Entropy and Nonsense

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A Review of Daniel R. Brooks and E. O. Wiley, *Evolution as Entropy*, University of Chicago Press, 1986. xiv + 335 pp., $25.00.

At the beginning of the Preface, the authors of *Evolution as Entropy* point out that first and foremost evolution is a *process*. With this in mind the title can be restated as "a process as an extensive thermodynamic state variable." It has the same grammatical status as *Evolution as Volume* or *Evolution as Mass*. I begin with this linguistic nitpicking because it is important to realize that this book uses imprecise meanings and poor writing to cover up the fundamental nonsense and emptiness of the underlying ideas. The only reason for reviewing such a work is that a number of biologists untrained in thermal physics and information science have been fooled into believing that there is some content in the "Unified Theory" of Brooks and Wiley, and it is important to release these biologists from being mesmerized by the language and equations of physics that are used to support the core hypothesis: "biological evolution is an entropic process." Since all changes involve global entropy increases, the core hypothesis is hardly anything to get excited about.

Since C. E. Shannon introduced the information measure in 1948 and showed a formal analogy between the information measure ($-\Sigma p_i \ln_2 p_i$) and the entropy measure of statistical mechanics ($-k\Sigma f_i \ln f_i$), a number of works have appeared trying to relate "entropy" to all sorts of academic disciplines. Many of these theories involve profound confusion about the underlying thermal physics and their authors use the language and formulae of the physical sciences to bolster otherwise trivial and vacuous theories. The book by Brooks and Wiley is of that genre. This is seen on page 5 in a restatement of their theme.

> The major thesis we will present is that evolution is an entropic phenomenon, but because the levels of organization to which the second law applies in a causative manner in biology are levels that are absent in atomistic physical systems, biology cannot be reduced to atomistic physics.

I have pondered that sentence trying to make sense of it. Since every possible state of the system is ultimately an atomic configuration, what

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meaning can we give to this array of words? Also, "levels of organization to which the second law applies in a causative manner in biology" is a noun phrase which lacks a referent. It is obvious that large scale biology cannot be reduced to atomic physics since random events at the molecular level, i.e. mutations, can have effects on organisms and ultimately on ecosystems. While there may be laws at higher levels of organization, these follow from properties of molecules, organelles, cells, organs, organ systems and populations. To assume that such laws arise from some underlying thermodynamic principle again reflects a deep misunderstanding of thermal physics.

Nowhere do the laws of thermodynamics generate structures. Structures come from mechanics, kinetics, quantum mechanics, hydrodynamics, etc.; thermodynamics provides limitations on structures which arise due to the forces of nature. The authors may have been confused by the work of the Prigogine school on symmetry breaking structures, but in all of these cases the structures arise from the kinetic equations of hydrodynamics and chemical kinetics rather than from thermodynamic considerations. Near to equilibrium thermodynamics provides domains within which kinetic considerations apply, but the structures come from the dynamic equations not the thermodynamic constraints. The optimistic claims of the Prigogine school about what they have in fact demonstrated are perhaps being picked up and misused by those who lack the symbol manipulation ability and depth of physics of Prigogine and his associates. All this filters down to the experimental biologist, who, unschooled to follow the subtle physical and mathematical arguments, assumes that insight is being given into his discipline. The moral has once been stated as follows (I'm afraid I forget the source): "Physics envy is the curse of biology." The further moral is that scientists should not uncritically accept statements from other disciplines that they are unable to understand and justify.

Chapter 2 is called 'Why Entropy?' (why, indeed) and consists of an oleo of confused writing and thermodynamic misconceptions. We present two quotations from pages 26 and 27 with annotations.

> Text 1. *Hydrogen gas in a bottle (type 1).* Consider some hydrogen gas injected into a bottle in compressed form. The gas molecules will expand and disperse to fill the bottle, releasing measurable amounts of energy as they do so. When the limits of dispersion (the walls of the jar) are reached, the gas no longer expands and no more net energy is released.

Comment 1: First note that molecules do not expand; the expansion is due to an increase in the mean distance between the molecules. Next I would like to present the same experiment as discussed in a standard classical physics text (L. Page, *Introduction to Theoretical Physics*, D. Van Nostrand, 1958).

Joule's Free Expansion Experiment — This experiment, performed by Joule in 1845, consists in allowing gas to escape from a vessel *A* into an evacuated vessel *B*. After the gas in the two connected vessels has come into equilibrium as regards both pressure and temperature, the temperature is measured and compared with the original temperature of the gas confined in *A*. As no external work is done and no heat is added, the intrinsic energy of the gas remains unchanged. Joule was unable to detect any change in temperature, and therefore concluded that the intrinsic energy of a gas is a function of its temperature only.

The difference between the two treatments is a measure of Brooks and Wiley's failure to understand the underlying physics.

Text 2. The transition toward equilibrium for type 1 systems is often given as

$$\Delta F = \Delta E - T\Delta S$$

where F is the free energy, or that released by the change in the system, E is the bound energy, or that which can be given up by the system. T is the absolute temperature, and S is the entropy, an abstract term relating to the macroscopic order or disorder of the system. . . . At equilibrium, $F = 0$ and no more work can be done.

Comment 2. Since E is not defined as one of the thermodynamic state functions, it is difficult to determine what free energy function is under discussion. In any case equilibria are described by free energy minima not by zero values of free energy. Since energy zero values are not defined, absolute values of free energy functions are not meaningful.

Every page of the thermodynamic section of the Brooks and Wiley book bristles with errors of the foregoing type. Attempting to read the book as serious physics is a surrealistic experience. The authors then proceed to a discussion of information and entropy in disregard of the following two concepts:

1. The entropy measure is only precise for equilibrium systems. A related measure exists for near to equilibrium systems. For far from equilibrium systems such as living organisms the entropy measure lacks meaning.

2. "Any contribution of the patterning to the thermodynamic functions is likely to be negligible until the pattern is constructed with molecular fineness" (P. Morrison, *Reviews of Modern Physics* **36** (1964), 517—524). Morrison goes on to show that visible patterning makes a contribution 10^{-18} of that of molecular patterning in any free energy function.

Thus the rest of the book tries to relate incommensurable quantities under conditions in which one of them is not defined. To discuss such theory further seems quite pointless.

There would be little purpose in this intemperate review were it not for the fact that theoretical biology is both difficult and important. To confound the discipline with this kind of meaningless patois masquerading as profundity is unconscionable. It is a serious matter that the time and efforts of large numbers get wasted in a ritual exercise devoid of content. The University of Chicago Press should be ashamed of themselves.

On June 17, 1910 William James wrote a letter to Henry Adams including the following:

> The "Second law" is wholly irrelevant to "history" — save that it sets a terminus — for history is the course of things before that terminus, and all that the second law says is that, whatever the history, it must invest itself between that initial maximum and that terminal minimum of difference in energy-level. As the great irrigation-reservoir empties itself, the whole question for us is that of the distribution of its effects of *which* rills to guide it into; and the size of the rills has nothing to do with their significance. Human cerebration is the most important rill we know of, and both the "capacity" and the "intensity" factor thereof may be treated as infinitesimal. Yet the filling of such rills would be cheaply bought by the waste of whole sums spent in getting a little of the down-flowing torrent to enter them. Just so for human institutions — their value has in strict theory nothing whatever to do with their energy-budget — being wholly a question of the form the energy flows through.

If Brooks and Wiley understood today what William James knew in 1910 this whole to-do would be quite unnecessary.