts. However, for a magnetically active fluid, a plasma, the opposite is the case. This is the significance of the paper of Frisch, et al. (1975), where they show that the additional conserved quantity of a plasma, the magnetic helicity, gives rise to cascades of energy similar to the two-dimensional fluid case where energy is transferred to longer scales. As will become clear below, a magnetic field often acts as a globally ordering effect, here by decreasing the effective dimensionality of the problem.

(2) Mathematically, this difficulty shows up in the fact that the two-dimensional system of vortices has temperature and entropy that cannot be rigorously defined to be independent of the volume or shape of the boundaries. In this sense, the system has no thermodynamic limits.

can rapidly shape the pattern of change for the system as a whole.

Fluid Studies of Self-Generated Vortex Motion

Von Neumann divided the study of turbulence into two general approaches, statistical ones and those based on stability. In the previous section I examined some of the statistical approaches to the problem. The prior approach historically concerned the stability of various structures and the conditions that undo this stability. There is also a line of historical development of the study of ordered phenomena in plasma that derives from this classical stability study. This classical treatment is most clearly laid out in Sir Horace Lamb's book on hydrodynamics (1936) where he described a large number of studies done of the structure and stability conditions of various hydrodynamic configurations, most of which involve vortical circulations.

The point I want to stress is that the methodology of this work is based on an assumption of the central nature of structure. For such an analysis, one studies a posited equilibrium pattern of flows and then looks at perturbations of this pattern. From the usual kind of treatment, one can then predict the kind of small waves this equilibrium will support and, more importantly, one can calculate whether any of these disturbances of the equilibrium grow or are damped. If the hypothesized equilibrium can support a growing wave, it is not a legitimate equilibrium and the system is linearly unstable. The difficulties that such a method brings with it will be noted below, but I want to emphasize that this approach starts with a very healthy respect for the system's ability to find such structured, globally ordered states and only then goes on to look at the behavior of the fluid. It is this element from classical fluid mechanics that has most clearly shaped the study of structure in plasmas.

The first studies in this sort of analysis of a plasma were done by Harold Grad in 1954 — although they were not published until later (Grad 1957) — as the beginning steps in the then-classified Project Sherwood. Grad's work was a very insightful transposition of the classical theory of fluid stability into the case of a conducting fluid — a plasma. Almost simultaneously, a similar methodology prompted several investigators to attempt to duplicate experimentally the phenomena of self-supporting plasma structures in the laboratory. It is important to note that there are two directions this work took from Grad's initial theoretical work. One was the direction of building machines that generated the magnetic fields, circulations, and so forth, that were predicted to lead to stable confinement schemes. The other was in the direction of attempting to generate plasmas in the laboratory that assumed the stable configurations on their own. The former were the guts of the first (and subsequent) research on controlled fusion power, but it is the latter that concerns us here. The first work on this was done by Winston Bostick and coworkers at the Lawrence Livermore Laboratories in California. (Bostick 1955, 1956a, 1956b, 1957)

Plasmoids

This line of study resulted in a description of a class of collective modes of a plasma, plasmoids, that were very distinctly vortex-like, and that lent themselves quite naturally to a classic analysis like that done on smoke rings, free vortices, and so forth. Bostick's experimental work showed quite dramatically the stability of smoke rings and balls of plasma and also gave evidence of a complicated pattern or current flows and self-generated magnetic fields that provided for this stability. This line of reasoning led to a flurry of papers in the Soviet Union, which focused on the phenomenon of ball lightning (Ritchie 1961). The work of Shafronov (1957) and Ladikov (1960), especially, used the experimental evidence provided by ball lightning and Bostick to infer the stability of rings, balls, and the like for a magnetically active fluid.

Another, simultaneous, theoretical line of development occurred with publishing of papers by Chandresekhaar (1956a, 1956b, 1958) and Woltjer (1958a, 1958b, 1958c, 1959), who, starting from some very preliminary work done earlier on astrophysical problems (Lundquist 1952; Lust 1954; Kruskal 1954; Ferraro 1954), indicated the importance of magnetic fields in a structured medium. These papers applied a variational method not only to determine the structures that might be stable, but also to predict the most stable kinds of structures a plasma might assume. These two approaches to the classical study of stable equilibrium structures cohere neatly. Together, they laid the basis for a significant amount of experimental work on the structure of the currents and fields that went to make up plasmoids and like phenomena in a Q-machine (Mosher 1970), in plasma guns (Waniek 1960; Alfven 1960; Lindberg 1960, 1961; Hogberg 1961; Komelkov 1961; Wells 1962, 1963, 1964, 1968; Jones 1966, 1967), in theta pinches (Clark 1962; Bostick 1963) in linear multipole systems (Ohkawa 1961; Dory 1966; Filimonov 1966; Barney 1966; Harries 1968; Chambers 1972), in torroidal octopole machines (Harries 1969, 1970a, 1970b; Yoshikawa 1969a, 1969b, 1969c), and theoretical work (Schmidt 1960; Dolique 1963; Busemann, 1965; Mercier 1966; Poukey 1967).

For accuracy, I will qualify some of the generalizations made above; not all the experimental work done on plasmoids took advantage of the point of view of classical hydrodynamic stability theory. A significant amount of this work was based on a study of the movements of plasma in a magnetic field (specifically on the motions possible across a magnetic field). As a number of investigators showed for example, Schmidt (1960, 1962), such motion might develop into a plasma structure that trapped magnetic fields and led to circulation within the plasma (Elmore 1958; Lindberg 1961; Dolique 1963; Bostick 1963; Poukey 1967; Chambers 1972). The observation of circulation cells in linear multipole systems is a spectacular instance of a rather amorphous self-generated structure.

This line of investigation tended to see any structure in the plasma as much more of an epiphenomenal, rather than essential, feature of the plasma. Indeed, in the cases where such nonthermal behavior was observed in a machine destined for use in the controlled thermonuclear program, the structure was treated as an anomaly that the machine and its designers had to smooth out! Thus, for a short while, the question of diffusion across a magnetic field was studied theoretically (Dawson 1971; Okuda

1973), and resulted in predictions of nonclassical diffusion due to vortices in the plasma *(3)* These investigations, however, were very much from the point of view that this anomalous behavior which entered calculations and studies of a plasma had deleterious effects for plasma confinement and had to be erased. Thus the possibility that these are in some way the more basic features of the plasma dynamics was ruled out from the beginning.

Vortex Motions

A quite opposite approach has led to the most significant work on vortex motions in plasmas. A student of Bostick, Daniel Wells, after several years of careful experimental work on self-contained and self-generated structures in plasmas (Wells 1962, 1963a, 1963b, 1964, 1966, 1968), published two mainly theoretical papers that outlined a very general and powerful theory of these structures. Starting from an assumption that these structures were an essential pattern of behavior in a plasma anytime that it had a nonzero velocity (whether or not it was traveling across a magnetic field), Wells' experiments showed hat the characteristics of such a moving plasma were highly repeatable and exhibited several recurring characteristics as follows:

(1) **They were force free.** This necessitated that the magnetic field lines, the current density, the vorticity, and the fluid streamlines be parallel to insure a zero-Magnus and Lorentz force density. This force-free structure is highly reminiscent of Alfven waves, and Wells initially speculated that these vortices were nonlinear stages of Alfven waves.

(2) **They contained closed currents and trapped magnetic fields.** Even qualitatively, this demands some sort of vortex configuration, and Wells' work reproduced long-standing results on the stability of vortex motions in both cylindrical and spherical geometries.

Based on these general observations, Wells develops a variational approach to determining the three-dimensional, low-lying, dynamic states that a plasma can support. I quote at length from his 1969 paper which clearly states his approach:

> What actually happens in any real plasma experiment in the laboratory is that the plasma is produced in the machine or injected from the outside the plasma in a violently dynamic state; i.e. the center of mass of a typical fluid element in the plasma is usually in a state of rapid motion. In this dynamic state the plasma interacts with the externally applied electromagnetic fields as well as any fields trapped in the plasma themselves and proceeds to lose energy until it either reaches a state of dynamic equilibrium in which the currents can decay without producing a violent mass motion in the plasma, or the plasma decays into a configuration which is unstable and anomalous diffusion or "pump out" results. The point is that the currents and-or fluid motions inside the plasma continue until the plasma is completely thermalized or until the plasma leaves the magnetic bottle at some earlier time owing to gross instability. If the plasma is to degenerate from a state of initial dynamic instability to one of hydrodynamic or hydrostatic stability, then it must be possible to calculate by some variational procedure the allowable stable configurations which the plasma can assume. If there is dynamic energy in the plasma when it is first produced, then this energy must be directed into a dynamically stable or statically stable configuration which exists until all currents decay and the plasma is thermalized. If the plasma is actually produced in a quiescent state, then the currents within the plasma must not generate mass motions and currents as they decay (unless, of course, the resulting current and flow structure is stable). If a thermonuclear reaction is to be sustained within a plasma which is confined by a magnetic bottle, the confining action will be effective only so long as the currents within the plasma are interacting with the externally applied fields. Complete thermalization, of course, results in complete loss of plasma. Thus it is during the dynamic decay period after injection or heating that one must calculate the stability. If the stability is to exist in a static configuration, then static stability must be shown to evolve out of the original dynamic state. The assumption that the system is conservative (i.e. that the effective potential is not an explicit function of the time) is justified only for a limited number of special configurations and plasma parameters.

This identifies the problem of dynamic systems very neatly.

My purpose here is not to present a technical assessment or summary of Wells' work; I want to point out the most important aspects of the approach he takes and take the shortcomings evident in his papers as a guide to the directions our attack on the problem must take. The most obvious distinction in Wells' approach is his stress on the importance of dynamic phenomena. As Wells notes, on the simplest level the disregard of the $(\mathbf{v}\cdot\nabla)\mathbf{v}$ term in the momentum equation of MHD (it is the source of the nonlinearity of the equation) leads to gross errors when the plasma has some zero order velocity, or develops such a velocity on a macroscopic scale (as we saw in the two-dimensional case). Including this term, however, leads to great mathematical difficulties, and Wells' treatment has the virtue of accepting the consequent and unavoidable nonlinearity of the equations. He then goes on to develop

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(3) My history of plasma behavior suffers an odd sort of diffusion at this point. The lines of investigation that I have separated as statistical studies and as stability studies cross paths with this question of cross-magnetic field motion. The first time the guiding center model of the plasma was used importantly was by Taylor (1971) in a paper called "Plasma Diffusion in Two Dimensions." This work was then used by Montgomery and his co-workers for an analogy with Onsager's paper. It is interesting that with Montgomery's work, the whole direction of the question of diffusion gets changed to one of investigating the origins and importance of the vortices that Dawson and Ohkawa also predict: diffusion becomes a much secondary question. At this crossroads in our story, the circulation cells are briefly noted as evidence of both the diffusion and tendency toward vortex formation. Even so, it is reasonably clear that this epistemological diffusion is anomalous — these currents of investigations have not yet crossed again.

an analytical procedure that is based on this nonlinearity. It is only out of a head-on grappling with the nonlinearity of the system that his treatment is able, for example, to show the full generality of the force-free type of structures.

The most fundamental contribution Wells made in these papers is to develop the interconnection between a variational principle, the geometric and dynamic symmetries (various gauge symmetries), and the boundary conditions on the plasma structures. I am unable to give a qualitative description of the results that Wells derives, but their implications for the theory are reasonably straightforward:

(1) The identification of relation between *conserved quantities* and symmetries is old, and has been applied to MHD. Wells extends this work by showing the very important property of the uniqueness of the list of conserved integrals that he derives. This is a problem that arises in all MHD theories relying on conservative theorems. (See Fyfe 1975, for example.)

(2) The question of what boundary conditions give a complete, but not overdetermined problem, is, in general, unsolved for the fluid equations. However, Wells has introduced a new way of dealing with the problem by focusing attention on the critical relation between the boundary conditions of a plasmoidal structure and its stability. Thus, for example, the question of conservation (or not) of angular momentum becomes a critical feature in the theory of cylindrical vortex structures.

(3) Wells' emphasis on global properties of the system has put in the forefront the question of effect on the magnetic field and boundary conditions in structuring not only the geometry of the plasma, but also its dynamics. Physically, this leads to the idea of the magnetic field of the external coils functioning as a force-bearing element for the plasma structures.

It is at this point, in fact, that Wells' work is unfinished. The questions concerning the details of nonsuperposable flow and the difficulties remaining in describing in a completely satisfactory way the boundary conditions on the plasma structures inside the plasma, have remained unanswered in the seven years since Wells' papers were published. It should also be noted that Wells, while deriving a dynamic, nonlinear equilibrium state, uses a formalism which does not deal with the approach to this equilibrium. His treatment is dynamic in that the final state he describes is not static, but the question of its accessibility is not dealt with. There is little question that solving this problem is intimately related to the question of the boundary conditions. It is difficult to assess the possibilities in Wells' work. My own guess is that Wells has taken the present formalism almost as far as it can go, without an *explicit* reformulation along the lines I described above. But, this is not to underplay a largely neglected, but very insightful attack on the problems of plasma structure.

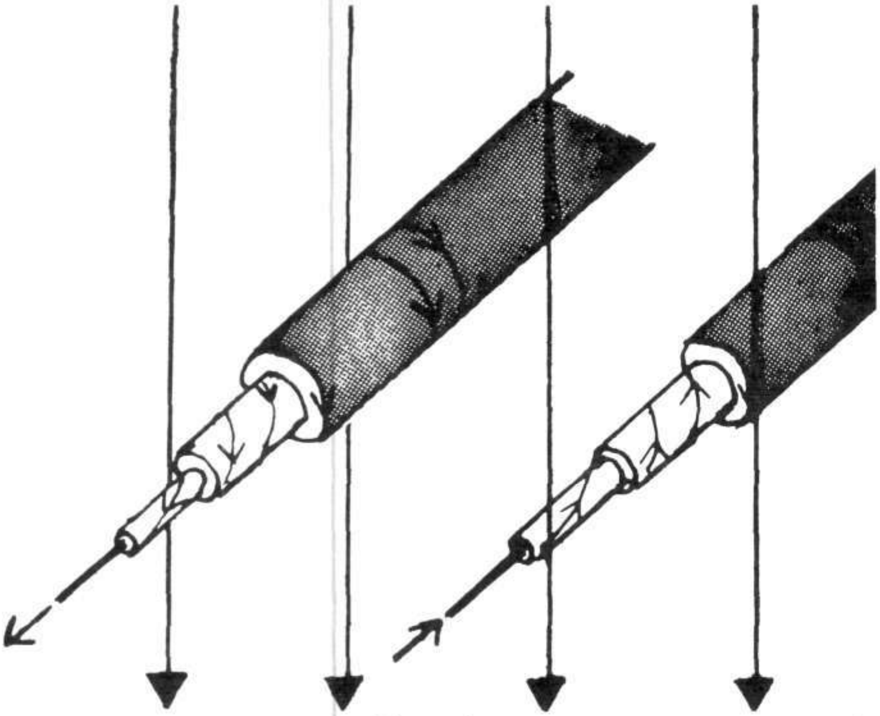

Figure 1

A schematic anatomy of a radial plasma filament. The vertical lines represent the background magnetic field. The left- and right-hand spindle-like filaments show that the convected force-free mass is made up of parallel constituents including the local magnetic field, electrical current density, fluid velocity, and fluid vorticity. (Bostick, personal communication)

Studies of Filamentary Structures in Plasmas

The last class of observed structures in plasmas is without doubt the most pathological example of the problem of seeing order in a plasma. There are several types of filamentary behavior that have been seen in plasmas, but I will restrict what I say here to those evident in the z- and theta-pinch discharges. *(4)* Here, more than in any other of these filamentary behaviors, there is unambiguous observation of

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(4) It is worth noting that the phenomenon of filamentation in relativistic electron beams is closely connected with the vortex formation phenomena. Analytically this has been mentioned by Benford (1971) and Book (1975). Numerical studies were done by Lee (1970).

the phenomena along with a decreasing interest in the phenomena as they become more evident!

Very early observations on the theta and z-pinches reported observations, by several diagnostic methods, of highly symmetrical patterns of striations that appeared as the fine structure of the current sheath. (Butt 1958; Kvartskhava 1961, 1966; Komarov 1963; Bodin 1963; Bostick 1963; Fadeev 1963; Lovberg 1965; Mather 1966a, 1966b; Kolesnikov 1966; Baconnet 1969; Post 1974; Gratton 1974; Bernard 1975) The accompanying diagrams of some of these observations show the general features of the filaments.

The initial reaction to these observations of the breakup of the current discharge in the plasma took several routes. By far the most common was to dismiss these so-called instabilities as the result of defects in the experimental apparatus and to ignore them as anything but a nuisance on that account. The introduction to a paper by the group at the Sakhumi sheds some light on this (Kvartskhava 1966):

> So far, insufficient attention is given to the experimental study of spatial plasma inhomogeneities in heavy-current discharges. (This was published in 1965.) Only a few publications are devoted to this problem; the reason is obviously that frequently observed inhomogeneities, (irregular and representing seemingly casual deviations from the expected general plasma motion) are considered by many to be the result of difficult-to-consider "fluctuations" of the initial experimental conditions. They could presumably be avoided by appropriate precautions....Not excluded, however, is the possibility that the observed irregularities of motion are largely due to a complex spatial structure resulting from a general law that is characteristic of heavy-current plasma.

It is useful to categorize in some detail the reasons that these filaments were called casual deviations:

(1) One school studied the phenomena as a subclass of general plasma rotations. In a fashion parallel to study of the cross-B diffusion, these investigators looked at the filaments and the observation of other rotating phenomena (the gross rotation of the current sheath, for example) as a structureless torque effect. (Linlor 1961; Rostocker 1961, 1962; Bodin 1962; Schmidt 1962; Lovberg 1965; Duchs 1968; Benford 1972).

(2) Another school held that the current sheath breaks up due to a set of linear instabilities, tearing modes, etc. (Furth, et al. 1962, 1963a, 1963b) The comment by the Sukhumi group is appropriate here:

> Furth and others explain the observed phenomena on the basis of developing instabilities. However, the linear theory of plasma instabilities is not adequate, since deviations from the initial unperturbed state cannot be considered small (the problem is essentially nonlinear), and their regular, quasi-stationary behavior, as concluded from the experimental data, does not accord with the general behavior of instabilities.

(3) A third school ascribed these filaments to an anomaly that was only interesting or evident in small machines. This was quite consistently the rationale for ignoring them as time went on and larger machines were built. Because of the insight that it provides, I quote an exchange between John Luce from Lawrence Livermore Laboratories and Winston Bostick on this point. (See Bostick 1975)

> **LUCE**: In regard to the striations, I have several questions. One is an old question, and that is that these striations are very much dependent upon the amount of energy that one uses in the plasma focus. That is to say, at higher energies, these striations apparently disappear, and one gets much better operation.
>
> **BOSTICK**: It is a fact that the bigger machines, when they're operating at high power, do not report evidence of striations. Even in our small machine, if we take an image-converter picture in which we overexpose the picture, we lost the striations. It's possible, by inadequate photography, to clean the striations from the film. There are some cases where we turn up the power in our machines and the striations disappear. That is, they seem to blend together. But if we take simultaneous pictures and shadowgrams, which are much more sensitive to variations in density of plasma, we can show that there are striations on the shadowgram. And, we would make the statement that even in the big machines with high power, the striations are there, but they're much more closely spaced than the optical resolution of the pictures.

The point here is clear. Without belaboring it, I note that the time and spatial resolution of any diagnostic is premised on a theory of what the significant time and space scales are. To a certain extent, however, that is a self-confirming situation. There are several exceptions to the general trends of ignoring these fine-scale and short-lived phenomena; a group in France (Bernard, et al. 1975) and at Stevens Institute (Bostick 1965a, 1965b, 1966a, 1966b; Nardi 1970, 1972, 1974) have both devoted attention to the evolution, structure, and importance of these filaments.

Filamentary Structure is Key

The essential point that both of these groups make is that the filamentary structures is the key to understanding the plasma focus. Bernard specifically says that it is the nonthermal effects, among which the filaments are the most important, that are responsible for the most spectacular of the effects that a pinch produces — the large number of neturons (whose distribution is definitely non-thermal), the hard deuteron spectrum, and the very hard x-rays. Bostick and his co-workers have done a large amount of very detailed experimental work on the fine structure of these filaments and have come up with appealing, although largely qualitative, mechanisms for the particle accelerations that produce these results.

It is interesting to note that Bostick's results confirm the centrality of force-free, vortex structure of the filaments (see Figure 1 for a more detailed description of this structure) and have been described by Nardi in terms closely parallel the statistical work discussed above (Nardi 1974). From all that has been said thus far, it is clear that the general approach of Bostick, for example, is correct, and that his experimental results cannot be disputed. I am emphasizing, however, that the methodology that treats these structures as the basic feature of the pinch machines is appropriate. Because of this, I will not examine the theoretical work that has been done either by the Russians (See especially

Komarov; Kvartskhava 1965) or by the group at Stevens. In both of these cases, the observations made above about fluid and statistical theories apply to their theoretical work.

III. Conclusion

All of this speculation about the real behavior of plasmas has an immediate urgency that cannot be obscured by the necessary discussion about the epistemology of science — the question of fusion development. Controlling the plasma that will allow the harnessing of fusion energy has, especially in the United States, been based on the assumption that only thermal ensembles would permit fusion to occur. The arguments in this regard, however, are all based on a totally unstructured nonthermal state. As I have tried to show, nonthermal states, when they occur, are not just different versions of random particle motion, but are very detailed macroscopic structures whose understanding demands a serious recounting of present day physics.

The importance of this observation is more than speculation. In the theta pinch, it has been shown that the bulk of the fusion neutrons and accelerated deuterons comes from the very small sources in the plasma associated with the collapse of the filaments. In the Tokamak, in a negative sense, the continued discrepancy between theory and experiment (In the prediction of transport coefficients, see Sleeper 1975), seems to be coming from collective motions in the machine. This is certainly the case for recent results from the Mirror machines. The centrality of these sorts of singular structures in inertial confinement systems is obvious.

At this point, a knowledge and competent understanding of ordered phenomena in plasmas is more than a strictly scientific question.

The implications of the apparent breakdown of statistical physics extend immediately into the life sciences and social sciences. This is not only true when the necessity for the development of fusion is considered. The fact that living matter and the processes that gave rise to life are part of the same universe described by physics demands that the laws describing physics be ultimately coherent with the highly ordered and self-ordering processes that make life possible. A physics that starts from the Second Law of Thermodynamics does not provide that coherence.

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THE INTERNATIONAL JOURNAL OF FUSION ENERGY

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The International Journal of Fusion Energy is sponsored by the Fusion Energy Foundation for the advancement of theoretical and experimental conceptions necessary for the realization of fusion power. The Journal aims to stimulate investigations of plasma dynamics from the standpoint of fundamental theoretical problems of physics, as well as to promote development of the revolutionary technologies and production techniques that are intrinsic to fusion processes.

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Republican Party Adopts Fusion Plank

The Republican Party convention which met in Kansas City, Mo. in August, adopted a platform that includes a strong fusion energy plank. Fusion Energy Foundation executive director Dr. Morris Levitt was at the convention and his testimony Aug. 10 before the Energy Subcommittee of the Platform Committee generated a high-level discussion of fusion energy and developments among committee members.

The 1976 Republican platform calls for an immediate expansion of coal production to "bridge the gap" between "oil and gas and future nuclear and other energy resources" and identifies the nuclear fission fuel uranium as an "intermediate" energy source. "Among alternative future energy sources," the fusion plank states, "fusion, with its unique potential for supplying unlimited and clean energy, and the promise of new methods of natural resources recovery, warrants continued emphasis within a national energy research program."

During the full platform committee hearings, Senator Hugh Scott (Pa.) tried to substitute the phrase "solar, geothermal, and solid wastes" for "new methods of natural resources recovery," but he was forced to back down after a vigorous defense of fusion from other delegates. One delegate told Mr. Scott, "The intention of this (plank) and the intention of the Energy Subcommittee is to draw particular attention to fusion energy because it is unique and merits particular attention."

Dr. Levitt's testimony and the discussions on the potential for development with fusion power at the Republican convention continued to generate interest especially among Midwest Republicans with an industrial base. The St. Louis Globe Democrat, for example, a paper associated with these layers, ran a lead editorial Aug. 30 entitled "Nuclear Fusion: The Ultimate Fuel?" The editorial concluded by urging that "President Ford and Congress consider a 'maximum effective effort' to develop nuclear fusion power." The Globe Democrat editor who wrote the editorial was familiar with the work of the FEF and drew on material published by the FEF.

Excerpts from Levitt Testimony at GOP Platform Hearings

Reprinted below is a brief excerpt from Dr. Levitt's testimony on fusion power to the Republican Platform Subcommittee on Energy, Conservation, and Natural Resources, Aug. 10. Copies of the full testimony are available from the FEF.

"The current global drought and international food shortage poses the problem of energy and resource utilization policy in its sharpest form. Energy-resource policy is no less importantly related to the issue of the growth of overall industrial output in the advanced sector of the world economy, particularly the United States.

The drought has already affected major grain-producing areas on four continents. California, parts of the northern Midwest, all of Western Europe, much of Australia, and parts of Brazil and Argentina have all been hit. In the case of Western Europe, the drought is the worst in 200 years. Overall crop losses are already estimated at 35 to 50 million tons and rising. Even before the drought, food requirements minimally necessary to prevent starvation and epidemic diseases in the underdeveloped sector were more than 100 millions tons above production. Under the present circumstances, the drought, unless rapidly counteracted, means death by hunger and plagues for millions.

Rectifying this situation — the prerequisite for restoring a healthy and growing world economy and markets — requires industrial inputs to the agricultural sector of millions of tractors, tens of millions of tons of fertilizer, and massive amounts of pumps and piping for irrigation. When translated into primary production categories such as steel and agrochemicals, it is clear that we are talking about increases in the rate of growth of energy production well beyond the recent several per cent per year.

In the face of these needs — which constitute a tremendous potential market for U.S. products — many key sectors of U.S. industry are operating well below full capacity (for example, steel is at 85 per cent). Even further production cutbacks as a result of rising energy prices are underway. The crippling effect of the recent natural gas price increases on the industrial heartland in Ohio (leading to decreases in production of both fertilizer and products important for industry) is typical of a general process.

We can dismiss in short order the arguments of those who say that cutting back on industry is desirable. Zero-growth quacks like Barry Commoner and other energy advisors to the Democratic Party claim that we should cut back on our use of energy and machinery and replace them with labor-intensive methods, thus creating jobs. Such back-to-the-caves incompetence ignores the simple fact that the massive decline in labor productivity per capita production which such policies would entail necessarily means a drastic reduction in consumption. Under present conditions this means starvation and death for millions in the underdeveloped sector. As for those who propose shutting down industry as the solution to pollution, as appears to be the policy of the Environmental Protection Administration, this is approaching the problem backwards. In fact, with sufficient supplies of cheap energy, the problem of cleaning up pollutants by breaking them down and recycling them will be easily solved. The current pollution problem generally derives from too little energy availability, not too much.

This brings us directly to the question of a truly alternative energy and resource program. There is hardly anyone who would disagree with the proposition that if thermonuclear fusion reactors and fusion-based technologies were around the corner, the nation's most pressing energy, resource and environmental problems will be solved. As is well known, the amount of energy available from fusion

— the reaction that powers the sun — is essentially unlimited, and, in principle, there is no lower limit to reduction of cost or pollution, including radioactivity. Fusion additionally provides potential energy forms and plasma reactions that can revolutionize industrial and chemical processing, as well as the extraction of pure substances from ores and recycled materials.

As a matter of practical policy, if we can safely project the rapid onset of technologically and economically viable fusion reactors by the end of the 1980s, we can today safely exploit our existing energy and other raw material resources to the hilt.

In reality, fusion is feasible as an operating energy source within the next decade, if the proper development policy is applied. On this basis, the only competent energy policy is one of fully utilizing **now** the cheapest available energy sources — conventional oil, gas and coal, and pushing as rapidly as possible for fusion. But the current energy program — the program of the Energy Research and Development Administration — is not based on that premise...or, in fact, any rigorous premise at all.

The Issue Is Progress: Fusion and the 1976 Campaign

What the U.S. Labor Party Says

"Provided we make an effective commitment to 'brute force' development of fusion technology (1982-85 target dates for initial operating facilities), the human race confronts no meaningful categorical limits to availability of either energy resources or any essential raw material....The proposals of solar energy, etc. as alternatives...are charlatanry....Only fusion development can assure continued human existence beyond this century....The cultural level of most of the world's labor is too poor to permit those persons to even approximate a U.S. standard of skilled productivity....It must be our policy to eliminate that discrepancy through development."

— 1976 USLP Presidential Platform, "How the International Development Bank Will Work," *first published May 1975.*

What the Institute for Policy Studies Says

"With the limits of growth clear in sight, with less than 40 years supply of oil and gas left in the U.S., these mineral resources must...be subjected to rigorous public control, so their use can be regulated and their consumption curtailed....Local boards would be given funds for expenditure in their geographic area on such alternatives as solar energy....Alternative modes of transport must be designed, including...bicycles."

— 1976 New Democratic Coalition Platform written by Institute for Policy Studies chief Marcus Raskin

What the Republican Party Says

"Among alternative future energy sources, fusion, with its unique potential for supplying unlimited and clean energy, and the promise of new methods of natural resource recovery, warrants continued emphasis within a national energy research program....We recognize that only when our technology is fully distributed can it be assimilated and used to increase our productivity and our standard of living....We will encourage our young Americans to study science and technology...."

— Republican Party platform adopted Aug. 18, 1976 at Kansas City

What Jimmy Carter Says

"The United States must shift from oil to coal, taking care about the environmental problems involved in coal production and use....U.S. dependence on nuclear power should be kept to the minimum...."

— speech at the United Nations, May 13, 1976

"Solar energy...is certainly a new kind of energy waiting to be used in this country. Small amounts of...funds would...provide large numbers of jobs...."

— speech at presidential forum, Boston, Feb. 23, 1976

IN THE NEXT ISSUE

The State of Fusion Research

The Witchhunt Against Fusion — The attempt of ERDA and other government agencies to harass and intimidate scientists working with the Fusion Energy Foundation and to stop the spread of the FEF's ideas.

Soviet Union to Build Test Reactors by 1980—The Soviets have scrapped plans for a T-20 Tokamak and are proceeding directly to the construction of a T-10 M Tokamak which will produce plasma conditions equivalent to those in a full-scale power-producing fusion reactor by 1980.

Soviets Offer Cooperation to the United States for Fusion in the 1980s — Dr. Leonid Rudakov from the Kurchatov Institute in Moscow proposed to close the "fusion gap" by combining U.S. technology with the Soviet's recently developed electron beam pellet fusion technology to produce a prototype fusion reactor.

U.S. Acts to Scrap Laser Fusion Research — An ERDA scientist announces that the agency is moving to scuttle laser pellet fusion research because it is producing anomalous effects which appear to preclude the application of laser beam-driven pellet compression to the simulation of hydrogen bombs.

Fusion Energy Foundation

Vortex Filaments from the current sheath in the plasma discharge from a Theta Pinch Machine. Photo courtesy of Dr. Winston Bostick.

The Fusion Energy Foundation was founded in November 1974 at a meeting attended by representatives of the U.S. Labor Party, the United Nations and the International Atomic Energy Agency, scientists who have made significant contributions to fusion research, and interested laymen.

The purpose of the FEF is to provide a forum of independent, high-level scientific discussion of fusion from the standpoint of comprehensive policy making.

The FEF publishes a bimonthly newsletter summarizing and analyzing all major developments in the fusion field and is cosponsoring, with the Baywood Publishing Company, the International Journal of Fusion Energy, which will stimulate and synthesize conceptual advances in the fusion-plasma field.

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