COMMITTEE I
Unity of Science: Organization and
Change in Complex Systems

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EVOLUTION: A TANGLED NOTION

by

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DISCUSSION PAPER

on

Robert U. Ayres's

SELF-ORGANIZATION AND TECHNOLOGICAL CHANGE IN THE ECONOMIC SYSTEM

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1. The topic of Professor Ayres's paper pertains to one of the most baffling issues, perhaps the most baffling of all. Is evolution a real phenomenon? And if it is, what are its sustaining factors? Numerous ideas have been set forth about this problem ever since the "obscure" Heraclitus, but the most salient of them belong to the past fifty years or so.

The task of Professor Ayres was truly arduous, all the more so since economic life transgresses not only the inorganic but also the organic domain. Man is not just a biological creature seeking only to feed, defend, and perpetuate himself. True, man's life, like that of any living creature, is entropic in all its material fibers: man, too, "sucks"--as Erwin Schrödinger explained--low entropy from the environment and discharges high entropy into it. But without taking into account man's specific faculties that have given rise to the unique form of human culture, we are not in the true economic domain. Man is not just a Carnot cycle. What is missing from Professor Ayres's paper is any consideration of man as an agent, especially as an economic agent. For a human is not just a price-taker, but as Adam Smith repeatedly noted, every individual continuously seeks to better his own economic position, a struggle that is responsible for most articulations of mankind's social evolution. As a social animal man is a complex--in the words of Thorstein Veblen--of "instinctive proclivities and tropismatic aptitudes" that is spurred by instincts, such as that of workmanship and idle curiosity. The specific human culture is dominated by Veblenian institutions--prevalent "ways of acting, feeling, and thinking," as A. L. Kroeber later explained again. By now there is hardly any doubt that a strong interplay exists between the institutional aspects and the mode by which man satisfies his wants by tapping and transforming environmental resources.

One would therefore have expected Professor Ayres to devote some attention to this interplay. Instead, he has spun his presentation only from a purely thermodynamic viewpoint. This epistemological temper--it is true--has become the fashion ever since ideas from several directions converged on the entropic nature of the biological processes. As a result, the main points--historical and analytical--are by now familiar knowledge not only to those who have a special interest in this crossroad of biology, thermodynamics, and the so-called information theory. For this reason the general review in the first three sections of Professor Ayres's paper does not seem to serve the substance of the argument announced by the title. Be this as it may, that review contains a few inadvertences that deserve to be pointed out.

A word should first be said about the substitution of "essergy" for "energy," even for "food." The terminological innovation is far from being innocuous. For "exergy," which is the preferred form, has been introduced by several European scholars to denote the amount of available energy supplied by a thermodynamic system in relation to its specific environment. But the element measured by exergy is just energy, as is the case for other thermodynamic measures say, enthalpy and Helmholtz's or Gibbs's free energy.[1]

2. There was indeed, as Professor Ayres observed a scientific crisis that grew out of Rudolf Clausius's studies. But that crisis (which is still unresolved and which is, in my judgment, deeper even than that caused by Einstein's relativity of time) did not turn upon the difference between the concepts of reversible processes (moving

at an infinitesimally slow pace) and the irreversible ones (with a finite speed). It turned upon the antinomy between the behavior of heat and the laws of Newtonian mechanics. We should not forget that Clausius's first (and still the most transparent) formulation of the second law is that "Heat always passes by itself from the warmer to the colder body, never in reverse." More snags appeared as one solution after another claimed to reduce that famous antinomy. But on close examination even the most sophisticated ones boil down to a defty petitio principii.

More subject to objection is the definition of thermodynamic equilibrium as "a changeless state in which all matter is uniformly distributed and there are no temperature or concentration differences" (italics mine). This definition describes a Chaos, not a "Heat Death." If a brick, for example, has the same temperature as the water into which it is immersed, the whole ensemble is a case of heat death, but not of chaos.

Thermodynamics has never been concerned with the dissipation of matter. The famous contribution of Ilya Prigogine and his school did extend the domain of classical thermodynamics (confined to closed systems) to open systems as well. But we shouldn't fail to realize that in Prigogine's framework matter plays a restricted role--not as matter per se, but only as carrier of energy. To wit, the classical formula involving only transfer of heat and work,

(1) $$dE = dQ - pdV,$$

was replaced by

(2) $$dE = d\Phi - pdV,$$

where $d\Phi$ represents the flow of energy "due to heat transfer and exchange of matter" (Prigogine, 1961, p. 11).[2] But as strange as it may seem, neither the Bruxelles school nor any other thermodynamicist asked what happens to matter when it serves as an effective support of a material process--say, the pistons and the cylinders in a motor or the graphite of a writing pencil. Friction, which is the main factor that degrades matter entropically, is mentioned in the thermodynamic literature solely as the cause of loss of available energy, very likely because friction is a most baffling phenomenon. Yet matter matters too.[3] We would greatly err to ignore this truth in seeking a conformable representation of material processes.

3. It was apposite for Professor Ayres to mention in his review the connection between entropy and "order:" most everybody nowadays defines entropy as the maximum of disorder. But his brief notice about order leaves the impression that, after all is said and done, the concept is not plagued by the immense difficulties exposed by renowned scholars--by Percy Bridgman (a physicist), Henri Bergson (a philosopher), Jacques Hadamard (a mathematician), as well as by Schrödinger. The familiar proposition that the disorder of a macrostate is proportional to the number of its possible microstates is a pure dictionary definition, not an elucidating one. The same is true of McGlashan's proposal endorsed by Professor Ayres. We speak quite sensibly of order and disorder, but only with ad hoc meanings associated with some purpose. A stack of book invoices may be in perfect order for the bookkeeper but in great disorder for the cataloguer. A highly edifying proof of the unsubstantiality of the concept of order occurred at a 1981 international colloquium on the bearings of thermodynamics as Jacques

Tonnelat displayed three pictures to illustrate the change from order to disorder. Almost everyone in the rather initiated audience thought that the gradient of order was the opposite of that intended by Tonnelat.[4]

4. For the description of thermodynamics vis-a-vis biology, Professor Ayres follows the view that permeates most of the accretive literature on the issue. Which is to take many things for granted, as is the case of the concepts of complexity, structure, organism and organization. Here perhaps one should not pass over F. A. Hayek's enlightening criticism of these concepts which are generally used without any ado.

Professor Ayres is right in recalling that there is no "evidence of any absolute contradiction" between thermodynamics and the life of an organism. (This, not the biological evolution, is the proper opposite.) But nor has anyone been able to show, except by modeling with pencil on paper, how thermodynamics may actually account for the numerous puzzling biological phenomena. It is not an accidental trivia that illustrious physicists have expressed definite reservations on the equivalence of an organism with a thermodynamic system. Lord Kelvin, one of the architects of thermodynamics, enunciated (in 1851) the second law for inanimate systems alone, and forty years later (1892) still clung to that restriction. About the same time Hermann von Helmholtz, who made memorable contributions to medicine, biology, psychology, and theory of music, and who set the first law on a solid foundation, also thought that "the delicate structures of the organic living tissues" could elude the second law. A fantastic experiment related by G. N. Lewis reveals as no other that I know the relevance of life in actual processes: some aimlessly running mice crowded in a corner of a box hinged down on its center ultimately disperse themselves so that the box be kept in horizontal equilibrium.[5] Undoubtedly, the same could not be brought about if the rats were replaced even by some billiard balls.

Of course, there was Clerk Maxwell with his demon.[6] The voluminous literature on Maxwell's fable is commensurate only with its great popularity. Wide-spread also is the belief that the paradox of the possibility of converting high into low entropy in an isolated system has been disposed of by Leo Szilard.[7] But now that an English translation of Szilard's paper is available, also the Anglophones who do not read German may see for themselves that Szilard's argument is based on "an inanimate device" and that his conclusion is that <u>such</u> a device would create an amount of low entropy just equal to that used up in observing and measuring. By any kind of logic, therefore, Szilard did not "exorcize" (as the claim goes) the demon.

But a paradox cannot be shattered by invoking the very principle(s) challenged by it, as Szilard did in his proof. Bertrand Russell, for instance, replaced the old principles of classes by a new theory of types so as to do away with the paradox of the class of all classes. It is indeed easier, as Norbert Wiener observed, to repel the puzzle as a paradox than to answer it. Suggestions to dispose of Maxwell's paradox have not been lacking. Georg Hirth set forth the idea taken over by F. Auerbach (1910) that in nature there is also an <u>ektropy</u> tendency opposed to entropy. Even a physicist of Sir Arthur Eddington's caliber argued that besides randomness, on which the modern theory of thermodynamics rests, an opposite factor, the anti-chance, must be at work in nature. Something thus must be added to physicochemical laws--as Werner Heisenberg argued--in order to account for the phenomena peculiar to life. There are some renowned physicists, however, such as E. Wigner, who do not share this belief. But as Manfred Eigen pointed out in his admirable essay, Wigner took for granted that any known organism can be assembled by a random process. Eigen (on whom Professor Ayres markedly relies) disagreed on this and pointed out that proteins, in particular, cannot reproduce themselves. Whether a new physics may explain directly how the intelligent human evolved from the single cell is still a typically moot question.

5. It is because in his general discussion of life phenomena Professor Ayres seems to accept the numerous formal equivalences about those phenomena as explanations valid on the workbench that I deemed it necessary to insert the ideas of the foregoing sections.

Eigen's basic diagram (p. 466), for example, serves only as a simile of the input-output in a self-reproducing cell, but it is not an explanation of how things go exactly where they have to be. The simulation presented by Zeleny and Pierre by a sequence of twelve diagrams also is only a simile of a particular feature.

Not to forget, an old theorem of John von Neumann stated that some Turing universal machine could reproduce itself if left floating in a medium together with plenty of its elementary parts. On paper, such a machine may be a simile of a protein, but only if we would have the protein to start with (the troublesome issue that arrested Eigen's attention). And interestingly a negation of Neumann's theorem is H. von Foerster's equally formal proof that "There are no such things as self-organizing systems!"

6. As is the fashion now for writers about complex structures, Professor Ayres mentions the results concerning the nonequilibrium structures obtained by the Bruxelles school. The crux of that endeavor, the concept of dissipative structure which is a landmark in modern science thought up by Prigogine (1967, chap. viii), who described it only by verbal, yet illuminating, considerations. Moreover, Prigogine was not then as self-assured of the general bearings of that concept as he became later. On the basis of a few processes--the transition of a liquid to solid, a hydrodynamic convection, some chemical reactions proposed by A. M. Turing, for example--Prigogine concluded that "Dissipative processes <u>may</u> therefore also lead to an increase of organization."[8] There, we are also told that through dissipative processes homogeneous turns into heterogeneous, an idea which in his subsequent writings Prigogine elevated to a universal law of nature.[9] The paradoxical face value of this tenet enchanted legions of writers who, following the very suggestions of Prigogine," snatched [it up] to justify all manners of social, psychological, [and] political" phenomena (<u>New York Times</u>, 2 June 1979).

What led to this quardary is certainly the improper use of "homogeneous," a fact manifest from Prigogine's most transparent illustration of the tenet (1980, p. 89), the process by which a highly organized pattern of convection cells forms in heated water. However, the initial situation being composed of a heat bath and some unheated water is not a <u>homogeneous</u> structure. An initial homogeneous structure would contradict Eigen's reservation mentioned earlier. Nothing has yet occurred to prove that from a true homogeneous structure a heterogeneous one could develop. The origin of the Universe was not Chaos, but Chaos will very probably be its fate.

7. I find myself in even sharper disagreement with Professor Ayres on his use of "information" and "negentropy." True, the so-called theory of information is in a muddle for the simple reason that information has been defined only implicitly, that is, as a vocable for negative (low) entropy (Georgescu-Roegen, 1976). The meaning of the earliest idea of G. N. Lewis (1930) that "gain in entropy always means loss of information" was clear: the player no longer knows where a card is in a shuffled deck. But Lewis's addition "and nothing else" set the stage for some otiose developments. For, if entropy is nothing but a mental event, then an engineer, for instance, would have to say that the entropy of an engine is just his degree of ignorance.

Claude Shannon was in order to define "information" as the measure of the capacity of a code to represent different messages, whether meaningful or not. That the formula for this measure coincides with that of Boltzmann's for entropy is a pure accident. So, to say that the Boltzmann-Shannon formula measures the information of, say, an income distribution or the structure of a cell, is a vacuous statement as long as "information" is defined only implicitly by Leon Brillouin's equation

(3) Information = Negentropy.

Brillouin did not supply a "famous proof of [this] equivalence," as Professor Ayres asserts;[10] he only shows the <u>algebraic</u> identity of Boltzmann's and Shannon's formulae.

However, often in his writings Brillouin replaced "information" by "knowledge," as he rightly argued that any gain of knowledge is necessarily acquired by an increase in entropy (a loss of negentropy) of the total system. This shift led to even greater exercises of verbalism, as is seen in Professor Ayres's paper. There, we find "stocks of information" (as a telephone directory?) regarded as "storehouses of negative entropy" (coal mines?). Moreover, "the ability of a dissipative system to capture (or embody) negentropy in structure" is intelligence; even an amoeba is thus intelligent. Yet a few lines further, embodied information is equated with "knowledge" and "natural" information with essergy.

8. Concerning the discussion of technological progress in relation to the growing scarcity of fossil fuels and even of uranium ores, Professor Ayres is confident that some of the familiar "alternative resources" will become operative. Whether or not this will happen is a matter of ball-gazing. What is certain, but we ignore, is that the sine qua non condition of any viable exchange between mankind and the material environment must generate a surplus as well. There have been only three such processes: agriculture, the mastery of fire, and the transformation of thermal energy (heat) into work by the heat engine. The hoped-for one must also be capable of the same.

9. Turning to the sections on economics, I must first point out two inexactitudes. Léon Walras's argument that his system has a solution because its number of equations is equal to that of the unknowns was of no avail. The proof of the existence of a solution under very restricted conditions was given by K. J. Arrow and G. Debreu only in 1951. Also, Vilfredo Pareto did not prove that a market economy "maximizes total welfare." The market economy only tends to a Pareto optimum,

a situation such that nobody may become better off without someone else becoming worse off.[11]

Finally, there is the theory of economic development of Joseph A. Schumpeter, which far from being a "simple conceptual model" as Professor Ayres claims, represents a vision not only unique in the history of economic thought but a novel understanding of the nature of evolution. Schumpeter's basic idea was that evolution is the result of qualitative novelties, which in economics have their roots in the continuous product of our minds: inventions. These, in turn, led to economic innovations, which, according to Schumpeter are not limited to the technological domain. We owe to Schumpeter the essential (albeit nowadays overlooked) distinction between growth (mere accretion) and development (in economics or in biology). His splendid aphorism "Add successively as many mail coaches as you please, you will never get a railway thereby," tells a lot about what evolution means.

Professor Ayres fails to mention these ideas as well as another, still more decisive thought. An important point in Schumpeter's system is that only great (substantial) innovations affect the economic conditions. And he took great pains to explain the difference between small and great innovations in a way that was certainly dialectic and which he also adopted in clarifying the quality of entrepreneur.

The reason why I consider Schumpeter's theory as the highest mark of economic thought is that his idea about evolution was independently thought up by a renowned biologist, R. Goldschmidt (1940), some thirty years later. Against the prevailing Neo-Darwinian view that speciation results from the accumulation of small, imperceptible modifications, Goldschmidt maintained that species derive from the emergence of "successful monsters." By analogy, a railway engine is a successful monster in comparison with a mail coach.

To gauge the depth of Schumpeter's vision we should note that the explanation of speciation by successful monsters has recently been revived by one of the greatest minds in biology, Stephen Jay Gould (1977). Interestingly, Gould (1980), too, recognizes the strict relation of dialectics with his new interpretation of speciation.

Professor Ayres is the author of many valuable contributions, in particular, he co-authored a volume (1970) that dealt with resources at a time when their problem was side-stepped. Therefore, I feel ill at ease to have found so many ideas in his paper with which I could not agree.

A

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Notes

[1]In the usual literature the concept of exergy is found under the transparent term "available energy," an original notion used by both Lord Kelvin and Walter Nernst in preference to entropy. For the more recent formula of availability, see K. Denbigh, *The Principles of Chemical Equilibrium*, Cambridge University Press, 1973). But for this concept, hence for exergy as well, there are two distinct formulae, one for closed, another for open systems (W. H. Giedt, *Thermophysics*, New York: Van Nostrand, 1971).

[2]A. Katchalsky and Peter F. Curran, *Nonequilibrium Thermodynamics in Biophysics*, Harvard University Press, 1965, are more precise: $d\Phi$ represents "all the energy transported into the [open system] including that resulting from the transfer of matter."

[3]To argue on the basis of Einsteinian equivalence between energy and mass, $E = mc^2$, that matter is a form of energy would not do. It would negate the difference between closed and open systems, and, implicitly, would deny all importance to Prigogine's contribution. But that argument also overlooks the important fact that thermodynamics is a science of macrosystems, of matter-energy in bulk.

[4]The curious may look up the 1982 special issue of *Entropie*, pp. 68 and 72.

[5]In fact it was Johann Loschmidt who imagined that demon long before Maxwell.

[6]"The Symmetry of Time in Physics," *Science*, June 6, 1930. Curiously though, by that article Lewis purported to refute the idea that life displays some special features.

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[7]Professor Ayres is inexact in attributing the introduction of "negentropy" to Szilard. Schrödinger introduced the troublesome term "negative entropy" in 1944, but it was L. Brillouin who coined "negentropy (abbreviation of negative entropy)" in 1950. Another inaccuracy is note 8. It was in Egypt that astronomy served to predict the flooding of the Nile in time for the agricultural works, not in Messopotamia whose slow rivers calmly descended only thirty feet over a length of 500 miles (Ronan, pp. 18, 31).

[8]The verb italicized by me appears almost regularly in that particular argument. And even later, Prigogine (1976) relies on some illustrations to explain the "concepts of dissipative structure and order through fluctuations."

[9]Prigogine (1980) attributed the tenet to Herbert Spencer. Indeed, Spencer spelled it out in 1847 and traced it back to two earlier embryologists, K. F. Wolff and E. K. von Baer, who refuted the old belief that the embryo contained the whole organism in miniature.

[10]Professor Ayres's reference to Brillouin (1953) is not exact.

[11]This concept is rather tricky so that even Paul Samuelson in the eleventh edition of his Economics (p. 435) slipped over it.